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PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *On the Use of the Sphygmograph in the Investigation of Disease.* By Balthazar W. Foster, M.D., &c. London: Churchill and Sons. 1866. Pp. 42.
2. *On the Application of the Graphical Method to the Study of Diseases of the Heart and Great Vessels.* By Balthazar W. Foster, M.D., &c. 'Medical Times and Gazette,' vol. ii, 1866; vol. i, 1867.
3. *On the Application of Physical Methods to the Exploration of the Movements of the Heart and Pulse in Disease.* By T. Burdon Sanderson, M.D., and Francis E. Anstie, M.D. 'Lancet,' vol. ii, 1866, and vol. i, 1867.
4. *Sphygmographic Observations on the Pulse of Typhus.* By Thomas Wrigley Grimshaw, A.B., M.B. 'Dublin Quarterly Journal of Medical Science,' February, 1867.
5. *Note on the Regulation of the Pressure on the Artery in the Application of the Sphygmograph.* By Balthazar W. Foster, M.D. 'British and Foreign Medico-Chirurgical Review,' July, 1867.
6. *Lectures on the Prognosis and Treatment of Certain Acute Diseases, with special reference to the indications afforded by the graphic study of the Pulse.* Delivered at the Royal College of Physicians of London. By Francis E. Anstie, M.D., &c. 'Lancet,' vol. ii, 1867.

7. *Handbook of the Sphygmograph: being a Guide to its Use in Clinical Research. To which is appended a Lecture delivered at the Royal College of Physicians on the 29th March,* 1867, *on the Mode and Duration of the Contraction of the Heart in Health and Disease.* By J. BURDON SANDERSON, M.D., &c. London: Hardwicke. 1867. Pp. 83.

8. *On a New Method of Increasing the Pressure on the Artery in the Use of the Sphygmograph.* By BALTHAZAR W. FOSTER, M.D., &c. 'The Journal of Anatomy and Physiology,' November, 1867.

IN glancing at the past history of medicine, the observer cannot fail to be struck with the extremely slow advance made during the earlier stages of its scientific growth. Science after science, born later, has reached a comparative maturity more quickly, and this, too, in spite of the fact that medicine has ever had its special votaries. So slow has been the progress that every now and then some impatient spirit asserts that medicine can never attain a scientific form. A little careful reflection leads to an explanation of this tardy evolution; for through the whole range of human knowledge there runs one invariable law of growth which determines the advance from the more simple to the more complex. Hence we see that those sciences which deal with the succession and relation of the more simple phænomena of nature, have been the first to assume a scientific form; for the method required to analyse their less complicated problems has been more easily acquired. The relatively high degree of perfection which astronomy has attained, and the rapid growth of geology, notwithstanding the late period at which its development began, seem almost discouraging to the investigator of biological phenomena when contrasted with the slow advance of his own study. A little closer scrutiny, however, reminds us that the phænomena with which such sciences as astronomy and geology are occupied are much less complicated with phenomena of a different kind than those of biology.

Observation alone has sufficed to penetrate into their more simple secrets; hence such sciences have been well called sciences of observation. On the contrary, when we come to consider medicine in relation to the other sciences, we cannot fail to mark the increased complexity of the questions with which it has to deal, questions requiring for their solution not only a knowledge of the laws which rule the phænomena of healthy life, but also those which regulate diseased action. Medicine in its true sense is a triad of sciences, of which two parts—physiology and pathology—are only partially developed, and the third—therapeutics—can only approach a scientific form, when the two former are more matured. Yet the three component parts exist together, each reacting upon the other, the more complex often aiding the development of the more simple. But in addition to the complexity of its phænomena, a complexity so great as to require all the other sciences to be impressed as auxiliaries, we can find another explanation of slow progress in the inefficiency of the method formerly applied. Medicine when first cultivated could only be approached by the method of observation, which, while it achieved vast conquests in the region of the simpler sciences, advanced but little the evolution of medicine.

Bernard has well shown that medicine could never arrive at any high development as a science of observation, for such sciences in their maturity lead simply to a knowledge of the invariable laws which regulate the succession and relation of phænomena, but give no power of modifying this relation and succession at will. Medicine entered on this stage of its growth long since, and has given the physician a certain amount of prevision with regard to disease, but little power of modifying its course. The empiricists of old were ever struggling against this tendency of medicine to become a mere science of observation, and in their rude way were pointing out what a later age has taught us, that only by employing experiment in addition to observation can medicine attain its highest development. To the recognition of the value of experiment in physiological research do we owe the great discoveries of modern times, and in chemistry we see the most brilliant results of the experimental method. Experiment is, however, only an extension of observation, by means of which the investigator repeatedly studies phænomena under known conditions, in order that he may render his analysis of their relation and succession more perfect. The observer studies phænomena which he cannot control in order to discover the laws which they obey. The experimenter modifies at will the conditions under which phænomena exist, and thus gains a deeper insight into their nature. In both cases it is of the greatest importance to render more precise the investigator's power of recognising the finer shades of resemblance and difference between phænomena, for in proportion to this precision will be the value of his results. Every means, then, which renders our observation of phænomena more exact gives an impulse to science. So intimate is this relation between instrumental aid and progress, that the position of a science might almost be inferred from the extent of its apparatus. How great a part the telescope has played in the advance of astronomy, how rapid has been the growth of our knowledge of the structure of the tissues since the invention of the microscope, must be familiarly known to every reader. More recently the invention of the spectroscope has given to chemistry a new vitality, and directed the energy of observers into fresh channels. Indeed, throughout the history of physics and chemistry the great landmarks of progress correspond to the invention of new instrumental contrivances to supplement the powers of the investigator.

Physiology bears witness also in almost every step of its recent advance to the great utility of improved instrumental aid. We may especially refer on this point to the recent work of Marey, 'Du mouvement dans les fonctions de la vie,' which tells us in every page how the accuracy of the experiments of the physiologist has been increased by mechanical contrivances. Medicine has not been ignorant of the benefits derived from this striking feature of modern science, for its most brilliant achievements have been made by the increased power of accurate observation which the microscope has given to the pathologist, the stethoscope to the physician. The laryngoscope, the endoscope, and the ophthalmoscope, have also added to our knowledge, and the last promises to quicken to a still greater degree than it has hitherto done, our insight into the morbid conditions of the deeper nervous structures. The thermometer has given a precision to our estimate of the essential phænomena of fever, and thus increased the accuracy of our diagnosis and our certainty of prognosis. To enumerate all the mechanical means which have advanced modern science would occupy too much of our space; we have already said enough to show how great has been their influence for good in our own special branch of study. Every fresh invention has given us, as it were, a new sense wherewith to attack more successfully the great problems which have hitherto defied our analysis.

The instrument which forms the subject of the present article has in this way contributed largely to our knowledge of the phænomena of the circulation. The finer features of arterial pulsation which escaped recognition by our mere sense of touch have been elucidated by its aid. What appeared to our fingers as a single or occasionally a double beat has been broken up into its component parts, and these parts have been gradually recognised in their true relation to other and antecedent phænomena. But, as we shall see, this development of physiological knowledge has stimulated our observation of disease, and given an accuracy to our power of distinguishing various forms of pulse never before generally attained. Once or twice in a century the tactus eruditus of a great master gave him a wonderful power in discriminating the various peculiarities of pulse movement, but the imperfection of the knowledge was shown by the impossibility of communicating it. The sensation perceived was too vague to be conveyed to the mind of another, and thus the exceptional knowledge was of little use to science. But now the sphygmograph enables the student to refer his sensation of a pulse-beat to a visible representation of the pulse movement, and thus promises to dispel that vagueness in the description of the same phænomena which is caused by the varying delicacy of the sense of touch.

Students need no longer crowd round the bedside, anxious but unable to perceive those finer features of the pulse to which their teacher refers, for in future the tracing of the pulsation will tell them, in unmistakeable language, the delicate shades of movement which scarcely any finger can appreciate, and still fewer physicians describe. That the invention of the sphygmograph has already led to a much more exact knowledge of the circulation, none will dispute; and, indeed, there can be little doubt that it is also destined to render great services in the hands of the physician. The few industrious investigators who have applied it to purposes of clinical research in this country have shown that it is capable of extended usefulness; and in the following pages we propose to consider the results which their publications placed at the head of this article contain. In so doing we shall first present our readers with a brief exposition of the recent advances of our knowledge of the healthy pulse curve, and then, after alluding to the suggested modifications in the form and application of the instrument, we shall proceed to discuss its importance as an aid in the study of disease.

Marey, in his admirable work, 'Physiologie Médicale de la Circulation du Sang,' did not notice some of the peculiarities of the pulse curve, which have since been shown to be of high importance, and consequently those who followed Marey's views too exclusively fell into the same error. Thus, in the earlier papers, the pulse-curve recorded by the sphygmograph was described as consisting of a line of ascension, a summit, and a line of descension, the last being broken normally by the occurrence of a large undulation, which was called the dicrotism or second beat. The forms of these several parts of the curve were stated to undergo considerable modification in physiological conditions, and still greater changes were referred to the existence of disease. The line of descension, the most important part of the curve, was soon noticed to vary very much in the number of its undulations: and the occurrence of several of these, instead of the single undulation or dicrotism, which observers had been taught to look for, suggested to some minds a doubt of the accuracy of the record. The sphygmograph was accused of untruthfulness, and the additional undulations were referred to acquired vibrations of the writing lever, totally independent of the pulse movements. The defence of the sphygmograph was undertaken by Wolff, who demonstrated that these undulations in the descending line of the curve were really normal features of the pulse movement, and that under a more accurate mode of adjustment the pulse could be always registered as distinctly *tricrotous* instead of *dicrotous,* as Marey described it. The curve of the normal radial pulse thus came to be recognised as possessing features which the earlier observers had not figured. The descending line of each pulsation was found to be marked by two notches, each followed by an elevation. The first notch occurring soon after the summit of the curve, was called by Wolff the first incisure, and the elevation which follows, the first secondary wave or undulation. The dicrotism of Marey was called the great ascension, and the notch which precedes it and separates it from the first secondary wave was designated the great incisure. In the bottom of this great incisure another small secondary wave, the second secondary wave, can be occasionally seen. This more accurate recognition of the finer features of each pulsation was first really arrived at by Wolff, and in this country his results have been fully confirmed. In speaking of the pulse-curve we shall, therefore, continue to use, for the most part, terms similar to those applied to its different features by Wolff, because such terms more conveniently adapt themselves to the original language employed in Marey's work. The next important step was taken in reference to the significance of each of these parts of the pulse trace. It was extremely interesting to know that these various little movements occurred in the radial pulse after each systole of the heart, but the value of the knowledge would have been unimportant if no explanation of the causation of these phenomena had been forthcoming. The pulse-curve, as described by Marey, consisted roughly of two elevations, the first of which was the greater, and corresponded to the ventricular systole; the second and smaller was termed the dicrotism, or, by Wolf, the great secondary wave, and corresponded to the period of diastole. Whenever the break in the line of descent of the curve which separated these two elevations could be distinguished the pulse-trace could be divided into its systolic and diastolic portions. To refer the primary elevation or ascending line of the pulse-curve to its cause seemed easy; it occurs immediately after the systole of the left ventricle, and corresponds to that sensation perceived by the finger and known as the pulse. Marey considered this part of the pulse-curve to be due to the increase of tension in the arterial walls, caused by the impletion of the artery produced by the passage of a mass of blood driven onwards by each systole of the heart.

This view was criticised by MM. Onimus and Viry, and afterwards by Dr. Sanderson. These writers revived in part the theory of Weber, which referred the pulse to a wave propagated towards the periphery, and originating in the increased pressure produced in the aorta, after each contraction of the heart. This wave causes a gradual expansion of the artery, but we must bear in mind Weber's expression, "Unda enim non est *materia* progrediens, sed *forma materiæ* progrediens." An experiment of MM. Chauveau and Marey proves this wave to be independent of the onward passage of the blood (*vide* 'Physiologie Médicale de la Circulation du Sang,' pp. 199, 200). The following simple experiment also supports the same view, and can be easily tested by any of our readers. The sphygmograph having been applied to the radial artery, or any other vessel to which it can be adjusted, the artery is compressed below the point on which the tactile spring of the instrument rests; and although by this means the onward flow of the blood is arrested, the pulse movements will be still recorded by the writing lever. We may, therefore, conclude that the systolic portion of the pulse-curve is not directly caused by the onward flow of blood, but is produced by a wave movement propagated along the arterial system. With this wave movement there occur also vibrations in the blood-column, of which we shall speak hereafter. The second elevation in the pulse-curve—the dicrotism or great ascension—has given rise to much dispute concerning its mode of production. Marey defined it as an oscillation in the blood-column in a direction alternately centripetal and centrifugal, originating in the arteries of the periphery. Marey's critics, Onimus and Viry, while supporting this view as to the direction of the dicrotic wave, define it as a return wave, produced by the obstacles to the passage of the blood through the capillaries. These obstacles are of two kinds—"the spurs formed by the bifurcation of the arterial trunks and the blood-globules contained in the capillaries." The blood-globules entangled in the smallest capillaries, form, these observers think, the chief obstacle to the onward passage of the blood, and consequently the most influential factor in the production of the return wave or dicrotism. This view has now been abandoned by many in favour of the view originally propounded by Naumann, and recently ably advocated by Dr. Sanderson, which refers the phænomenon to the closure of the aortic valves, and which regards the dicrotic wave as central in its origin and centrifugal in its direction. Dr. Sanderson's investigation of the carotid-pulse-form strongly supports the view that the notch preceding the dicrotism is due to the reflux of blood which closes the aortic valves, and, consequently, the succeeding elevation on the pulse-curve is referred to the wave produced in the blood-column by the rebound of the blood in the aorta from the tensely closed valves. The notch preceding the dicrotism, termed by Wolff the *grosse incisur*, may be now more conveniently called the aortic notch, as it is synchronous with the reflux which closes the aortic valves. This reflux begins after the contraction of the ventricle ceases. In cases where the tension in the aorta is high the valves are closed almost instantaneously, and the notch is not very distinct in the radial pulse. On the other hand, when the aortic tension is low, the valves close more slowly, the reflux towards the heart lasts longer, and possibly a slight amount of regurgitation from the aorta into the ventricle occurs; the notch is consequently very distinct in the radial pulse. The state of arterial tension stands, therefore, in close relation with the occurrence of dicrotism.

The small secondary waves to which we referred above as occurring in the descending line of the pulse-curve, now alone remain to be explained. They appear in the systolic portion of the curve, and we have therefore to look to conditions connected with the cardiac systole for their origin. When the ventricle contracts the first step is the tightening of the mitral valve, the next the elevation of the curtains of the aortic valves. The blood resting on the aortic surface of these valves is thus thrown by their elevation into a series of vibrations which are propagated rapidly towards the periphery, especially when the arterial tension is low, for then the aortic valves can open suddenly. These vibrations produce corresponding movements in the arterial walls, which appear in the systolic portion of the pulse curve. The true wave of the pulse occupies, it must be remembered, the whole of the curve up to the aortic notch: the vibrations modify the form of this part of the curve. Thus, the summit of the pulsation, especially when sharp-pointed, is due to a vibratory movement, and the wave which follows, that is to say, the first secondary wave is due to the distension of the artery following the heart's systole. In conditions of higher tension the first secondary wave, or wave of distension, becomes rounder and more distinct, giving to the pulse the quality of fulness. In states of still higher tension the pointed summit wave or vibratory wave, is blended in the summit of the curve with the wave of distension, or pressure wave, as it is sometimes called. In a pulse-curve then exhibiting all the features we have described, the summit-wave may be considered as due to a sudden vibration in the blood column, synchronous, or almost so with the elevation of the aortic valves: the first secondary wave as due to the wave of distension, or pressure wave, following the passage of blood from the heart into the aorta. The second secondary wave, which is not often seen, is most probably vibratory; and the great ascension or dicrotism represents the rebound of the blood from the closed aortic valves. The first notch represents the sudden collapse of the artery following the brusque elevation caused by the vibration in the blood column, and the second or aortic notch corresponds to the centripetal reflux which precedes the closure of the aortic valves, and marks the termination of the ventricular systole.

We may now consider briefly the modifications in the mode of application of the sphygmograph of Marey which have enabled later observers to record pulse-curves so much more perfect than those originally published. The form of instrument has remained substantially the same, but a nicer adjustment has led to improved results. Wolff, with that patient spirit of investigation which has made German observers so great, discovered, early in his inquiries, that variations in the pressure exerted on the artery produced considerable change in the form of the pulse curve. The English observers appear to have soon discovered this fact independently, and to have endeavoured to remedy this possible source of error. In the instrument of Marey there is a screw by which the tactile portion of the spring can be made to descend and press upon the artery, and the use of this screw seemed to offer a solution of the difficulty. The screw, as it was first made, however, was of very little use, for although it permitted the increase of pressure, it gave no information as to the amount of pressure exerted, and consequently no two observations on the same pulse could be accurately compared. In an instrument likely to be used much in investigations as to the effects of remedies on the circulation, a greater defect can scarcely be imagined, for all such inquiries must demand as a primary condition, a power of comparing with precision the states of arterial tension at different times. To remedy this defect Dr. Foster first proposed that the screw should be made self-recording by the addition of an index, which pointed out on a circle described round the screw, the increase of pressure. By this simple arrangement observations could be compared with some approach to accuracy.

The great objection to this plan consisted, however, in the fact that when the screw was used to exert any considerable pressure on the artery, the tension of the spring was at the same time so much increased that it ceased to follow with the necessary delicacy the finer movements of the pulse. The next plan proposed was that while the screw we have referred to should only be used occasionally, the pressure should be applied directly over the artery by placing little weights on the head of the screw which brings the writing lever in contact with the movements of the tactile spring. Additional weight can no doubt be thus applied in the best possible position with regard to the artery, but, in order to render either plan complete, it is necessary that the equivalent of the pressure exerted by the simple application to the wrist should be also known. This might be easily done, in our opinion, by fixing to the upper surface of the spring a very fine upright ivory scale, which would indicate the distance between the upper surface of the spring and the brass framework of the instrument. The shorter this distance the greater pressure the spring would necessarily exercise on the blood-vessel, and thus the distance marked on the ivory scale would always indicate the pressure. This simple plan would have the great excellence of making the instrument itself record in every application the actual weight exerted upon the artery. In Dr. Sanderson's 'Handbook,' we find still another method described by which satisfactory results seem to have been obtained. The pressure screw is used to fix the tactile spring permanently, and the pressure is ascertained by measuring the distance between the spring and the under surface of the lever, whenever the instrument is applied to the arm. To diminish the pressure the framework of the sphygmograph is raised by slipping beneath the carpal end little blocks of brass and the withdrawal of similar blocks has the opposite effect. A pressure of at least 100 grammes is recommended as the most expedient to work with. Would not the ivory index of which we have spoken render this plan also more simple? We hold that the accurate adaptation of the pressure of the spring to the various conditions of the pulse is of the highest importance. Attention to this point has already led to a much more complete knowledge of the pulse form, and no observations of the highest value can be made in either physiological, pathological, or therapeutical inquiry, without an accurate method of estimating the pressure exerted upon the artery examined. We have dwelt at length upon this part of our subject because notwithstanding the good results obtained by the accurate adjustment of Wolff and others, French authors appear inclined to underestimate its importance. For instance in M. Souligoux's little work just published, 'Du Diagnostic Médical et Chirurgical par les moyens Physiques,' we find no reference to this point; and the pulse tracings contained in the interesting researches of Dr. Moréno y Maïz on the physiological action of Coca, and those figured in the chapter on "The Circulation in Cholera" of Prof. P. Lorain's valuable treatise, are much diminished in value by the want of any record of the pressure.

Another practical point of importance consists in the reduction to a minimum of the friction between the receiving plate and the point of the pen. Spurious tracings may be manufactured if this be not carefully attended to, and when the record is marked by ink on glazed paper such worthless tracings are not unfrequently obtained. To obviate this difficulty the pen at the end of the lever should be made of very flexible metal; it can then be easily adjusted. Most observers in this country have now wisely gone back to the old plan of using smoked glass instead of paper and ink. The great advantages of this plan are, that the pen seldom if ever fails to leave its record, that the friction can be reduced more effectually than by the other methods; and lastly, that the traces when fixed, as they can easily be by photographer's varnish, are much more convenient for reproduction by photography.

We may now turn to the results yielded by the application of the sphygmograph to the study of disease; and here we must confess, *in limine*, that Marey and his earlier followers were too sanguine in their expectations. We nevertheless feel confident that we have had at present but a very insignificant portion of the fruits which the use of the instrument in clinical research is destined to bear. Dr. Sanderson has said—

"The sphygmograph is not to be regarded, like the laryngoscope or the ophthalmoscope, as an aid in the discovery and discrimination of organic diseases, for affections the most diverse communicate to the pulse the same graphical characters. Its use is to enable the physician to investigate the state of the circulation and circulatory organs in diseases of which the general nature is already recognised, with reference to (1) the mode and duration of the contraction of the heart; (2) the soundness of the arteries; and (3) the relative quantity of blood contained in the arteries and veins, or, in other words, the balance of pressure between the venous and arterial systems."

The reaction against the undue expectations raised by Marey has led Dr. Sanderson a little too far in the opposite direction. As the quotation itself shows, he has limited the uses of the instrument somewhat too narrowly, but even his estimate of its value is sufficient to teach us how indispensable it is to accurate clinical work. The observations which have been published in this country have been directed chiefly to the study of the modifications of the pulse in aneurisms and valvular diseases of the heart. Drs. Grimshaw and Anstie have also studied, with interesting results, the changes of the pulse tracing in acute disease. It is in the diagnosis of aneurysms, however, that the sphygmograph has hitherto afforded the most valuable information: in some cases leading to the correct diagnosis of the seat of the disease, and in others to the discovery of the lesion before unsuspected. No investigation of a case in which there is the slightest ground for suspecting the existence of an aneurysm, can now be considered complete unless a careful sphygmographic observation has been made. Marey points out[1] that the modifications in the pulse produced by aneurysm are chiefly directed to a change in force of the pulse, and a change in the intensity of the dicrotism. A still more valuable sign, often the only one in aortic aneurysms, is also mentioned—the want of a parallelism between the tracings of the radial arteries of opposite sides of the body. Marey relates some ingenious experiments which induced him to refer the modifications in force to the transforming effects of the elastic walls of the sac; but he also adds, that in many cases the walls lose much of this elasticity, and consequently exert much less influence. In the first-mentioned little book on our list, a very interesting case of subclavian aneurysm is recorded, in which the variations in the pulse tracings taken below the aneurysm at different times were very remarkable. In one observation the left radial tracing was reduced to a series of curves almost semicircular in form, while in a subsequent record which the pulse gave, the form was much more developed. Such a change can, we think, scarcely be accounted for by the supposition of an alteration in the elasticity of the sac, but would much more likely be due to the influence of the clots within the aneurism in impeding the force of the blood current. We believe that in the great majority of cases the chief changes in the pulse form are referable to the modifying influence of the clots; to produce anything like the great modifications of form observed in the case we have referred to, and in another very similar case published by Dr. Brondgeest, and quoted by Marey, it would have required an enormous aneurysmal sac with most elastic walls; the history of neither case tells of such conditions.

The number of complete cases of aneurysm recorded are as yet too few to warrant any very decided conclusions; all of them, however, point to the correctness of Marey's views on the mode in which the form of the pulse curve is influenced. In the 'Lancet,' vol. i, 1866, will be found a very remarkable case, in which a correct diagnosis of the situation of an intrathoracic aneurysm was formed from the study of the pulse tracings. In this case the question of operation depended on the decision arrived at. The pulse traces pointed by their slight want of parallelism to the aorta as the seat of the disease, and consequently the idea of operative interference was abandoned. The event proved the perfect accuracy of the testimony of the sphygmograph. A brief account of an aneurysm of the descending thoracic aorta will be found in the 'British Medical

[1] 'Physiologie Médicale de la Circulation du Sang,' chap. xxii, xxxiii.

Journal,' vol. i, 1867. The nature of the case had not been suspected, as the physical signs did not point to the existence of aneurysm; the pulse tracings of the right and left radial arteries, however, differed considerably in form; the right pulse not only gave a more ample tracing, but also exhibited an unusual development of the dicrotism. These features led to the diagnosis of aneurysm which the autopsy afterwards confirmed. The situation of the aneurysm, we may remark, just below the origin of the left subclavian was little calculated to influence the force of the blood entering the innominate artery and the vessels of the right arm, but on the contrary the mouth of the sac attracted the current intended for the left subclavian, and thus rendered the left pulse so much smaller than the right. A somewhat similar explanation has been offered by Dr. Foster[1] to account for the relative smallness of the right pulse tracing in cases of dilatation of the ascending portion of the aorta. In such cases the dilatation towards the right side tends to impede the current entering the innominate artery, while the vessels of the left side receive an unchecked wave. We are greatly in need of more sphygmographic observations in cases of aneurysm, and we were much disappointed at not finding any information on this subject in Dr. Sanderson's 'Handbook.' The few cases we have been able to refer to, indicate how much important knowledge has to be gathered in this direction, and we trust so rich a field will not long want a greater number of skilled observers.

The pulse-curve, in cases of *aortic regurgitation,* as might be expected, is very noteworthy, and we call attention to the tracing in this form of valvular disease more particularly, because it was formerly considered to possess specific characters, sufficient of themselves to justify the diagnosis of the lesion. More extended observation has proved this view to be incorrect, although we are sorry to say it still holds a place in some recent French publications. The pointed summit which the curve usually presents, and the vertical line of ascension, were considered by Marey as the distinctive characters of this affection. Functional disorder is now known to produce similar appearances. When the heart contracts quickly and violently, as it often does in anæmic patients, a pulse curve simulating that of aortic regurgitation, in its vertical ascension line and pointed summit, is often recorded. The valvular incompetency in aortic disease, however, is not without its effect on the pulse tracing; when the regurgitation is at all copious, the descending line of the curve presents a marked peculiarity in its sudden

[1] 'Med. Times and Gazette,' vol. ii, 1866.

fall. The collapse of the artery is so complete during the diastolic period, that the dicrotism is often almost suppressed. No centrifugal wave rebounds from the closing aortic valves to distend the blood-vessels, for these valves by their incompetency allow the blood to flow back into the ventricle. The suppression of the dicrotism and the form of the aortic notch vary of course in proportion to the incompetency of the valves, and within certain limits the amount of the disease may be inferred from the extent to which these features of the curve are modified. The vibratory phenomena of the pulse are highly developed in these cases; but the other feature, which the low arterial tension normally developes, the dicrotism, is lessened by the valvular lesion. In this particular, and in the fact that the vertical line of ascent in this disease is composed of the two elements of vibration and distension, and consequently is not to be diminished by increased pressure on the vessel, we have possibly the means of distinguishing the pulse of aortic regurgitation from that which occasionally simulates it.

The irregular pulse which often, but not invariably, accompanies *mitral regurgitant* disease, gives a striking sphygmographic record, but one not generally useful for diagnostic purposes. The compressibility of the pulse, and the change of form produced by varying the pressure on the artery, however, often yield valuable information of the amount of regurgitation. The irregularity of the pulse in this condition appears to be intimately connected with the rhythm of the respiratory act; it obeys in many cases, as we have long known, a certain law in its irregularity. Prof. Marey and Dr. Sanderson have referred this to the effect of inspiration, but we have not yet had the evidence laid before us on which this view is based.

Some of the most interesting observations of Marey were made in connection with the pulse-form of old age. The healthy elasticity of the arteries being diminished by the senile change in the arterial coats, its transforming effect on the blood-movement was lessened, and the pulse approached more nearly in its form to the aortic movement. Marey's tracings were, however, in many respects imperfect, and were generally taken in conditions of too advanced arterial change to be of much use to the physician. When an artery feels like a bony tube under the finger, no sphygmograph is required to tell us of its senile state. It is in the earlier and more obscure stages of arterial degeneration that it is important for the physician to obtain information.

At present we have no sufficient evidence to show that atheroma limited to the great vessels produces any material alteration in the pulse-form. To solve this question we want a series of carefully observed cases in which the state of the great and smaller arteries has been carefully investigated. In certain morbid conditions, however, in which the primary change occurs in the capillaries, such as that recently noticed by Dr. George Johnson, the pulse-trace gives a striking evidence of the difficulty the blood meets in its onward passage. The systolic part of the curve is prolonged in its duration, and the first secondary wave is very marked, being relatively much greater than the dicrotism, hence the fulness of the pulse which the finger perceives. The summit of the curve resembles, in its rounded or flattened form, that occurring in old age, but, in the latter case, the dicrotism is much less developed. Morbid conditions of the capillaries inducing undue tension of the arterial system, seem likely to yield sphygmographic evidence of their occurrence long before they can be recognised by other means.

The pulse-trace of undue impletion of the arterial system leads us by an easy transition to the consideration of the pulse peculiarities in the opposite conditions. In the former case the high arterial tension showed itself by the striking development of the first secondary wave; in the latter this wave gradually disappears, and the dicrotism is developed in a corresponding degree. A very simple experiment will enable any observer to study the changes in the pulse-curve, between these opposite conditions. A whiff or two of nitrite of amyl will produce all the different phases in rapid succession. As soon as the physiological action of the drug is felt, the pulse-curve begins to lose its first and second secondary waves, which are soon swallowed up in the aortic notch, while the dicrotism is proportionally increased. Soon the aortic notch dips below the level of the curve-basis, and the dicrotism becomes blended more and more with the ascending line of the next pulsation, on account of the rapidity of the heart's action. It is remarkable that similar changes occur in acute febrile diseases, and Wolff has given a very striking diagram which has been borrowed by Anstie to illustrate these phases of the pulse-change.

This diagram shows how the increase of the aortic notch in the pulse of acute febrile diseases corresponds with the height of the fever. In the healthy pulse-curve the tricrotous form exists, but the febrile pulse ever tends to become dicrotous and may become monocrotous. These changes are chiefly effected by the deepening of the aortic notch, and the classification of febrile pulses depends on this feature. When the notch has not sunk down to the level of the curve-basis, and has not quite swallowed up the first secondary wave, but has annihilated the second and slightly retarded the dicrotism, the pulse is said to be hypo-dicrotous. With this form of pulse the temperature of the body seldom exceeds 100° Fahr.

When the notch sinks to the level of the curve basis, the first secondary wave having almost disappeared, and the dicrotism being still more retarded, the pulse is called dicrotous or perfectly dicrotous. In this condition the temperature is about 103° F., and the pulse-rate about 100 per minute. When the aortic notch sinks below the level of the curve-basis, and the dicrotism appears partly blended with the line of ascent of the next pulsation, the pulse is called hyper-dicrotous, and the temperature usually ranges above 104° F. The value of these researches of Wolff can scarcely be overestimated, and the lectures delivered by Dr. Anstie at the College of Physicians, were of interest as confirming and popularising such important information. Other signs occur in acute febrile diseases besides the modifications of which we have spoken, and afford useful indications on points of vital interest. On account of the low state of arterial tension in these maladies, the heart while acting well, gives a lofty and vertical line of ascent, terminating in a sharp apex. On the other hand a short and non-vertical ascension-line with a square or blunt summit indicates weak and failing heart-action. The occurrence of irregularity in the pulse-curve at the height of the pyrexia is another grave sign. In its mildest form the irregularity betrays itself in a want of exact similarity in the successive pulsations, which affects the systolic portion more particularly and tells of a varying vigour of ventricular systole. When, however, there is an undulatory irregularity of the general line of the pulse-trace, we have a sign of still graver import which informs us that the power of the ventricle is momentarily changing. This form of irregularity has no relation to the respiratory movements, and must not be confounded with that undulation of the general line of the tracing which is produced by the varying tension of the arterial system caused by respiration. In the latter case the undulations form a series of equal curves occurring at regular intervals, characteristics not to be recognised in the former.

When the hyper-dicrotous pulse changes at an advanced stage of the fever into the monocrotous or imperfectly monocrotous form, both Wolff and Anstie concur in regarding it as an almost certain indication of death.

Such are the chief points on which the sphygmograph informs us in acute disease. Dr. Anstie has certainly not overestimated its value in the following words which form part of the conclusion to his first lecture:—

"Let me conclude this lecture by again enforcing the general estimate of the prognostic value of the sphygmograph in acute disease, which I expressed at the commencement. Used in conjunction with the strictest and most diligent observance of other means of clinical research, I believe that the instrument affords us an additional test of the progress of acute disease, and the patient's chances of safety, which is of very high value."

The action of alcohol occupied Dr. Anstie's second lecture, and some of the observations on its influence on the pulse deserve much attention. Alcohol seems to modify the pulse-form in accordance with its action as a stimulant or narcotic. When given in the typhoïd stages of acute diseases its effect is to diminish the dicrotism, and to slow the pulse, in other words to increase the arterial tension. On the contrary when alcohol acts as a narcotic it quickens the pulse and increases the dicrotism. These observations of Dr. Anstie's, which we can ourselves confirm, are very important, and point out what a ready guide the sphygmograph may become in the treatment of acute diseases. Hitherto we have been sadly in want of some rule to help us in the administration of stimulants, and some means of ascertaining their effects when administered. The sphygmograph seems to promise this aid, and if it were proved useless in the investigation of all those other problems concerning which it has already told us much, and has given us good reason to expect still more, this one result of its use is surely sufficient to earn for it our gratitude, and to induce all who seek to advance medicine to apply it to the study of disease at the bedside.

Review II.

Études sur les Causes du Cretinism et du Goître endémique. Par le Dr. J. Saint-Lager. Paris, Baillière. 1867.

On the Causes of Cretinism and Endemic Goître. By Dr. J. Saint-Lager. Paris, Baillière. 1867.

It is remarkable enough that the old saying, "obsta principiis, sero medicina paratur," is more generally accepted now, in spite of the marvellous and continued progress in the art of healing, than at any previous time in the history of medicine. Its truth is most strikingly illustrated by the case of acute specific diseases; for, while our increased knowledge of them has led us to adopt heartily the principles of treatment laid down by the English Hippocrates, we can no longer endorse his saying, that God is the author of acute diseases, and we ourselves of chronic ones; we are convinced that the former are the well-merited and natural results of a general abuse and neglect of the gifts of Providence; and that, although care would prevent the formation of animal poisons, we are powerless to do much more than watch their effects upon individuals.

Again, a more careful study of the phenomena of local diseases has shown us, that we very frequently cannot hope to effect a substantial cure, and that our efforts should be directed to averting those anatomical changes which, when once made, we cannot alter.

For the purposes of preventive medicine, a thorough knowledge of the causes of disease is necessary, and hence the importance now attached to the study of ætiology even where no immediately practical result is to be anticipated.

The work before us would commend itself to us by its subject, if it treated only of morbid causes, which from their nature and complexity, were beyond human control; but goître and cretinism are so very probably dependent on the quality of water drunk in the districts where they prevail, that the discovery of their origin would be a certain prelude to their suppression. It is difficult to overestimate what humanity would gain by such a result; but, to give some idea of the extent of the evil, we may mention that in France alone, at least 450,000 persons suffer from goître, and about 30,000 from cretinism. In our own country, though we are comparatively free from this latter scourge, bronchocele is common in some of our most favoured and otherwise healthy counties; and both are endemic in many parts of our Indian and American possessions.

We believe, therefore, that our readers will be interested in learning the conclusions to which Dr. Saint-Lager has been led by a very careful study of this important question. They are novel, and it will be seen that, without somewhat extensive observation and experiment, we should not be in a position to do more than present an analysis of the book; but we are bound to say at once that its author is evidently a man of great industry and care, whose statements, so far as we have verified them, are accurate, and whose conclusions appear to be warranted by the premises.

He begins by some general remarks, such as that goître is much more common in women than men,[1] while, on the other hand, there are more male than female cretins.

Some modern writers have supposed that these are two

[1] The excess among women is very much greater here than in France; evidently because the French statistics are compiled with a view to exemption from military service, so that men would register more carefully than women; while in our dispensaries and hospitals, women present themselves more readily than men.

essentially different diseases, but our author points out that exact observation will show that some amount of bronchocele almost invariably accompanies cretinism (when it has been overlooked, the tumour has developed laterally and posteriorly, and is to be detected rather from its interfering with respiration than from its external projection), and that, on the other hand, many who have goître are, if not cretins, at any rate, weak-minded and eccentric. Cretinism would appear to be the ultimate result of a poison which, in a less degree produces goître; thus, in the Alps and Pyrenees, on entering the regions where these diseases abound, goître is first met with, and cretins are only found in any number near the centre of the district.

Dental caries, albinism, stammering, and deafness, are unusually common in those suffering from goître or cretinism, and in the countries where these are endemic; where the proportion of deaf and dumb people is also greater than elsewhere. These disorders seem, therefore, to be symptoms of what our author calls the "cretinous diathesis," modified by circumstances of which we at present know nothing. The conditions, whatever they may be, which produce goître in man, affect the lower animals in the same manner. Thus, dogs, cats, pigs, sheep, horses, mules, oxen, have all been observed to have bronchocele; the wool or hair at the same time becoming rough, the voice hoarse, the hearing obtuse: the animal falls at last into a state of torpor, and dogs have even been noticed in a state of true cretinism.

Many authors (among others Mead, White, S. Cooper, in England, and Bazin, in France) have supposed that goître is only an extreme form of scrofula. But the weight of authority is against this opinion, which is disproved by the different geographical distribution of the two diseases, bronchocele being on the whole common, where scrofula is rare, and *vice versâ*, and by the different pathological characters of the two affections; thus, goître is by far most common between the ages of twenty and thirty, is rare before puberty, is not hereditary, has no connection with disease of bones, or of other glands, the mind is dull or imbecile, instead of being precocious, and the tumour does not suppurate,—in all of which points it differs from scrofula.

Cretinism, again, has been looked upon by some Italian physicians as a result of pellagra; but nothing (according to the Lombard commission and other authorities) can be more unfounded, since there is even a sort of antagonism between the two diseases, at any rate as to the places of their occurrence.

The cretinous diathesis attacks alike each of the chief races of mankind; it is not inherited, since healthy parents, who have previously borne healthy children, on going to live where cretinism is endemic, have often had other offspring who were cretins. Nor has intermarriage any apparent influence; for, in Italy especially, the inhabitants of affected districts are in the habit of marrying among their more fortunate neighbours; while in many other villages not a single cretin is to be seen, though all are connected by ties of marriage.

So much for the indirect evidence that goître is an endemic disease, due to local conditions. As direct evidence, we have the testimony of Pliny, Vitruvius, and Juvenal, that it existed in the Alps and in Lombardy in their day. In mediæval times the references to it become more numerous and precise, so as to leave no doubt that it has always prevailed in many of the places on the continent of Europe, where it is now observed.

What are the local conditions producing the cretinous diathesis?

De Saussure, Demme, and Güggenbuhl believed it to be unknown at a greater height than 1200 metres (nearly 4000 feet) above the sea-level. The fact is true as far as regards the Swiss Valais; but that the connection is merely accidental is proved by its existence at very much greater altitudes in the Himalayas and Andes, and by its prevalence on one bank of several rivers (as the Isère and the Aral), while the inhabitants of the other bank enjoy perfect immunity.

The popular idea that mountainous gorges are the favorite habitat of this disease is equally unfounded; witness the plains of the St. Laurence, Danube, Ganges, and Po, and the flat country of Piedmont, Alsace, the Palatinate, and Ceylon. When it does occur in valleys, it is not confined to those which take any particular direction, or which are open to any particular wind.

Nor can climate and electric conditions have any effect on the development of a diathesis which is observed alike in the oases of the Sahara, in Ceylon, Java, and Brazil; in the perpetual spring of the "tierras templadas" of Mexico; in the variable temperature of the Himalayan valleys, and in Europe; and in the wintry climes of Canada, Finland, and Siberia.

A favorite opinion at the present day, and one supported by the authority of Vingtrinier, Morel, Virchow, and Kœberlé, ascribes the production of cretinism and goître to some aërial miasm, either ordinary paludal malaria or some independent poison. Now, on the whole, malarial fevers are most common in low, marshy countries, and the cretinous diathesis in hilly ones; but they may coexist, so that there is evidently neither connection nor antagonism between them. And if an aërial "cretinising" miasm of any kind existed the disease could be acquired by a few hours' sojourn in an affected district, and ozone would be deficient, which is not the case.

The vapour of sulphurous acid has been said by some authors to be a cause of goître; and it is true that the disease is observed near the "solfataras" of Naples, Tivoli, Java, Sumatra, and Mexico. We shall see the probable explanation of this fact by and by; at present it is enough to remark that we have no account of goître being ever caused by exposure to the fumes of sulphurous acid when used in the arts; it occurs, too, in many places where the acid is not found.

Stagnation of air and absence of direct sunlight have been set down as causes of cretinism by eminent authorities; but it is not found in many of the worst ventilated Swiss valleys, and is common, on the other hand, in some of the healthiest positions.

It is unnecessary to fatigue the reader with more than a passing mention of the theories which have ascribed goître to drunkenness, or to the use of various kinds of food. The Sardinian commission had more plausible reasons for supposing poverty, and a generally defective hygiene, to be the true cause, for it is certain that the proportion of those suffering from cretinism or goître is much greater in the lower than in the middle or upper classes, but the absence of the diseases in the midst of the deep distress of our large towns, and their frequent occurrence in persons of even the highest rank in countries where it is endemic, prove that unfavorable hygienic circumstances may indeed be set down as predisposing to the cretinous diathesis in the same sense as they predispose to the tuberculous or syphilitic, but that they are by no means the proximate causes.

Putting, therefore, all these assigned causes apart, there is abundant evidence to show that the real source of mischief is some peculiarity in the water consumed. Many instances are cited by our author, in which either individuals or whole villages or towns having obtained a fresh water supply, became free from goître and cretinism, which had previously affected them: many families are mentioned who have preserved themselves in places where these diseases were decidedly endemic, by drinking rain water, or water that had been boiled or filtered; nay, it seems to be matter of notoriety that there are in Savoy springs, which will infallibly give goître to all those who drink of them, and which are for this reason frequented by young men who wish to escape the conscription. The few cases in which goître has been acquired, although only rain or filtered water has been used, are probably to be explained by the presence of the common water of the district in bread and other articles of food.

So much may be safely assumed as certain; but the greatest difficulty is to determine which of all the numerous substances present in water is the one to blame.

There are many cases on record (McLelland gives, we believe, one very remarkable one) where goître and cretinism are common near the source of a stream, and rare a short distance below. This has led some persons to doubt the influence of water in causing these diseases; but it is evident that a stream in its course deposits sediment, and that the result is a sort of natural filtering. At Saillon in the Valais, the villagers have unintentionally performed an *experimentum crucis;* for, on their taking their water supply some hundred yards higher up the stream than usual, goître, previously unknown among them, made its appearance.

The epidemics of goître, which have occurred from time to time, have almost always affected regiments which have been freshly sent to garrison towns where goître is endemic, and therefore are readily accounted for on the assumption that the water is at fault.

Coxe and Deluc, towards the end of the last century, first suggested that carbonate of lime was the noxious ingredient in water; and, more recently, M. Grange has supposed the salts of magnesia to be in fault. Both of these opinions have acquired a certain popularity, and it is true that the waters of many goitrous districts do contain a large proportion of calcareous and magnesian salts; but the case of the city of Paris, where the water is extremely hard, and contains sulphate of magnesia enough to purge most new comers, but where goître is almost unknown, is alone sufficient to make us reject them.

McLelland, whose excellent work was fully noticed in the number of this Review for January, 1861, is often said to have maintained that the cretinous diathesis is due to the use of water containing calcareous salts; but this is not the case. He confines himself to showing that it is much more frequent upon limestone than upon any other soil, and supposes "some subtle combination, derived perhaps from those strata of the rock called by miners *copper-slate,* so distinguished from the quantity of metals it contains," is the noxious principle.

Prevost of Geneva, after Coindet had discovered the curative action of iodine in goître, suggested that the disease might be perhaps owing to the absence of that element from the water used. This hypothesis received some confirmation from Inglis's remark, that goître did not occur in the town of Harrogate, where the waters contain iodine, although endemic in the surrounding country: it has, however, been shown that iodine may be either present or absent in places where goître prevails; —nay, that some of the Alpine springs most notorious for their goître-producing powers contain a very large proportion of iodine.

The ancient opinion ascribing goître to the use of snow or ice-water is decidedly negatived by the absence of the disease in Lapland, Greenland, and other arctic regions, where ice-water is used all the winter; and its occurrence in tropical climates, such as Brazil, Java, and Sumatra. We are compelled to conclude from these instances that the epidemic of goître which occurred in Captain Cook's voyage, in 1772, among the men who had drunk water from the melted fragments of an iceberg was due to some other cause. The ice was melted in large iron cauldrons: this will possibly supply an explanation, according to the theory we shall presently develope.

Van Helmont and Cardan were the first propounders of an opinion, which has since been held by such men as Lugol and Kœberlé, that the presence of some organic substance in the water is the cause of the cretinous diathesis. The ordinary products of decomposing animal and vegetable matter may be at once set aside, as, on the whole, goître is more common on the banks of fresh mountain streams than of polluted rivers. M. Saint Lager informs us also that in many analyses of waters known to produce goître, he has found the organic matter of every kind to vary from the merest trace to a very large proportion.

We may be assisted in coming to a conclusion as to the nature of the offending substance, if we remember that filtering the water used, or even allowing it to stand for some time before using it, deprives it almost entirely of its noxious qualities. This proves that the poison is evidently some solid substance, either already existing in the water, or precipitated on exposure to the air, which enables us to put out of the question the soluble salts of lime and magnesia, and makes us look upon a study of the geological character of the soils on which goître prevails as the only key to the mystery.

Dr. St. Lager proceeds, therefore, to a very careful examination of the geology of France, Switzerland, and Lombardy, noting especially the places where goître and cretinism occur. His account of the rest of Europe, and of the other countries where these diseases are known to exist, is necessarily less minute, but, as far as we have been able to test it by the case of England, apparently correct, both in its geological and statistical data. Of course we are unable to follow closely these tables, which fill nearly 200 pages of the work, but we may state briefly the conclusion he arrives at, viz., that where goître and cretinism are endemic, the soil will always be found to contain iron pyrites; copper pyrites, galena, and baryta are also frequently found.

To give a few examples:—In the miocene strata these diseases are endemic on the molasse (containing pyrites and lignite) of Switzerland, Baden, &c.; they are unknown on the "nagelfluc," which is in Switzerland in some places superposed on the molasse. They are endemic, in our own country in Surrey, Sussex, Hants, Bucks, and Norfolk, on chalk containing silex and pyrites, and on the greensand and gault, in which pyrites also occur. The coal-measures of Durham, York, and Northumberland, do not contain pyrites, and the diseases do not prevail there; but they are endemic over the coal, containing pyrites, of Derbyshire, Nottingham, Belgium, and Pennsylvania. The old red sandstone in England does not coincide with the presence of goître; but in Scotland, where the two are found together, it is traversed by veins of serpentine, iron, and copper. Cretinism and goître occur only upon granite, gneiss, and porphyry, when those strata contain metallic veins; and on volcanic soil they are only observed near the solfataras where sulphurous vapours are in contact with ferruginous clays, as in the Terra di Lavoro, in Java, Sumatra, and the Azores. When they occur on alluvium, the soil has been brought down from strata containing pyrites, as in the valleys of the Rhine and the Po.

Many apparent exceptions to this rule of coincidence of the cretinous diathesis with iron pyrites will be found; they are due, according to our author, either to the presence of a thick superficial layer of alluvium containing pyrites, or to the water-supply being derived from some distant source.

M. St. Lager has, of course, foreseen the objection that will, no doubt, have occurred ere this to our readers, that preparations of iron are constantly administered medicinally, without even producing these diseases. He replies that medicines vary according to all the elements of which they are composed (as for example, the chloride of sodium and the sulphate of soda), and that the bi-sulphide of iron is never employed in medicine. We would hint, in addition, that it is, perhaps, conceivable that in some of the cases of Graves's disease, the goître may have been produced by the excessive use of iron for long standing anæmia; at any rate Trousseau has taught us that iron, apparently so plainly indicated, is injurious in this remarkable malady.

The epidemic of goître which occurred during Captain Cook's voyage in 1772, presents difficulties, whatever hypothesis we may adopt: it is, however, probable that the water having been heated in foul iron cauldrons, held in suspension a certain amount of sulphide of iron.

M. St. Lager has performed some experiments on the lower animals with the object of testing his theory. He has found that mice acquire goître very readily; out of a dozen of these animals, whom he kept for three months, three only had enlargement of the thyroïd gland at the end of the time; these three had had sulphide and sulphate of iron mixed with their food, while the rest had had various other mineral substances administered to them. He also experimented on two dogs, but both of these came to an untimely end by devouring the "appâts" which the French police employ to destroy stray dogs; one, however, who had been taking small doses of the sulphide of iron daily for four months, had a decided, though slight hypertrophy of the thyroïd gland.

He suggests that, in case any one should desire to repeat these experiments, either dogs or pigs should be selected; as the herbivora acquire goître less frequently where it is endemic. As we have already said, we are not in a position to offer any serious critical remarks on a theory which has been so elaborately constructed; we will merely mention in support of it—*valeat quantum*—that in two parts of England where we have special personal knowledge of goître, viz., Sussex and Dorset, the springs are in many places decidedly chalybeate. We would also suggest that iodine may possibly cure goître in the same way as it relieves lead-poisoning,—by favouring the excretion of the offending substance.

We have been most favorably impressed by the very unusual amount of research exhibited by M. St. Lager. The only omission of matter bearing on his subject which we have noticed, is that of Chevreul's account of the mode in which the "eaux sulfureuses accidentelles" are produced.[1] Being evidently a novice in the great art of book-making, our author has introduced a number of learned digressions, which, in the hands of a skilful manipulator, might be turned into as many separate works; an antiquary might read with profit his account of "touching" for goître, an ethnologist learn much from his history of the "cagots" of Western France; and a geologist derive much information as to the Alpine coal-measures.

Should his theory, however, be confirmed by further observation and experiment, he may fairly claim a much higher praise than for mere learning; he will be reckoned among the greatest benefactors of humanity.

[1] By the decomposition of alkaline or earthy sulphates in their passage through soil containing organic matter. (See Wurtz, 'Chimie Médicale,' i, p. 119.)

Review III.

1. *Études sur la Tuberculose.* Par J. A. Villemin.

Observations on Tuberculosis. By J. A. Villemin. Paris. 1868. Pp. 640.

2. *Die Krankhaften Geschwülste.* Von Rudolph Virchow. Einundzwanzigste Vorlesung. Band. II. Hälfte II.

On Tumours. By Rudolph Virchow. Twenty-first Lecture. Vol. II. Part II. Berlin. 1864. Pp. 555—749.

3. *The Nature and Affinities of Tubercle; being the Gulstonian Lectures for the Year* 1867. By Reginald Southey, M.D. London, 1867. Pp. 118.

4. *Sulla Struttura dei Tubercoli prodotti per Inoculazione. Nota del Dottor Giulio Bizzozero.*

Remarks on the Structure of Tubercle produced by Inoculation. By Dr. Julius Bizzozero. Milan. 1867. Pp. 10.

5. *Beiträge zur Experimental-Pathologie.* Von Dr. Lebert und Dr. Oscar Wyss.

Contributions to Experimental Pathology. By Dr. Lebert and Dr. Oscar Wyss. Virch., Archiv., 1867. Pp. 142—170; and 532—580.

6. *Lecture on the Artificial Production of Tubercle, at the Royal College of Physicians.* By Wilson Fox, M.D.

The history of tuberculosis has been so recently and so fully related in the pages of this Journal, that a renewal of the subject might, for the present, well seem to be unnecessary and superfluous: but the original observations by Virchow on the anatomy of tubercle, and the close affinity that it has to other kindred diseases; above all, the remarkable views concerning its causes and nature lately put forth by Villemin, and endorsed by other trustworthy observers, furnish abundant and most important material for another review.

That which will chiefly force itself upon the attention of any one who will refer to the list of books here introduced to notice, is the fact that the very groundwork of our pathology is rapidly undergoing a complete transformation. It requires no prophetic spirit to predict the extinction of those humoralistic doctrines of plastic exudation and crasis with which we were as students imbued, so completely are they becoming supplanted by the teachings of the new cell-pathology. We see a pathologist of Lebert's fame enunciating cellular doctrines as a matter of course. Southey, an ardent admirer of Virchow, puts forth in an agreeable English dress for English readers the teachings of the great Berlin master. Bizzozero's writing breathes of them from the first to the last page of his pamphlet. But, most remarkable of all, a Frenchman, Villemin,—and the French have been notoriously the last and the least inclined to accept Prussian authority—not only builds up the first part of his excellent book on the strictest precepts of cellular pathology, but shows by his writing that he has been led by a contemplation of this new pathology to the discovery that he has made.

We purpose first to give a short account of tubercle, its birth, life, and death; secondly, to inquire into the marks of relationship that it bears to other kindred diseases, as scrofula, glanders, syphilis, typhoid fever, cancer; thirdly, to discuss the influence of supposed causes in its production; fourthly, to make mention of the lower animals in which it is naturally met with; fifthly, to enumerate the experiments that have been made to show its capability of communication from man to the lower animals, and from one animal to another, by means of inoculation; sixthly, to examine the arguments that have been drawn from these experiments, and from other facts, in favour of its communicability by contagion from man to man, and to endeavour to show how far we are justified, in the present state of our knowledge, in accepting any such conclusions on this most engrossing subject, all-important as it is for the welfare of mankind.

It may, perhaps, be well, before proceeding with our subject, to say a few words against the tendency, which has long and much prevailed, to regard diseases as strange entities, or to confound disease itself—a complication of effects produced by certain causes in the tissues of the body—with the causes of disease. Thus fallaciously guided, the student of biology, instead of being led step by step up a gradual ascent from the study of physiological to that of pathological processes, is made to spring over a wide gulf into a new and strange land. The truth is, that we pass by a scarcely perceptible transition from the phenomena of healthy nutrition to hypertrophy, inflammation, tubercle, tumours, fevers, and other forms of perverted nutrition or disease. A hypertrophy may be an excess of healthy nutrition, or it may be what we call disease; this distinction being not one whit founded on intrinsic differences, but being an arbitrary distinction of our own according to the usefulness or harmfulness, as observed by us, of the changes which take place. We see a blacksmith's arm enlarge as he works at his forge, and we see a thyroid or a cervical gland enlarge from some cause which we cannot so clearly appreciate. We examine the condition of the tissues in both, and we find in both alike what we call hypertrophy or hyperplasia. The elemental increase in both is identical, but we call the one "healthy action," because we see why it occurs and what a useful purpose it subserves; while we denominate the other "disease," because we cannot see its purpose in the economy, and therefore regard it as hurtful. From hypertrophies to tumours is but another mere step or shade of gradation. Between certain tumours, in their early stage, and products of inflammation, no distinction can be made: and we shall hereafter see how identical in their structure are tubercles with products of inflammation on the one hand and certain tumours on the other. Hence, from the merest hypertrophy up to the most aberrant form of tumour, disease is nothing more than an unusual activity or perversion of the very changes which are ceaselessly going on in the nutrition of the body, and which constitute what we understand by the life of the individual.

Perhaps nowhere is the *birth* of tubercle better studied than in the pia mater of a person who has died of acute tubercular meningitis. A piece of this membrane, spread out and examined under a moderately high power of the microscope, is found to be the seat of an extraordinary cellular increase. The cells and nuclei which mark this increase or proliferation are seen part scattered profusely and at random through the membrane, giving it the well-known milky appearance that it offers to the naked eye, part clustered into little whitish or grayish knots or particles—the individual tubercles. A close scrutiny of these latter will show that they are especially abundant along the smaller branches of the cerebral arteries, and seem here to take their origin in the cells of the connective tissue which composes the adventitia or outer coat of these blood-vessels. So markedly is this the case that, although the tubercle cells must be allowed to arise from the connective-tissue-cells of the membrane generally, yet the adventitia appears to be certainly the part most actively concerned in their development. The tubercle-cells generally are rather smaller than a white blood-cell, have faintly granular contents, and are very brittle, so as very readily to rupture and set free their shining nucleus. Towards the border of the tubercle are seen cells larger than the above, often containing many nuclei, and manifestly representing connective-tissue-cells in a state of active hyperplasia or tubercular development. In and among the cluster of cells which compose the tubercle is a faint stroma, the original connective tissue, together with, occasionally, blood-vessels, not newly formed, for tubercle is non-vascular, but belonging to the tissue in which the tubercle arises. Very much the same appearance is obtained from fine sections of liver or kidney, and from serous membranes affected with tubercle, the connective tissue in all cases serving as the matrix for development. In the lungs, wherever connective tissue is to be found there may tubercles be present: but whether tubercle may also be developed out of the epithelium lining the alveoli and grow inside the air-cells, is a point on which we find our authors at issue. Virchow denies that the little deposits found *inside* the air-passages and air-cells are tubercles. For him these are mere products of inflammation or catarrh which choke up the small bronchi and, on section, so closely resemble tubercle proper that they may be called "spurious tubercle." He professes himself able to distinguish between these spurious tubercles and the true tubercles of the connective tissue, a distinction which, if it be real, must require unusual skill and assurance to make. Villemin, on the contrary, maintains that there are positive intravesicular tubercles, which are no more the products of catarrh than the extravesicular. Careful microscopical observation enables him to assert, that the fine wall which partitions off the air-sacs from one another is not a homogeneous membrane, but encloses in its substance a cellular element peculiar to itself. Under a high magnifying power a beautiful network of capillaries is seen covering this wall, between the meshes of which network he can detect a cellular element occupying nearly the whole of each open space. There is, he asserts, no epithelial lining to the alveoli, but the tubercles are formed out of the above cells, and grow into the cavities of the air-sacs, filling them up. Lebert, again, believes that pulmonary tubercles or granules, as he calls them, take origin indifferently in the cells of the connective tissue or in the epithelium of the alveoli, being both intra- and extra-alveolar. If, he says, you call the little particles outside the alveoli tubercles, you must call those that are within likewise tubercles. They are all a part of the same disease, a result of the self-same irritant. Bizzozero likewise holds that the little intra-alveolar masses which accompany tubercle are really tubercles and spring out of the epithelium that lines the alveoli. In the case of the lymphatic and ductless glands, Virchow, though he alludes on more than one occasion to the closeness of the link which binds the connective tissue with the lymphatic system, yet insists that it is in the connective-tissue-framework of these glands, not in the gland-cells, that the tubercles originate. But Villemin, looking at the connective tissue and lymphatics as parts of one great system, which he designates the "lymphatico-connective system," believes that tubercle may originate both in the cells and in the connective tissue of the lymphatic and ductless glands. Hence, in tubercular disease of these organs, the microscope cannot of itself distinguish tubercle from simple hyperplasia, inasmuch as elements identical with those already existing in health are simply reproduced in excess. It is, consequently, a matter of extreme difficulty, nay, in some cases impossible, to say whether a given change in one of these glands is simple hyperplasia or tubercle. This question, then, must remain for the present an open one. That tubercle originates mainly in the connective tissue is the expressed opinion of all these modern authors. That it may also find a nidus of development in epithelium and gland-cells is probable, but not yet clearly determined.

Tubercle has the shortest *life* of any pathological product; it is born but to die. Hence its power of growth is exceedingly limited, and its hurtfulness, when solitary, but small. Herein, however, consists its deadliness—that it is the expression of a general disease; that the same cause which produces it is circulating through the body, and may produce thousands similar to it wherever connective tissue is present; and that it has, as Virchow was the first to point out, manifestly malignant properties, spreading to the tissues in its neighbourhood and infecting distant organs by dissemination of its germs or juices. Thus, though there is a marked limit to the size of the individual tubercle, there is no limit to the size of the masses that are formed by agglomeration of tubercles, or to the number of tubercles which may be scattered through the body. Do we not see in these properties something that reminds us of the outbreak of an eruptive fever, and something, too, that recalls the behaviour of cancer?

An early *tendency to die* is, then, the most characteristic and distinguishing mark of tubercle. Fatty degeneration, beginning first at the centre of the knot, gradually spreads to the circumference, and gives it the well-known yellow colour from whence the name of "yellow tubercle" is derived; till, little by little, the whole particle softens and breaks down into a "cheesy" granular *débris*. We have, therefore, but to picture to ourselves a large cluster of tubercles passing through this change in the midst of some organ, as the lung or the kidney, in order to understand what wholesale devastation may be thus wrought, and how a cavity or "vomica" may be formed, in which are contained the remains of the softened tubercles, and of the broken-down tissue involved by the tubercles in destruction. On the other hand, if the part diseased be a free surface (as a mucous membrane), the disintegration and softening of the tubercular mass will give rise to the formation of an ulcer which differs from the ordinary process of ulceration by the fact that the base and sides of the ulcer are walled in by the remaining tubercular matter.

Foremost among *the diseases which, either anatomically or clinically, bear decided marks of relationship to tubercle,* stands scrofula. The word "scrofula" seems to have been originally employed to designate a chronic swelling of the cervical lymphatic glands, in consequence of which the neck loses its contours and comes to bear some resemblance to the neck of a "scrofa" or pig. It was then noticed that certain eruptions of the skin or affections of the mucous membrane were accompanied by this same glandular enlargement, and it was agreed to include them too under the title of scrofula. After a while, it followed that every glandular enlargement, no matter where situated, must be a manifestation of scrofula. Certain internal glands, as the bronchial or mesenteric, were now observed to be often in a state of hypertrophy and cheesy degeneration, while, at the same time, tubercles were present in the lungs or bowels. What more natural than to look at the ganglionic lesion and the tubercular products as the common effects of the same cause, scrofula? The thing seemed clear enough, especially when the French school detected the same "specific cells" in the cheesy scrofula of the glands as had been found in the cheesy tubercle of the lungs and intestines. Thus the terms scrofulous and tubercular came to be synonymous, and a scrofulo-tubercular diathesis was a convenient refuge for the destitute in all doubtful cases. Gradually, however, facts in contradiction of this view began to accumulate, and to be noted down by independent observers. Foremost among those who combated the identity of the two diseases was our own Jenner, whose masterly exposition of the subject, ('Med. Times and Gaz.,' March, 1860), is well known to all. Virchow at the same time completed Jenner's clinical picture by upsetting the doctrine of specific tubercle-elements, and showing that the cheesy metamorphosis is peculiar neither to scrofula nor yet to tubercle, but is met with as a termination of simple inflammation, of syphilitic tumours, of farcy-buds, and sometimes of cancer. Both Virchow and Villemin look on scrofula and tubercle as distinct things. In the opinion of both pathologists, scrofula signifies a weak, "impressionable," or "vulnerable" state of the body, in consequence of which, irritants act upon the tissues with unusual severity and *persistence.* From this point of view, a simple glandular swelling following an ordinary irritant is not scrofulous; but when a superficial irritation of the skin or mucous membrane *persists,* and is followed by swelling of the adjacent glands which likewise *persists* even after the primary irritation has been removed, we have a scrofulous person before us. Three great groups of glands may thus be affected with a scrofulous or chronic change,—the cervical, the bronchial, and the mesenteric. Villemin is of opinion that no anatomical distinction can be made between the scrofulous and tubercular gland; that in both there is simple hyperplasia with a retrograde tendency. He thinks that it is in the exciting cause that the true distinction is to be sought; scrofula being a mere tendency to chronic glandular enlargement in consequence of slight external and *local* causes or irritants, in a child whose lymphatic system is unusually impressionable; tubercle being, on the contrary, due to a specific cause or virus which provokes a more *general* outbreak. That which holds good for the glands holds good equally for the bones, in which scrofulous disease is a mere chronic and very persistent inflammation, while tubercular disease is a specific lesion. Scrofula is, further, a disease almost peculiar to childhood; it is, therefore, intimately connected with the development of the body, and manifests its presence at a time when the lymphatico-connective system is in the zenith of its functional activity. Tubercle, on the contrary, prevails between twenty and thirty, at an age when scrofula has ceased to disturb the body. Scrofula is not hereditary as a disease; but the exaggerated lymphatic vulnerability which favours its attack is strongly hereditary. Virchow goes still further, and finds a positive anatomical difference between the scrofulous and tubercular lesion in the diseased gland as well as in the lung. In the gland, scrofula is for him a simple chronic hypertrophy of the glandular tissue generally, with a tendency to "nekrobiosis" or degeneration; tubercle is a true neoplasm forming in the connective tissue only of the gland and not involving, save by extension, the proper glandular structure. In the lung, tubercle is in the connective tissue alone; scrofula, or "scrofulous pneumonia," is a chronic inflammation of the bronchi and air-cells, which become stuffed up with their accumulated contents in a state of cheesy degeneration, and form cheesy deposits of larger or smaller size according to the extent of the inflammation. No one who has had much to do with children's diseases can hesitate to accept the accuracy of this distinction. These cheesy patches in the lungs, accompanied by cheesy degeneration of the bronchial glands, without a trace of anything like a tubercle in any part of the body, are, from time to time, met with. We cannot avoid, therefore, accepting a scrofulous pneumonia in youth as distinct from tubercular disease.[1] At the same time

[1] A very striking example of this scrofulous pneumonia has lately occurred to the writer in the case of a boy, aged 12, who, after having been some months under observation, died and disclosed the following appearances. The right lung was converted, from apex to base, into a solid compact yellow substance, which, in all the lower part, cut firm like new cheese, but towards the apex was softer and more putty-like, breaking down readily under the finger. All round the root of the lung, the large blood-vessels, and the trachea, were masses of enlarged glands,

we agree with Villemin that Virchow and his followers ride their hobby a little too hard in the case of the cheesy infiltrations which accompany real tubercular disease in adults. These, the Germans assert, are not softened tubercular masses, but are mere patches of chronic inflammation that accompany the tubercles, and have passed through the same cheesy metamorphosis that tubercle itself undergoes. It is in vain that you put before them their own assertion that degenerate products of inflammation and degenerate tubercle are exactly similar in appearance, and that these patches are, therefore, at least as likely to be tubercles as inflammatory products, or that you point to the tubercles grouping themselves around these patches, as if ready to form fresh ones. You are told that what you see are not tubercles, but sections of small bronchi filled with catarrhal accumulations. Lebert simplifies this matter most agreeably by assuming that there is no such thing at all as tubercle, but that all these deposits, intra- and extra-alveolar, miliary and diffuse, grey and cheesy, are nothing but inflammatory products. We shall return to this difficulty under the head of inoculation. It is thus seen that tubercle and scrofula, though they have, anatomically considered, certain marked affinities with one another, are yet essentially distinct diseases.

It is in *glanders* that Villemin thinks he has found the closest marks of analogy with tubercle, not only in its anatomy, but also in its symptoms and causation. He seems to have been conducted from the study of glanders direct to the inoculation of tubercle. The characteristic lesion of glanders is a small tubercle which is strewn either in the mucous membrane of the nasal passages, or in the lungs, or, more rarely, in the liver and spleen. At first a greyish-white firm granulation, composed of cells and nuclei apparently developed by hyperplasia of connective tissue, it soon tends to soften centrally and form ulcers on the mucous membrane, cavities in the lungs. Like miliary tubercle, it occurs isolated or in clusters. Together with this little granulation, streaks and bands of fibrous tissue, as well as patches of cheesy infiltration, are not infrequently met with in the lungs of glandered horses. It is interesting, too, that the same doubts have been raised concerning the real nature of these "infiltrations"

yellow in colour, some of them firm like the firmer parts of the lung, others soft like rotten cheese. In the left lung were several patches of the size of a hazel nut, exactly similar in appearance to the firmer part of the right lung; but the rest of the lung was healthy, without a trace of tubercle. The mesenteric glands were in the same state as the bronchial and cervical. The ileum contained a considerable number of ragged ulcers, one of which had perforated and been the immediate cause of death. Neither in the neighbourhood of the ulcers nor in the peritoneum was there a single tubercle. Here was a genuine instance of uncomplicated scrofula, and it seems impossible to deny the existence of scrofula as distinct from tubercle when one encounters such cases as this.

in glanders as in tubercle. They are regarded by Villemin as one form of glanders, just as in man they are one form of tubercle. As to which is the part primarily affected in glanders—the nasal membrane or the lungs, there is some difference of opinion; Virchow maintaining that the deposits in the lungs are always secondary and by metastasis from the nasal membrane; Phillippe and Bouley being convinced by repeated post-mortem examinations that the primary lesions are always in the viscera, more particularly the lungs, and that the formations in the nasal membrane are invariably secondary. If, say they, a horse has the "jetage" (discharge from the nose), he is already thoroughly glandered. It really matters very little which part of the body is first affected. In either case the analogy with a tubercular outbreak remains as strong as can be. The intestinal ulceration of tuberculosis—in which we see the counterpart of the nasal ulceration in glanders—is more often secondary to the pulmonary disease, but occasionally shows itself before any evidence of mischief can be detected in the lungs. Again, glandular enlargement of a severe and persistent kind, constitutes an important part of glanders as it does of tubercle. The mode of invasion is likewise identical in the two diseases; now acute, foudroyant, destroying life in a few days as by an overwhelming blood-poison, now chronic so as to last for years. Further, in the chronic form, the same recurrence of acute attacks complicating and adding to the chronic mischief is observed in glanders as in tuberculosis. To read a description of chronic glanders is, *mutatis mutandis*, to read an account of chronic phthisis. It is, therefore, not surprising that Dupuy goes so far as to say that glanders is a tubercular disease in the horse. In speaking of the supposed causes of tubercle, we propose presently to follow out still further this remarkable thread of resemblance; but for the present it will suffice to say that glanders is transmissible by inoculation, and contagious from horse to horse, and that it is also unmistakeably communicable from horse to man. Can we hesitate to believe, says Villemin, that the parallel between tubercle and glanders must here find its completion? To conclude, glanders and tubercle are so closely akin that they must be looked upon as nearly related species of the same genus.

Syphilitic formations have in certain organs, more particularly the brain, so close a resemblance to tubercle that the one may well be mistaken for the other. We see in both, the same little cells and nuclei forming apparently by proliferation of the connective tissue and heaped together in an inter-cellular substance; in both, too, a tendency to fatty degeneration, though less prominent in the syphilitic gummy tumour than in the tubercular deposit.

Syphilis must therefore take its place with glanders and tubercle as one of a family.

Who is there that has watched a case of acute tuberculosis from its outset, but must have been impressed with the remarkable similarity that it bears to the eruptive fevers, and, more especially, *typhoïd fever*. It is, indeed, doubtful whether in the first few days of the attack it be possible to make a diagnosis in such a case. It is only as the disease makes progress that special symptoms declare themselves, which enable a careful physician to recognise the presence of this most deadly of diseases. The question as to how far tubercle and typhoïd fever are antagonistic is disputed. Villemin is strongly in favour of such an antagonism. He maintains that those who speak of fever as a frequent precursor of tubercle have been led into error by the great likeness of acute tuberculosis in its early stage to typhoïd fever. What they have witnessed has been tuberculosis throughout, and not fever followed by tuberculosis. Murchison's authority is as strongly against any such antagonism. He says:—"Whether it be true or not that persons labouring under phthisis are rarely attacked with pythogenic fever, an attack of pythogenic fever is often followed by tubercular deposit in the lungs." But this is certain, that the post-mortem appearances in the liver, spleen, kidneys, and intestines of persons who have died of acute tuberculosis have sometimes a most striking resemblance to the appearances in the same organs after death from fever.[1]

From *cancer* in its early stages tubercle in its early stages is structurally indistinguishable. Both take origin from the connective-tissue-cells, and tubercle has been already shown to have malignant properties which assimilate it to cancer. There are, likewise, cases of acute general cancerous eruption which have some points of resemblance to acute tuberculosis. Cancer, how-

[1] The writer of this review examined, a few weeks ago, the body of a young woman, aged 23, who died of tubercular meningitis, after an illness of three weeks. In the pia mater were the usual appearances met with in this disease, but the condition of the spleen, liver, and kidneys was quite noteworthy. The spleen was swollen, soft, and rotten, breaking down under the finger exactly like the spleen of typhus. The liver was large and swollen as in cases of blood poisoning. The kidneys were very large, congested, and swollen; the capsule stripping off more readily than natural; the tubules stuffed with cells. In none of these organs was there any tubercle present. An interesting feature in the disease was this, that, at the end of the second week of the disease, the urine having been from the commencement daily examined and no albumen having been hitherto detected, albumen began to appear in considerable quantity, and was detected daily in increasing quantity till the end of the third week, when death ensued. The time of the first occurrence and the amount of the albuminuria, as also the post-mortem appearance of the kidneys, were just what might have been looked for in scarlatina. It seems hard to deny that acute tuberculosis, like acute glanders, finds its natural place in classification among the zymotic diseases.

ever, has an innate vigour and power of growth which sufficiently characterise it as contrasted with the sorry degraded tubercle and its kindred.

In criticising *the influence of supposed causes in the production of tubercle,* Villemin, convinced as he is of the specific contagious character of tubercle, uses all his energies to upset the current doctrines. He first endeavours to show that tubercular disease is not hereditary as a disease, but only as an "aptitude for disease." A child born of parents, both of whom have died of tubercular disease, inherits a very strong tendency to be affected by the tubercular virus, whenever he is brought in contact with it. On the other hand, there are people who will never catch tubercle, just as there are people who will never catch smallpox even when exposed to the action of a concentrated poison. Now this seems to strike at the foundation of the whole system of modern life assurance, which is mainly based upon the acknowledged hereditary transmission of the so-called tubercular diathesis. But not so in reality; for, this aptitude for contagion being strongly inherited, and man being a gregarious animal, and tubercle literally infesting mankind wherever they congregate, so his may be considered the best "life" whose power of withstanding tubercular disease is greatest. There is, therefore, no such thing as a "phthisical temperament" in the ordinary acceptation of the term, and there is no such thing as a conformation of thorax which predisposes to tubercle; the only peculiarities of thoracic conformation connected in any way with tubercle are those which follow as the effects of tubercular disease. Villemin certainly seems here to have overshot his mark, and to be trying to prove too much. For, supposing tubercle to be a zymotic or contagious disease, and supposing it to be thus capable of influencing the offspring through the parents, it seems reasonable to believe that the influence thus propagated would rather tend to prevent the child from catching the same disease during its lifetime than to produce any aptitude for taking the disease. It is difficult to see why he has gone out of his way to argue in favour of any such extraordinary and improbable hypothesis. He need only have looked to syphilis to find a parallel disease which is contagious, but is, nevertheless, communicable as a disease to the offspring. The inherited tubercular type, as sketched by Jenner, is as surely seen and recognised as is the inherited syphilitic type portrayed by Hutchinson. It would be just as ridiculous to say that a child can inherit from its parents an aptitude to catch syphilis, because they have both had the disease, as to affim that an aptitude to catch tubercle can be inherited by the offspring of tubercular parents. Much more probable and convincing are some of the following remarks. And first, as regards the influence of trades and professions on tubercle, there is not, he thinks, any satisfactory proof to show that it is in the exercise of his trade, or in the substances which he handles that the artisan finds the cause of his phthisis. Cold is equally powerless to excite tubercle. The soldier, exposed to all the inclemencies of a campaign, sees phthisis disappear from the ranks, while in the comfort and warmth of garrison life he finds his comrades decimated by it. Both Villemin and W. Budd, ('Lancet,' 1867, p. 452), have hit upon precisely the same arguments in dealing with the topography and geographical distribution of tubercle. Tubercle, like the zymotic diseases, loves low-lying places, and is not met with at certain altitudes: rare at the poles, it increases in intensity towards the tropics, where it becomes exceedingly "malignant." It finds its habitat in crowded places, and is intense in direct proportion to the concentration of the population. This is singularly exemplified in the case of prisons, barracks, manufactories, and convents. Baly, in his observations on the prevalence of phthisis in Millbank, draws a comparison between the mortality there noticed and that in London generally, and shows that phthisis is three times more prevalent in the prison than in the metropolis, the large majority of the prisoners having contracted the disease after their admission. Pietra Santa arrives at the same conclusions in the prisons of France and Algeria. Still more striking is the evidence afforded by barracks. Here we have picked men coming from the country, well cared for, well clad, well fed, in time of peace certainly not over-worked. But yet the mortality among soldiers is decidedly greater than among the rest of the population. On the other hand phthisis spares those who live a nomad life, and visits with less severity soldiers on the march. The very slight mortality from phthisis among the French soldiers in the Crimean War is commented on by Tholozan. Interesting it is too that, as with phthisis in the trooper, so with glanders in the horse, barrack life is alike unfavorable to both. The barrack is to the soldier in the production of phthisis what the stable is to his horse in the production of glanders. Lastly, the fact that tubercle was unknown in certain parts of the world, as the South-Sea Islands and America (Rush), before the contact of the aborigines with Europeans, and that, as Budd points out, tubercle is common along the African sea-board, wherever there has been contact with whites, while it is unknown in the interior where the white man has not penetrated, is a strong argument in favour of the contagious nature of tubercle and its propagation by some virus or germ.

Tubercle is only met with in a limited number of zoological species, and in this respect has a decided claim to be regarded as a specific disease. Thus, glanders is a disease peculiar to solipeds and man. Cholera, measles, and scarlatina are the appanage of mankind. Were tubercle, as some assert, a mere form of inflammation in which the products are characterised by a tendency to degeneration, it is difficult to say why it should be thus restricted to certain species, and why all animals alike should not be subject to its evil influences. The greatest care is necessary in distinguishing genuine from spurious tubercular disease in the lower animals. Thus, in certain animals, as the sheep, it is not uncommon to meet with a disease termed "verminous phthisis," in which symptoms very like those of tubercular disease—cough, dyspnœa, wasting—are present, and in which the post-mortem appearances very closely resemble those in real phthisis. Masses, having the closest possible similitude to tubercle, and, like tubercle, subject to cheesy or calcareous degeneration, are found scattered through the various organs of the body, more particularly the lungs and the liver. A careful microscopical examination will, however, in all cases, reveal the presence of a parasite occupying the centre of the mass, and is therefore indispensable in all examinations of animals that are supposed to be tubercular. Innumerable mistakes have been made in consequence of this omission, and many an animal has been condemned as tubercular which was really infested with parasites. Real tubercular disease is met with in the apes, in cows, in rabbits, and a few other of the rodents.

Pathologists had forgotten or neglected the experiments of Erdt, who, thirty years ago, strewed the lungs of horses with tubercular nodules by inoculating them with a so-called scrofulous matter from men, when Villemin, in 1865, astonished the world by the publication of his *experiments showing the inoculability of tubercle from man to the lower animals and from one animal to another.* His experiments consisted in the introduction of portions of gray or miliary and agglomerated tubercle, and of that which is called the cheesy infiltration, but which, in his opinion, is as strictly tubercular as the rest, beneath the skin of rabbits and guinea-pigs, by means of which he obtained, with scarcely a single failure, the following results:—For two or three days after the operation nothing appeared; but, at the expiration of that time, the part operated on became red; and on the fifth or sixth day a little nodule was felt under the skin, recalling the primary induration of syphilis. This nodule slowly increased in size, and at last, softening centrally, discharged through a hole in the skin a cheesy substance. In some of the experiments, those, namely, where the inoculation was from man to rabbit, the animal did not suffer much in health, but kept its flesh and was at last killed; but in others, those more particularly where the inoculation was from cow to rabbit or from rabbit to rabbit, the animal began, somewhere between the tenth and fifteenth day, to suffer in health, refusing food and slowly wasting till it died, generally before the completion of the third month. There was now found at the seat of inoculation a sore, sometimes covered with a crust, and having a hard base, in, and for some little distance around, which, were distributed numbers of small, miliary, yellowish granulations, single or agminated, in a thickened, sometimes lardaceous connective tissue. The adjacent lymphatic glands were generally swollen and filled with scattered nodules and granulations, sometimes undergoing the regular cheesy transformation, and often connected with the centre of inoculation by hard and knotted cords of lymphatics, whose walls were infiltrated with tubercles, and which recalled the lymphatic cords of glanders. The lungs and other viscera were strewn with tubercles for the most part miliary, but often agglomerated, and occasionally cheesy. He next inoculated a rabbit with tubercular matter from a cow, and produced a still more rapid and general tuberculosis. Again, he inoculated from rabbit to rabbit, taking the matter from the one animal before its heart had ceased to beat, and obtained now a more intense and widespread disease than in any previous experiment. Very interesting also is his statement that the cheesy matter which forms at the seat of inoculation is itself virulent, and produces tuberculosis when inoculated. In like manner, the sputa of phthisical patients and the blood of tubercular rabbits produced, without fail, tuberculosis in rabbits. He draws special attention to the fact that the cheesy pneumonia which accompanies tubercle is just as inoculable as the gray miliary tubercle, perhaps more so, and points to this as strong evidence in favour of all these substances being alike tubercular in their nature. On the other hand, he failed altogether in the inoculation of cancer, of ordinary hepatised lung in pneumonia, and of pus in its different forms. Least satisfactory of all were his experiments with genuine scrofulous matter—*i.e.*, cheesy substance taken from cervical glands in cases where there was some chronic eruption of the skin of the head or face. There is but one trustworthy experiment of this kind recorded, in which the inoculation proved fruitless. These startling results, which we have thought right to detail somewhat at length, were then tested by Simon in this country, who ('Lancet,' 1867, p. 367), corroborated Villemin's discovery in all respects, remarking that both the yellow and the gray tubercle are inoculable from man to the rabbit, and from rabbit to rabbit, with more intense and general results in the latter than the former case. The results of his experiments were even more conclusive than Villemin's, in that he inoculated the smallest possible quantity of tubercular matter, "not more than is employed in vaccination." He concluded that, "whether called tubercle or not, the action must be allowed to be specific." A commission of inquiry appointed by the Pathological Society to report on Simon's specimens, "thought the tuberculous nature of the specimens was beyond reasonable doubt." In like manner, a French commission ('Lancet,' 1867, p. 135) gave Villemin credit for revealing "a fact of the highest interest, the transmission of phthisis by the inoculation of tuberculous matter." Bizzozero's evidence has likewise confirmed Villemin's discovery. After alluding to the little metastic abscesses produced in the lungs of animals by the subcutaneous injection of decomposing fluids, and pointing to the difference which exists between them and tubercles in the eyes of an experienced pathologist, he states that he set himself carefully to ascertain by experiment and microscopical observation—1, whether the neoplasms produced in the lungs by inoculation of tubercular matter are themselves really tubercular; 2, how, supposing them to be tubercular, they originate. 1. He, as well as Biffi of Milan and Mantegazza of Pavia have satisfied themselves that the deposits in the viscera of rabbits inoculated with human tubercle are really tubercles. 2. He next watched the tubercles in different stages of their formation, and after very numerous investigations, concludes positively that the tubercles in inoculated rabbits are formed by proliferation both of the connective tissue and epithelial elements. The abundant nuclei, partly free, partly surrounded with a scanty protoplasma (the tubercle cells), may be seen in process of development in four different parts of the lung—the subpleural and interstitial connective tissue, the adventitia of the blood-vessels, the peribronchial tissue, and the walls of the alveoli. In addition to this, in a lung where tubercle is rapidly developing, the epithelial cells of the alveoli may be also seen in a state of active hyperplastic change; but, after a time, the elements become so crowded and compressed at the centre, that it is no longer possible to mark the limit which separates the small cells of proliferating connective tissue from the larger cells of proliferating epithelium: the two seem to coalesce. Next, Hérard ('Arch. Gén. de Méd.' July, 1867), seeking to verify Villemin's experiments, took seven rabbits and inoculated five only of the seven. Three of these five were inoculated with tubercular granulations, either gray and semitransparent or yellowish, taken from the pleura and peritoneum of a phthisical patient. The two others were inoculated with cheesy matter from what is called catarrhal pneumonia as contradistinguished from tubercle. At the end ot two months, all the seven rabbits were killed. The two which had not been inoculated were in every respect healthy; the two which had been inoculated with cheesy substance were likewise healthy; two of the three which had been inoculated with tubercle were manifestly tubercular. Hérard concludes, first, that tubercle is inoculable from man to the rabbit; secondly, that the cheesy inflammatory products .are not inoculable, and therefore not tubercular; thirdly, that the miliary tubercle is alone inoculable and is the specific lesion of tuberculosis.

Thus far, the evidence of all these observers pointed but to one conclusion—that the disease which we call tubercle in man is directly communicable by inoculation to the lower animals: but the experiments of Lebert and Wyss, performed last year at Breslau, and, more recently, the very interesting facts communicated to the Pathological Society by Sanderson, as also the convincing testimony of Wilson Fox, have thrown a new light on the whole matter. The contributions of Lebert and Wyss are chiefly interesting from the wider range their experiments have taken. For they have employed not only tubercle, and the cheesy products of inflammation in the lungs and lymphatic glands, but also melanoma, cancer, cancroid and sarcoma, and they have compared the results of these inoculations with the effects produced by the injection of simple mechanical irritants, as charcoal and mercury, into the veins of animals. In the first seven of their experiments, rabbits and guinea-pigs were inoculated with various kinds of tubercle, gray or miliary, and yellow or cheesy. In five of these seven, tubercle was unmistakeably communicated. In the sixth, the lymphatic glands in the shoulder were found swollen and filled with yellow matter, but there were no tubercles in the viscera. In the seventh, the rabbit died in a month with an enormous superficial abscess, but with no marks of tubercle. Four inoculations were then made with what are described as cheesy products of inflammation, or patches of degenerate lung-tissue following simple chronic inflammation. In not one of these was tubercular disease produced: in one only was there found cheesy infiltration of some of the lymphatic glands. Pus from an ordinary abscess was injected into the veins of two dogs. In one of these dogs the usual small circumscribed abscesses were found in the lungs, and what are described as small tubercles in the liver. In the other dog, numerous gray half-translucent granulations were found both in the lungs and the liver. Next followed nine inoculations with the secretions from bronchi and vomicæ, muco-purulent and gangrenous sputa. In all of these, save one, death occurred so rapidly (within four or five days), that no results were obtained. The case of a dog is afterwards instanced, in which, some time after a fistulous opening had been made for another purpose, death followed, and there were found, beneath the pleuræ and scattered through the lungs, numerous granulations identical with tubercular granulations. Lebert attributes the formation of the tubercles in this case to the mere traumatic effects of the fistulous opening, and regards the case as a striking argument in favour of his views, to be presently cited, concerning the real nature of tubercle. In one only of three rabbits, inoculated with melanotic cancer from the horse, were small nodules found scattered beneath the skin in the proximity of the wound: in the other two, no results were obtained. One of two rabbits, inoculated with sarcomatous tumour from the tibia of a man, was found, after death, to have diffused throughout the parenchyma and beneath the pleura of its lungs, small "infiltrated spots of tissue," soft in consistence and composed of large, round, well-formed cells, with large round nuclei. The other was unaffected. A dog, into whose jugular vein the juice from a human cancer, diluted with water, was injected, died in fourteen days, and disclosed well-marked cancerous nodules dispersed through its heart and liver. On the other hand, three rabbits, inoculated with cancerous matter from the liver of a woman, gave a negative result. A fourth rabbit, inoculated with the juice from an epithelial cancer of the œsophagus, died at the expiration of a month, and disclosed some small granulations in the lungs, together with a number of superficial abscesses. Lastly, finely divided charcoal and mercury were injected into the veins of animals, in order to compare the effects of mechanical obstruction or embolism of the small pulmonary branches with the so-called tubercles. It was found, according to the statements of these two pathologists, that such an artificial capillary embolism gives rise, by propagated irritation, to changes very nearly akin to those observed in induced tubercular disease. Thus, there were noticed hyperplastic changes in the cells lining the alveoli, in the interstitial connective tissue, and in the adventitia of the smaller pulmonary branches, as well as lobular, and even lobar, consolidation when the obstruction was more considerable, the changes in all these cases being seen to originate in the immediate neighbourhood of the occluded vessel.

Lebert is evidently inclined to think that the general laws of inflammation are dominant in the production of all these morbid appearances after inoculation, rather than that any specific property resides in the inoculated matter. We see, he says, that the same cells (connective tissue) are stimulated into excessive development by the most diverse kinds of irritants, and that like irritants throw into a state of hyperplastic increase different kinds of cells (connective tissue and epithelium). If he is asked the question,—"Have you obtained real tubercle by inoculation," he answers,—"What is real tubercle?" His endeavour throughout is to show that there is no real difference between tubercle as a supposed neoplasm and the products of inflammation, and that the granulation of the so-called tubercle is nothing more than a secondary, often metastatic, product of some primary inflammation. "An undeniable primary inflammation which can show all grades of transition from a small punctate granulation up to an extensive infiltration, and which finds its habitat in the connective tissue as well as in the epithelium, produces ordinarily as a secondary effect, by transport or dissemination, the small granules termed genuine tubercle." Such is his view of the would-be tubercular inoculation. He thus combats the dualistic doctrine, which would separate tubercles proper (miliary granulations) from cheesy products of inflammation (cheesy pneumonia), and thinks that they are both to be regarded, not as parts or stages of a specific affection called tuberculosis, but as effects of inflammation; the pneumonia, or cheesy dépôt, being the primary, and the tubercular eruption the secondary or metastatic, disease. The authority of Dr. Sanderson's name has lately added fresh weight to these views of Lebert's.[1] He, like Lebert, has found that not only will inoculation of tuberculous matter in the rodents be followed, in the large majority of cases, by a diffusion of miliary granulations throughout the various internal organs of the body, but that the self-same results may be also obtained in these animals by any severe and prolonged subcutaneous irritation. The instance of Lebert's dog[2] with the fistulous opening, which, at first sight, seemed so improbable as to be almost incredible, is fully confirmed by Sanderson's experiments of inserting setons in rabbits and producing morbid changes in their internal organs not to be distinguished from those which we have seen to follow after inoculation of tubercle. Sanderson's minute description of the way in which these miliary granulations are developed, bears valuable testimony to the correctness of the account that has been given of the birth of tubercle—an account condensed from Virchow's writings

[1] Dr. A. Clarke deserves the credit of having, even prior to Lebert's publication, stated that he had, by employing other non-tuberculous pathological products, succeeded in producing the same results as could be obtained by the inoculation of tubercle.

[2] That which Lebert here observed in the dog, Barwell had observed in rabbits whose bones had been injured.

on the subject. We find in them, therefore, additional and weighty evidence in favour of the cellular pathology. Lebert, Sanderson, and Fox, working independently and without any knowledge of each other's conclusions, have thus overthrown Villemin's doctrine of specific tubercle-inoculation. They have shown unmistakeably that the inoculation of human tubercle in certain of the lower animals produces in them a condition undistinguishable from general tuberculosis in man, not, however, by virtue of any *specific* properties present in the tubercular matter inoculated, but by the mere irritating effects of the inoculation. When, on the one hand, we find Fox rendering guinea-pigs tubercular, not merely by introducing minute atoms of ordinary putrid muscle, bone, and kidney beneath their skin, but also by *vaccination* and inoculation of pus; or, on the other hand, we hear of Sanderson and Fox causing tuberculosis in these rodents by setons and cotton threads; we must be convinced that it is not tubercle which is communicated, but a series of inflammatory changes of a low kind, which may be set up in these susceptible animals by almost any irritant. At the same time, we cannot, with Lebert, argue from these appearances as to the real nature of tubercle in man: nor can we look at every outbreak of miliary tubercle as a mere metastasis of inflammation: for then we must, in every case of tuberculosis, presuppose the existence of some inflammatory dépôt or centre, from whence such a metastasis of germs can arise. But, as Southey very justly observes—

"In acute tuberculosis, without any local abscess, or any such source of primary infection, we find new growths of apparently the same age and date springing up in various and distant parts: be it granted that this is very rare, still the one single instance would be enough to prove that tubercle was not then produced by the re-absorption of retrograde tissue products into the blood."

We would add that the one single instance would also be enough to prove that tubercle was not then produced by dissemination from a primary focus. This difficulty, however, must remain unsolved, as long as these experiments are performed on the lower animals. The time may, perhaps, be not far distant when our confrères on the other side of the channel, who, in their devotion to science, have not shrunk from syphilis-inoculation in man, will put an end to all doubts by practising tubercle-inoculation either on themselves or on each other. We know that the introduction of setons in men will not cause them to become tuberculous, but we do not yet know whether tubercle is communicable by inoculation from man to man.

Let us now shortly examine *the arguments deduced from these* *experiments, and other facts, in favour of the transmissibility of tubercle by contagion from man to man.* "Phthisicorum cadavera fugi adolescens, fugio etiam senex," wrote Morgagni in the eighteenth century: and now again, after the notion of the contagion of phthisis has been so long scouted from us as absurd, we, in this advanced nineteenth century, are coming round again to the doctrines of the old pathologist. Laennec, J. Frank, Sir James Clark, and Perroud, have all upheld the belief that the inhalations from a phthisical person certainly predispose those who are long submitted to their action to become phthisical: while Andral ('Auscult. Médiate,' t. ii, p. 179) has gone so far as to assert that it is possible for such tubercular emanations to act as a positive source of contagion. Watson's views on this matter, as coming from a true physician, have a peculiar interest. He says, in answer to the question, "is phthisis contagious?" "No, I verily believe it is not." But a few lines farther on we read, "Nevertheless, if consulted on the subject, I should, for obvious reasons, dissuade the occupation of the same bed, or even of the same sleeping apartment, by two persons, one of whom was known to labour under pulmonary consumption." He had here probably in his mind some cases of apparent contagion that he had seen. There has been, doubtless, a latent suspicion in the mind of many a physician that a tubercular husband may infect his wife, or *vice versá*; and that not through the medium of the fœtus, concerning the possibility of which there can be little doubt,[1] but by direct contagion; for there are few practitioners who do not occasionally come into contact with such cases.

If, now, passing over as not trustworthy the facts of inoculation, we consider the very close resemblance which tubercle has to glanders, not only in its anatomy, for this is of small account, but in its symptomatology and in the laws which it seems to obey; if, secondly, we compare typhoid fever with acute tuberculosis; if, thirdly, we put tubercle side by side with other zymotic and contagious diseases in its habitat, its geographical range, and its presence in only a limited number of zoölogical species; if, lastly, we look at the fact, which Virchow, who is no contagionist, was the first to point out, that tubercle will sometimes, more especially in the summer months, occur as an epidemic in its acute and miliary form, it must be conceded that not without reason are men inclined now-a-days to revive the teachings of old Morgagni, and to believe that tubercle will yet somehow or other prove to be really contagious. The strongest argument as yet adduced against any such belief is that which Cotton has employed. He quotes ('Lancet,' 1867, p. 550) statistics from the Brompton Hospital to show that among resident

[1] See this Review, April, 1867, p. 327.

medical officers, chaplain, matron, secretaries, and nurses working in the institution, no evidence whatever of contagion from phthisis is to be found. So that in the place which ought, if phthisis be contagious, to be a very pest-house, contagion is a thing unknown. But what of typhoid fever? Here is a disease which, in most of the metropolitan, and in all the large continental hospitals, is mixed up indiscriminately in the medical wards with every other kind of disease. During a few years' residence in London, Paris, or Vienna, some hundreds of cases of typhoïd fever may be seen scattered hap-hazard among other medical cases; and yet such a thing as contagion from typhoïd fever is so rare that many of the best authorities disbelieve altogether in its possibility. Our greatest authority on fever, Murchison, states (p. 428) that, during fourteen and a half years, while 2,506 cases of typhoïd fever were admitted into the Fever Hospital, eight cases only were reported to have originated there. On the other hand, one case of typhoïd fever shall come to a village where the houses, wells, and cesspools lie close huddled together, and in a few weeks the disease shall have spread from house to house in a way that rivals the contagion of typhus.[1]

As with typhoïd fever, so with cholera, contagion in the wards of a hospital is by no means a marked phenomenon;[2] and yet one case of cholera under the circumstances above mentioned, may spread the disease far and wide in a community which has been exempt up to the time of the new arrival. Now it is only

[1] In the summer of the past year two boys came home from a school in which typhoid fever was prevalent, the one to an isolated farm-house in which there had been no previous history of fever; the other to a large village situated several miles from the farm house. Both boys were attacked with typhoïd fever directly after their arrival. He in the farm-house died. His father, mother, two sisters, three labourers, and a farm boy, who worked in the house, all sickened with typhoïd fever one after another, and two out of the eight died. He in the adjacent village recovered, but his nurse caught the disease and took it home to her cottage, where she communicated it to her own child and to a labourer and his child in the adjoining cottage. Two children, who lived at some little distance from these cottages, but who were in the habit of going backwards and forwards to inquire after the sick people, and who, as was ascertained on inquiry, occasionally had a drink of water in the house where fever was present, next sickened with fever, and from them the fever, cases of which have been till recently (March, 1868) under observation, spread around the neighbourhood. The water in the nurse's cottage, which supplied also the adjacent cottage, came from a well which was only a few yards from, and on a lower level than, the privy, and was found on examination to be so impure as actually to stink.

"Persons labouring under pythogenic fever," says Murchison, "sometimes transport it into localities where it was before unknown, but where it then spreads as from a centre."

[2] "Cholera," says Goodeve (Reynold's 'Syst. of Med.,' vol. i, p. 142), "seldom spreads from bed to bed in a ward; on the contrary, when people are attacked in hospitals, they lie generally in distant corners or in another ward. Cholera does not spread from the sick to the whole by any rapidly acting emanation."

quite recently, and thanks more particularly to the investigations of Snow, Budd, and Acland, that we have learnt to understand how it is that typhoïd fever and cholera may thus be propagated —namely, by contamination of the water-supply: and that which twenty years ago would have been treated as an absurdity is now not only discussed as a matter of course in the medical journals, but has become public property; so that every housewife buys her filter and talks learnedly of disinfection. Again, there can be no doubt that glanders—cousin-german to tubercle —is strictly contagious; and yet how subtle and mysterious are the ways of its contagion. Leblanc enclosed the head of a healthy horse in the same sack with that of a glandered horse, and thus forced the healthy animal to inspire, directly and undiluted, the exhalations of the diseased one for a period of seven or eight hours. In eight horses thus experimented on, not one single instance of contagion occurred, and yet it is certain that glanders is inoculable from horse to horse, and from horse to man, and that it is, in some mysterious way, contagious. We are, therefore, hardly warranted in denying that tubercle is contagious, because we do not see the disease communicated from one to another in hospitals, or in refusing to accept the possibility of its successful inoculation in man because we cannot produce any instances of students who have cut their fingers at post-mortem examinations of tubercular subjects, and have become tubercular. Is it, we ask, too much to hope that we shall ere long get a nearer insight into the real cause of this pest of mankind? that what has happened for typhoïd fever may yet happen in the case of tubercle? and that we shall be able to prevent, or at least to hold in check, the ravages of the one as we shall certainly restrain the spread of the other? We think that such an expectation is by no means Utopian.[1] "By the destruction of the

[1] Since the above was sent to the press, the writer's attention has been called to the highly interesting facts communicated by Dr. Buchanan, in the 'Ninth Report of the Medical Officer of the Privy Council.' It is there clearly shown (p. 17 and 48) that, while typhoïd fever and cholera have been to a considerable extent diminished by the able sanitary administration of our authorities, the mortality from phthisis has been in like manner decidedly reduced. It has been found, to quote Mr. Simon's words, "that the drying of the soil, which has in most cases accompanied the laying of main sewers in the improved towns, has led to the diminution, more or less considerable, of phthisis." No less interesting is the fact announced that "diseases of the lungs, other than consumption, have undergone no regular reduction in their amount;" and "that neither directly nor inversely did the class of lung disease fluctuate according to the fluctuations of phthisis." These facts, though they prove nothing as to either the communicability or non-communicability of phthisis by contagion, do at any rate show that phthisis, in common with typhoïd fever and cholera, can be directly influenced by sanitary improvements; and that the very improvements, which limit the spread of phthisis, have no effect on the ordinary *inflammatory affections* of the lungs.

specific morbid matter of tubercle," says Budd, "as it issues from the body, by means of proper chemicals, and by good sanitary conditions, there is reason to hope that we may eventually, and at no very distant time, rid ourselves entirely of this fatal scourge."

Review IV.

St. George's Hospital Reports. Edited by John W. Ogle, M.D., &c., and T. Holmes, F.R.C.S. Vol. II, 1867. London. Pp. 486.

The publication of hospital reports on the plan of this and the preceding volume, as edited by Dr. J. Ogle and Mr. Holmes, needs no disquisition to prove its utility and value as a means of recording and preserving the results of experience, particularly to those engaged in actual practice; moreover, such volumes serve also as, what may be termed, excellent centres of attraction to old students of the hospital identified with their production, not only by affording them an admirable medium for placing on record the results of observation, but also by inciting them, from an abiding feeling of interest and esteem for their alma mater, to become contributors.

Without further preface, we may observe that this second volume contains twenty-two communications on medical and surgical topics, illustrated, where requisite, by wood engravings. They are of an eminently practical character; some of them are brief clinical records with remarks, whilst a few partake rather of the nature of essays on special subjects. Mr. Prescott Hewett, in continuation of the series of "Contributions to the Surgery of the Head," commenced in the previous volume, leads the way with a very complete notice of *exostoses of the skull,* describing therein the varieties, connections, consequences, origin and treatment of those morbid outgrowths. Among other remarkable forms mentioned is that found in some of the lower animals, and especially the ox tribe, to which from the form it often assumes, and a misconception of its true nature, the name of petrified brain was given. In short, the supposed petrified brain was nothing more than an ivory exostosis of the frontal sinus, which, following a course inward, breaks down the wall of the frontal sinus and projects into the brain-case.

Internal exostoses may give rise to epilepsy and to various cerebral symptoms, of which instances are cited. But the extreme density of exostoses is something remarkable, and many examples are cited in which saws and chisels have failed to make any decided impression upon them, so much so that attempts at their removal by such mechanical means have had to be given up in despair. Mr. Hewitt, indeed, recommends them not to be meddled with at all unless for some very cogent reason. If their removal be decided upon, sawing them off on a level with the skull, when feasible, is perhaps the best way of proceeding, but mostly the use of caustics is required, and is also of much easier application.

Clinical Cases of Insanity is the subject of a short paper by Dr. Blandford. Three cases of chronic insanity are described in which recovery took place after illness of seven, six, and five years' duration, and following these are notes of five instances of mania transitoria or acute delirium, in the production of which epilepsy was considered to be not concerned. The practical question put is—How are such transitory cases to be diagnosed? To this question Dr. Blandford attempts a reply, stating his impression to be that, in such transient cases, the invasion is very sudden, and a definite and sufficient mental cause discoverable.

This paper should be read in conjunction with that by Dr. Handfield Jones, *On Delirium,* found in this same volume (p. 123), under the title of *Reports of Cases of Nervous Disease.* Eight cases are detailed, differing widely among themselves in character and causation. Each case is followed by a commentary, and the paper concludes with some speculations on the cerebral nature of delirium. The point on which Dr. H. Jones wishes to lay stress is—

"that though we have no exact knowledge as to what is the peculiar state of the cerebral tissue which conditionates delirium and convulsions, we are assured that it is, at least in most cases, a very similar if not an identical one. The essential features of delirium are undue excitability and mobility. The very same may be said of convulsions generally; and we may conclude that the difference in the phenomena depends much more on the locality affected and on the special endowments of the tissue than on any alteration in the pathological process itself."

Dr. H. Jones proceeds to enlarge upon this hypothesis of the nature of delirium, and viewing it as a type of *irritation* affecting a certain tissue and locality, justly observes that clinical experience proves to us that irritation

"is not a constant condition, even where its principal phenomena are apparently identical. Thus, taking a very simple and typical instance of irritation, the so-called strumous ophthalmia, where the hyperæsthesia (photophobia) is most intense, we find that the same remedies are by no means always appropriate. Quinine, iron, and cod oil, are successful in many instances, but in others small doses of tartar emetic are of much more avail, as stated by Mr. Chesshire. This is very much what we find to occur in disorders of the hemispheres and other nervous centres, and while it proves, I think, the varying *quality* of morbid states of nervous tissue, it also shows the general similarity of the derangements which occur both in the peripheral and central organs."

Dr. John Charles Hall, of Sheffield, commences, as would appear, a series of papers on the *Diseases of Artisans.* The Sheffield file-cutters' disease is the subject of the present communication. The mode in which the workpeople, chiefly file-cutters, carry on their trade, and the way in which they become exposed to lead by contact and by inhalation of dust, are described; and this description is followed by the record of a few cases of lead-poisoning, exhibited by wrist-drop and other paralytic symptoms and by colic. Dr. Hall is not an advocate for iodide of potassium in such cases, but prefers sulphuric acid with sulphate of magnesia. He appends some useful rules to be observed by the operatives to prevent the "file-cutters' disease."

The next essay, on *Certain Epileptic Phenomena,* by Dr. Edward Fox, is of considerable length, but will repay perusal. Of late years epilepsy has been so largely written about that little remains to be said of the disease, considered from a symptomatic point of view, although ample scope for speculation exists when the intrinsic cause of the malady is made the topic. Dr. E. Fox attacks the subject from both sides, first reviewing the phenomena of epilepsy, and then speculating on their causation. In his apprehension the blood is the true seat of lesion; arterial spasm is the proximate cause of most of the epileptic phenomena, and the pathological appearances met with in the brain of epileptics are the effects of the attacks. As bearing upon the opinions last quoted, we would call Dr. Fox's attention to Mr. Hewitt's memoranda on internal exostosis of the skull as a cause of epilepsy. Blood lesion is undoubtedly but one of many causes.

Mr. G. F. Hodgson narrates an interesting case of *Encephaloïd Disease of a Retained Testicle.* A woodcut is given in illustration, and serves well to display the immense size which the morbid growth acquired before death carried off its victim. Mr. Hodgson enters into the question of operating, and in determining it, examines the records of similar cases. The conclusion is, that an attempt at removal is undesirable where the tumour has attained a considerable size, and especially if the peritoneum have to be opened. Yet if he saw such a case early he would operate, believing, as he does, the doctrines of cancer as laid down by Mr. Moore, Dr. Hughes Bennett, and others. Moreover, he appears to approve of the removal of the testicle when arrested in the groin, quoting in favour of the practice the fact that such testicles are useless, without ability to secrete true spermatic fluid, and that, besides this, they are more prone to cancer than the normal organ, and also will give rise to more suffering when attacked by inflammation of any sort.

Dr. Reginald E. Thompson describes the results of *Thermometrical Observations in Typhoïd Fever*, carried out by himself, and which proved in general confirmatory of those made and recorded by Wunderlich. Dr. Thompson found that the thermometer afforded no reliable criterion for diagnosis between typhoïd fever and certain other diseases, though it sufficed to distinguish it from acute granular kidney, meningitis, and peritonitis, as likewise to appreciate intestinal lesions before they were recognised by the ordinary symptoms. Moreover, the thermograph of typhoïd supplies an additional distinction between this fever and typhus. A table of observations in a case of typhoid fever is appended.

Aphasia and Agraphia constitute the subject of one of the longest essays in the volume. It is a contribution by Dr. William Ogle, presenting a summary of the present state of knowledge of those maladies, accompanied by critical remarks and brief records of cases that have occurred in St. George's Hospital. Dr. Ogle upholds the hypothesis of MM. Dax and Broca, as to the localisation of the faculty of articulate speech in the inferior or third frontal convolution of the left hemisphere, and attempts to overturn the arguments against it advanced by Trousseau and others. To meet the objection that in a double organ so symmetrical as the brain it is hard to conceive a difference of function between its two sides, he makes the most of the absence of exact symmetry in the arrangement of the convolutions; but being dissatisfied, apparently, with the cogency of the argument so derivable, he resorts to the remarkable suggestion of Dr. Moxon, that we may, and, if Broca be right, universally do, educate the left hemisphere as the organ of language in a much higher degree, or well nigh to the exclusion of the right half of the cerebrum. And in support of this notion he appeals to anatomy, which, as he conceives, shows that the left hemisphere receives by the left carotid a more direct, and therefore freer supply of blood than does the right, by reason of the mode of origin of that carotid from the aortic arch. He farther urges that the left hemisphere is more fully developed at an earlier period of existence than the right. We must, however, confess that these anatomical arguments do not at all satisfy our mind, even were they valid, which we doubt; and we are likewise unwilling to believe the right hemispherical frontal convolutions to be in vain and of no purpose in the cerebral economy, an inference Dr. Moxon's hypothesis would involve. If they remain normally so uneducated or so unused, how is it they acquire and retain equal dimensions with those on the left side? We have, however, no space at present to take up with this discussion. In further notice of this essay, we may state that Dr. W. Ogle follows Trousseau in recognising two forms of Aphasia, termed respectively, *Amnemonic* and *Atactic;* the former implying that condition in which appropriate words fail the patient, owing to a defect of memory, the latter failure in the power of co-ordination of the muscles necessary to articulation. The two disordered conditions are frequently associated together.

Agraphia is the term invented to designate inability to express ideas in written language, and this faulty condition is distinguished as of two kinds, just as in the case of aphasia, viz. the amnemonic and the atactic. In the former the patient misuses words, or writes a confused series of letters, having apparently no connection with the words intended; in the latter, the power of writing even separate letters is lost, and only meaningless strokes made with the pen.

Loose cartilages in the knee-joint, especially such as result from accident, are briefly considered by Mr. Brodhurst, who draws the conclusion that, as a rule, they are detached portions of semilunar cartilage, set free within the cavity of the joint. A case is related where the loose cartilage was removed as early as six weeks after the accident producing it.

Mr. Edgcombe Venning opens the question of diagnosis between *infecting and non-infecting chancres,* and, after a brief discussion, illustrated by cases, expresses his belief that the infecting form is always attended with the amygdaloïd enlargement of the glands of the groin, an opinion not many will endorse.

The limits allotted to this review constitute a bar to even the shortest adequate analysis of the contents and conclusions of many of the valuable contributions collected in this volume of reports. We must consequently pass over some of the remaining papers, very unwillingly indeed, with little more notice than that of their titles.

The serious and rare form of morbid growths known as *naso-*

pharyngeal polypi are well described, and their treatment carefully discussed by Mr. Thomas P. Pick. These tumours do not, as generally supposed, invariably grow from the base of the skull, for M. Robert describes one that appeared to be connected with the fibrous tissue in front of the cervical vertebræ. They increase rapidly, and individuals afflicted with them live but comparatively a brief period. To diagnose their nature and attachments is not generally so easy as might be imagined. Various plans of treatment have been suggested and adopted: these Mr. Pick has carefully noted, citing published cases, and in summing up his inquiries remarks:

"That the methods by the palate and by the ligature are inadmissible on account of the fact that the root of the disease cannot be reached. Of the method by the galvanic current we do not possess sufficient experience to speak with any degree of certainty. There remains, then, only the methods by the nose, and by the removal of the superior maxilla. The former of these methods is applicable to and is to be preferred in polypi of small size; whilst the latter must be employed in cases where the tumour is large, and especially where it has prolonged itself in the manner mentioned above."

The liability of such tumours to return after their excision is as yet a problem not satisfactorily solved.

Croup and Diphtheria, and their treatment, is the subject of a communication by Mr. J. W. Haward. This writer advocates tracheotomy early in diphtheria, to avoid the exhausting effects of the dyspnœa, otherwise urgent, and he "goes so far as to think that no child should be allowed to die of suffocation, however bad and hopeless the case; for we have at present no means of judging how far the exudation has extended, and I do not think the existence of bronchitis should exclude the operation." He adds the caution, that the operation should not be hastily performed. The differential diagnosis is well examined.

The *Signification of Skin Affections in the Classification of Disease* is a philosophic disquisition of much merit, by Dr. C. Allbutt, which must be read through to be appreciated. Mr. Nourse narrates more or less briefly some seventeen "*Cases of fever*" of various sorts, with the view of elucidating their origin. A much larger series of cases, however, is needed to give weight to the conclusions he would draw. At the same time it may be remarked, Mr. Nourse's inferences agree in the main with those generally received by the profession.

On the Forms of Pneumonia. This is a contribution of considerable length, wherein the author, Dr. Sturges, examines critically the various morbid conditions comprehended under the general appellation pneumonia. In carrying out this examination Dr. Sturges avails himself of the cases placed on record at St. George's Hospital during the last twenty years, excluding such as were connected with tuberculosis, or with secondary deposit. The cases collected, and which are also most carefully tabulated, exhibited, for the most part, that stage which has been called red hepatisation; and whether they be held to prove the author's deductions or not, they constitute in themselves a monument of his industry, and a repertory of observations valuable to all future inquirers into the pathology of pneumonia.

In the course of the labour of tabulating the cases, Dr. Sturges observes that he

"Soon found that all the fatal instances of so-called pneumonia occurring in a series of years fell naturally, in view of their clinical histories, into four classes. The *first* and largest class would comprise patients who died of tedious and exhausting diseases of whatever kind, such as the constant drain of an abscess ...... or generally where lingering was unusually prolonged, and emaciation extreme. Lung consolidation, indeed, is a familiar appearance in connection with this form of decay......A second class would consist of the subjects of a specific fever, or of some definite affection of a secreting organ, and conspicuously of uræmic poisoning and the poison of typhus......In a third class hepatisation would seem due almost entirely to mechanical causes, and quite independent of any inflammatory affection whatever......Fourthly, hepatisation occurs, there is reason to suppose, as the result of idiopathic inflammation of the lung. It is then invariably connected with pleurisy, and often with pericarditis. It runs a rapid and tolerably uniform course, and would seem to be but rarely fatal."

Having thus indicated four varieties of morbid action, the author proceeds to examine each in turn from a pathological stand-point; and he points it out as a curious circumstance, that wherever recent pericarditis is associated with marked pneumonia, it is always the *right* lung which suffers either mainly or solely.

The general conclusion is, that the term pneumonia, if retained, can apply only to a certain combination of physical signs, which, in themselves, are diagnostic of no one disease or morbid entity. This being so, the "laudable attempt" made of late years by Dr. Hughes Bennett to test the comparative efficacy of various modes of treatment in certain definitive diseases by means of statistics must be, so far at least as concerns the selection of pneumonia for that purpose, both inconclusive and abortive. For

"Simple pneumonia is rarely a fatal disease under any mode of treatment; and as a secondary disease the associations of pneumonia are so many and various, that it seems almost hopeless to attempt a classification sufficiently precise to bring cases into fair comparison. In a word, if the term pneumonia be used to express all the cases of consolidation which I have been noticing, or the majority of them, no disease can so little be treated in this way or that because of its name. If, on the other hand, the word is to be restricted to pure uncomplicated cases of inflammation of the lungs, it is clear that our knowledge at present does not enable us always during life to discriminate such cases. The argument of treatment derivable from tables must concern itself with simpler diseases than this."

Dr. John W. Ogle, one of the editors of the volume, recounts a *case of death from hæmorrhage into the pericardium*, as a result of rupture of one of three true and circumscribed aneurysms of the coronary artery of the heart, and follows with observations on aneurysm or aneurysmal dilatation as a result of embolism or thrombosis. A woodcut shows the position of the aneurysms and of the rupture. The writer carefully examines the relation discoverable between the lesions found after death and the symptoms presented by the patient during life. Cases of aneurysm of the cardiac coronary are very rare, and Dr. Ogle has done good service in culling examples from all available records in illustration of the history and pathology of the lesion, and also in reviewing them in connection with the instance that fell under his own observation. The same writer has likewise contributed a most instructive and copious collection of "*Instances of some of the rarer varieties of morbid growths, swellings, &c., connected with the organs contained within the abdominal cavity,*" and purposes, in a future number, to add to this present collection. He informs the reader that he has gathered the instances now published from the hospital records, and from practice among the out- and in-patients, as illustrating difficult points of diagnosis, or as presenting features of interest as regards morbid anatomy determined by post-mortem research. The cases related, their features during life, and the appearances noted after death, may be divided into three groups:—"1. Affections of the peritoneum, stomach, intestines, liver, pancreas, and lymphatic glands. 2. Affections of the uterus and urinary bladder. 3. Affections of the bones, arteries, &c."

This catalogue of cases offers, we think, a valuable supplement to Dr. Bright's excellent clinical essay on abdominal tumours.

The same storehouse of facts—the case-books of St. George's Hospital—furnishes the other editor, Mr. T. Holmes, with the material for the "*Statistics of Strangulated Hernia*," as adduced to elucidate some facts relative to this lesion and its surgical treatment. This paper will recommend itself to our surgical readers, especially to those who desire to ascertain the results of cases at St. George's Hospital with the view of comparing therewith their own experience.

A case of convulsions, occurring after delivery, is narrated by Dr. A. D. Mackay, who in the accompanying remarks expresses his opinion to be that the puerperal convulsions occurring in women whose urine is albuminous depend on an impure state of the blood, or excrementitious matter in it; and it also appears to be his belief that albuminuria is, as a rule, associated with convulsions, though these do not necessarily follow on its existence.

The volume concludes with reports on the medical and on the surgical cases admitted into St. George's Hospital during the year 1866-67. The medical report is from Dr. Reginald E. Thompson, and the surgical from Mr. Edmund C. Ring, the surgical registrar. Each report is accompanied by copious tables. The first table of medical cases exhibits the nature of the disease, the total number admitted, the total number of deaths, the percentage of deaths, the circumstance of complications with other diseases, and the deaths among complicated cases. To these particulars are added brief observations. A second table sets forth the cases where lardaceous or amyloïd degeneration was discovered after death, and shows the age of the patients, the disease for which they were especially treated, and its duration and symptoms. Subjoined is a notice of the organs found degenerated. A third table is occupied with various annotations on some of the cases of pneumonia admitted during the year. These cases were thirty-one in number. The tables of surgical cases and of operations performed are even more extensive. The first set refer to compound fractures, and indicate their cause—the limb injured, the state of fracture, and the treatment and results. A few brief remarks are appended in a final column. The cases of pyæmia are likewise tabulated in such a way as to convey a clear conception of their history. Last of all comes a tabular statement of operations performed during 1866, divided into classes according as the operations were on the head, neck, or face, on the upper extremity, on the thorax, on the abdomen, on the genito-urinary organs, or on the lower extremity. The surgical report concludes with a tabular outline of the surgical cases admitted during the year 1866.

The medical and surgical registrars of the hospital deserve great credit and encouragement for the immense pains taken by them in framing these tables, and in collecting the large amount of information conveyed in their several columns. Indeed, to these reports of the cases admitted into the St. George's during the year in strict language belong the title of reports of St. George's Hospital. It is these that especially demonstrate the amount of work done—of good effected by the hospital. Many of the other contributions in the volume are founded upon observations made in the course of private practice by former students, and, consequently, in strict parlance, constitute no portion of the St. George's Hospital Reports.

Morever, both Dr. Thompson and Mr. Ring supply notes on very many cases and groups of cases, conveying memoranda of peculiar symptoms, of remarkable pathological conditions, and of particulars of treatment; so that, indeed, the student is supplied with a very excellent sketch of the year's practice, both medical and surgical, at the hospital, whilst every medical man is furnished, both by notes and tables, with particulars and summaries of great value in instituting investigations relative to almost every disease of temperate climates.

We trust that the plan of publishing such volumes of hospital reports may prove no temporary fashion, but will assume a permanent character; a consummation to be attained, however, only by an encouraging list of subscribers.

Review V.

1. *The Physiology of Man, designed to represent the existing state of Physiological Science, as applied to the functions of the Human Body.* By Austin Flint, Jun., M.D., Professor of Physiology and Microscopy in the Bellevue Hospital Medical College, New York, &c. &c. Vols. I and II. New York. 1866 and 1867. Pp. 502 and 556.

2. *A Treatise on Human Physiology, designed for the use of Students and Practitioners of Medicine.* By John C. Dalton, M.D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons, New York, &c. &c. Philadelphia. 1867. Fourth edition. Pp. 695.

3. *Outlines of Physiology, Human and Comparative.* By John Marshall, F.R.S., Professor of Surgery in University College, London; Surgeon to the University College Hospital. Illustrated by numerous woodcuts; in two volumes. 1867. Pp. 607 and 699.

4. *Handbook of Physiology.* By WILLIAM SENHOUSE KIRKES, M.D. Sixth edition. Edited by W. MORRANT BAKER, F.R.C.S., &c. London. 1867. Pp. 802.
5. *Lehrbuch der Physiologie für Akademische Vorlesungen und zum Selbst-Studium.* OTTO FUNKE. 1863-6. Band i and ii.

A Treatise on Physiology for Academic Lectures and for Self Instruction. By Dr. OTTO FUNKE, Professor of Physiology in the University of Freiburg. Fourth thoroughly revised edition; in two volumes. Pp. 1014 and 1182.

6. *Lessons in Elementary Physiology.* By THOMAS H. HUXLEY, LL.D., F.R.S. London. 1866. Pp. 319.
7. *Quain's Anatomy.* Seventh edition. Edited by Dr. SHARPEY, Dr. THOMSON, and Dr. CLELAND. 1864-67.
8. *On the Elimination of Nitrogen during Rest and Exercise on a Regulated Diet of Nitrogen and on a Diet without Nitrogen.* By E. A. PARKES, M.D., F.R.S. Proceedings of the Royal Society, Nos. 89 and 94. 1867.
9. *Lehrbuch der Physiologischen Chemie.* VON Dr. W. KUHNE, Leipsic. Pp. 605. 1868.

Treatise on Physiological Chemistry. By Dr. W. KUHNE.

10. *Archives de Physiologie normale et Pathologique,* publiées par MM. BROWN-SÉQUARD, CHARCOT ET VULPIAN. Paris. 1868. Parts I and II.

Archives of Normal and Pathological Physiology. Edited by MM. BROWN-SÉQUARD, CHARCOT, and VULPIAN.

11. *The Journal of Anatomy and Physiology.* Conducted by G. M. HUMPHRY, M.D., F.R.S., and W. TURNER, M.B., F.R.S.E. Second series. Nos. 1 and 2, with nine plates. Pp. 452. Macmillan and Co. 1868.
12. *Essais de Physiologie Philosophique.* Par M. J. P. DURAND (de Gros). Paris. 1866.

Essays on Philosophical Physiology. By M. J. P. DURAND.

WITH slight modification the old Hanstown motto of

> "Nurembergs hand
> Geht durch alles land

may be applied to physiology. With each succeeding year its hand reaches into more and more distant provinces of science, and its connection with other branches of knowledge becomes more extensive and close. Occupied of old as its etymology implies, with the interpretation of nature in all her varied aspects, its scope subsequently became limited to the consideration of the general phenomena exhibited in the life of animals and vegetables—embracing, therefore, the science of biology as now constituted. Still more recently its aim and object has been held to be restricted to the investigation of the laws of life, and to the description of the functions of the several organs in opposition to their general and minute anatomy. Within the last few years, however, in spite of these limitations, physiology has again begun to extend her relations, and is now most intimately connected with chemistry, botany, and physics. That its progress should be slow is by no means surprising when it is remembered that it is engaged with the examination of incomparably the most complex phenomena of nature, but the very complexity and variability of these phenomena possess a powerful charm for the highest class of minds, and we may refer to the researches of a host of observers on the electrical properties of nerve and muscle, as well as to the recent investigation of Stokes on Cruorine; of Haughton and Frankland on Muscular Force; and of Helmholtz on the Eye and Ear, as evidence of the interest it excites in those whose studies for the most part lie in a different direction, as well as of the light which may be thrown upon its several departments by those who are pursuing widely different lines of inquiry. The direct connection that exists between physiology and pathology, and the evident and immediate bearing that all questions of physiology have on medicine and surgery, is constantly becoming more clearly recognised, and we trust that in a few years the impropriety, to use no harsher term, of permitting a student to enter on the practice of his profession without a sound knowledge of what must always be considered as one of its most important bases will no longer exist.

The two works at the head of our list by Dr. Austin Flint and Dr. Dalton are highly creditable to our American brethren. Both constitute excellent text-books of physiology, well arranged, perspicuously written, and enriched by original observations. The treatise of Dr. Flint is as yet incomplete, the two first volumes only having been published; but if the remaining portions are compiled—for every physiological work embracing the whole subject must be in a great measure a compilation—with the same care and accuracy, the whole may vie with any of those that have of late years been produced either in our own or in foreign languages. Dr. Flint is already favorably known as the author of various physiological essays published in the 'American Journal of Sciences;' and as he occupies the important posts of Professor of Physiology and Microscopy in the Bellevue Hospital Medical College, New York, and in the Long Island College Hospital, it is natural that he should furnish his pupils with a text-book as a supplement to his lectures. We may remark in passing that there are some advantages as well as disadvantages connected with the publication of such a work as the present, in parts. On the one hand, it enables the author to round off each portion perfectly, making every section complete in itself, whilst a sheet or two more or less is a matter of little consequence. On the other hand, the exhaustive manner in which he is tempted to treat the particular subject to which he is devoting his attention, necessarily takes up much time; and when he has completed it, the reading, repetition of experiments, &c., required for the next part, delays the progress of the whole work to so great an extent, that the first sections published are rarely up to the time, especially in so progressive a science as physiology. A well-known instance of this occurred in the case of Dr. Todd's 'Cyclopædia;' and in spite of the originality as well as the acumen of the authors, was also perceptible in Messrs. Todd and Bowman's Physiology, the publication of which extended over some fifteen or sixteen years. The first volume of Dr. Flint's work, though published in 1866, has a preface with the date of October, 1865, and the second, of June, 1867, which, certainly considering the labour involved, implies very steady application ou the part of the author; but there still remain the two most difficult sections in the whole range of physiology, the nervo-muscular apparatus and reproduction, to be considered; these will, doubtless, occupy two if not three more volumes; and allowing the same space of time between the appearance of each, Dr. Flint will have concluded his labours at the end of 1870, an interval of at least five years occurring between the commencement and the termination of the same edition—a period which is sufficient, as is actually the case with the extensive subject of the nutrition, and microscopic anatomy of muscle, very materially to modify the statements made in the earlier parts of the work.

The first volume of Dr. Flint's treatise commences with a description of the saliva and organic constituents of the body, which, on the whole, is sufficiently good. The remarks on the sugars may be taken as an example of the style of these introductory sections. After giving an account of the composition and properties of the various saccharine substances met with in the body and in the food, the several tests are fully given, including Moore's, Trommer's, Barreswil's, Maumenè's, the fermentation test, and the evidence derived from the growth of torulæ. The origin and functions of sugar are then briefly discussed, and he concludes by remarking that, in the present state of science, we are justified in saying that sugar is important in the process of development and nutrition at all periods of life, though the precise way in which it influences these processes is not fully understood. Dr. Flint makes no mention of Dr. Pavy's observations in regard to glycogene, which are certainly deserving of consideration, but holds with Bernard, that sugar is "continually manufactured in the economy by the liver, whence it is taken up by the blood passing through this organ. It disappears from the blood in its passage through the lungs. Sugar is found, then, in the economy constantly in the substance of the liver, in the blood coming from the liver, and in the blood of the right side of the heart; and after the ingestion of saccharine or amylaceous articles of food in the blood of the portal vein. It is not found in other organs, nor does it normally exist in arterial blood." A few of the details of the introduction appear to us to be somewhat antiquated; thus Dr. Flint still admits the existence of margarine and margaric acids, though it is now well known that the former is only a combination of tri-palmitin and tri-stearine. So also the essential albuminous constituent of muscle called by Dr. Flint musculine, and by which we suppose syntonine is meant, is dismissed in a dozen lines, no reference being made to myosin; nor do we find any mention made of myelin. The account of fibrine, again, is scarcely up to the present time, no notice being taken of C. Schmidt's important experiments, confirming and extending the older ones of Dr. Buchanan, of Glasgow, and now very generally admitted to the effect that fibrine is composed of a fibrinogenous and of a fibrino-plastic substance, both of which exist in a free state in the blood during life; but after death, or when the blood has been removed from the body, and under various other conditions, unite to form the coagulating material.

The remainder of the first volume is taken up with the blood, circulation and respiration. To each of these is prefixed a succinct and well-selected historical sketch, though we are rather in doubt whether the space thus occupied might not have been advantageously engaged with the consideration of points of minute anatomy, that are only glanced at, or in giving more fully the bearings of physiology on pathology and medicine. Dr. Flint, in common with most modern microscopists, holds that the red blood-corpuscles possess a perfectly homogeneous structure, presenting no nuclei or granules, and being destitute of an investing membrane; but we are surprised to find he only alludes, in the most cursory manner, to the crystallisation of the blood, the few lines devoted to it occurring in a note, and the credit of their discovery being attributed to Sir Everard Home, whilst their nature is most incorrectly given. The importance that the forms of the crystals obtained from blood-spots may possess in medico-legal investigations, apart from the great interest attached to the crystallisation of a substance so nearly allied to albumen, should have led to its introduction into the text, and to references to some more recent chemical handbooks than those of Robin and Verdeil's 'Chemie Anatomique' and Nysten's 'Dictionary,' published in 1858. Dr. Flint has paid considerable attention to the various modes of analysis of the blood, and gives the following as the result of certain modifications which he has suggested.[1] It will be seen that it differs to a considerable extent from the tables given by other observers. We have slightly condensed the proportions of the salts. The blood was taken from a healthy male, aged 27.

Composition of the Blood.

Water	154·870
Corpuscles	495·590
Albumen	329·820
Fibrin	8·820
Serolin ?	0·025
Cholesterine	0·125
Oleate margarate and stearate of soda	1·400
Chloride of sodium Chloride of potassium (a trace)	3·500
Phosphate of lime and magnesia, other salts, carbonate of soda being the most abundant	2·500
Iron	0·550
Extractives	2·450
	1000·000

For purposes of comparison, the fibrin, albumen, and corpuscles, were desiccated and weighed, giving the following proportions of dry residue:

Fibrin	2·50	parts per 1000 of fresh blood.
Albumen	71·53	" "
Corpuscles	125·00	" "

The chapter on circulation commences, as we have said, with an historical sketch of the subject from the time of Hippocrates to the present day, the foremost place being justly assigned to Harvey, whose descriptions and experiments are freely quoted, and acknowledged to be models of simplicity and pertinence. The physiological anatomy of the heart is fully given, and reference made to the observations of Hiffelsheim and Robin, who endeavoured to ascertain the capacity of the cavities by injecting them with wax, in an animal recently killed, subsequently calculating the amount of liquid displaced by the moulds. He states that these observers found the capacity of the right auricle and ventricle to be respectively from one tenth to one third greater than the left auricle and ventricle; that the capacity of each ventricle exceeded that of the corresponding

[1] See also 'Amer. Journ. of Med. Sci.,' Oct., 1863.

auricle by from one fourth to one third, and that the absolute capacity of the left ventricle is from 4·8 to 7 ounces; but, he continues, all we can say is that from observation on the heart during its action, it never seems to contain much more than half the quantity in all its cavities that it does when fully distended by injection; but it is the right cavities which are most dilatable, and probably the ordinary quantity of blood in the left ventricle is within one fifth or one sixth of its extreme capacity. In opposition to the statement of Kölliker, but, we think, correctly, Dr. Flint considers that the fibres of the heart are destitute of sarcolemma; he gives the old drawing from Kölliker, showing the anastomosing character of the fibres; but since the period at which the publication of the work has occurred, Eberth has proved that the muscular fibres of the heart closely resembles those of the unstriped variety, being broad, flat fibres, which are branched and divided at their extremities, but the processes of which do not actually fuse with one another. Several pages are occupied with a discussion of what was long ago clearly explained by Dr. Halford, now professor of physiology at Melbourne, namely, the apparent elongation and real shortening of the heart's action, the apparent elongation being due to the elastic recoil of the vessels at the base of the heart. As regards the impulse, we are at issue with Dr. Flint, who describes it as produced by a direct blow from the apex of the heart. "If," he says, "the impulse of the heart be felt while the finger is on the pulse, it is evident that the heart strikes against the thorax at the time of the distension of the arterial system. The impulse is due to the locomotion of the ventricles;" and he quotes an observation of Harvey's, to the effect that at the moment of systole the heart is erected, and rises upwards to a point, so that at this time it strikes against the breast, and the pulse is felt externally. Notwithstanding the high authority of Harvey, we are convinced, from personal observation, that there is no such thing as a blow given by the heart against the parietes of the chest, in the sense of the whole musculature of the organ being withdrawn from the ribs, so as to leave a space in diastole, and then being suddenly shot out in systole. It is simply a case of suddenly increased pressure giving the impression of a blow, the conditions being almost precisely identical with those which are perceived when the finger is placed on the cheek, and the masseter muscle is caused suddenly to contract. Moreover, the sensation of a blow is occasioned, not by the apex, but by the anterior surface of the heart, nearly an inch above the apex, the apex, as Dr. Flint previously correctly states, being slightly moved from left to right. The account of the succession and duration of the movements of the heart is mainly taken from

Marey and Chauveau, and he summarises their observations in the following propositions :

> "*Auricular Systole.*—This occupies two tenths of the heart's action. It is feeble compared with the ventricular systole, and relaxation immediately follows the contraction.
>
> "*Ventricular Systole.*—This occupies four tenths of the heart's action. The contraction is powerful, and the relaxation sudden. It is absolutely synchronous with the impulse of the heart.
>
> "*Diastole.*—This occupies four tenths of the heart's action."

In a recent essay by Donders, translated in the January number of the 'Dublin Medical Journal,' the whole period of activity of the heart is estimated at rather less than one half of an entire cardiac revolution; the period of diastole would, therefore, be rather more than five tenths, the actual proportion being as ·428 : ·572.

Dr. Flint points out what we do not remember to have elsewhere seen any notice of—a safety-valve action at the orifice of the pulmonary artery, which he believes to be as important as that ascribed by King to the tricuspid valve, and, indeed, even more important in protecting the lungs than the insufficiency of the latter. It may be shown by cutting away a portion of the ventricles in the heart of a large animal, securing the nozzles of a double syringe in the aorta and pulmonary artery, and forcing water into the vessels. In performing this experiment it will be noticed that, while the aortic semilunar valves oppose the passage of the liquids so effectually that the aorta may be ruptured before the valves will give way, a considerable degree of insufficiency exists, under a high pressure, at the orifice of the pulmonary artery.

The relative duration of the sounds of the heart and of the period of silence is stated to be such that the first sound occupies the period of the ventricular systole or four tenths of the heart's action, the second sound about three tenths, and the repose three tenths. Perhaps this is accurate, though most of the text-books consider the pause to occupy as much time as the two sounds together.

The causes of the first sound are stated to be complex, and to include the closure of the auriculo-ventricular valves, the muscular sound, and the impulse of the heart against the walls of the thorax. These statements would drive Dr. Billing and Dr. Halford nearly wild. These gentlemen would object to the term "closure of the valves." In their opinion it is the sudden tension, not the closure which has already occurred, of the valves that produces the sound; and they would deny alike the muscular sound, and especially the impulse, seeing that the heart never leaves the inner surface of the chest. There can be no doubt, however, we think, that the muscular sound is a genuine element of the first sound; but we are disposed to agree with them, in opposition to Dr. Flint, in ignoring the impulse as a cause of sound, and in admitting the tension of the valves as the essential cause of the first sound.

Dr. Flint, sen., whose essay gained the prize of the American Medical Association in 1858, is of opinion that auscultatory experiments may be made by which all but the valvular element of the first sound may be eliminated, and the character of the first sound is thus found to resemble that of the second. Thus:

"1. If a folded haukerchief be placed between the stethoscope and integument, the first sound is divested of some of its most distinctive features. It loses the quality of impulsion, and presents a well-marked valvular quality.

"2. In many instances, when the stethoscope is applied to the præcordia, while the subject is in a recumbent posture, and the heart by force of gravity is removed from the anterior wall of the thorax, the first sound becomes purely valvular in character, and as short as the second.

"3. When the stethoscope is applied to the chest a little distance from the point where the first sound is heard with its maximum of intensity, it will present only its valvular element."

Dr. Flint then quotes Chauveau and Faivre's experiment, in which the first sound was abolished by the introduction of a wire ring through a little opening in the auricle into the auriculo-ventricular orifice, so arranged as to prevent the closure of the valves. "But," he continues, "whilst these observations settle beyond question the fact that the closure of the auriculo-ventricular valves produces one element of the first sound, there are other and less prominent elements which serve to give it its prolonged and booming character." This, we confess, we do not quite comprehend. It seems to us that if the first sound of the heart is really abolished in Chauveau's experiments, the heart still continuing to beat, there is no necessity for curiously investigating the possibility of other causes, since this one, the sudden tension of the auriculo-ventricular valves, has been shown to be sufficient.

Dr. Flint sides with those who maintain that the activity of the pneumogastrics is not affected by woorara, at least until a much later period than the motor nerves of the body generally, and he gives the following interesting experiment in support of his views:

"Desiring to demonstrate to the class at the New Orleans School of Medicine the action of the heart in the alligator, a specimen, six feet in length, was poisoned with woorara, and the heart exposed. The animal came under the influence of the poison in about thirty minutes, when the dissection was commenced, and was quite dead when the heart was exposed. The pneumogastrics were then exposed and galvanised, with the effect of promptly arresting the action of the heart. This observation was verified in another experiment. We were at first at a loss to account for the absence of effect of the woorara on the motor filaments of the pneumogastric nerves; but on reflection thought it might be due to slow absorption of the poison in so large a cold-blooded animal. With a view of ascertaining whether there is any difference in the promptness with which different nerves in the body are affected by this agent, we made the following experiment upon a dog. The animal was brought under the influence of ether, and the heart, the pneumogastrics and the sciatic nerve were exposed. Galvanisation of the sciatic produced muscular contraction, and of the pneumogastrics arrested the heart promptly. A grain of woorara dissolved in water was then injected under the skin of the thigh. One hour after the injection of the woorara the sciatic was found insensible to the action of galvanism, but the heart could be arrested by galvanisation of the pneumogastrics, though it required a powerful current. A weaker current diminished the frequency, and increased the force of its pulsations. In this experiment, the operation of opening the chest undoubtedly diminished the activity of absorption of the poison, and consequently retarded its effects upon the nervous system. Taken in connection with the observations on alligators, it shows that the motor nerves are not affected at the same time, and that the pneumogastrics resist the action of this peculiar poison after the motor nerves generally are paralysed. This shows a conservative provision of nature which guards particularly the important influence exerted by these nerves upon the heart."

In the description of the capillary system, we observe that Dr. Flint denies the presence of an epithelial lining to the capillary, though this was proved to exist by Dr. Eberth in the early part of 1865, and is now admitted by all microscopists, since it may easily be shown by Recklinghausen's plan of injection with nitrate of silver. He gives some important experiments corroborating the view of Dr. Reid, that in asphyxia the blood experiences a difficulty in traversing the systemic capillaries. If the medulla oblongata of a frog be broken up, and the capillary circulation of the web be watched, no important change occurs; but if the cutaneous surface be then coated with collodion, excepting only the web under examination, an immediate diminution in the rapidity of the circulation is observable, and at the expiration of twenty minutes it will have wholly ceased. If now the entire coating of collodion be peeled off, quite a rapid circulation commences, though it soon again begins to decline, and in twenty minutes will have almost ceased. These experiments, as Dr. Flint observes, taken in connection with observations on the influence of asphyxia upon the arterial pressure, conclusively show that non-aërated blood cannot circulate freely in the systemic capillaries. At the same time he is a disbeliever, and we are much disposed to agree with him, notwithstanding the ingenious arguments adduced by Dr. Carpenter and others in favour of the opposite view, that there is any such thing as a capillary power. He believes that physiologists generally who have admitted this power have not appreciated the action of the arterioles, the contractility of which is competent to produce all the variations which are observed in the capillary circulation.

The section on Respiration contains little requiring notice here. The author approves of and corroborates the views of Dr. Sibson on the functions of the intercostal muscles, holds with Robin, we think incorrectly, that the black appearance of the human lungs is due to the presence of carbon, which has been inhaled and deposited in the parts where it is found, and gives various original observations made by his father. In reference to the auscultatory phenomena accompanying the act of respiration, Dr. Flint has some interesting remarks, and has made some original investigations on the location and cause of the respiratory sense. He observes—

"1. If the chest be opened in a living animal, and artificial respiration be carefully performed, inflating the lungs sufficiently but cautiously, and taking care to change the air in the bellows every few moments, as long as this is continued the animal will make *no* respiratory effort, showing that for the time the respiratory sense is abolished.

"2. When the artificial respiration is interrupted, the respiratory muscles are thrown into contraction, and the animal makes regular and at last violent efforts. If we now expose an artery, and note the colour of the blood as it flows, it will be observed that the respiratory efforts only commence when the blood in the vessel begins to be dark. When artificial respiration is resumed, the respiratory efforts cease only when the blood becomes red in the arteries. The invariable result of this experiment seems to show that the respiratory sense is connected with a supply of blood containing little oxygen and charged with carbonic acid, to the systemic capillaries by the arteries, and that it varies in intensity with the degree of change in the blood.

"3. If while artificial respiration is regularly performed a large artery be opened, and the system be thus drained of blood, when the hæmorrhage has proceeded to a certain extent the animal makes respiratory efforts, which become more and more violent, until they terminate just before death in general convulsions. The same result follows when the blood is prevented from getting to the system by applying a ligature to the aorta. These facts, which may be successively observed in a single experiment, remain precisely the same if we previously divide both pneumogastric nerves in the neck, showing that these are by no means the only nerves which convey the respiratory sense to the medulla oblongata. The conclusions which may be legitimately drawn from the above-mentioned facts are the following:

"The respiratory sense has its seat in the system, and is transmitted to the medulla oblongata by the general sensory nerves. It is not located in the lungs, for it operates when the lungs are regularly filled with pure air, if the system be drained of the oxygen carrying fluid. It is due to a want of oxygen on the part of the system, and not to any fancied irritant properties of carbonic acid; for when the lungs are filled with air, and the system is gradually drained of blood, though all the blood which finds its way to the capillaries is fully oxygenated, as the quantity becomes insufficient to supply the required amount of oxygen, the sense of want of air is felt, and respiratory efforts take place. The experimental results on which these conclusions are based are invariable, and have been demonstrated repeatedly; so that the location of the respiratory sense in the general system, and the fact that it is the expression of a want of oxygen, seem as certain as that oxygen is taken up by the blood from the lungs, and distributed to the tissues by the arteries. With this view we can explain all the reflex phenomena, which are connected with the respiratory function.

"The supposition of Bérard, that the respiratory sense is due to distension of the right cavities of the heart, is disproved by the simple experiment of sudden excision of this organ. In that case, as the system is drained of blood, efforts at respiration invariably take place, though the supply of air to the lungs be continued."

In the second volume of his work Dr. Flint treats of alimentation, digestion, and absorption, and of the lymph and chyle. He commences with a general account of hunger and thirst, and of the nature of the different kinds of food in which the sugars, oils, and albuminous compounds are again considered, but in reference only to their dietetic characters; and two excellent chapters follow on compound alimentary substances in which the comparative value of meals, eggs, milk, and animal food generally, is contrasted *inter se*, and with the various kinds of vegetable diet; whilst full consideration is given to the various beverages in common use. He remarks that "the flesh of various non-domesticated animals is esteemed highly as food. In some parts of this country buffalo meat is largely used. This is somewhat coarser and of a more decided flavour than beef, but does not differ in its physiological properties. Venison is a meat very highly esteemed. This resembles mutton, but as a constant article of diet is by no means as agreeable. The flesh of the wild boar is used as food in many European countries. It is darker and more highly flavoured than ordinary pork, and is generally regarded as a delicacy. In this country the racoon (Procyon lotor), the woodchuck (Arctomys monax), and the opossum (Didelphys Virginiana), are occasionally eaten. These can hardly be ranked among the delicate varieties of game. They are not, however, unpalatable, but are excessively fat."

In regard to the use of alcohol, Dr. Flint holds with MM. Lallemand, Duroy, and Perrin, and in opposition to Dr. Anstie, that a considerable proportion of alcohol ingested into the stomach is eliminated by the skin, lungs, and kidneys. He considers there can be no doubt that it may temporarily give tone and vigour to the system when the energies are unusually taxed, and that it retards the destructive assimilation of the tissues; but that it cannot be regarded as an alimentary principle, that if its use be long continued it weakens the assimilative power of the system, and that under ordinary conditions, where the organism can be adequately supplied with food, it is undoubtedly injurious. He states that "in many parts of the United States the manufacture of wine from native grapes has assumed considerable importance. The Catawba wines of Ohio, the California, and the North Carolina wines have become quite celebrated. Though these are of rich flavour, and possess many good qualities, it will be many years before wine can be produced in this country equal in delicacy to the products of the vineyards of the old world."

In his observations on mastication Dr. Flint states that the upper jaw undergoes a slight movement of elevation in opening the mouth; which becomes somewhat exaggerated when the mouth is opened to the fullest possible extent, and he refers to an experiment suggested to Monro by Pringle, namely, that if the blade of a knife be so placed as to correspond exactly with the line of contact of the teeth, it may be observed in a mirror that the upper teeth are sensibly elevated every time the mouth is opened. This movement, he considers, may be partially effected by the posterior belly of the digastric, but probably, also, by the contraction of muscles placed too deeply to be explored experimentally, but is certainly not due to the contraction of those of the posterior muscles of the back, which have for their chief function the elevation of the head.

The account of the saliva and of the act of deglutition are very fully and completely given, and there is little to add to them.

The description of the stomach, gastric juice, and gastric digestion is exceedingly good, the only point of importance omitted being, we think, the characters of pepsine itself and the mode in which it may be obtained by precipitation successively with phosphate of lime and with cholesterine. The question of the nature of the acid of the gastric juice, which is treated of at considerable length, is admitted to be still doubtful. "On what," he remarks, "does the acidity of the gastric juice depend? This is the simple question to which the foregoing discussion naturally leads, and it is one which can be answered almost with positiveness, though it is not settled to the satisfaction of all physiologists, since there are some conflicting observations which can be harmonised only by new researches. Aside from the conditions under which acids, such as the acetic, butyric, or the lactic are developed from articles of food taken into the stomach, the evidence is strongly in favour of free lactic acid as the principle on which the gastric juice mainly and constantly depends for its acidity. There also exists a certain proportion of the biphosphate of lime, and this is the only condition in which a phosphate of lime can exist in the presence of free lactic acid. The observations of Bidder and Schmidt indicate apparently a quantity of chlorine in the gastric juice, not to be accounted for by the proportion of bases obtained by ultimate analysis. There is evidence sufficiently positive to show that there is no hydrochloric acid in the gastric juice in a condition which allows the fluid to present the reactions which are observed when the acid exists in a free state. If there be any hydrochloric acid not in combination with metallic bases, it is united with organic matter in such a way as to prevent the manifestations of its ordinary properties, excepting that of acidity. The fact that some of the mineral acids can be made to unite in this way with albuminoid substances lends colour to this supposition; although further investigations are necessary to demonstrate that this takes place in the gastric juice."

The section on the nature and properties of the bile is very well drawn up, and some valuable experiments, which it is interesting to compare with those of Dr. Dalton, are given. In one case, after the formation of a biliary fistula in a dog, the abdomen was somewhat tumid, with some rumbling in the bowels for five days. "The first alvine discharge took place on the evening of the second day. The fæces seemed in all regards normal. After that time they became very infrequent, though the animal ate very well every day. The fæces that were passed after the third day were of a greyish colour and moderately soft. They had an exceedingly offensive and penetrating odour. At about the fifteenth day the fæces became more frequent, and from that time were passed three or four times a day. Generally they were clay-coloured, but on one or two occasions were quite dark. They always had a peculiarly offensive odour; the weight of the animal remained stationary for about four days." It then, with some interruptions, progressively diminished until death occurred thirty-eight days after the operation, the loss of weight amounting altogether to $37\frac{1}{2}$ per cent. His appetite was good throughout, and towards the later periods ravenous. He also became very cross, snapping at every animal that came near him; but there never was any icterus, fœtor of the breath, or falling off of the hair.

In reference to the variations in the flow of bile with digestion, he gives a table in which are set down the quantities of bile discharged in thirty minutes at various periods after feeding, and he gives the following conclusions. Disregarding slight variations which might be accidental, "it may be stated in general terms that the bile commences to increase in quantity immediately after eating, that its flow is at its maximum from the second to the eighth hour, during which time the quantity does not vary to any great extent; after the eighth hour it begins to diminish; and from the twelfth hour to the time of feeding it is at its maximum. The experiments of Dr. Dalton, made on a dog with a fistula into the duodenum, show that the bile passes into the intestine in by far the largest quantity immediately after feeding, and within the first hour." Dr. Flint adds in a note that, according to Bidder and Schmidt, the flow begins to increase about two hours after feeding, its maximum being from twelve to fifteen hours after; whilst Arnold found the maximum to occur soon after feeding, decreasing after the fourth hour, and Kölliker and Müller found the maximum to be between the sixth and eighth hours.

We observe that Dr. Flint does not mention Dr. Brinton's views on the nature of antiperistaltic action, but he gives a good account of vomiting; and here, as elsewhere, he seems to have very conscientiously referred to original authorities, and is thus enabled not unfrequently to rectify errors that have appeared in edition after edition of other well-known works.

Considering the importance of the epithelial cells covering the villi, we are somewhat surprised to find no description of their peculiarities. It is a subject that is at the present time attracting much attention, and recent researches and microscopical investigations will, doubtless, lead to very important modifications of the modern views respecting the nature of absorption.

After noticing excretine, Dr. Flint gives an interesting description of a new substance, to which he has applied the term "stercorine:"

"This principle, which we discovered in the fæces in 1862, was described by Boudet in 1833, as existing in excessively minute quantity in the serum of the blood, and was called by him seroline. As we found it to be the most abundant and characteristic constituent of the stercoraceous matter, we proposed to call it stercorine, particularly as our researches led us to the opinion that it really does not exist in the serum, but is formed from cholesterine by the processes employed for its extraction. Stercorine may be extracted in the following way: the fæces are first evaporated to dryness, pulverised, and treated with ether. The ether extract is then passed through animal charcoal, fresh ether being added until the original quantity of the ether extract has passed through. * * * The ether is then evaporated, and the residue extracted with boiling alcohol. This alcoholic solution is evaporated, and the residue treated with a solution of caustic potash for one or two hours at a temperature a little below the boiling point, by which all the saponifiable fats are dissolved. The mixture is then largely diluted with water thrown upon a filter, and washed until the fluid that passes through is neutral and perfectly clear. The filter is then carefully dried, and the residue washed out with ether. The ether solution is then evaporated, extracted with boiling alcohol, and the alcoholic solution evaporated. The residue of this last evaporation is pure stercorine.

"When first obtained, the stercorine is a clear, slightly amber-coloured, oily substance, about the consistence of the Canada balsam used in microscopical preparations. In four or five days it begins to show the characteristic crystals. These are few in number at first, but soon the entire mass assumes a crystalline form. In our analysis we obtained from seven and a half ounces of normal human fæces (the entire quantity for the twenty-four hours), 10·417 grains of stercorine, the extract consisting of nothing but crystals. This was all the stercorine to be extracted from the regular daily evacuation of a healthy male twenty-six years of age, and weighing about 160 lbs. In the absence of other investigations, the daily quantity of this substance excreted may be assumed to be not far from ten grains. In many regards stercorine bears a close resemblance to cholesterine. It is neutral, inodorous, and insoluble in water and in a solution of potash. It is soluble in ether and hot alcohol, but is almost insoluble in cold alcohol. A red colour is produced when it is treated with strong sulphuric acid. It may be easily distinguished from cholesterine, however, by the form of its crystals. It fuses at a low temperature, 96·8 Fahr., while cholesterine fuses at 293° Fahr. Stercorine crystallises in the form of thin delicate needles, frequently mixed with clear rounded globules, which are probably composed of the same substance in a non-crystalline form. When the crystals are of considerable size, the borders near their extremities are split longitudinally for a short distance. The crystals are frequently arranged in bundles. These crystals[1] cannot be confounded with excretine which crystallises in the form

[1] Of which Dr. Flint gives drawings.

of regular 4-sided prisms, nor with the thin rhomboidal or rectangular tablets of cholesterine. They are identical with the crystals of seroline figured by Robin and Verdeil.

"There can be no doubt with regard to the origin of the stercorine which exists in the fæces. We have found that whenever the bile is not discharged into the duodenum, as is probably the case, for a time, in icterus, accompanied with clay-coloured evacuation, stercorine is not to be discovered in the ejections. In one case of this kind in which the fæces were subjected to examination, the matters extracted with hot alcohol were entirely dissolved by boiling for fifteen minutes with a solution of potash, showing the absence of cholesterine and stercorine. In another examination of the fæces from this patient, made nineteen days after, when the icterus had almost entirely disappeared, and the evacuations had become normal, stercorine was discovered. Taking the estimates which have been made of the entire quantity of bile discharged into the intestine in the twenty-four hours by Bidder, Schmidt, and Dalton, a comparison of the total quantity of cholesterine contained in the bile, with the quantity of stercorine actually discharged, shows a correspondence which serves as an additional argument in favour of the view that stercorine is formed from a modification of cholesterine in its passage along the intestinal canal. These facts show conclusively that the cholesterine of the bile, in its passage through the intestine, is changed into stercorine. Both of these principles are crystalline, non-saponifiable, are extracted by the same chemical manipulations, and behave in the same way when treated with sulphuric acid. The stercorine must be regarded as a slight modification of cholesterine, the excrementitious principle of the bile. We have found that the change of cholesterine into stercorine is directly connected with the process of intestinal digestion. If an animal be kept for some days without food, cholesterine will be found in the fæces, though for a few days stercorine is also present. It is generally recognised by those who have analysed the fæces, that cholesterine does not exist in the normal evacuations; but whenever digestion is arrested, the bile being constantly discharged into the duodenum, cholesterine is found in large quantity. For example, in hybernating animals cholesterine is always present in the fæces. The same is true of the contents of the intestines during fœtal life; the meconium always containing a large quantity of cholesterine, which disappears from the evacuations when the digestive functions become established."

The following extract from an official report by Dr. Jones, dated Oct. 19th, 1864, respecting the condition of the Federal prisoners in the Southern prisons, is so interesting and instructive, not only in a physiological but in a medical point of view, that we shall not hesitate to extract it:

"Immediately after the brief report upon hospital gangrene had been forwarded to the surgeon general, Dr. Jones repaired to Camp Sumpter, Andersonville, Georgia, and instituted a series of investigations upon the diseases of the Federal prisoners. The field was of great extent and of extraordinary interest. There were more than 5000 seriously sick in the hospital and stockade, and the deaths ranged from 90 to 130 each day. Since the establishment of this prison on the 24th Feb., 1864, to the present time (19th Oct., 1864), over 10,000 Federal prisoners have died; that is, nearly one third of the entire number have perished in less than seven months. Dr. Jones instituted careful investigations into the condition of the sick and well, and performed numerous post-mortem examinations, and executed drawings of the diseased structures. The medical topography of Andersonville and the surrounding country was examined, and the waters of the springs, streams, and wells, around and within the stockade and hospital carefully analysed. Diarrhœa, dysentery, scurvy, and hospital gangrene, were the diseases which have been the main causes of the extraordinary mortality. The origin and causes of the hospital gangrene which prevailed to so remarkable a degree and with such fatal effects amongst the Federal prisoners, engaged his most serious and earnest consideration. More than 30,000 men crowded upon twenty-seven acres of land, with little or no shelter from the intense heat of a southern summer, or from the rain and dew, with coarse corn bread from which the husk had not been removed, with scant supplies of fresh meat and vegetables, with little or no attention to hygiene, with festering masses of filth at the very doors of their rude dens and huts, with the greater portion of the banks of the stream flowing through the stockade, a filthy quagmire of human excrements alive with working maggots, generating by their own filthy exhalations and excretions an atmosphere that so deteriorated and contaminated their solids and fluids, that the slightest scratch of the surface, even the bites of small insects, were frequently followed by such rapid and extensive gangrene, as to destroy extremities and even life itself. A large number of operations had been performed in the hospital on account of gangrene following slight injuries and mere abrasions of the surface. In almost every case of amputation for gangrene, the disease returned, and a large proportion of the cases terminated fatally. * * * The rations consisted of one third of a pound of bacon with one pound and a quarter of meal. The meal was unbolted, and when baked the bread was coarse and irritating, producing diseases of the organs of the digestive system (diarrhœa and dysentery). The absence of vegetable diet produced scurvy to an alarming extent, especially among the old prisoners. * * * From the sameness of the food, and from the action of the poisonous gases in the densely crowded and filthy stockade and hospital, the blood was altered in its constitution, even before the manifestation of actual disease. In both the well and the sick the red corpuscles were diminished, and in all diseases uncomplicated with inflammation the fibrinous element was deficient. In cases of ulceration of the mucous membrane of the intestinal canal, the fibrinous element of the blood appeared to be increased, whilst in simple diarrhœa, uncomplicated with ulceration, and dependent upon the character of the food and the existence of scurvy, it was either diminished or remained stationary. Heart-clots were very common, if not universally present, in the cases of ulceration of the intestinal mucous membrane, whilst in the uncomplicated cases of diarrhœa and scurvy the blood was fluid, and did not coagulate readily; and the heart-clots and fibrinous concretions were almost universally absent. From the watery condition of the blood, there resulted various serous effusions into the pericardium, into the ventricles of the brain, and into the abdominal cavity. * * * The Federal prisoners, as a general rule, had been reared upon wheat bread and Irish potatoes, and the Indian corn so extensively used in the South was almost unknown to them as an article of diet previous to their capture. Owing to the impossibility of obtaining the necessary sieves in the Confederacy, for the separation of the husk from the corn meal, the rations of the Confederate soldiers, as well as of the Federal prisoners, consisted of unbolted corn flour, and meal, and grist. This circumstance rendered the corn bread still more disagreeable and distasteful to the Federal prisoners. Whilst Indian meal, even when prepared with the husk, is one of the most wholesome and nutritious forms of food, as has been clearly shown by the health and rapid increase of the Southern population, and especially of the negroes previous to the present war, and by the strength, endurance, and activity of the Confederate soldiers, who were throughout the war confined, to a great extent, to unbolted corn flour; it is nevertheless true that those who have not been reared upon corn meal, or who have not accustomed themselves to its use gradually, become excessively tired of this kind of diet, when suddenly confined to it without a due proportion of wheat bread. Large numbers of the Federal prisoners appeared to be utterly disgusted with Indian corn, and immense piles of corn bread could be seen in the stockade and hospital enclosures. Those who were so disgusted with this form of food that they had no appetite to partake of it, except in quantities insufficient to supply the waste of the tissues, were of course in the condition of men slowly starving, notwithstanding that the only farinaceous form of food which the Confederate states produced in sufficient abundance for the maintenance of armies was not withheld from them. In such cases an urgent feeling of hunger was not a prominent symptom; and even where it existed at first it soon disappeared, and was succeeded by an actual loathing of food. In this state the muscular strength was rapidly diminished, the tissues wasted, and the thin skeleton-like forms moved about with the appearance of utter exhaustion and dejection. The mental condition connected with long confinement, with the most miserable surroundings, and with no hope for the future, also depressed all the nervous and vital actions, and was especially active in destroying the appetite. The effects of mental depression and of defective nutrition were manifested, not only in the slow feeble motions of the wasted skeleton-like forms, but also in such lethargy, listlessness, and torpor of the mental faculties, as rendered these unfortunate men oblivious and indifferent to their afflicted condition. In many cases even of the greatest apparent suffering and distress, instead of showing any anxiety to communicate the causes of their distress, or to relate their privations and their longings for their homes and for their friends and relations, they lay in a listless lethargic uncomplaining state, taking no notice either of their own distressed condition or of the gigantic mass of human misery by which they were surrounded. Nothing was so appalling and depressing as this silent uncomplaining misery. It is a fact of great interest, that notwithstanding this defective nutrition in men subjected to crowding and filth, contagious fevers were rare, and typhus fever, which is supposed to be generated in just such a state of things as existed at Andersonville, *was unknown*. Neuralgia and malarial fever were also very rare."

What a picture of the horrors of war! Dr. Flint also quotes another report by Prof. Wallace, by which it appears that the rations in the Southern prisons, when at the maximum, consisted only of half a pint of soup, containing about 2 oz. of beans or peas or sweet potatoes, 10 oz. of bread, and 6 oz. of beef; whilst the minimum amounted to 4 oz. of bread and 1 oz. of beef, a quantity far too small to sustain life. One of the most remarkable effects produced by this diet was the feeble power of resisting cold—frostbite and gangrene of the extremities being very frequent even in the mild climate of the south.

We must here conclude our notice of Dr. Flint's work. The description we have given of it, as well as the numerous extracts we have taken from it, will enable our readers to judge not only of its plan and scope, but also of its execution, and we trust soon to see the remaining parts. It is printed on agreeably toned paper with good type. It is singular amongst modern works on physiology in containing only some half dozen woodcuts, the reason of the introduction of which, to the exclusion of others of at least equal value,we are unable to see.

Respecting Dr. Dalton's work, which has reached its fourth edition (a tolerably good evidence of the estimation in which it is held) our comments must necessarily be brief. In many parts excellent, and in some superior, to any other treatise on physiology, it is with regret that we find some of the sections dismissed with a brevity wholly disproportioned to their importance. Thus the entire subject of the circulation, including the physiology of the heart, arteries, capillaries, and veins, occupies only forty-two pages; as a consequence, some very important topics are omitted altogether, whilst others are scarcely more than alluded to. Not a word is said respecting the nervous supply of the heart, and the experiments of Weber v. Bezold, Ludwig, and a host of other writers, are wholly ignored. The account of the venous system, again, is singularly imperfect. The author unintentionally, no doubt, leaves the impression that the heart has little to do with the passage of the blood through the veins, the circulation in this part of the vascular system being stated to be essentially maintained by the combined action of the following forces: 1. The force of the aspiration of the thorax, the influence of which in the normal movements of respiration extends to the farthest extremities of the venous system; yet he admits that in forced or laborious respiration the movements do not assist, and may retard, the flow of blood; 2. The contractions of the voluntary muscles; and, 3. The force of the capillary circulation, which he considers to be the most important of all, as it is the only one which is constantly and universally acting. We presume that Dr. Dalton refers the force of the capillary circulation to the force primarily derived from the heart; but as many physiologists still hold that there is a certain force in the capillaries derived from a mutual action and reaction taking place between the blood and the tissues, the importance of the cardiac force should have been more clearly expressed. We do not find any notice of the pressure of the blood in the veins, nor any remark on the effects of the introduction of air into them, one of those points of physiological interest which has so immediate a bearing on surgical practice that it should at least have been alluded to.

The whole subject of respiration is despatched with equal brevity with that of the circulation in barely twenty pages, and there are some inaccuracies. The bronchial ramifications are stated to be 1-25th instead of 1-50th of an inch in diameter, and the average size of the air-vesicles is given at 1-75, though this is really the maximum. Dr. Dalton holds that the external and internal intercostals alike minister to the act of inspiration, which is in accordance with the older views, and is now admitted by most writers of eminence. The estimate given for the total amount of air inspired per diem, 600,000 c. i., is certainly below the average, or at least holds only for a man at perfect rest. He adopts the curious and wholly unnecessary supposition that the interchange of air entering, with that already contained within the lungs, is assisted by the vibratile action of the cilia lining the tubes. It may indeed be assisted, but it must be to an infinitesimal extent only, since the cilia are extremely short and are immersed in the fluids which cover the bronchial membrane. The old experiments of Magnus, which are very incorrect, are referred to in speaking of the amount of oxygen and carbonic acid in arterial and venous blood respectively; whilst no allusion is made to the more recent, and far more trustworthy, observations of Meyer, Schroffer, Setschenow, and others; and it is stated that both these gases are in solution in the blood-globules, and not in the plasma; whilst it is now well known that it is only the oxygen that is combined with the corpuscles, the carbonic acid being contained in the plasma, either in the state of bicarbonate of soda or as a compound of phosphate of soda and carbonic acid. Lastly, the experiments of Dr. Edward Smith appear to have been entirely overlooked, though without question they constitute, on some points in the physiology of respiration, the most important that have as yet appeared, and materially modify the conclusions arrived at by Andral, Gavarret, and others, whose calculations were made at a time when animal chemistry was almost in its infancy. We have noted these defects in Dr. Dalton's work somewhat pointedly, because the remainder of the book is far above the average.

The subject of the bile and the whole section of reproduction are admirably written, and the woodcuts, of which no less than 266 out of a total of 274 are original, are extremely well done. The section on reproduction embraces eighteen chapters, and commences with a short account of the nature of reproduction, and of the origin of plants and animals, in which he makes a vigorous attack upon the doctrine of spontaneous generation. He states, on Prof. Wyman's authority, that although infusoria will make their appearance in infusions of animal and vegetable substances which have been boiled at the ordinary temperature of the air, for periods varying from fifteen minutes to two hours, in glass vessels so arranged that the atmospheric air could only gain access to their interior through tubes filled with red-hot iron wire, yet that the appearance of such infusorial life is absolutely prevented by exposure of the organic solution to ordinary boiling for four hours, or to boiling for fifteen minutes under a pressure of five atmospheres. The observation of Dr. Mitchell which is also referred to is curious, to the effect that vibriones will grow and thrive in the venom of the rattlesnake, when beginning to putrefy, and when it is still a deadly poison to all the higher animals.

To this follows a description of the egg in the various classes of the animal kingdom, and the mode of its discharge; the structure of the spermatozoa, and the function of menstruation; the formation of the corpus luteum is excellently described and depicted as it presents itself both in the virgin and in the impregnated uterus.

Then comes an account of the segmentation of the vitellus and the further process of development in the frog, the mode in which the umbilical vesicle appears in fish, and the amnios and allantois in the chick, and finally the development of the chorion in the human subject. The remainder of the section is occupied with the changes that take place in the uterus, the formation of the placenta, and lastly the development of the several organs. This, in our opinion, is the proper way to teach this difficult subject, and we venture to make one extract from Dr. Dalton's work, which we are sure will induce many of our readers to purchase the work for themselves and master its contents:

"*Amnion and Allantois.*—We shall now proceed to the description of two other accessory organs which are formed, during the development of the fecundated egg, in all the higher classes of animals. These are the amnion and allantois; two organs which are always found in company with each other, since the object of the first is to provide for the formation of the second. The amnion is formed from the external layer of the blastodermic membrane, the allantois from the internal layer. In the frog and in fish, as we have seen, the egg is abundantly supplied with moisture, air, and nourishment, by the water with which it is surrounded. It can absorb directly all the gaseous and liquid substances which it requires for the purposes of nutrition and growth. The absorption of oxygen, the exhalation of carbonic acid, and the imbibition of albuminous and other liquids, can all take place without difficulty through the simple membranes of the egg, particularly as the time required for the formation of the embryo is very short, and as a great part of the process of development remains to be accomplished after the young animal leaves the egg. But in birds and quadrupeds the time required for the development of the fœtus is longer. The young animal also acquires a much more perfect organization during the time that it remains inclosed within the egg; and the processes of absorption and exhalation necessary for its growth, being increased in activity to a corresponding degree, require a special organ for their accomplishment. This special organ, destined to bring the blood of the fœtus into relation with the atmosphere and external sources of nutrition, is the allantois.

"In the frog and the fish, the internal blastodermic layer, forming the intestinal mucous membrane, is enclosed everywhere, as above described, by the external layer forming the integument; and, consequently, it can nowhere come in contact with the investing membrane of the egg. But in the higher animals the internal blastodermic layer, which is the seat of the greatest vascularity, and which is destined to produce the allantois, is made to come in contact with the external membrane of the egg for purposes of exhalation and absorption, and this can only be accomplished by opening a passage for it through the external germination layer. This is done in the following manner by the formation of the *amnion*.

"Soon after the body of the fœtus has begun to be formed by the thickening of the external layer of the blastodermic membrane, a double fold of this external layer rises up on all sides about the edges of the newly-formed embryo, so that the body of the fœtus appears as if sunk in a kind of depression, and surrounded with a membranous ridge or embankment as in fig. 1. The embryo *c* is here seen in profile with the double membranous folds above mentioned, rising up just in advance of the head and behind the posterior extremity. It must be understood of course that the same thing takes place on the two sides of the fœtus, by the formation of lateral

Fig. 1.

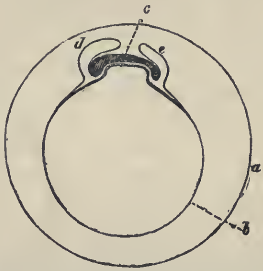


Diagram of fecundated egg, showing formation of amnion.
a. Vitellus. *b.* External layer of blastodermic membrane. *c.* Body of embryo. *d d.* Amniotic folds. *e.* Vitelline membrane.

folds simultaneously with the appearance of those in front and behind. As it is these folds which are destined to form the amnion, they are called the 'amniotic folds.'

"The amniotic folds continue to grow and extend themselves forwards, backwards, and laterally, until they approach each other at a point over the back of the fœtus (fig. 2), which is termed the 'amniotic umbilicus.' Their opposite edges afterward actually come in contact with each other at this point, and adhere together, so as to shut in a space or cavity (fig. 2 *b*), between their inner surfaces and the body of the fœtus. This space, which is filled with a clear fluid, is called the amniotic cavity. At the same time the intestinal canal has begun to be formed, and the umbilical vesicle has been partially separated from it by the constriction of the abdominal walls on the under surface of the body.

"There now appears a prolongation or diverticulum (fig. 2 *c*) growing out from the posterior portion of the intestinal canal, and following the course of the amniotic fold which has preceded it, occupying it as it gradually enlarges and protrudes the space left vacant by the rising of the amniotic fold. This diverticulum is the commencement of the allantois. It is an elongated membranous sac, continuous with the posterior portion of the intestine, and containing blood-vessels derived from those of the intestinal circulation. The cavity of the allantois is also continuous with the cavity of the intestine.

"After the amniotic folds have approached and touched each other, as already described, over the back of the fœtus, at the amniotic umbilicus, the adjacent surfaces thus brought into contact fuse together, so that the cavities of the two folds, coming respectively from front and rear, are separated only by a single membranous partition (fig. 3 *c*) running from the inner to the outer lamina of the amniotic folds. This partition itself soon after atrophies and disappears, and the inner and outer lamina become consequently separated from each other. The inner lamina (fig. 3 *a*), which remains continuous with the integument of the fœtus, inclosing the body of the embryo in a distinct cavity, is called the amnion (fig. 4 *b*), and its cavity is known as the amniotic cavity. The outer lamina of the amniotic fold, on the other hand (fig. 3 *b*), recedes farther and farther from the inner, until it comes in contact with the

Fig. 2.

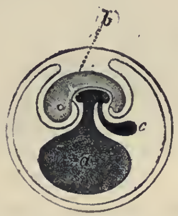


Fig. 3.

Fig. 2.—Fecundated egg, farther advanced. *a.* Umbilical vesicle. *b.* Amniotic cavity. *c.* Allantois.

Fig. 3.—Fecundated egg with allantois nearly completed. *a.* Inner lamina of amniotic fold. *b.* Outer lamina of ditto. *c.* Point where the amniotic folds come in contact. The allantois is seen penetrating between the inner and outer laminæ of the amniotic fold.

original vitelline membrane, still covering the exterior of the egg; and by continued growth and expansion it at last fuses with the vitelline membrane, and unites with its substance, so that the two membranes form but one. This membrane, formed by the fusion and consolidation of two others, constitutes then the external investing membrane of the egg.

"The allantois during all this time is increasing in size and vascularity. Following the course of the amniotic folds as before, it insinuates itself between them, and of course soon comes in contact with the external investing membrane just described. It then begins to expand laterally in every direction, enveloping more and more the body of the fœtus, and bringing its vessels into contact with the external membrane of the egg. By a continuation of the above process, the allantois at last grows to such an extent as to envelope completely the body of the embryo, together with the amnion, its two extremities coming in contact with each other, and fusing together over the back of the fœtus, just as the amniotic folds had previously done. It lines, therefore, the whole internal surface of the investing membrane with a flattened vascular sac, the vessels of which come from the interior of the body of the fœtus, and which still communicates with the cavity of the intestinal canal.

"It is evident from the above description that there is a close connection between the formation of the amnion and that of the allantois. For it is only in this manner that the allantois, which is an extension of the internal layer of the blastodermic membrane, can come to be situated outside the fœtus and the amnion, and be brought into relation with external surrounding media. The two laminæ of the amniotic folds, in fact, by separating from each other as above described, open a passage for the allantois, and allow it to come in contact with the external membrane of the egg."

Such writing as this, we repeat, is deserving of all praise. It is eminently intelligible, perfectly correct, and, considering the difficulty of the subject, very concise. If Dr. Dalton would work up the other sections of his book to the same extent as this we should be inclined to concede to him the palm of having written the most readable book on physiology extant.

We have read Mr. Marshall's work through with great care, and we may add with great pleasure. Taking it altogether, it is one of the most complete treatises on physiology we possess, and it is surprising how much information is here compressed into the compass of two moderate-sized volumes. Not limiting himself to human and comparative physiology alone, as the title of the work might lead one to expect, Mr. Marshall has introduced a succinct but clear outline of the anatomy of the body of the various vertebrate and invertebrate classes, a very good account of the microscopical appearance of the different tissues, and an excellent summary of the results of modern histo-chemical research. Nor is the vegetable kingdom wholly passed over, a section being devoted to the consideration of the general structure and functions of plants, which are instructively compared with those of animals.

The first volume is occupied with the consideration of general physiology, including the vital properties of the tissues and the relations of man to external nature; with the minute anatomy of the textures; and chiefly with the animal functions, motion, and sensation, including voice and speech. The second volume embraces the vegetative functions and reproduction. The plan adopted in regard to each section is to give first the general anatomical features, then the physics and chemistry of the organ when practicable or requisite, then the pure physiology with the relations that may exist to pathology, and finally the comparative anatomy. Thus, under the head of respiration, we find successively considered the organs of respiration, including the general and minute anatomy of the thorax, trachea, bronchi, and lungs; the mechanism of respiration, where we may note in passing that Mr. Marshall agrees with those who hold that the external intercostals and a small portion of the internal are muscles of inspiration, whilst the remaining posterior portion of the internal intercostals, including the greater part of their fibres, are muscles of expiration; the movement and sounds of the air in respiration and the capacity of the lungs; the changes in the air resulting from respiration; the effects of respiration on the blood and tissues, in which Prof.

Stokes' experiments are capitally summed up; the general theory of respiration, including the consideration of the two points, in what part of the circulation, and at the expense of what constituents of the blood and tissues does the oxygen absorbed in respiration become united with carbon to produce the carbonic acid given off; the conditions which modify the chemical processes of respiration, as purity of the air, age, food, exercise, &c.; the nature and treatment of asphyxia and suspended respiration and animation, the rules of the Humane Society, as drawn up by Dr. Silvester, being given; the effects of bad air and imperfect ventilation; the chapter concluding with a very full account of the organs and function of respiration in animals. As every great division of physiology is treated in nearly the same fashion, our readers will see that Mr. Marshall's book is one of no ordinary character. Everywhere there is evidence of its being written with care, and we are especially disposed to praise the lucid manner in which the numerous topics cognate to, and yet scarcely forming a part of physiology, are treated; we allude to such subjects as osmosis, spectrum analysis, the description of various instruments used in physiological researches, as the galvanometer and sphygmograph; the cardiac, and respiratory pathological sounds, &c.

The section upon locomotion in man and animals, which occupies fifty-five pages, is one that is rarely introduced into English works on physiology, and is here very interestingly given. After describing the various forms of joints and the kinds of levers employed in the body, the modes of progression on solids and in fluids, both in man and animals is described at length; and we will transcribe the section on *walking*, which is a fair sample of the whole:

"The act of walking is accomplished by means of alternate unsymmetrical movements on the two sides of the body, performed at the ankle, knee, and hip-joint, the trunk being kept as nearly as possible in a state of equilibrium, though, as we shall immediately show, its centre of gravity is not merely carried forwards, but undergoes both vertical and lateral oscillations. One leg is first lifted from its base of support with a slight flexion of the knee and foot, so as to prevent the latter from touching the ground, and is advanced a certain distance, chiefly by swinging, as will be presently mentioned, but also by flexion of the thigh upon the pelvis, and by extension of the leg and foot; it is soon permitted to touch the ground in advance of the body, the centre of gravity at the same time descending a little, as well as advancing forwards, and also inclining over in the direction of the advanced limb. As the forward foot advances, the hinder one inclines in the same direction, and the centre of gravity, now moved beyond the original base of support, is slightly curved. When the forward foot has touched the ground, the hinder one is raised by extension of the foot, which continuing to press on the ground, assists in urging the centre of gravity forwards, a little upwards, and still more over to the opposite side. The centre of gravity having now reached a secure point of support over the advanced and stationary limb, the hinder limb completely leaves the ground; the thigh is slightly bent upon the pelvis; the leg is a little bent on the thigh, and the foot somewhat on the leg; in this position of the segments it is shortened by about one ninth part of its length, so that the toes may keep clear of the ground. The limb in its turn is now swung forwards, to be planted on the ground in advance of the body, the centre of gravity being again carried forwards, downwards, and over to the same side, and the foot being finally planted on the ground as before. * * * In rapid walking almost every muscle of the body is exercised: the duration of the step is shortened, and so also is the length of time during which both feet touch the ground together; the length of the step may be either shortened or increased. In the case of a man walking at the rate of four miles an hour, and whose legs were thirty-four inches in length, the number of steps taken in fifteen minutes was 2000, the length of each step 2·64 feet, and the period of each step ·45 of a second (Vasey). In very quick walking the rate has been nearly $5\frac{1}{2}$ miles per hour, or about 7·9 feet per second." * * *

The chapter on "Animal Statics and Dynamics" is also exceedingly good, and gives a *résumé* of all the recent physiological researches occasioned by the important observations of Fick and Wislicenus, which have led to such material modifications of the older views of the nature and mode of development of muscular force. After recounting the details of these well-known experiments, and the further investigations of Frankland and of Parkes, Mr. Marshall continues—

"There are many facts which indicate the necessity for large amounts of non-nitrogenous food for the due performance of muscular work. It is in the larval stage that insects generally consume the most albuminous food, and perform the least amount of work, whilst in the perfect condition, as in bees, butterflies, and moths, their muscular activity is remarkable, although their food is almost purely saccharine or non-nitrogenous. The goat, chamois, gazelle, and many other ruminants, are singularly swift and active creatures; their food, however, is not highly nitrogenous, but chiefly consists of carb-hydrates. It is not probable that the muscular work in any of these cases is performed by the oxidation of albuminoid matters only; for in that event the muscles, especially the minute ones of insects, would soon be entirely oxidised, and could not be restored by the scanty supply of nitrogen in the food. The remarkable provisions for digesting the carb-hydrates, and rendering them absorbable, appear therefore to have reference, not only to their use as heatgivers, but also as sources of motor power. The production of sugar from starch is a universal action of the saliva of all animals, and long-continued digestion in the ruminant stomach will even change the cellulose. It has been remarked that the chief food-manufactures are concerned with non-nitrogenous articles of diet, that eggs contain, when dried, forty per cent. of fatty matter, that fat is always present in meat, that the poor consume much bacon fat and the rich, who eat most albuminoid food, likewise take more butter, sugar, and alcohol (Lawes and Gilbert). The use of bacon by the agricultural labourer has given rise to a familiar epithet for him. The chamois hunters prefer a store of bacon-fat and sugar to any other provisions on a hunting expedition; and Fick and Wislicenus ascended the Faulhorn on non-nitrogenous diet, without special fatigue. But on the other hand, Parkes found that, on the second day of severe exercise, on a non-nitrogenous diet, healthy soldiers complained of unusual fatigue. Practically it would seem that sufficient nitrogenous food, being supplied for the nutrition of the muscular and nervous system, then the most effective diet for a labourer is that which contains a large proportion of non-nitrogenous substances. Athletes should *train* on meat, but enter into their contests upon amylaceous, saccharine, or fatty food."

As a means of showing the mode in which Mr. Marshall deals with the comparative anatomy he has introduced, we take at random his account of the circulation in mollusca, which runs thus:

"*Mollusca.*—The most perfect condition of the circulatory system in the non-vertebrate animals is met with in this sub-kingdom, and in the class Cephalopods. In the cuttle-fish, for example, there is found a *systemic* ventricular heart, provided with valves at its orifice; it is usually rounded, has strong muscular walls, and even internal columnæ corneæ. Arteries proceed from it to all parts of the body, excepting to the branchiæ or gills, the liver even receiving branches. The blood is returned into a large vein, or venous sinus, which is surrounded by a remarkable cellular organ filled with blood, and from which symmetrical lateral branches, two or four in number, according to the number of the gills, proceed to those organs, each presenting, as it enters the gill, a pulsating dilatation or so-called bronchial heart, which helps to propel the blood through the gills. From the gills the blood is returned into large venous sinuses, which being contractile, act as auricles, and thence passes into the systemic ventricular heart already described. In the Pteropods, and in the Gasteropods, there is but a single heart which is always systemic, distributing its contents by one arterial trunk and numerous branches to the body and liver, from which, having passed through *lacunæ* or spaces, it is again collected by veins, and by them conveyed to the respiratory organs; from these it is collected by other canals, the branchio-cardiac veins, and is so brought to the heart again. In the Terrestrial Pulmo-gasteropods, as in Helix, the venous blood from the body passes through small vessels on the walls of the pulmonary air-sac, and is then collected into a larger vessel, which conveys it to the heart; whilst in the aquatic Branchio-gasteropods, as in Doris, the blood returning from the body is carried by special vessels into the gills, and is then conveyed by other vessels back to the heart. In both kinds of Gasteropods, the heart consists of an auricle and ventricle, between which there is found a minute but distinct valve, which serves accurately to direct the course of the circulating fluid. In the Lamellibranchiata, the heart, usually single but sometimes double, in correspondence with the bilateral arrangement of the parts of the body of these animals, and often perforated by the intestine, is placed in a pericardium situated near the adductor muscle, which closes the shell; when single it has sometimes one and sometimes two auricles connected with its simple ventricle; when the heart is double, each has only one auricle."

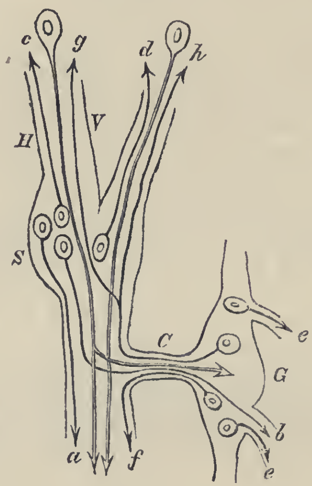
In like manner, a short exposition is given of the vascular system of the several invertebrate and of the vertebrate classes.

Here and there some slight corrections might be made: thus, after the careful researches of Dr. Anstie and Dr. Dupré, it can scarcely be said that after the injection of alcohol "a great portion escapes unchanged by the lungs, skin, and kidneys." The proportions of gases in the blood is given on the old statement of Magnus, which, as has been shown by many succeeding observers, are very incorrect—he, in fact, only obtaining about three fourths of the actual quantity present. We do not find any reference to the peculiarities of the circulation in erectile organs. We can hardly agree with Mr. Marshall that the milk is most abundant, as well as most nutritious, in nursing women from the age of fifteen to twenty. Had we to select a wet-nurse on purely physiological grounds, we should prefer one of from twenty to thirty; and so on. These, however, are but trifles; and on the whole, we are bound to say there are very few alterations or emendations needed. We may remark in conclusion that the topography of the book is excellent, and that it is embellished by 122 woodcuts, some of which are original. It fairly represents the existing state of physiological science, and whilst cordially recommending it to all our readers, we would most strongly do so to those students who are reading for the physiological examination of the University of London, to whom the section on comparative anatomy will render it extremely valuable.

The physiology of M. Otto Funke, which is founded on the older treatise of Wagner, and of which the fourth greatly extended and improved edition has, after several years' delay, been completed, is a fine work, and is as deserving of translation into our language, as was the philosophical treatise of Johann Müller. Dealing lightly with the subjects of alimentation, circulation, and respiration, the purely vegetable functions of our bodies, it enters most elaborately into all the important points of the nervo-muscular apparatus. His description of the nerves of special sense is very complete, and especially is this the case with the eye, which occupies more than three hundred closely printed pages, or one seventh of the whole work, being at once an indication of the inexhaustible field of research presented by this wonderful organ, and the extraordinary perseverance with which it has been worked out by a multitude of inquirers.

We have taken the trouble to epitomise the section on the sympathetic nerve, which shows well Prof. Funke's mode of dealing with a difficult subject, and will, perhaps, prove of service to some of our readers. He commences by pointing out that the work of Bidder and Volkmann was the commencement of a new era in the history of the sympathetic nerve. These observers showing by actual counting, in opposition to Valentin and Remak who held that it was to be regarded as merely an offset of the cerebro-spinal system, that it was fairly impossible that all the fibres of the sympathetic could take origin in the brain and spinal cord, but that the ganglia were properly to be considered as centres of origin for new fibres. Again, whilst Valentin considered the rami communicantes as being exclusively composed of fibres passing from the spinal cord to the sympathetic, Bidder and Volkmann assured themselves that the greater part of these fibres turned at the point where they joined the spinal branch towards the periphery, a small number only bending towards the spinal cord, which last could alone be regarded as the spinal root of the sympathetic; and they further maintained that many even of these fibres proceeded from the ganglion on the posterior root of the spinal nerves, since an examination of this root on the proximal side of the ganglion showed that it contained only about 2 per cent. of fine or sympathetic fibres, which is much below the proportion found in the mixed nerves, and still less than in the sympathetic branches. The work of Bidder and Volkmann received important support from the excellent researches of Kölliker; but, at the same time, much correction and extension. Kölliker corroborated the origin of the nervous fibres from the spinal and sympathetic ganglia, and indeed not only indirectly by comparative enumeration, but directly through the discovery of their origin from the cells of the ganglia. He denied, it is true, the specific nature of the sympathetic fibres, and consequently regarded the independency of the sympathetic, as caused not by any peculiarity of its elements, but essentially as a result of the origin of its fibres; whilst he yet rightly attributed to it, in consequence of a portion of its fibres being derived from the spinal and cerebral ganglia, a certain degree of dependency upon these centres. Kölliker's modification of Bidder and Volkmann's theory received very general acceptation, and even Valentin felt constrained to modify his views in accordance with them. More recently the question of the dependency of the sympathetic has again become the subject of dispute, some endeavouring to establish the original theory of Valentin, others exerting themselves to prove the entire independency of the sympathetic as regards the cerebro-spinal system. We shall here take into consideration the anatomical points of the controversy, only reserving the physiological for subsequent discussion. Axmann's results are best explained by a reference to the accompanying figure. In the cells of the

Fig. 4.

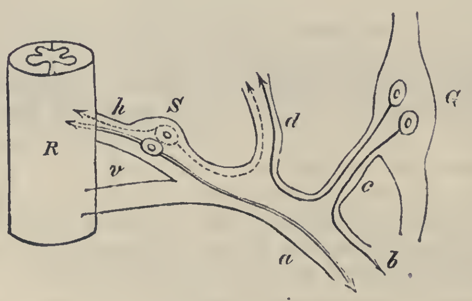


spinal ganglion (*s*) fibres originate, which partly run towards the periphery and emerge with the spinal nerves (*a*), or enter the sympathetic through the ramus communicans (*b*), and partly pass towards the centrum either through the anterior (*d*) or the posterior root (*c*). From the nerve-cells of the sympathetic ganglion fibres originate, which partly remain in the sympathetic nerves (*e*, *e*), partly pass to the spinal nerves through the ramus communicans, in order either to course downwards pherically (*f*), or to pass upwards without communicating with the cells of the spinal ganglion, and to enter the spinal cord either through the anterior or the posterior root (*g*, *h*).

This scheme of Axmann's contains many propositions of an unproven and doubtful nature. At the first glance it is obvious that Axmann only admits a unipolar origin of the fibres from the sympathetic ganglionic cells; as also in the spinal ganglia, although he himself has especially observed the existence of bipolar cells with the fibres arising from their pursuing opposite directions in the spinal ganglia. On this ground his description of the fibres marked *c h* and *g h* appears particularly doubtful, since he attributes their origin to the ganglionic cells; whilst he regards the spinal cord as their peripheric terminal organ."

After some further criticism of Axmann's views, Funke continues: "The scheme proposed by Küttner, founded on microscopical sections and experiments on the living animal, is wholly different. Previously to the observations of Küttner, the degeneration of the distal part of a nerve which follows its section established by Walter and Budge, had been employed by Schiff to decide the question whether the sympathetic speaking generally was an independent system or a derivative and sectional portion only of the spinal cord. Schiff took for granted that destruction of the spinal cord would certainly be followed by this peculiar degeneration of all the fibres of the sympathetic if the spinal cord constituted their central organ. Experience corroborated this supposition, but whether this circumstance is to be regarded as incontestable proof of the origin of all the ganglionic nerve-fibres from the medulla appears to us extremely doubtful, since other observers who have repeated the experiment have not been able to discover such degeneration. Küttner has indeed gone to the other extreme. He denies that any communication takes place between the fibres originating in the sympathetic ganglion and the spinal cord, especially any passage of fibres to the sympathetic in the path of the ramus communicans, whilst he admits that a communication occurs between the nerve-cells of the special ganglion and the spinal cord, but does not reckon the 'broad' fibres originating from them as sympathetic fibres. He maintains, also, that the peripherically directed fibres remain in the proper spinal nerves. He founded his statements on the following experiments. He divided in frogs either the *ramus communicans* (*c*, fig. 5), or the nerves of the spinal cord on this side (*g*), or on that side (*b*) of the point of insertion of the former, or the roots *v h*; and after a certain time made examinations, with the view of determining the presence of degenerated or of normal 'fine' fibres, which, in common with Bidder or Volkmann, he regarded as specifically sympathetic and easily recognisable as distinct from the spinal fibres; upon whichever side of the section the degenerated fibres were observed, he attributed their centre of origin to the part beyond or on the opposite side of the section. After he had convinced himself that in the frog the fibres contained in the ramus communicans, at the point of union with the spinal nerves, were directed partly peripherically in these, and partly turned towards the centre (in point of fact, in the upper spinal nerves chiefly centrally, and in the lower almost exclusively peripherically); he divided the ramus communicans *c* at its middle; after the lapse of three months the fibres of the part still in connection with the sympathetic ganglion were normal in appearance, whilst the fibres of the portion attached to the spinal cord were degenerated, as were also even the finer fibres of the spinal nerves themselves at *a* and *b*. On the other hand, in the roots *v* and *h*, Küttner could neither discern normal nor degenerated fine fibres; he thence came, in direct opposition to Bidder and Volkmann, to deny their existence in these roots. If the spinal nerves were divided between the entrance of *c* and *s* at *d*, degeneration was observable after three months, even to the naked eye, in the peripheric segment. Under the microscope, all the *broad* fibres appeared degenerated; whilst, on the contrary, all the *fine* fibres, both here and in *c*, were perfectly normal. In *v* and *h* he was unable to find either normal or degenerated thin fibres. In another frog Kuttner divided on the right side both, on the left the posterior root of the ninth spinal nerve. Subsequent examination showed in *c*, *a* and *b* all the *fine* fibres on both sides normal.

Fig. 5.



The remarkable difference in the behaviour of the roots themselves, the degeneration of the central part of the posterior and of the peripheric portion of the anterior root, is fully discussed in another part of the work.

Küttner concludes from these facts that all the fibres contained in the ramus communicans originate in the sympathetic ganglion which constitutes their centre; that those which, after the junction with the spinal nerves, pass towards the spinal cord neither terminate in the spinal ganglion of the posterior root, nor in the ganglionic cells of the spinal cord, but enter the dorsal branch of the spinal nerve (*d*), and are distributed peripherically. He considers that the centripetally coursing fibres originating in the bipolar nerve-cells of the spinal ganglion pass to the spinal cord, whilst those that run peripherically are distributed with the spinal nerves; but since he finds in the dorsal branch (*d*) of the lower spinal nerves a far greater number of fine fibres, the majority of which run peripherically, than can proceed from the ramus communicans, he maintains, in opposition even to his own statements, that the greater number of these fine fibres arise from the spinal ganglion, the cells of which, he elsewhere states, give origin only to broad fibres.

Küttner claims, therefore, for the fibres of the sympathetic the most complete independency in an anatomical point of view; still more complete, even, than that maintained by Bidder and Volkmann, who at least admit that a few of the sympathetic fibres course in the roots of the spinal nerves. M. Funke elsewhere already indicates the grounds on which he is disposed to doubt Küttner's theory and the physiological considerations which are opposed to it.

The theory of Remak stands in strong opposition to that of Küttner. According to this, the whole of the sympathetic fibres proceed immediately from the brain; all pass through either the anterior or posterior roots of the spinal nerves and enter the multipolar ganglion cells of the sympathetic, through which they are continuous, with fibres passing either with the peripherically distributed branches of the sympathetic, or with those of the spinal nerves. Remak denies the existence of fibres primarily originating in the sympathetic, and also the existence of cerebro-spinal fibres in the sympathetic nerves, which have proceeded directly from these cerebro-spinal centres without communication with the sympathetic ganglion cells. This theory is, however, as one-sided, and rests on foundations as unsatisfactory, as those of Küttner or of Axmann. So stands the controversy respecting the anatomical relations, and especially concerning the origin of the sympathetic. In view of physiological facts which indisputably point to a communication between the sympathetic and the cerebro-spinal system, we regard the theory of Kölliker as incontestably the best established and most probable. According to this, the rami communicantes are to be regarded as in part branches, in the proper sense of the word, of the sympathetic, through which fibres originating in the sympathetic ganglion-cells enter the spinal nerves, but in part also as roots containing fibres passing to the sympathetic, from the anterior and posterior roots, originating in part from the spinal cord and partly from the ganglion on the posterior root of the spinal nerve.

As regards the functions of the sympathetic, M. Funke remarks upon the difficulty of determining them, in consequence of the close connection that exists between the sympathetic and the cerebro-spinal system, so that doubt and obscurity exist even in reference to the first and most prominent question, whether the sympathetic fibres can conduct sensory impressions. It is certain that the parts supplied by the sympathetic nerves are sensitive, but who is to say whether the impressions are conveyed through sympathetic or through cerebro-spinal channels? He himself thinks that impressions of pain are certainly received through the sympathetic fibres, though they are incapable of transmitting simple impressions of touch; and he further holds that the impressions derived from the sympathetic system are perceived, not by the ganglia of the sympathetic, but by the cerebral ganglia or sensory centres of the brain; as regards the motor properties of the sympathetic, it is indubitable that motor impulses can be transmitted by its fibres, and the independent origin of the impulses is shown by the continued contraction of the heart removed from the body, and of the intestines and other parts after the removal of the entire cerebro-spinal system. It seems, however, that no impulses of the will can be conveyed to the parts supplied by the sympathetic. The motor impulses, under ordinary circumstances, appear to be excited automatically or reflectorially, though this explains little or nothing of the nature of the process. He then refers to, and corroborates, the remarkable inhibitory influence exerted on the movements of the small intestine by galvanisation of the splanchnic, discovered by Pflüger; but admits there is much difficulty in giving any satisfactory explanation of the phenomena observed. The most likely, he thinks, is that, as in the case of the heart, the intestines are excited to contract, rather through automatic, or reflectorial, or direct excitation of the peripheric ganglia. The fibres of the splanchnic nerve are in direct continuity with the same ganglion cells as those which give origin to the motor fibres of the intestine and the influence exerted by the former set of fibres, is to effect the inhibition of the motor impulses developed in the cells.

He then fully discusses the well-known vaso-motor powers of the sympathetic fibres, giving an excellent *résumé* of the observations of Bernard, Waller, Budge, Schiff, Brown-Séquard, and others, in regard to the dilatation of the vessels which results from section of the nerve in the cervical region, or from section of the anterior roots of the spinal nerves from the fifth cervical to the third dorsal, and of the contraction that occurs on galvanizing the upper cut extremity. The experiments of Callenfels are also described, founded on the observation of Schiff, who, however, was preceded by Wharton Jones on the rhythmical contractions of the arteries at short but variable intervals (of 6·60 sec.), and which propagate themselves towards the capillaries. Callenfels found that the galvanic stimulus applied to them when dilated caused their contraction, but when contracted their dilation. Finally he has a very complete section on the trophic influence of the sympathetic nerve, *i. e.* its influence on nutrition. Axmann's views, of which we have seen no translation, are thus stated:

"Axmann came to the conclusion, that the ganglia on the spinal nerves are trophic centres, whilst the proper sympathetic ganglia govern the 'vital contractility,' and are therefore to be regarded only as motor centres. He grounded this opinion on the fact, that section of the roots of the spinal nerves above the ganglia produced no change in the nutrition of the parts supplied by them, nor was any alteration visible in frogs after removal of the whole brain and spinal cord. The animals lived for a long time, wounds healed, and broken bones united with the greatest facility. It was observed after section of the posterior roots alone above the ganglia that parts of the spinal cord soon underwent inflammation and softening, which he attributes to the division of the fibres marked *c*, fig. 4, which originating in the ganglia, pass to the spinal cord. So also, when section of the spinal nerves was made below the ganglia, between them and the junction of the ramus communicans, considerable trophic disturbances occurred. The frogs became pale from retraction of the processes of their pigment-cells, general anasarca occurred, the fluid containing uric acid; relaxation of the muscles took place, and numerous small extravasations of blood were found in their substance. The mucous membrane of the small intestines was soft and injected, the kidneys softened and the liver filled with extravasations. From the seat of the section, these results could not proceed from the division of the proper sympathetic fibres; and as they did not occur after section of the posterior roots above the ganglia, the only conclusions he could arrive at was, that they were the consequences of the division of the fibres arising from the ganglionic cells in the ganglion on the posterior root."

Pincus, who instituted an extensive series of researches, to control the results obtained by Axmann, generally corroborates them. These, with those of Bidder and Valentin, are likewise given at length. Our readers will be able to see from these extracts the very full and complete manner in which all the questions relating to the nervous system are treated. The only deficiency of the work, taken as a whole, seems to be the almost entire omission of all English references, standing in this respect in remarkable contrast with the learned work of M. Milne-Edwards, whose 'Leçons,' when completed, will constitute, perhaps, the most perfect physiological treatise in existence.

We can cordially recommend Kühne's Physiological Chemistry to those of our readers who understand German.

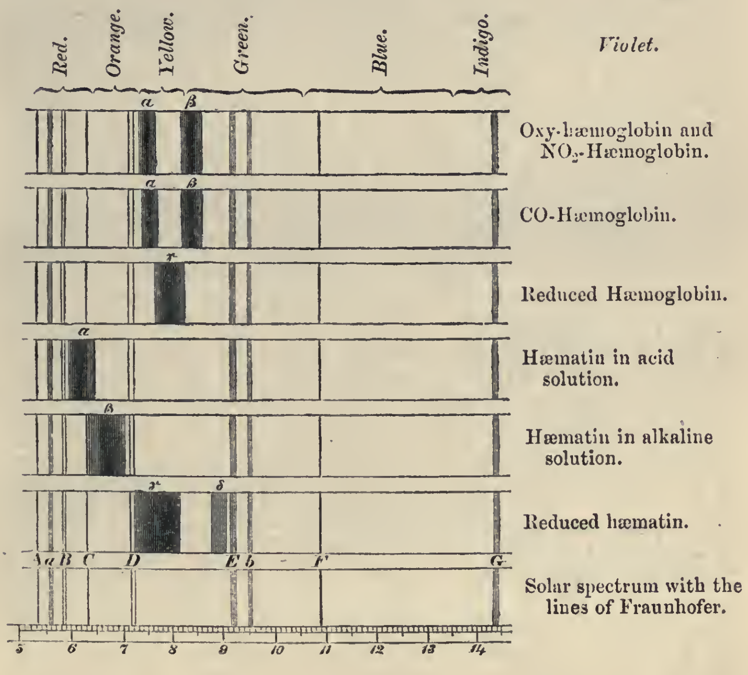
The work is divided into five sections, digestion, the chemistry of the animal fluids, the chemistry of the tissues, the chemistry of the fluids, and, lastly, the chemistry of the secretions.

The first section is very complete, it is written in a plain and easy style; but though worked up to the very latest period, gives comparatively few references to authorities. It includes the consideration of the properties of the various kinds of saliva and their action on starch, the changes that the food undergoes in the stomach, the chemical character of the biliary, pancreatic, and intestinal fluids, the constitution of the fæces, and the nature of the gases contained in the intestinal canal. In the last section, special praise is due to the account of the urine. Throughout the book, the physiological and chemical aspect of each subject are happily blended; and it is, in contrast with v. Gorup Besanez's work on the same subject, not a book of reference only but one that is eminently readable. As a specimen of Dr. Kuhne's work, we subjoin a translation of the part relating to the spectrum analysis of the blood.

"THE OPTICAL RELATIONS OF THE BLOOD.

"It has already been mentioned that the crystals of hæmoglobin possess double refraction, and are pleochromatic; and that, when dried below 0°, they form a red powder. Their solution if prepared with access of air, is likewise of a beautiful red tint. In order to determine the influence it exerts upon transmitted light, Hoppe-Seyler first adapted cells to the spectrum apparatus, permitting solutions containing various proportions of the colouring material, but of equal thickness, to be examined. If, in the first instance, a concentrated solution be placed in the apparatus, the whole of the red will be found obscured, at most, about three fourths of the space between the Fraunhofer's lines, C and D remaining clear; the yellow is also obscured. On gradual dilution, clearing up occurs as far as D, then light appears between the lines E and F in the green. With further dilution, the part beyond F. clears up, and the spectrum extends to the violet. At this degree of dilution there still remains two absorption-bands in the green parts of the spectrum, between D and E, which are most distinctly seen in a solution rather less than one third of an inch in thickness, and containing 1-1000th of hæmoglobin, but even in 1-10,000th they cannot be overlooked. The first absorption band of the hæmoglobin (α) which lies nearest to the line D is smaller, darker, and better defined than the second,

Absorption Spectra of the Blood and of its Colouring Matter.



β, which is close to E—a clear space intervenes between the two. With still further dilution, β is the first to disappear.

"Stokes now made the interesting observation, that on the addition of oxygen absorbing fluids, these absorption bands gradually disappeared, whilst coincidently a broad shadow with softened off edges appeared in the previously clear intervening space (γ). By agitation with air the broad shadow again vanishes, and the striæ α and β return. For this purpose mixtures of sulphate of iron, tartaric acid and ammonia, sulphide of ammonium, or ammoniacal solution of tartrate of zinc oxide may be employed. These fluids, mingled in small proportion with the solution of hæmoglobin, rapidly produce the changes in the absorption of light above described. Sulphide of ammonium acts somewhat more slowly. Since the reducing solutions oxidise themselves at the cost of the oxygen in the hæmoglobin, and since the reduced hæmoglobin by agitation with oxygen again presents the former optical relations, the experiment may be repeated as frequently as may be desired. Even without the application of the spectrum, it may be noticed that the application of colourless sulphide of ammonium causes the solution of reduced hæmoglobin to assume another tint. It becomes darker, resembling venous blood, becomes somewhat bluish or violet, and in thin layers green. Thus, through reducing agents, the mono-chromatic hæmoglobin becomes dichromatic, whilst oxygen again restores its monochromatism. Hoppe has shown that the reduced hæmoglobin absorbs rather less of the blue part of the spectrum. From all this the conclusion may be drawn, that the reduced hæmoglobin represents a colour which is compounded of red, green, and blue, for these parts of the spectrum remain clear, whilst the yellow is obscured by the shadow γ. But since this part of the spectrum is the most brilliant, it follows that the hæmoglobin becomes by reduction darker (more opaque), through oxydation brighter (more transparent). Similar differences in the brightness are observed if solutions of hæmoglobin in the oxidised and reduced conditions are compared with one another, the concentration of which is so considerable that the absorption bands α and β are imperceptible. In that case all light is absorbed by the reduced solution, with the exception of the red bands between the Fraunhofer's lines a and β, and even these are much darker in the 'reduced' solutions of equal strength. The same changes in the optical relations are all exhibited by hæmoglobin after the introduction of CO_2, CO. and H. CO gas causes only a shifting of the absorption band a towards E, but blood thus treated does not become dark by any of the reducing agents α, the bands a and β remain without the occurrence of the shadow γ. "NO gas, with exclusion of air, passed through reduced hæmoglobin, restores the striæ a and β, but these cannot be again made to disappear by reducing agents. In blood treated with CO, the transmission of NO brings back the shifted band α to its original position. Under all destructive operations which *produce* hæmatine, the optical relations of hæmoglobin are modified, as is clearly shown by the passage of the beautiful red into brown or green. The now remarkable absorption of light depends upon the presence of hæmatine. As this exhibits different colours in alkaline or acid solution, its spectrum is different, according to whether it is united with acids or bases. In order to recognise the absorption bands, especially of the former somewhat concentrated hæmoglobin solutions must be employed corresponding to the relatively small proportion (4 per cent.) of hæmatine proceeding from the hæmoglobin. The simplest experiment consists in the addition of a little acetic acid. When this is done, the absorption bands α and β vanish immediately, and another absorption band appears which covers the Fraunhofer's line C (α) and extends towards D.

"Supersaturated with ammonia, or some other alkali, the stria is shifted towards D (β), so that the part in the immediate vicinity of C becomes free from shadow. This second band is somewhat less defined than the former on acidification. β readily changes to α. According to Hoppe's experiments, these appearances are still clearly visible with a solution of 1 gramme of hæmatine in 6667 Ccm. of fluid, 1 Ctm. thick.

"If the solution be treated with the above-mentioned iron-oxide solution, it causes these absorption bands to disappear; but two new ones appear of dark colour, which, on superficial observation, might be referred to an exchange for the α and β of the hæmoglobin solution. They are, however, the striæ of the reduced hæmatine, γ and δ, γ commencing with a soft shading between D and E, in which position α of the hæmoglobin also lies. Only the former (γ), reaches nearer to the spot where β of the hæmoglobin begins, and is therefore much broader than α of the hæmoglobin. The very dark line β of the reduced hæmatine includes in its centre the Fraunhofer's line E. On agitation with air these bands disappear, but those of the original unreduced hæmatine do not reappear. This is opposed to the opinion of Stokes, that by the use of reducing agents hæmoglobin can be produced from hæmatine. As already stated, solutions are produced with all the optical properties of hæmatine under all those circumstances which develope hæmatine out of hæmoglobin, as by long exposure, by drying, by long transmission of CO_2, by warming, by coagulation, &c. If dried and decomposed hæmoglobin be extracted with water, a solution is obtained which gives the band β of the acid hæmatine, because the fluid is acid from the presence of the products of disintegration (formic and butyric acids, &c.). Hoppe draws the conclusion, that the hæmatine is here combined with the albuminous bodies, which are not precipitable by neutralisation, to form a peculiar substance called met-hæmoglobin, which possesses similar optical relations to those of acid albumen. But this solution always still contains undecomposed hæmoglobin, as is proved by the presence of its two spectral absorption bands. In fact, we can artificially out of weak acetic acid solution of albuminate of alkali, hæmoglobin and hydrochlorate of hæmatine, obtain a mixture which behaves itself exactly as the so-called met-hæmoglobin."

The two papers written by Dr. Parkes may be taken as the types of the kind of work that is required for the advancement of physiological science. They reflect the greatest credit on Dr. Parkes' industry and intelligence, and they show in the most conclusive manner that were the efforts of original investigators directed in different paths with some common object in view, we should soon be rewarded by important advances.

The subject of the first series of Dr. Parkes' essays is replete with interest. It consisted in an endeavour to determine whether during exercise any increase takes place in the amount of nitrogen eliminated from the body on a non-nitrogenous diet. In a second series, the effect of rest and exercise respectively on the elimination of nitrogen, when the diet contained a regulated amount of this substance, was investigated. Throughout all modern treatises on physiology it is admitted as an undoubted fact that muscular substance is constantly undergoing degeneration, or, in other words, oxidation, and that for the repair and renewal of their tissues fresh supplies of food are required. The degeneration is supposed to take place even when the vessels are at rest, but much more actively when they are brought into play. A strong argument in favour of this view is drawn from the fact that the elimination of nitrogen in the form of urea continues even in complete inanition to the last day of life, and this was supposed to represent the minimum amount produced by the heart and respiratory muscles (as well as to a small extent by other nitrogenous tissues), in their persistent activity; and further, the experiments of Lehmann and others seemed to show that with vigorous exercise more urea was eliminated than when the body was at rest, due apparently to the increased degeneration of the tissue when in action, which was further exhibited by consequent increased demand for food. Of late years, and especially through the observations of Bidder and Schmidt, a luxus-consumption of the food came to be admitted; that is to say, it was believed that, although a certain proportion of the nitrogenous food was applied to the maintenance of the muscular tissue, yet if more food were consumed than was required for this purpose, it was used up for the general purpose of the economy, and, probably, chiefly for the production of heat. In either case, whether the nitrogenous constituents of the food were applied to the renewal of the muscular tissue, or were oxidized in the blood, the terminal products were carbonic acid, water, and urea. Some doubts were thrown on the validity of these conclusions by the observations of Dr. E. Smith, which showed that with even violent exercise—such as that of the treadmill—there was comparatively little increase in the amount of urea discharged from the body. Several subsequent observers noticed the same fact, but the most important observations were those of Fick and Wislicenus which, owing to Dr. Frankland having given a lecture upon them at the Royal Institution, are now well known. These gentlemen endeavoured to ascertain whether there was any increase in the elimination of nitrogen from the body during violent exercise on a non-nitrogenous diet. They climbed the Faulhorn, exerting an amount of muscular force which, when added to the muscular force employed for circulation and respiration, was equal to 159,637 kilogrammeters for M. Fick, and 184,287 kilogrammeters for M. Wislicenus. In accomplishing this severe effort, it was found that during the ascent, and during six hours after the ascent, only 5·74 grammes of nitrogen were eliminated, which corresponds to the disintegration of 37·17 grammes of muscle, and the experiment corroborated upon the whole what had already been stated by others, and especially by Dr. E. Smith, that active muscular exertion causes little or no increase in the amount of urea discharged, and, therefore, of nitrogenous tissue used up.

Dr. Parkes' observations are more valuable than those of MM. Fick and Wislicenus on two grounds: first, because they extended over a much longer period (seventeen days); and secondly, because attention was paid to the possible exit of nitrogen by the bowels, which had been overlooked by the German observers. The mode of proceeding adopted in Dr. Parkes' experiment was, that two healthy and remarkably intelligent and docile soldiers were allowed for six days a general diet of meat, bread, vegetables, &c., without any absolute restriction. The quantity of nitrogen contained in the urea and other nitrogenous constituents of the urine and in the fæces was carefully examined, in the former daily, and in the latter on one occasion. The tissue-changes were found to be very closely the same, and the men quite comparable and well fitted for the experiment.

During a second period of two days, the men were placed on a non-nitrogenous diet of arrow-root, sugar, butter, and tea, and were kept *at perfect rest*. The effects of this diet were very similar on both, and a satisfactory basis of comparison was obtained for the period of exercise. During a period of four days the men returned to their former regulated diet and usual occupations, during which their weight, which had fallen away a little with the farinaceous diet, returned to its usual amount. In the fourth period, the diet was the same as in the second, viz., arrowroot, sugar, and butter; but the men now walked, on the first day, 23·76 miles, and on the second 32·78 miles. In the fifth period (of four days' duration), the men returned to their ordinary diet and exercise.

The general results obtained by Prof. Parkes were that, during the four days which followed the period of non-nitrogenous diet *with exercise,* more urea and more ureal nitrogen was discharged from the body than during the four days which followed the period of non-nitrogenous diet with *rest,* as shown by the following table, in which the quantities are expressed in grammes.

	S.	T.
Excess of urea in four days in after-work period .	3·364 .	7·700
Excess of total nitrogen do. do. .	1·492 .	4·560

Whence it would at first sight appear that on a non-nitrogenous diet exercise *does* increase the elimination of nitrogen, but both the men were more hungry after the long walks of the fourth period, and took so much more food containing nitrogen, that it is possible the excess was derived from this source. Dr. Parkes, therefore, believes the conclusion arrived at by MM. Fick and Wislicenus is certainly borne out, that, on a non-nitrogenous diet exercise produces no notable increase in the nitrogen of the urine, although when the whole period is considered it does produce a slight increase. It may now also, he thinks, be said that under similar conditions, exercise produces no increase in the excretion of nitrogen by the bowels. He is careful to add, however, that,

"Although it is thus certain that very severe exercise can be performed on non-nitrogenous diet for a short time, it does not follow that nitrogen is unnecessary. The largest experience shows not only that nitrogen must be supplied if work is to be done, but that the amount must augment with the work. For a short period the well-fed body possesses sufficient nitrogen to permit muscular exertion to go on for some time without a fresh supply; but the destruction of nitrogenous tissues in these two men is shown by the way in which, when nitrogen was again supplied, a large amount was retained in the body to compensate for the previous deprivation."

There was some probability, also, that the muscles and nerves of these men were becoming structurally impaired, as shown by great exhaustion occurring on the second day.

In the second paper, Dr. Parkes considers the variations that occur in the elimination of nitrogen when the body is at rest, and where exercise is taken respectively, on a regulated diet of nitrogen. The course of the experiments precisely the same as those just described, except that the diet was during sixteen days exactly the same on each day. During four days the men were at their ordinary employment; during two days rested, returned to ordinary work for four days; took very active exercise for two days; and were then for four days more on ordinary occupation. The food consisted of bread and butter, meat, and vegetables, and tea and coffee, sufficient in quantity exactly to maintain the body at its normal weight. The general results obtained by Dr. Parkes were—1. That with an unchanged ingress of nitrogen there was a slight excess of nitrogenous excretion during rest, as compared with a period of ordinary exercise. 2. There was a decrease of urinary nitrogenous excretion during active exercise as compared with a period of rest, and this was perceptible both when the ingress of nitrogen was stopped as well as when nitrogen was supplied in regular amount. 3. There was an excess, not great, but long-continued, in nitrogenous excretion after exercise. 4. There was a retention of nitrogen in the system when it was again supplied after having been cut off after both rest and exercise, and greatest in the latter case, showing that it is needed in the system, and that an insufficient supply at one time must be subsequently compensated. In addition, Dr. Parkes observes, we cannot leave out of account the well known dietetic fact, based on experience, that much muscular work always demands the supply of a larger amount of nitrogen. He considers both the old and new theories of the chemical changes involved in muscular action are alike insufficient to account for all the above facts. Upon the old theory, muscle undergoes disintegration during action, and it is natural to suppose that the amount of action is measurable by the quantity of nitrogen eliminated. The new view founded on the theory of Fick and Wislicenus is, that the nitrogenous framework of a muscle is merely the machinery which allows changes in the non-nitrogenous substances to take place, and that in itself it undergoes during exercise no changes.

Dr. Parkes suggests the following as a theory more consonant with the facts than either of the above.

"When a voluntary muscle is brought into action by the influence of the will, it appropriates nitrogen and grows; the stimulus, or the act of union, gives rise to changes in the non-nitrogenous substances surrounding the ultimate elements of the muscular substance which cause the conversion of heat into motion. The contraction continues (the will still acting) until the effete products of these changes arrest it; a state of rest ensues, during which time the effete products are removed, the muscle loses nitrogen, and can again be called into action by its stimulus. . . . . . This theory shows why the muscle requires nitrogen for its action, and why increased action requires increased nitrogen. The food must either supply this, or the store of nitrogen in the blood and other organs must be lessened. It enables us to understand why, in a well-fed body, it may be some time after nitrogen is cut off before the muscles have any difficulty in obtaining what they want, and why in a body ill supplied with nitrogen, exertion lessens, or if kept up, produces bad effects. If exertion is persevered in under such circumstances, a failure somewhere is always observed. Frequently the nervous system, or the heart, shows signs of weakness, a result which could hardly be explained by the view of the Swiss professors."

The objection that presents itself to our minds to Dr. Parkes' explanation is the difficulty of conceiving the muscle to appropriate more nitrogen in contraction, at a time when its supply of blood is considerably diminished. It seems to us more rational to suppose that the tissue really disintegrates during violent exertion, and that the products are thrown out, though not taken up, by the vessels. When they have accumulated beyond a certain proportion, the sense of fatigue is induced, rest then allows of the gradual absorption and removal of these secondary compounds which are forthwith discharged from the economy and produce the increase observed in the succeeding period of rest.

To Mr. Morrant Baker's edition (the sixth) of Dr. Kirkes' 'Physiology' the merit may be conceded of being, for its size, the most satisfactory text-book to put into the hands of a beginner. It is clearly written, it avoids controversy, it is remarkably free from errors; and under Mr. Morrant Baker's able editorship it very fairly represents the broad outlines of the present state of physiological science. A chapter has been added on the principal elementary tissues of the body, which we are sure will prove very serviceable to the great body of students. A small text-book of histology seems to be much wanted. With the exception of the introduction to Quain and Sharpey's Anatomy, there is literally not one in the language that is at all up to the present time. Will no one of our professors of physiology fill up the hiatus?

The little elementary work on Physiology by Prof. Huxley is well adapted for the purpose for which it is designed. The general features of the subject are clearly rendered, and few debatable points, except that of the actions of the intercostal muscles, which are stated with a refreshing simplicity, are introduced. It is pleasantly written, and will, we are sure, be of great service to the school-teacher and others for whom it was written.

The 'Introduction to Quain and Sharpey's Anatomy,' which has just been published, though unobtrusive in its character, is yet a very admirable manual of histology. Almost entirely free from controversy, there may be found in it a plain statement of facts, which will give the student a thorough insight into the microscopic structure of the tissues. Nothing can better prove the care with which this subject was drawn up in the former edition than the fact that it has been requisite to modify it in so slight a degree in this.

In the first section, devoted to the general consideration of the textures, an account of the researches of Prof. Graham on Dialysis has been introduced, whilst the chemical characters of the proximate constituents has been much condensed. The varieties and properties of the vegetable cell are then discussed, and from thence the transition is easy to the animal cell. In reference to the employment of the term cell, Dr. Sharpey remarks:

"The existence of animal cells destitute of envelope, although more insisted on of late years, has been all along recognised in the study of cell-development, and was expressly pointed out by Schwann himself. It has appeared to some that another name should be used to designate bodies which thus exist in a naked non-vesicular form. Brücke proposes to call them 'elementary organisms,' a term too cumbrous for use; as the first 'shaped' products of organisation which appear in the development of all but the lowest organised beings, they might be named 'protoplasts,' or as that name has been already used in a widely different sense—'monoplasts,' but after all seeing the universal currency of the term 'cell,' it is probably most convenient and best to adhere to it, with the understanding that in many cases it is used in a conventional sense."

The progress of microscopical investigation has rendered necessary some modification of the account contained in former editions of the production of new cells, which is now reduced to the following heads:

"1. The subdivision or fusion of pre-existing free cells, as occurs in the ovum and white blood corpuscles; and this may be either into two or into more than two. A similar process occurs in cartilage, the chief difference being that the cells are enclosed in a matrix with which the cell wall of the parent cell is continuous, and which takes no part in the division. 2. The development of cells from nuclei, which may either be contained in cells or may be free."

Lister's and Kühne's observations are referred to as well as those of Dr. Beale, whose views, however, respecting "germinal matter and formed material" are by no means accepted unreservedly, Dr. Sharpey remarking that he presumes it is not meant that "formed material" is incapable of undergoing further organization; for otherwise the proposition would be in opposition to well-known facts, such as the formation of fibres in the matrix of cartilage. A few lines are devoted also to Dr. Bennett's molecular theory of organization, respecting which Dr. Sharpey observes, that for his own part he—

"Is disposed to think that in the process of organization, as distinguished from its result, the cognisable form and mass of the organisable material, whether as cell or molecular, are of altogether subordinate consideration to the nature of its substance."

The descriptions of the tissues require no remark here; they are admirably clear, and most of the observations of late years receive due notice. The best section is undoubtedly that on bone, to which Dr. Sharpey has, as is well known, paid great attention. It is embellished by numerous excellent woodcuts, nearly the whole of which are original, though one or two are copied from Kolliker and the best foreign authors.

M. Durand's work is a series of philosophical essays of such wide scope and general character, that it is almost impossible, without going very fully into them, to do more than indicate the principal points treated of. The titles of the several essays are as follows: 1. A *Coup d'œil* of the Physiological Relations existing between the Organism and the Outer World; 2. The Experimental Physiology and Medicine of the Soul; 3. On Vital, as compared with Inorganic Properties and Forces; 4. What is an Organ? a Theoretical Investigation in General Anatomy; 5. On Function: its Faculty, its Organ, and its Specific Agent; 6. An Introduction to the Physiological Theory of Instinct; 7. Physiology of the Correlations of Physics and of Morals. Almost all these topics are treated of metaphysically; and we can, therefore, recommend the volume to such of our readers as are engaged in inquiries of this nature. The mental faculties are very freely discussed in somewhat of a materialistic spirit, and broad views are given of the functions appertaining to the several parts of the nervous system. In the fifth essay on instinct, M. Durand dwells on the importance of the study of Comparative Biology; and after reviewing and sharply criticising some of the definitions of instinct that have been given by preceding writers, he gives one which, perhaps, it is hardly fair to take from the whole of the preceding train of reasoning, but is certainly remarkable. It is that—

"Instinct, in the proper acceptation of the word, must be considered as a kind of local hypertrophy of the soul, more and more localised in proportion as we ascend in the scale of animated beings. Amongst the lower tribes, it is only perceived by those acts which tend to the satisfaction of the alimentary and reproductive appetites. Reduced to its narrowest passional and intellectual limits, to its most rudimentary simple and uniform expression, it is, in our opinion, the motor power which has its seat in the ganglia of the sympathetic system, and which is only rendered apparent in the work of nutrition."

M. Durand advances some singular views regarding the nature of the corporeal frame; thus, after referring to the opinion of M. Lacaze-Duthiers, to the effect that, in almost all the invertebrata, the individual is made up of a number or colony of distinct individuals, which may be designated zoonites, each of which possesses its own heart, respiratory orifice, &c., he proposes to extend this doctrine to the vertebrata and to consider their bodies as being also collections of zoonites, differing only in degree from those of the invertebrata—that is, in the existence of a greater division of labour and a greater specialisation of the individual parts, and he adduces the various arguments for and against his suggestion. The work will be useful to those who have devoted themselves to the study of Speculative Physiology.

We are glad to see that M. Brown-Séquard has again undertaken to edit a journal, a task for which, as may be concluded from the success attendant on his former efforts, he is in every way so well qualified. He has secured the co-operation of two gentlemen, Messrs. Charcot and Vulpian, whose labours in the field of experimental physiology are already well known, and if, as appears probable from present results, the work contain papers as valuable as those written for the 'Journal de la Physiologie,' of which it is to be regarded as the continuation, there is no doubt it will prove a great success. The parts already published contain seventeen separate memoirs, with short accounts of the chief advances in French and foreign physiological literature. The second part closes with a bibliographical index of the labours of our distinguished countryman, Mr. Lockhart Clarke, whose entire works are here brought into view at a glance, with the dates of their publication, and the mode (whether in journals or otherwise) in which they have been published. Amongst the original treatises we may notice 'Anatomical and Physiological Researches on the Spheno-palatine Ganglion,' by M. Prévost; 'On Osteitis,' by Dr. Ranvier; 'On Tubercle,' by M. V. Cornil; 'On the Pathogeny of Cerebral Hæmorrhage,' by MM. Charcot and Bouchard; 'On the Pathological Conditions found in Sclerosis of the Posterior Roots of the Spinal Cord, with Atrophy of the Posterior Roots,' by M. Vulpian; 'On the Cardiac Pulsation of the Frog,' by M. Prompt; 'On the Oïdium Albicans,' by M. Quinquand; 'On the Movements of Certain Organic Bodies at the Surface of Water, and the Relations of such Movements to a Theory of Smell,' by M. Liégeois, &c. If, in future numbers, papers of equal value are contributed, there is little question but that the Journal will obtain an extensive circulation, not only in France, but in this country also.

We have often been surprised that the United Kingdom was unable to support a journal devoted exclusively to anatomy and physiology. But the truth is, we suppose, that publishers turn a deaf ear to propositions involving considerable outlay, and resulting in the establishment of a serial of a purely scientific character. Everything they conceive must be practical. We have always seriously doubted this, and we have great pleasure in seeing that one publisher at least has had the courage to attempt to cater for the profession on these subjects, and we wish Professors Humphrey and Turner every success in their new and arduous undertaking. The first volume of the new series of their 'Journal of Anatomy and Physiology' contains, indeed, many interesting papers, and promises well for the future. Amongst the more important ones in the first part may be mentioned those of Mr. Gulliver, on the white and red blood corpuscles, and of Mr. Hair on the arrangement of the muscular fibres of the alligator, the amusing as well as learned disquisition of Professor Rolleston on ancient and modern domestic cats, the excellent paper of Mr. Mivart on the osteology of the Insectivora, and the contribution to the anatomy of the pilot-whale, by Professor Turner: whilst the second contains a suggestive essay by Drs. A. Crum Brown and Fraser, on the connection between chemical constitution and physiological action, an account of certain American crania by Professor Huxley, an interesting paper by Dr. Beigel, on the nature and action of Indian and African arrow poison, and a carefully drawn up account of the myology of the Orycteropus capensis and of the Phoca communis, by Professor Humphry, besides more than twenty other shorter communications. Each part contains also capital reports on the progress of anatomy by Professor Turner, and on recent English and foreign physiology, by Drs. Rutherford, Fraser, and Gamgee.

Review VI.

1. *Procès-Verbaux de la Conference Sanitaire Internationale, ouverte à Paris le 27 Juillet,* 1851. Tomes I et II. Folio. Pp. 396 and 412. Paris, 1852.

Proceedings of the International Sanitary Conference opened at Paris, 27th July, 1851.

2. *Procès-Verbaux de la Conference Sanitaire Internationale, ouverte à Constantinople, le* 13 *Fevrier,* 1866. 4to. Pp. 762.

Proceedings of the International Sanitary Conference opened at Constantinople, 13*th February,* 1866.

3. *Rapports faits à la Conference.* 4to. Pp. 379. Constantinople, 1866.

Reports made to the Conference.

In this article we purpose to give a summary of the principal contents of the proceedings of the late International Conference held in Constantinople on the subject of epidemic cholera, in order that the profession may be enabled to form their own opinion of the practical conclusions which were then adopted, and of the evidence on which these conclusions are based. The two volumes in which the proceedings are recorded are as yet very rare in this country, so that few persons can have an opportunity of examining them for themselves. It is the more necessary, too, at the present time, that a connected analysis of their contents be laid before the reader, as not only several of the topics discussed and opinions expressed by the Conference are occasionally being commented on in the press and at medical meetings, but also, more than once, reference has been made and questions asked in the Legislature as to what our Government propose to do in respect of the recommendations which have been officially made to them. But, before proceeding to our immediate task, it will not be unprofitable to take a retrospective brief notice of the sister work, the first on the list at the head of this article. It was undertaken now seventeen years ago for a similar object, and under similar circumstances; yet, strange as it may seem, its contents have (as far as we are aware) never been made known to the profession in any medical journal down to the present day. The truth is that, although printed as an official document, it was not published or circulated, in this country at least. The Conference of 1851 was more comprehensive in its scope than that of 1866; for it undertook to discuss and determine the whole subject of quarantine, in respect not of cholera only, but of other diseases, more especially of yellow fever and the plague. Delegates, medical and consular, were appointed to attend it by the governments of France, Great Britain, Russia, Austria, Piedmont, Tuscany, the Papal States, Naples, Turkey, Greece, Spain, and Portugal. The medical members were Drs. Melier, Sutherland, Rosenberger, Menis, Bo, Betti, Cappello, Carbonaro, Bartoletti, Costi, Monlau, and Grande. The conference sat for eight months, and held forty-three meetings. We shall confine our notice of their work to what has reference to the subject now in hand.[1] The delegates differed much in opinion as to the necessity for any

[1] A full analysis of the proceedings of the Conference will be found in a paper by Dr. Milroy in the 'Transactions of the National Association for the Promotion of Social Science,' 1859.

stringent quarantine in respect of cholera. The French, British, Austrian, and Piedmontese members were decidedly opposed to "measures of rigour." On the other hand, the Neapolitan and Papal members urged their necessity as much for the cholera as for the plague; the island of Elba and many places in Italy had been, it was alleged, preserved intact by the segregation or exclusion of all suspected arrivals. The Spanish and Portuguese members—while admitting that it is mainly by adopting sanitary measures on board merchant vessels, and also in sea and river ports, that the spread of the disease can be checked—contended that, until these measures were universally carried out, quarantine must be continued. Russia, it was stated, had not come to a definite conclusion on the question; she awaited further inquiry. On two points, however, experience seemed to her to be conclusive, viz. (we quote from Dr. Milroy's analysis)—

"That the disease, when occurring only in sporadic and occasional cases, is certainly not importable by intercourse; and, secondly, that the only *fomites* or articles capable of transmitting the cholera poison are bed or body clothes fouled with the excreta of the sick.

"The final decision of the Conference, as carried by a majority of votes, was that all arrivals whatever from a place where cholera exists should be liable to a quarantine of observation of five complete days, the voyage being included in this period, before free pratique is granted.

"If a case of the disease occurred during the voyage, the quarantine to date from the arrival of the vessel; and, if during the performance of quarantine, a fresh detention to be imposed from the date of each such occurrence.

"With respect to cargoes, it was decided that they shall never be required to be disembarked into a lazaret, or be subjected to any other measures of purification except free ventilation on board, and due attention to the cleanliness of the vessel itself.

"These remarks apply to arrivals from countries actually infected with the cholera. A shorter quarantine of observation, namely, for three days only including the voyage, might be imposed on arrivals from countries which a local board of health should consider to be compromised, either by proximity to an infected place or otherwise, although the disease may not yet have manifested itself."

To make assurance doubly sure on the side of presumed safety by these precautionary measures, it was determined that, even after the certified cessation of cholera in a place, an interval of ten days should be required to elapse before clean bills of health should be permitted to be issued therefrom. It need only be added to this short notice of the Conference of 1851 that the convention, which was drawn up in accordance with the views of the majority of the delegates, was accepted only by France, Piedmont, Portugal, Tuscany, and Turkey. Our Government declined to accede to it, on the ground of the proposed restrictions upon freedom of intercourse, from the apprehended risk of importation of the several diseases, being deemed extreme, and unnecessarily oppressive. Within a year or two afterwards, the quarantine regulations of some of the States represented at the Conference were more rigorous and severe than they had been previously.

The Conference of 1866 was of larger dimensions, although the scope of its inquiry was more limited, than that of its predecessor. The governments represented at it were seventeen in number; and the number of delegates who attended was in all 35, of whom 14 were diplomatic, and 21 were medical. France was represented by Dr. Fauvel; Britain, by Drs. Goodeve and Dickson; the Netherlands, by Drs. Van Geuns and Millingen; Prussia, by Dr. Muhlig; Austria, by Drs. Sotto and Polak; Russia, by Drs. Lenz, Pelikan, and Bykow; Spain, by Dr. Monlau; Portugal, by Dr. Gomez; Greece, by Dr. Maccas; Italy, by Drs. Bosi and Salvatori; the Papal States, by Dr. Spadaro; Sweden and Norway, by Dr. Hubsch; Turkey, by Salih Effendi and Dr. Bartoletti; Egypt, by Dr. Salem Bey; and Persia, by Dr. Sawas. Belgium and Denmark were represented only by diplomates; and the United States of America, which had been invited to join, did not send any delegate. The first meeting was held on February 13th, and the last on September 26th, 1866.

The Conference was opened by an address from the Minister for Foreign Affairs of the Porte. No sooner was this over and business commenced, than the French delegates (Count Lallemand was the diplomatic member), with an autocratic energy which characterised them throughout the proceedings, moved the immediate appointment of a committee to determine what precautionary measures should be forthwith urged upon the Ottoman Government for adoption, in the event of cholera again appearing this year among the pilgrims assembled at Mecca. The scheme recommended by France was nothing less than the total suspension of all maritime communication and intercourse whatever between any part of the Egyptian coast in the Red Sea, and all ports on the Arabian coast, as long as the disease continued among the pilgrims, and for fifteen days after the occurrence of the last case among them. Until then, the pilgrims should be required to remain in the Hedjaz, unless they preferred to proceed on their return journey by caravan along the desert;—whereby the disease was invariably, Dr. Fauvel asserted, got effectually rid of. To carry into effect these measures would of course require, among other means, the posting of ships of war at various parts of the coast of the Red Sea, so as to bar the approach of all vessels to the infected points, and prevent the possible escape of any of the pilgrims to Egypt by sea. As to the pilgrims returning to India and other lands to the east of the Red Sea, they might possibly be allowed to embark at some port considerably to the southward of Djeddah; although it would be wiser on the whole, it was thought, to subject all pilgrims without exception to one general rule until all trace of the disease among them had vanished.

This proposition at once gave rise to much controversy. Mr. Stuart, the British diplomatic member, took exception to it as at variance with the very terms of the original invitation, addressed by the French Government to England and other states in respect of the Conference, whose object was professed to be—

"De réchercher les causes primordiales du cholera—d'en étudier les caractères et la marche—d'en déterminer les points du départ principaux—enfin elle aurait à proposer les moyens pràtiques de le circonscrire et de l'étouffer à son origine;"

the ultimate and great object being to prevent, if possible, the recurrence of epidemic visitations of cholera in Europe. The present proposal was, he contended, "beginning with the end;" and, moreover, the consequences, maritime and commercial, involved in its adoption would manifestly be so very serious, that he declined to take any part in its consideration, without first consulting his Government. The Turkish and Persian delegates pointed out the disastrous results that might ensue from the sudden enforcement of measures of such extraordinary rigour, and this too, without any previous intimation to the tens of thousands of pilgrims that would soon be assembled at Mecca.[1] Dr. Pelikan contested the necessity of the alleged urgency for immediate action, on the ground that the past history of cholera proves that—

"the pestilence, always proceeding from India, has never followed two years in succession the same route in reaching Europe, the reason doubtless being that *the epidemic development of the cholera is not explicable solely by its transmissibility.*"

Moreover, the wide dispersion of cholera-infection already through-

[1] In 1865, the number of pilgrims was unusually great, not less, according to Dr. Gianelli, than 200,000. The majority arrived at Djeddah by sea. In that year, between 18,000 and 20,000 returned to Suez by sea. The total number of pilgrims this year, 1866, would probably be considerably over 100,000.

out Europe constitutes, he said, a much more formidable danger than "the conjectural re-importation" of the disease by the pilgrims assembled at Mecca. The Committee appointed to consider the French proposition could not, after much discussion, arrive at any satisfactory decision; of seven members only three were in favour of it, and one (Mr. Stuart) abstained from voting. It was then brought before the full Conference. Dr. Fauvel again strongly urged its necessity. M. Kalergi (Greece), quite approved of the suspension of all maritime communication between the Arabian and Egyptian coasts; but he thought that a strict quarantine might be necessary by land also, when the caravans reached Suez, or any other point of their destination. Drs. Muhlig and Sotto, and other members, were decidedly in favour of the French proposal. The former gentleman considered that the proposed restrictions were, in some respects, less rigorous than they ought to be, especially in respect of infected or suspected arrivals at Suez from India and other places beyond the Straits of Babel-māndeb. The Turkish delegates protested, in the name of humanity, against the scheme of barring all escape of the pilgrims by sea, when the means of transport by land were notoriously utterly inadequate for the purpose of their removal; and when the due supply of provisions and of water, for those who were forcibly detained at Mecca, could not be depended on. Dr. Goodeve asked what was proposed to be done with the numerous vessels, bringing not only pilgrims but stores of provisions and other cargoes, which would congregate at Djedda, the only considerable port of the Red Sea? Were they at once to be compelled to proceed to sea, whenever it was announced that cholera had appeared at Mecca? and, if so, where were they then to go to? and what were they to do? Dr. Hubsch, in reply, was of opinion that the interests of the public health should over-ride all mere commercial and maritime considerations whatever. On the question being put to the vote, seventeen of the twenty-six members present were in favour of the French proposition, eight were against it, and one member did not vote. The discidents were the British, Turkish, Persian, and Russian commissioners. Thus already, and before any evidence had been taken or examination commenced, the opinions and views of the majority of the Conference, on a most important practical point of inquiry, had been distinctly manifested and made known. In the original programme of the French delegates, it was suggested that the proposed embargo and interdiction upon all vessels arriving at Suez from infected ports should not be made applicable to arrivals from ports out of or beyond the Red Sea, whether they had touched at Aden or not; and that such arrivals should continue to be merely subject to the practice which had hitherto been in force there under such circumstances. This limitation or exception was obviously designed to meet the case of the Peninsular and Oriental steamers from India, which were fortnightly arriving at Suez; and which, it was admitted, during twenty years and more, had not been known to have introduced the disease into that port in a single instance. The Prussian delegates, notwithstanding, insisted upon the necessity, for the sake of uniformity and consistency in the recommendations of the Conference, of a much more rigorous quarantine by sea being now enforced upon all arrivals, without exception, from infected places. Moreover, they contended that, in the event of any cases of cholera occurring at Suez, a military cordon should forthwith be drawn round the town, so as to intercept all communication with any other place in Egypt, with the view of preventing the spread of the pestilence. Many of the other members of the majority appear to have taken the same view of the question; but as the proposal could never, it was well known, be carried into execution, it was finally resolved to omit altogether any notice of this point in the programme. Neither the British nor Russian delegates took any part in the discussion, and the former requested that a note of their abstention be entered on the minutes of the sitting. Having determined this point, the Conference proceeded to nominate committees to examine and report upon different sections of the great subject they had taken in hand, and which they sought to investigate in the most exhaustive manner. The several reports were, of course, submitted to the general body of the members before being finally adopted. It is but right to state that there was no small discrepancy of opinion on many of the points discussed, and that this was more especially the case when the practical application of the restrictive and coercive measures, proposed for the arrest or subjugation of the pestilence, came to be considered. But we must not anticipate.

The *first* report discusses the questions of the origin or genesis of cholera, its endemicity and epidemicity in India, and the propagation of the disease from that country. It is considered as proved that cholera had its origin in India, where it is still permanently endemic; it is extremely doubtful whether it is, or has been, so in any other land. Nowhere else can it be shown to have sprung up spontaneously. With respect to the Hedjaz, or district around Mecca, the disease has invariably been imported from the East; and this event has always coincided with the period of the annual pilgrimages to the Holy Places. As to India, the mother-seat of the pestilence, it would seem that the valley of the Ganges has been, and still is, the principal *habitat* of its endemic prevalence. The special conditions which give rise to its spontaneous production in India, and which occasion its persistence in certain localities in that country, are as yet unknown. That the pilgrimages to the many noted shrines in different parts of the peninsula are the most potent causes of its propagation and increase, can scarcely however, be doubted. It seems probable that in India, as in every other region of the world, away from and out of the endemic foci, the importation of the disease *ab extra* is the necessary condition of its epidemic development.

That the cholera is transmissible by human intercourse from one place to another is demonstrated by indisputable facts. The general course of each great European visitation has proved it. The rapidity of the successive epidemics has gone on increasing with the increased velocity and frequency of the means of transport; and the earliest cases in all countries have usually occurred in sea-port towns. Among the most notable instances in former years are those of the importation of the disease into Mauritius in 1819, into Quebec in 1832, into New York and Quebec in 1848, into Varna[1] in 1854, and into Madeira in 1856. The epidemic of 1865 supplied also many convincing proofs, as in the case of Constantinople, of Odessa and other places in the South of Russia, and of Altenburg in Saxony. Again, the efficaciousness of restrictive measures, on many occasions, affords additional confirmation of the same truth. Both Greece and Sicily escaped entirely in 1865, and in both countries a most rigorous quarantine was maintained. The frequent unsuccess of quarantine, in defending countries against the incursion of the pestilence, has been probably due to the insufficiency or defective application of the proper means to be employed for this purpose. Moreover, cholera when imported into a place, is not invariably or inevitably transmissible. For the transmission of the disease there must be adjuvant circumstances, and these are, happily, not always coincident; "otherwise the ravages of the pestilence might soon bring about the extinction of mankind."

On the important question whether the cholera is ever propagable to a distance by the medium of the atmosphere, independently of human intercourse, it is declared that, although it has not been always possible to demonstrate an inter-communication between an attacked locality and one already infected,

[1] The statements of the French and British naval medical officers as to the date of the first appearance of the cholera at Varna, among the allied armies, differ considerably. According to Dr. Bryson, the disease had manifested itself there three or four weeks before the date assigned by Dr. Marrouin, the physician-in-chief of the French fleet. Moreover, the Danubian provinces had been infected for months previously, and had suffered in the preceding year, 1853.

still wherever due inquiry has been made, such previous intercourse has been clearly established. And, yet, the atmosphere seems to be the chief, if not the sole, vehicle of the morbific element in its diffusiou; only (just as with the poison of typhus), it is never wafted to a great distance from a focus of infection. On no occasion, has epidemic cholera spread from one locality to another in a shorter time than that in which it might be conveyed by human intercourse. For an outbreak to occur in any locality two coincident conditions are requisite, viz., the arrival of some person or thing from a place already infected, and the existence there of favouring circumstances. It has been long believed, in respect of ships, that the mere lapse of a few days without any manifestation of disease on board during the voyage, or after arrival, afforded a sufficient guarantee against the possibility of their importation of cholera. This, it has been clearly shown, is a mistake; for even a long voyage does not always suffice to extinguish the danger of transmission of the disease in this way. Every ship arriving from an infected port should, therefore, be regarded as 'suspected,' or capable of introducing the pestilence. There is, moreover, no proportion between the amount, so to speak, of the imported poison—or, in other words, of the number of infected persons or things landed in a place—and the intensity of the subsequent outbreak. A single case of cholera arriving in a locality may give rise to an epidemic. The intensity of the epidemic depends on the more or less favouring conditions of the locality, just as in a conflagration the ravages are proportionate to the combustibility and quantity of materials the flames encounter. Nor does it require that the sick person arriving in a place, hitherto healthy, should be affected with developed cholera. If he has only premonitory diarrhœa upon him at the time, that may suffice to transmit the pestilence. As to whether persons being in perfect health themselves can be the channel of its transmission, there may be some doubt; but, be it remembered, a slight diarrhœa may exist without any visible signs of ill-health otherwise, or the possibility of its being found out. And, even supposing the absence of any premonitory symptoms among all the individuals, are we not entitled, should cholera break out after their arrival from an infected place, to conclude that "ce sont ces individus sains qui ont, par eux-mêmes, importé la maladie? et n'ont-ils pas pu porter avec eux des objêts contaminés?"

The incubation of the cholera poison, or the period between its reception into the system and the manifestation of the first symptoms, is usually short; in most instances, it does not exceed a few days, and in some cases not many hours. Occasionally, however, the period seems to extend to twenty days and even more. In these exceptional cases, the precursory diarrhœa has been probably included in the period of incubation; or the reception of the poison may possibly have occurred, after the sailing of the vessel, from the effluvia proceeding from contaminated baggage or other articles on board in which it had been latent.

That the clothes and baggage of persons arriving from an infected place, particularly if they have been used by cholera patients, and still more so if soiled with choleraic discharges, can communicate the disease, is now fully admitted; yet, it must be confessed that very few instances of a decisive and thoroughly demonstrative character can be quoted; "presque toujours ils se presentent entourées de circonstances qui permettent des interpretations differentes." Washerwomen, laundresses, and other persons who have had much to do with the clothes of cholera patients, are particularly liable to be attacked.[1] Certain facts seem to show that the disease may be transmitted to a great distance by articles of clothing, when these have been shut up for several months from free contact with the air. At the same time, it must be admitted that the importation of the disease by personal clothing, sent from an infected locality, is extremely rare. When exposed freely to the atmosphere, all such articles quickly lose their infective power.

Although no evidence whatever exists that ordinary mercantile cargoes have ever conveyed the cholera from an infected port, still such an occurrence may be regarded as possible; and especially, of course, in regard of such articles as rags, hides, &c.

It has been a question whether the corpses of cholera patients are liable to be the vehicles of infection. Many medical men have confidently asserted that they are, while others have as strongly denied the dictum. Certain it is that medical men who have been much engaged in post-mortem examinations,

[1] The question as to the greater liability to cholera of washerwomen than other persons in the same social position, &c., has not been worked out with that precision which its importance demands. At the Marine Hospital of Constantinople, only one of the persons so employed, during the severe outbreak there, was attacked; and this occurred at first, and before the clothes had begun to be treated with chloride of soda or lime prior to their being washed. Out of 300 women employed in washing the linen of cholera patients in the Paris hospitals, in 1865, not one died. The articles were always first steeped in a chloride solution. At the London hospital, in 1866, and where the same precaution was used, only one of the washerwomen died. She had been engaged at the work for a fortnight before she was attacked; she slept out of the house and in a locality where the epidemic was prevailing at the time. Dr. Goodeve states that the native washers at the Calcutta hospital have not suffered more than other persons in the same condition of life.

and the porters in hospital deadhouses, have not suffered more frequently than other persons.[1] Nevertheless, it is prudent to consider the corpses of cholera patients "comme dangereux." Another subject of dispute has been whether animals are liable to become infected with the disease; and if so, whether they can communicate it to man. From some experiments of Thiersch, Meyer, and others, it has been inferred that the ingestion of the matter of choleraic dejections into the stomach of animals is capable of producing cholera in them.

"Mais tous ces faits sont bien loin d'être probants, et quand même on admettrait l'análogie de certaines epizootics avec le cholera, et que les souris blanches empoisonnées par Thiersch aient presenté tous les symptomes de cette maladie, on serait loin d'être autorisé à conclure à l'identité de nature, et encore moins à la transmissibilité de l'animal à l'homme."[2]

Whether the hair or fur of a living animal can serve to retain and give off the choleraic poison, is not absolutely determined; possibly it may.

As to the influence of the different modes of intercommunication between countries in favouring the spread of the cholera, maritime transport is unquestionably the most frequent and the most dangerous in the conveyance of the disease to a distance. Next to ships, railroads must be regarded as the most common channels of distant transmission. Sandy deserts usually prove to be an effective check to the persistence, and to the diffusion, of the pestilence. Caravans speedily become free from it, after they had been a few days on their journey in the desert. The disease has never been imported into Suez or into Syria from the side of the desert.

With respect to the liability of ships coming from an infected

[1] During the Paris epidemic in 1865, out of 911 persons employed as bearers of coffins, or otherwise engaged at the funerals of hospital patients, two were attacked. No details are given (in the 'Gazette des Hôpitaux') respecting the circumstances of these two cases. Chloride of lime was put into the coffins, and the corpses were strewn over with sawdust, moistened with carbolic acid.

[2] Thiersch's experiments appear to be considered anything but conclusive, either in Germany or in France. At the Weimar Conference it was stated that they had been repeated both in Berlin and in Vienna, but without confirmatory results; and the Commissioners of the French Academy, in reporting on the *concours* in 1866 for the "Prix Breant," confine themselves to the simple statement that some of the authors, including M. Thiersch, in experimenting with choleraic discharges, "ont determiné chez les animaux des symptomes et des lesions semblables à ceux que l'on observe sur les hommes atteints du cholera." Dr. Sanderson's experiments failed in producing any results in dogs, hedgehogs, and pigeons. It was only in mice that the choleraic liquid had any decided effects in a considerable number of the animals experimented upon; and, when the experiments were repeated in cooler weather, they were altogether resultless. On the whole, experiments on animals seem to have, as yet, contributed very little to our useful knowledge of the disease.

port to transport the cholera, it must not be inferred that merely because they have had no sickness, or, perhaps, only a few cases of slight diarrhœa during the voyage, and because they appear to be innocuous on arrival, they are not likely to import the disease. This would be a dangerous mistake; it has been long, unfortunately, believed, but is clearly contradicted by several notable facts observed in the epidemic of 1865.

"The great majority of the vessels from Alexandria remained free from cholera; but have they the less not propagated it, even when no choleraic accident could be ascertained to have occurred on board? The decisive proof is that nowhere did the disease appear, except only where such vessels arrived."

Even in respect of the case of the Guadaloupe invasion, although there has been some difference of opinion whether it was due to a vessel from Marseilles or to one from Bourdeaux,

"the capital fact is, that the outbreak of the pestilence took place after the arrival of a vessel from a country which was infected. The immunity of other similar arrivals in other ports proves nothing against the importation in this case."

All evidence goes to show how much the propagation of the disease is promoted by the accumulation of masses of individuals in pilgrimages, armies, &c.; and how much good may be done by a timely thinning and dispersing the crowds over suitable localities. But however beneficial this may be to the persons immediately concerned, such dispersion

"ne devrait jamais être operée dans des localités indemnes où elle aurait pour resultat d'importer la maladie, en même temps qu'elle serait sans avantage pour les individus contaminés; elle devrait au contraire rester renfermée dans la circonscription de la localité où s'est manifestée l'infection."

The influence of bad hygienic and sanitary conditions of peoples and places in predisposing to invasions of cholera, and in aggravating their force and fatality, is now universally admitted. And as choleraic excreta appear to contain the 'generative principle' of the disease, it may be reasonably inferred that sewers, privies, and

"the contaminated waters of a town may become the channels of infection. The soil of a locality, once impregnated with choleraic dejections, seems to retain for a considerable period the property of disengaging the morbific principle, and thus to keep up the continuance of an epidemic, or even to regenerate it after it has ceased."

It has been already stated that the atmosphere is the principal vehicle of the cholera poison, and the chief agent in its dissemination, the generation of the poisonous element taking place only by successive renewals and multiplications within the human body, and never occurring spontaneously. As to the question how far the morbific poison can be wafted by the air, facts seem to show that the distance from the focus of infection is generally very inconsiderable; it probably never exceeds 100 metres. The statements, which have been made of its transport through the atmosphere to the distance of one mile and more, have not been duly authenticated.

That water, whether it be directly polluted with choleraic excreta, or whether it has become impregnated with the morbific poison diffused in the atmosphere, may be the vehicle of its transmission, has been shown by the experience of England. It is, however, mainly by the respiratory passages that the poison enters into the system; but the alimentary canal is, most probably, also a channel of its admission. It is within the stomach and intestines that the generation and multiplication of the cholera germs seem to be effected; and that the alvine dejections contain them, is incontestable. Whether the morbific principle may be evolved from the body by the lungs as well as by the digestive passages, it is not easy to determine:

"Le fait n'a pas été demontré, et en outre les phenomenes morbides de cholera le rendent peu probable."

The last point discussed in this report is the question, for what length of time may a person affected with precursory diarrhœa, or with confirmed cholera, be liable to communicate the disease? Some of the members held that "cette diarrhée infectieuse," may continue for several weeks; while others maintained that in the great majority of cases the duration of the disease, as well as its incubation, is short—usually not exceeding three days, and very rarely a week—and that, consequently,

"On pouvait, en toute securité, tenir pour non-cholerique un individu isolé de toute cause de contamination, dont la diarrhée se serait prolongée plus de huit jours après son isolement, sans qu'il ait presenté aucun signe caracteristique de la maladie."

The majority adopted this view of the question, but without explaining how the "premonitory diarrhœa" might be discriminated from other kinds of diarrhœa, prevailing at the same time during the epidemic prevalence.

The *second* report is devoted to describing "the march and mode of propagation of the cholera in 1865." The professed object of the Committee which drew it up is declared to be, not to give a simple historical narrative of the epidemic, but rather, "de reunir les faits les plus saillants qui se rattachent à sa marche afin d'en tirer la preuve de son importation par des hommes d'un lieu malade à un lieu sain, ou bien, dans le cas contraire, d'etablir le principe de la diffusion de l'epidemie par l'air et sans le concours de malades où d'objêts contaminés."

It is of course unnecessary to enter into any of the details of this report, as the whole subject has been gone into at length in a recent number of this journal. We shall therefore here only glance at the practical conclusions, which, in the opinion of the Commission, are fairly deducible from the evidence which has been brought together. Premising that the earliest cases of the cholera, wherever it manifested itself, were (with one or two trivial exceptions) consequent upon the arrival, by sea or land, of persons or things from places previously infected, attention is then drawn to the fact that the disease appeared at Constantinople, at Gibraltar, at Guadaloupe, and at Marseilles, after the arrival of ships which had not been subjected to quarantine. Among the places which, by means of a complete sequestration of all cholera arrivals (*provenances*) escaped the pestilence, may be mentioned Cavalla (on the coast of Macedonia), Volo (on the coast of Thessaly), and the islands of Chio and Crete; these places established encampments on small islands which had no communication with the mainland. At other places, as Bourgas and Sinope in the Black Sea, at Mytilene and Rhodes, and at Benghazi (on the African coast), the same fortunate result was obtained by camping out all persons who arrived from infected localities at a distance from human dwellings, and strictly watching them. This experience shows that lazarets, to be a secure means of prophylaxis, should be established as much as possible on islands, and with a large amount of airy space round them. Greece affords a striking instance of exemption, attributable to her system of quarantine, which is more strict than anywhere else. She refused to admit all arrivals from infected places into any of her ports on the mainland; they were obliged to go to the islands of Delos or Skiathos, where no fewer than 25,000 persons performed quarantine in 1865. The islands of Samos and Sicily also, surrounded, so to speak, with foci of infection, owed their immunity to the system of repulsion which they strictly maintained, from the beginning to the complete disappearance of the epidemic. The experience of New York, too, confirms in the most conclusive manner the efficacy of quarantine measures, properly applied, in preventing the spread of cholera.

To the report is appended a chart, indicating the march of the epidemic of 1865 from its presumed starting points at Singapore and Calcutta to Mecca, and thence on to Alexandria. From this, as a centre, radiate numerous red lines in every direction to the several places to which the disease was conveyed directly from Egypt. These, again, are, in like manner, shown to have become secondary foci, from which it emanated to other localities, which are designated as tertiary foci. Moreover, the exact course of the pestilence being always shown by the direction of the arrows affixed, and the precise dates of its origin in each spot being also given, no traveller's route, when indicated on a map, could be laid out more definitely or made more easily intelligible. If the data from which the chart has been constructed are accepted as thoroughly trustworthy, the migratory movements of the epidemic of 1865 cannot certainly be regarded as in any way obscure or problematical. In respect of Great Britain, the only locality indicated is Liverpool, in connection with the transmission of the disease thence to Halifax in the spring of 1866. Southampton and Epping are not marked as infected localities in 1865.

The *third* report treats of "the hygienic measures to be taken for preservation against the Asiatic cholera." These measures are subdivided into six classes, viz.:—1. Measures to be adopted in places or countries deemed to be permanent foci of the disease; 2. Measures to prevent, as far as possible, the importation of cholera by sea; 3. Measures to diminish the chances of reception of the disease in sea-ports by sanitary regulations; 4. Measures to diminish the predisposition or receptivity of towns and dwellings by sanitary regulations; 5. Measures to arrest, as far as possible, the spread of the disease in the interior of a country; and, 6. Measures to prevent and extinguish foci of infection by destroying the germs of the disease in the atmosphere, or in contaminated objects, by means of disinfecting agents.

In reference to the first of these points, allusion is made with commendation to the great sanitary works that are now being carried out in the three presidencies of India; to the improvements which have been, and are being, effected in barracks and other military establishments; to the regulations relating to pilgrims and places of pilgrimage in the country; and to the regulations affecting the conveyance of pilgrims from Indian ports, under the Native Passenger Act of 1858.

"The transmissibility of cholera having only recently been recognised in India, as almost everywhere else, it has been within the last few years merely, that sanitary measures have begun to be based on this principle." In the Bombay presidency, pilgrims on returning from their journey are now not allowed to enter towns or military stations until it has been ascertained by examination that they are free from diarrhœa or other symptoms of cholera; otherwise, they are detained under observation for forty-eight hours, or longer if need be, and till all traces of disease have ceased; due attention being of course paid to maintaining cleanliness in the encampment, and to providing proper food and medical treatment. Most satisfactory results have followed the adoption of these measures. But, however useful hygienic precautions may be, they will never suffice of themselves to extinguish the cholera. "It is obvious that measures directed against the importation and propagation of the disease are indispensable, and will continue to be so for a long time;" for "its transmissibility may give rise to deadly epidemics as long as there shall remain a single permanent focus of infection,—we might almost say, as long as there remains a single individual affected with the malady."

On the second point enumerated, which deals largely with naval hygiene, it is unnecessary to say more than that the importance of sanitary arrangements in merchant and passenger ships is fully discussed; and with respect to the third one, that which relates to the necessity for the sanitary regulation of seaport towns, there can be one opinion. Equally important is the purification of inland towns, villages, and detached dwellings. The experience of England, during the last twenty years, is largely quoted to prove the salutary effects of such measures. Not a single reference is made to the experience of any continental country—a fact of no small significance, when coupled with the relative slightness of the last cholera visitation in Great Britain, and contrasted with its fatality in Paris and most other large cities of Europe.

In conformity with the doctrine that the excreta of choleraic patients alone contain the infective elements of the disease, and believing that the dejections of a single person, when received into a common sewer, may suffice to poison the neighbouring houses which drain into that sewer, through the medium of the refluent gas finding its way into the dwellings, more especially during the night when doors and windows are shut, the Commission give a decided preference to the system of "fosses mobiles" during a cholera season over that of ordinary privies or waterclosets which drain into a common sewer; as the former can be immediately removed and disinfected before being buried in the earth: the risk of spreading the poison is thus greatly diminished. It would not be unreasonable to go so far as absolutely to prohibit the use of common latrines, and to make obligatory the general disinfection of excreta, &c. Such a measure, if rigorously enforced from the beginning of an invasion, might arrest the development of the pestilence, especially if its adoption was combined with a system of daily house-to-house visitation. Many facts, related by Pettenkofer and others, attest the power of disinfectants in checking the spread of the disease.

With respect to the removal and interment of the corpses of cholera patients, the more quickly it is done the better; the burial should be within twenty-four hours of the decease. The body should be put at once into a pitched coffin, and covered with a layer of quicklime; and, when placed in the grave, quicklime should be sprinkled over it. "The conveyance of the corpses of persons who have died from the cholera to a locality free from the disease should be interdicted."

On the question of hospital accommodation for the sick, special hospitals should be provided. If a house be hired for the purpose, the adjoining ones should be evacuated. If general hospitals must be made use of (which, considering the contagiousness of the disease, should if possible be avoided), separate wards must be assigned to cholera patients. Special vehicles for the conveyance of the sick from their homes are indispensable. For attendants upon the sick, persons who have passed through the disease themselves are the fittest.

Appended to this report is a special one by Dr. Muhlig, a member of the Commission, on "disinfection applied to the cholera;" also an additional note relating to the treatment of ships, their cargoes, &c., in epidemic seasons. These need not detain us.

The *fourth* report deals with "the measures to be taken in the East for the purpose of preventing new invasions of Europe by the cholera." Against the importation of the disease by sea, three series of defences or obstacles, "*echelonnés sur le trajet parcouru par le fleau,*" are proposed, viz.:—1. Precautionary measures at the entrance of the Red Sea; 2. Measures to preserve Egypt, if the coast of the Red Sea become infected; and 3. Measures to protect Europe against Egypt if the disease has penetrated to it. The prophylactic measures by land against the transmission of the pestilence from India to Persia, and thence to Europe, are, of course, more difficult than those by sea; "et cependant là etait le point stategique de la preservation générale." As to the measures at the entrance to the Red Sea, it is recommended that a quarantine station should be established there on an island, if possible; and that all vessels coming from the East should be required to touch at it for examination, and, if need be, for detention and purification. The institution should be of an international character, *i. e.* be under the direction of commissioners from different European countries as well as from Egypt and Turkey.

For the purpose of arresting the spread of the disease to Egypt, in the event of its being introduced into any part of the coast of the Red Sea, notwithstanding the surveillance proposed at the above station in the Straits of Babel-mandeb, sanitary posts should be established at several ports on the African as well as on the Arabian shore; and there should be at least two lazarettos, one for the use of pilgrims exclusively, and the other for ordinary arrivals of ships and passengers. For the general superintendence and direction of quarantine and sanitary measures in the Red Sea, a permanent international commission should be made resident at Suez, with full power to decide on all questions relating to the public health. If cholera should appear in the Hedjaz during the period of pilgrimage, it was decided that all communication whatever by sea between the infected part of the coast and the Egyptian littoral at Suez, and elsewhere, should be suspended during the continuance of the disease, and for ten days at least after all traces of it had ceased, as officially declared by the sanitary authority resident at the spot. The British and Egyptian members of the Conference objected to this recommendation, but their objections were overruled by a large majority. To the objection that the proposed interruption of all transit from the East to Europe, *via* Egypt, would inflict enormous loss and inconvenience, it was suggested by one of the delegates that the route by the way of the Cape of Good Hope would still be open. In the event of the pestilence reaching Egypt in spite of the above precautionary measures, it is recommended—for then "le danger est aux portes de l'Europe, et l'importation y est infaillible si des mesures serièuses ne s'y opposent pas,"—that the same heroic measure of a temporary embargo on all intercourse between that country and every part of the Mediterranean seaboard should be at once established, and maintained while the disease lasts. Again, the British and Egyptian commissioners objected, but to no avail.

Our limits prevent our going into details respecting the quarantine, and other prophylactic measures, proposed with the view of preventing the importation of the cholera by land on the Turko-Persian frontiers, or by the way of Bokara and Tartary, or on the Russo-Persian frontier; and with these details the report closes.

The *fifth* and last report is devoted to the consideration of the "Quarantine measures applicable to cholera arrivals (*provenances choleriques*)." The first questions examined are, What have been the results of quarantine as hitherto practised against the cholera? and then, What are the fundamental principles derived from experience which should guide us in the matter? It is admitted that, on the first European visitation of the pestilence, stringent quarantine measures, both by sea and land, were everywhere tried, and that on the whole they signally failed; but, at that time, the knowledge of the disease was notoriously inexact and incomplete. From 1847 to 1850, Sweden made a much more sustained attempt to safeguard herself than any other country; "cette fois encore le cholera a franchi ces barrières élèvées au priz de très-grandes sacrifices." As to the recommendations of the International Conference in 1851, what could be expected, it is asked, from a quarantine of from three to five days, without taking any regard either of the incubative period of the disease, or of the premonitory diarrhœa, or of the influence of contaminated articles, and of bed and body clothes, soiled with cholera dejections—all conditions which determine, in the present day, the laws of the transmission of cholera? At that time, too, lazarets were badly situated, often crowded, ill-ventilated, and unwholesome, so that they were more likely to communicate a disease to the neighbouring residents than to protect them from its assaults. The experience of former times is deemed, therefore, to be quite inconclusive. Greece, however, then as now, has afforded a signal example of what may be realised by stringent quarantine. By the complete sequestration of all infected arrivals on small islands, duly watched, she has escaped in every European visitation, except only in 1854, when she was prevented from carrying out her rigorous system of exclusion by the Piræus being occupied by foreign troops. The experience of 1865 has furnished numerous other strong proofs to the same effect, as at Crete, Volo, New York, &c. The conclusion from these facts is that—

"il est incontestable que des quarantaines etablis sur des bases rationelles et conformes aux progrès de la science peuvent servir de barrière efficace contre l'envahissement du cholera."

With respect to the utility and applicability of sanitary cordons around infected localities, the Conference, after quoting several instances of their reputed efficacy in Russia during the epidemic of 1830-31, and in Syria during the late visitation,[1] came to the conclusion that—

"Employés au milieu de populations nombreuses et serrées, ils sont d'un effet incertain et souvent sont dangereux; que, par contre, employés dans les localités limitées, ou des contrées dont a population est clair-semée, comme dans certains pays Asiatiques, les

[1] Nowhere has the trial of sanitary cordons been so systematically and repeatedly made, and that, too, under circumstances of very effective isolation, arising from the nature of the locality, as by Spain in respect of Gibraltar when cholera has been in that citadel. The results have hitherto not been encouraging; a vast amount of distress has been inflicted on the inhabitants, not only of the Rock but also of the adjacent district of Spain; and, on no occasion, has the disease been kept out.

cordons sont appelés à rendre de grands services contre la propagation du cholera."

To cut off all communication with an infected place is unquestionably, whenever it is practicable, the best safeguard against the dissemination of the disease; but the measure is generally very difficult to maintain for any length of time. To attempt to prevent the flight of the panic-stricken inhabitants from a city which has become the seat of the pestilence, would scarcely be possible; yet, something requires to be done to mitigate the evils consequent upon unlimited dispersion. Nowhere was the flight of the people from infected localities, in 1865, more conspicuous than in Spain, where some attempts were made in several places to confine them. A third of the inhabitants fled from Valentia; and nearly the half of the large population of Barcelona, 190,000, fled from that city for a time. There were between 30,000 and 40,000 fugitives from Alexandria. The Commissioners recommend that there should be, at least, a regulation in force restricting the number of persons to be received on board vessels at such a time; and, moreover, that a strict examination of them, and of their baggage, should be enjoined before they are permitted to leave the port.

Minute details are given respecting the suitable sites, and the proper construction, of lazarets. "Parloirs," or "parlatorios," are condemned, because the germs of the disease might possibly be transmitted by the air from the *detenus* to visitors, who might thus convey them back to the town. From the moment, as remarked by one of the delegates, that the Conference admitted that the choleraic poison may, in certain cases, be conveyed by the atmosphere to a distance of 100 metres, it would stultify itself by sanctioning "parloirs." Common privies or closets in lazarets are disapproved of; and moveable closets, provided with disinfectants, are recommended. A proposal by two of the members that each person should have a separate commode, so that "le medicin à sa visite journaliere put inspecter les dejections alvines pour reconnaitre la nature, et constater par ce moyen le debut d'une diarrhée," was voted impracticable in a place where hundreds of persons might be congregated at a time. The *detenus* should be arranged in different categories, according to the degree of suspicion, and the dates of their admission; and these categories must be kept quite separate from each other; otherwise a healthy person would be exposed to contract the disease, as long as he remained in the lazaret. To every lazaret there should be three physicians attached: two of them to be always resident in, and not to leave, the establishment. Two kinds of lazarets are required, viz., 1. Lazarets of observation, where vessels having a clean bill of health, but which may be deemed suspected by the authorities, may perform quarantine without disembarkation of passengers, or discharge of cargoes; and 2. Foul-bill lazarets, where rigorous quarantine, necessitating the landing and systematic purification of passengers and cargoes, is performed.

With respect to the length of quarantine to be enforced, the Conference, believing that a detention for two or three days over and above the ordinary *maximum* of the incubative period would afford a sufficient guarantee against the transmission of cholera, recommend that, as a general rule, all persons arriving from an infected place should undergo a quarantine of ten full days, commencing from the date of arrival. Should any case of cholera, or of choleraic diarrhœa, occur among the *detenus*, "les personnes saines, après la separation des malades, devraient recommencer la quarantaine de dix jours pleins." Some of the members were of opinion that the period of detention should not be less than fifteen or seventeen days. As choleraic diarrhœa is not always distinguishable from ordinary diarrhœa, all persons suffering from looseness of the bowels should be regarded as "suspected," and pratique should not be granted to any one until the medical officer has satisfied himself of the innocuousness of his case. All contaminated ships must be kept in strict quarantine for ten days; if any case or cases of disease have occurred during the voyage, other rigorous precautions, including the discharge and disinfection of the cargo, are declared to be necessary. If the voyage has exceeded fifteen days, and without any sickness on board, the quarantine may be reduced to five days; but no length of voyage can be deemed sufficient to render a ship, in which a case of cholera may have at any time occurred, free from the taint of infection, and, therefore, admissible to pratique on arrival. A ship from Calcutta, for example, which may have had the misfortune to have had one of her crew or passengers sick in going down the Hooghly, although perfectly healthy during the rest of the voyage, must be considered as "suspected," and be treated accordingly. Under certain conditions, the period of the voyage may be taken into account in determining the duration of the quarantine to be performed. This relaxation was, however, opposed by several of the delegates, as resting on a false basis, and likely to frustrate the object of the whole system.

In regard to arrivals from infected places *by land*, (which are considered to be less dangerous and less liable to transport the disease than arrivals by *sea*,) a detention of eight full days is recommended as a general rule; but pilgrims, emigrants, and troops, should be kept in quarantine longer. "Si les provenances de terre partaient d'un foyer rapproché d'un à trois jours de marche, la quarantaine serait de dix jours pleins." This regulation would, of course, apply to the intercourse between all the cities on the continent, including Paris, and this country, during an epidemic season.

Minute particulars are given as to the most efficient mode of disinfecting ships, and everything on board of them. The articles of cargoes are divided into "susceptible" and "non-susceptible," according to their presumed liability to convey the morbific germs from infected places. Letters and dispatches, which are exposed to contamination by contact with persons affected with cholera, should be inclosed in a box, and be disinfected with chlorine, but, "sans être percées." As ready-made clothes and such goods are manufactured by workmen who, if sick, might soil and infect them, they should be disinfected by aeration, immersion in water, or by fumigation, according to the nature of the articles. As to ordinary cargoes, always excepting rags, hides, &c., their liability to transmit cholera has not been demonstrated; and it seems certain that goods imported from India into Suez, or directly into Europe, have never been known to introduce the disease. The same degree of doubt exists as to the liability of living animals to convey the infective poïson, when they have been exposed to it. Nevertheless it will be prudent to subject them, "à des mesures restrictives et de desinfection dans des circonstances dont l'appreciation dependra des autorités sanitaires."

What time should elapse between the cessation of epidemic cholera in a place, and the granting of free pratique to arrivals thence when provided with a clean bill of health? To grant free pratique at once, on the mere announcement by the authorities (always desirous to get rid of restrictions on the intercourse and commerce of their community) that the disease had ceased in the place, would be an imprudence which might lead to serious consequences; as occasional and scattered cases are often known to occur for some time afterwards. It is, therefore, recommended that all boards of health should agree not to grant free pratique to arrivals from any infected port, until fifteen days have elapsed after the date of the entire cessation of the epidemic.

In conclusion, the Conference point out the necessity that heavy penalties should be attached, on the part of every Government, to all evasions or violations of quarantine enactments and regulations—

"Les reticences, les fausses declarations, rendent illusoires le systeme restrictif le mieux combiné, et compromettent la santé publique. Elles doivent être severement punies par les lois de chaque pays."

And with this recommendation the labours of the Conference close, and our analysis comes to an end. This, it will have been seen, has been expository, not critical. A work which has been the offspring of so much combined labour among medical men of different nations, and which comes forth with all the prestige of diplomatic authority, should obviously be well known by the profession in this country; for no country in the world is more concerned, few so much, in the practical questions discussed by the Conference, and none has certainly contributed more abundant and valuable materials to assist in their rightful solution. That, moreover, it will in future be much appealed to in various parts of the continent, and also abroad, as a guide and directory on many of the points treated of, can scarcely be doubted; for, already, its influence has been made manifest in the character of the prophylactic or defensive measures resorted to for the exclusion of cholera in several of the Mediterranean ports, and nowhere more conspicuously than in our own colony of Malta. On the other hand, exception has been taken in this country to the accuracy of many of the statements, and to the soundness of not a few of the conclusions, sanctioned by the Conference. Some of the former have been clearly shown to be inexact. All, for example, that relates to the limitation of the endemicity of the disease exclusively to certain parts of India, and much relating to the history of its epidemic outbreaks in the East in former years, are regarded by competent authorities as conjectural and illusory. The narrative, too, of the circumstances connected with the development of the disease in the Hedjaz in the spring of 1865, tracing that event back to direct importation from one point, viz., the distant settlement of Singapore, without having duly ascertained whether it existed in intermediate regions or localities prior to the arrival of the infected vessels from the Straits of Malacca, has been deemed anything but satisfactory. And the same remark has been made in respect of several of the allegations, put forth as indisputable facts, regarding its traceable transmission from Egypt throughout the Mediterranean, and from one point of the continent to another.[1] Sufficient

[1] To take one instance, and that from a country which more than any other exercises a most vigilant surveillance at all her sea ports, and must, therefore, be the better enabled to communicate exact intelligence of all that occurs in respect of arrivals from other countries. It is alleged that Barcelona received the infection from Malta by the British fleet, and that the first case occurred there (Barcelona) on July 22. No details whatever beyond these two statements appear to have been given by the Spanish delegates to the Conference. It now appears that the fleet left Malta harbour on July 1st, and was at sea till the 12th, when it reached Barcelona, where it remained until the 22nd. On that day, it sailed

scrutiny, it is maintained, has at least not been exercised in ascertaining the accuracy of the data made use of. Certain it is that the entire account of the epidemic has been drawn up under the dominant influence of an accepted idea, viz., that the cholera is a virulently transmissible or contagious disease, and that its propagation and spread are due solely to the transmission of its infective poison from man to man, either directly, or through the medium of *fomites* which have been impregnated with the morbific matter. In these respects it resembles, it will be observed, the cattle plague. Assuming the above proposition as demonstrated, and coupling it with the annex that the infective germs of the disease are generated in the alimentary canal, and are contained in the alvine excreta (whether the ejecta from the stomach also contain them, appears to be yet undecided), the Conference have built up an elaborate superstructure complete in all its details, alike of theory and of practice, each part conformable to and supporting the others, so that the whole fabric has the aspect of harmonious consistency. In the language of the schools, the work is planned on the deductive method of investigation, in contrast with the inductive method, followed in the somewhat analogous work of the Royal Academy of France on the subject of the Plague, about twenty years before, and to which an incidental reference is made.

As the quarantine measures recommended by the Conference for the protection, in future, of Europe from visitations of the cholera lead back to the re-adoption of the system that was in force at the end of last century against the apprehended invasion of the plague, and as grave doubts are entertained among medical men on the Continent, as well as in this country, as to the wisdom or the practicability of these recommendations, it obviously behoves the profession to consider what advice should be urged upon our Legislature and government with the view of obtaining a thorough investigation, on a large and comprehensive scale, of this confessedly difficult and cogent question of State Medicine.

The memorial on the subject which was recently addressed to the Privy Council by many of the most eminent men in our ranks, and which we published in the last number of this journal, sets forth in the clearest manner the necessity there is for such an inquiry by means of a Royal Commission appointed

for the Bay of Rosas, arriving there on the 27th. The fleet was healthy on leaving Malta, and no case of choleraic sickness occurred in any of the ships before reaching Barcelona, or afterwards. No quarantine was imposed upon it on arrival there. It may be added that the disease had appeared in the port of Valentia on the same coast, about a hundred miles to the southward, a fortnight before it was discovered in Barcelona, and ten days before the first case occurred at Gibraltar.

for this purpose. But, notwithstanding its acknowledged importance, both nationally and internationally, no one can be sanguine that this desirable object will be speedily attained. To no subject could the attention of the forthcoming meeting of the British Medical Association at Oxford be more appropriately, and more beneficially, directed: we commend it to their consideration.

Review VII.

Mémoires de l'Académie Impériale de Médecine. Tome xxviii, 1re Partie. 4to. Paris, 1867.

It is a considerable period since we noticed in this Review the publications of the French Academy of Medicine, and before adverting to the contents of the present volume it may be of interest to some of our readers if we furnish some account of the learned body under whose auspices it is published.

The Académie de Médecine is, so to say, the heir or resuscitation, after a long interval of abeyance, of the famous Académie de Chirurgie and the Société Royale de Médecine, both suppressed during the troublous times of 1792. It was founded by royal charter in 1820, opened in 1824, and since that period has pursued the even tenour of its way, undergoing no other change during the various political vicissitudes that have occurred, except that of altering its title from "Royal" to "National," and then again to "Imperial." Within its walls have been embraced almost all those who have attained reputation in the various branches of medical science since the period of its foundation, and the honour of its membership is as highly appreciated and as keenly sought for at the present day as at any former period. It is true that from time to time other important scientific societies have been established, corresponding to various sections of the Academy, such as the Sociétés d'Anatomie, de Biologie, and de Chirurgie, but these are in no sense rival bodies, having in view only a more active development of their respective branches of investigation than could be attained in the academical sections, composed as these are rather of the seniors who have attained their reputation than of those who are still laboriously working.

The Academy embraces medicine under its widest acceptation, and consists of 100 members, who are distributed into the following eleven sections:—Anatomy and Physiology, 10 members; Medical Pathology, 13; Surgical Pathology, 10; Therapeutics and Medical Natural History, 10; Operative Medicine, 7; Pathological Anatomy, 7; Obstetrics, 7; Public Hygiene, Legal Medicine, and Medical Police, 10; Veterinary Medicine, 6; Medical Physics and Chemistry, 10; Pharmacy, 10. There are also at present nine "Free Associates," among whom we observe the names of MM. Chevreul, Milne-Edwards, Littré, Husson, Director of Public Assistance, and Conneau, the Emperor's physician. Next we have lists of twelve "National Associates" and fourteen "Foreign Associates." These may be increased each to twenty and it is remarkable that the only English names are those of Travers, Faraday (both dead), and Professor Simpson. Finally, there are 120 "National Correspondents," which number is to be reduced gradually to 100; and 61 "Foreign Correspondents" —to be reduced to 50. The English names are those of Farre, Owen, Roget, Wardrop, Hodgson, and R. Lee. The respected physician, Dr. Farre, elected in 1835, has been long dead. When vacancies occur in any of the academical sections, the titles to admission of the various candidates are referred to the section in question, which has to make its report to a secret sitting of the Academy, giving in a list of not less than three, and not more than four, names arranged in order of merit. These have to be balloted for at the next public meeting of the Academy, an absolute majority of the members present being necessary to secure election. As a general rule, the Academy selects the name placed first on the list by the section; but this is by no means always the case, for the contest is sometimes very keen and close, and the custom of the candidates having to visit and personally solicit the votes of the electors is considered by many as a very burdensome and objectionable procedure.

The Academy has two forms of publication, its 'Bulletins' and 'Mémoires,' both of which have now become long and important series, constantly consulted. In the 'Bulletins,' commenced in 1836, are furnished authentic reports of the memoirs, correspondence, reports, and discussions of the Academy. Some of these last reach portentious dimensions, but many of them are of great importance, and characterised by high ability and deep research. The 'Memoirs' or 'Transactions,' the first volume of which appeared in 1828, are usually published annually, and besides certain official reports contain the more elaborate papers that have been deemed worthy of selection by the committee of publication. We may here notice that all essays, memoirs, &c., submitted to the notice of the Academy are referred for examination to committees, the names of whose members are made public, and that the reports of these committees upon such memoirs are always published *in extensô* in the 'Bulletin;' so that the grounds of their recommendations as to the ultimate disposal of the memoirs in question, whether by publication, simply thanking the author, &c., are made publicly known. Not only are they known, but they may have to be defended, for some of the most important discussions at the Academy have arisen upon the presentation of these reports. In this way the subject becomes thoroughly ventilated, and complete justice is secured to the author.

The Academy, being a body supported by the State, has certain public functions to perform. Thus it conducts public gratuitous vaccination, furnishing an annual report to the Government upon the subject, prepared by M. Depaul, the Vaccine Director of the Academy, but previously submitted to this body for its approval or modification. The Academy also possesses a chemical laboratory, the chief business of which seems to be to conduct the analyses of the new mineral waters which are continually being discovered in France. Reports are made concerning these by the director of the laboratory, and upon them are based the recommendations of the Academy as to the admission of the new spring into the authorised list of mineral waters. The body is also frequently consulted by the Government as to various matters connected with public health.

Before describing the contents of the volume of 'Memoires' before us, we may for an instant advert to the question which has at times been mooted as to how far it is practicable or desirable to establish a similar Academy here. The fusion of the Royal Medical and Chirurgical and the other Medical Societies has been suggested as a possible means of accomplishing this. This might, if properly organized, give rise to much needed improvement in the working of our medical societies; but in the face of the increasing disposition to separation rather than amalgamation manifested by the formation of new societies, it probably would not be practicable. At all events it could never constitute an institution analogous to an Academy, as long as mere respectability and a willingness to pay a subscription constituted a claim to membership. This must rest on scientific merit alone, to be strictly determined by competent judges; and if we are desirous of an Academy, such an institution can only be modelled *de novô*, and not by any attempt to amalgamate bodies founded on a mere monetary basis. In Paris there are and have always been numerous medical societies flourishing beside the Academy, the object of all such societies, in fact, being as much social as scientific.

1. The first article in the present volume reminds us of the practice which has always prevailed in the Academy of having

eulogia of its most distinguished deceased members pronounced by its secretary. Those delivered by M. Pariset, its first secretary, have been collected into two volumes of interesting reading, although somewhat too eulogistic. Those which have been delivered by M. Dubois, the present secretary, and M. Beclard, the assistant-secretary, are more critical and discriminating productions. Although such orations cannot be always expected to be characterised by complete soundness and impartiality of judgment, they are of considerable utility in furnishing the authentic facts of the careers of a series of distinguished men, together with accounts of their writings and some estimate of the positions they had acquired. Certainly, whether right or wrong, men of distinguished merit on the continent are not allowed to slip out of the world in the same unnoticed way they do with us. Where can we point to any properly appreciative account of the career of Abernethy, Cooper, Brodie, or Lawrence, to say nothing of lesser men who have yet played an important part in the progress of science? It will have to be sought for, even if then found, in the pages of some fugitive journal, or at the fag end of some presidential address. The continental practice of enumerating the merits of their great men at the brink of their graves or in the halls where their voices once resounded, may be sometimes too showy and exaggerated, but our own demeanour is one of indifference and repulsive coldness. The *éloge* in the present volume is that pronounced by M. Béclard on M. Gerdy, and as he died so long ago as 1856, a sufficient time has elapsed to allow of a calm appreciation. Although he did much good work in his day, both in the provinces of anatomy and physiology as well as in practical surgery, he cannot be said to have occupied the highest rank; but it is very desirable that an authentic record of his career should be procured in so accessible a form. We do not, however, propose to notice this *éloge* further than to transcribe a passage in relation to the *concours*, the suppression of which after 1848 excited so much ill-feeling in France. Before that event the eloquence of Cousin had already threatened the institution, and it is in relation to Gerdy's enthusiastic defence of it that M. Béclard refers to the subject delivered to an approving auditory as his address was in the presence of such celebrities created by it as Trousseau, Rostan, Velpeau, and Bouillaud. He says:

"Like all other human institutions, the *concours* has its defects and even its errors. But one must feel a strange confidence in the assurances of renown, that equivocal power, to seek in it for more serious guarantees than in public trials conducted before competent judges. Loyal struggles of the intellect will always exert an irresistible attraction. The *concours* is acceptable, because a sentiment of justice is the principle whence it is derived, its roots stretching into the very depths of the heart. By the publicity of the struggle, it exerts a profound influence, and bestows on the aristocracy of intellect a legitimate and durable popularity. At the epoch in which we live, when feebleness of moral sentiment is indicated as one of the signs of the times, what can be more suited to elevate and strengthen the mind than these noble spectacles which, snatching it from indolence, excite emulation, and spread, among the youth of our schools, the beneficent contagion of example, the more certain and rapid in its operation in proportion to the height whence it has descended.

"The reproach that has been most especially directed against the *concours* is, that it paralyses original work, and that it keeps away the so-called men with ideas, giving to the clever in speech precedence over the true savant. Does not such language seem to imply that the interests of teaching should be delivered up as a sacrifice to some exceptional personalities? How often have we not seen such men, surrounded as they were by the reflection of a just celebrity, compromise in a chair without auditors the whole of a glorious past? It is too much forgotten that the chief mission of our schools is not the formation of savants, for savants create themselves, but of instructed and useful men, and to assure France as to the service of the public health. The investigator has his pen, the press, academies, and the higher professional chairs connected with his own special researches. As to genius, it raises itself above all social categories, and institutions are not designed for it. It has better than all this the glory of the present and the renown of the future. I repeat, in the words of Gerdy, 'The professor is a man seldom to be met with, who unites an extensive memory for the retention of facts, sound judgment for their appreciation, and severe reasoning for the deduction of the consequences that flow from them. He is the industrious bee who, accumulating from every side, elaborates from the products of his labour a delicious honey to the profit of the entire human race.'" (xxxi.)

2. *Report by M. Dubois on the Prizes awarded by the Academy in* 1866.—The Academy performs, through the agency of committees, a very laborious duty in the award and distribution of its numerous prizes, most of these being legacies, and some of them of considerable value. It is the business of the secretary to state the grounds upon which these awards have been made, as well as the reasons which have dictated their withholdal or postponement. This report is, in fact, a summary of the various decisions arrived at by the respective committees. We need not advert to the details of this report, as these were long since published: but we may observe that the awards of the Academy are not confined to original memoirs sent in competition, but are often bestowed on the authors of works already published, which are considered to have advanced the depart-

ments of medical science they relate to. Whatever may be the reason, the prize system seems to be much more flourishing on the continent than with ourselves, judging from the large sums devoted to it, and the amount of emulation it gives rise to. But even there the reporters have not infrequently to lament the insufficiency of many of the essays submitted to them.

3. *Report on the Epidemics observed in France during* 1865 By M. Bergeron, the reporter of the committee on epidemics.—This report is addressed to the Minister of Agriculture, Commerce, and Public Works, and was approved of by the Academy at the end of 1866. The material is derived from the reports of a class of public medical officers distributed over the whole of France, designated as Medical Officers of Epidemics, and one of whose duties is to transmit annually to the Academy accounts of any epidemics that may appear in their respective localities. Whether from insufficient remuneration or other causes, this part of their functions seems to be executed in a very imperfect manner, and it is a standing complaint of the successive academical committees, that, while a few of the reports submitted to them are creditable and careful productions, the majority of these officers either send in no reports at all, or these are so meager as to be of little utility; so that, what with this deficiency of communications, and the want of authenticity and uniformity in the statistical information supplied, the Academy during the forty years it has been at this work has, in the words of the present reporter "never yet been in the position to present to the administration a complete and exact representation of the epidemics prevailing in France." The committee on the present occasion is anxious to draw the minister's attention to the fact of how few real sanitary improvements have resulted from the repeated representation as to their necessity it and its predecessors have addressed to head-quarters. It is of opinion, also, that a disproportionate amount of attention has been paid to epidemic as compared with endemic affections. These latter are infinitely more amenable to hygienic influences, for while epidemic scourges are of only occasional occurrence, endemics are of much more constant operation.

"Just as the pathogeny of the greatest number of epidemic diseases is independent of conditions of locality, so on the other hand there is an intimate causal relation between endemics and the places in which they arise. This fact has long been demonstrated as regards ague, goitre, and cretinism, and there is every reason to believe that a similar demonstration might be made with respect to phthisis, scrofula, rickets, and *dartres*, if, on the one hand, it were established by irrefutable documents, that one or other of these diseases notably prevailed in certain localities, and, on the other, the topographical conditions of such localities were perfectly known." (lviii.)

It is in this direction, at all events, the committee believes that attention should be directed for the purpose of obtaining exact geographical, geological, and ethnological accounts of these various localities, as well as of the manners, habits, and occupations, *i. e.*, of the hygienic conditions of their inhabitants. Sad as are the results of epidemic visitations, the endemic conditions of ill health, more abiding, exert a permanent deleterious influence from generation to generation, sapping the very vitality of the race itself.

The reporter passes in review the various epidemics which prevailed during 1865, as far as the information furnished him by the reports from fifty-eight departments allowed. The year was below the average in these visitations, until towards the end the terrible cholera made its appearance. While speaking of *variola*, he says that it has during the last few years been committing greater ravages than heretofore. This is due to the diminution in the popularity of vaccination, founded in some measure upon the dangerous prejudice that, under its influence, typhoid fever has been substituted on a large scale for variola. Then, again, vaccination is too often carelessly performed, and not sufficiently supervised, while certificates of its performance are neglected to be enforced as a condition of admission to schools. In no less than twenty-six departments epidemics of *diphtheria* appeared, and in one or two instances they were attended, on a small scale, with the disastrous effects described by Trousseau in the epidemics in Touraine in 1819 and 1820.

No official work is complete in France without its medals, and accordingly the reporter furnishes a long list of Medical Officers of Epidemics, to whom the committee recommends that silver and bronze medals and honorable mentions should be accorded. An industrious officer may in this way in time easily accumulate a little cabinet of these precious objects.

4. *Report on the Medical Service of the Mineral Waters in France during* 1864. Addressed to the same minister on the part of the Committee of Mineral Waters, by M. Guérard.—The management of the mineral waters in France is another of those State affairs, better carried out on paper than in practice. At all places where mineral waters are authorised to be drank, a "Medical Inspector of Mineral Waters," whose duty it is to furnish to the Academy reports as to the number of persons frequenting them, their efficacy, and various other facts desirable to be known. Formerly these officers were invested with a power they do not now possess of regulating the cases fitted for the use of those

medicinal agents. Indeed, the abolition of the office altogether has been contemplated. In the mean time reports are annually sent to the Academy, whence the committee compiles its general account. We may observe that, on the discovery of any new mineral water, an occurrence which seems of great frequency, samples are forwarded to the Academy for analysis in its laboratory, and upon a favorable report being received, a permission is granted to "exploiter" the source, such preliminary proceedings being indispensable.

The Academy, through its successive reporters, has often sought to place some limits on the indiscriminate use of these mineral waters by patients of their own accord, without the sanction of the medical inspector, or other medical practitioner. The present reporter is convinced that much mischief results from this procedure, and is of opinion that so important an agent in the treatment of chronic disease ought not to be tampered with by the ignorant. The absence of any compulsory regulation of this kind also renders it impossible for the inspectors to furnish full reports of the number of persons resorting to these places, or the results of treatment. Moreover many of the inspectors exhibit great remissness in reporting, especially from the smaller places; and thus, while the present account is drawn up from reports of some forty establishments, there are more than a hundred which furnish none at all. From a table given of thirty-eight establishments sending in reports, it seems that in these were treated 58,000 paying, and 7000 gratuitous patients, the various establishments being farmed out by the Government for sums varying from 1,000,000 francs at Vichy, to 500 francs at some place in Corsica. The reporter passes in review the various reports that have come to the hands of the Academy, the different establishments being classed according to the composition of their waters; and his report forms a useful document for those to consult who wish to have the latest information on the repute attached to the various springs. Notwithstanding the laziness of the great bulk of the inspectors in the matter of reports, the Academy finds some twenty of the more industrious of them worthy of medals.

5. *Report on Experiments in Animal Vaccination conducted at the Academy.* Addressed to the same Minister by M. Depaul, Director of Vaccination.—The minister, early in 1866, allotted to the Academy the sum of 6000 francs for the purpose of conducting a series of experiments upon direct vaccination from the cow, which, introduced from Naples by M. Lanoix, has been a good deal patronised of late among the easier classes of Paris. A committee was appointed for this purpose composed of MM.

Leblanc, Blot, Jacquemier, Guérin, Bouley, Reynal, and Bousquet, having M. Depaul as its reporter. Heifers were provided for on the premises of the Academy, and were vaccinated through nine punctures made at the lower part of the abdomen between the udder and the groin, where the skin is not only very delicate, but is well sheltered from contact with foreign bodies.

The trials made proved that by this means an ample supply of lymph might be easily kept up for the use of the public. The cow-pock employed in them was derived from two sources, viz., from a heifer furnished by M. Lanoix, to which the lymph derived originally from Naples had been transmitted through several animals, and from a recent outbreak of cow-pock at Beaugency. The results obtained from each of these were identical—just as fine pustules also being produced by the latest as by the earliest inoculations. One great advantage of this direct mode of vaccination, in M. Depaul's opinion, is the security it furnishes against the transmission of syphilis by vaccination, a circumstance that may occasionally occur under the from arm-to-arm vaccination, however carefully the subjects be selected. On this head we may observe other reports on vaccination, from the pen of M. Depaul, show that he entertains very exaggerated opinions, and which, indeed, have been often rebuked as such in the discussions on the subject at the Academy. In proof of the non-transmissibility of syphilis to the lower animals, M. Depaul, after referring to the numerous experiments already on record, relates some others conducted on heifers by himself, M. Reynal, and M. Fournier, all of which were followed by negative results.

It was part of the plan of the committee to institute a series of comparative trials between the new and the ordinary modes of vaccination; and the results are given in this report in great detail. In the "from arm to arm" vaccinations, the "old virus" which has been in course of uninterrupted transmission for so many years was not employed, for since the prevalence of "animal vaccination" it has ceased to be obtainable in Paris. The virus of Neapolitan or Beaugency origin had, therefore, to be resorted to. After detailing the particulars of each vaccinal experiment, tabular views of the results are furnished, and from these we gather the following facts. There were 681 children submitted to "animal vaccination," but of this number 206 never re-appeared; of the remaining 475, in 54 no pustules had appeared at the end of the week, and in 421 the results were quite satisfactory, about four pustules appearing on the average. Of 885 children vaccinated from arm to arm, 324 were seen no more, and of the remaining 561, 18 were failures, and 543 successes, with an average of 4½ pustules. It is to be observed, however, that while, in this latter category, the children were all vaccinated on the seventh day from well-developed pustules, a distinction is to be observed with regard to the 681 submitted to the new mode. In 406 of these virus, taken from the third to the sixth day, from heifers perfectly well, was employed, and (deducting 123 absentees), there were only five failures to 278 successes, the pustules averaging 4½ in number. But among 275 vaccinations with lymph taken on the seventh day or later, or derived from a sick animal, not counting the 83 who did not return, there were 49 failures to 143 successes, only three pustules being produced on the average. While in the arm to arm vaccination six punctures were made, frequently only two were made in animal vaccination, in consequence of a fear of exciting too great inflammatory action, owing to the pustules which result from this undergoing a greater development.

The following are the conclusions the Committee arrived at :—

"1. The transmission of cow-pox by inoculation from heifer to heifer is obtained without difficulty. 2. We have inoculated forty-five, and always with the same success. 3. Punctures made either with the lancet or a needle succeed just as well as do incisions. 4. None of these animals suffered from any accidents that could properly be referred to the inoculations. 5. For the first three we employed the Neapolitan cow-pox, and for the other forty-two that of Beaugency. 6. The results obtained were perfectly identical. 7. Successive transplantations of the same virus do not seem to influence the development of the pustules. Those observed on the last animal presented the same characters and dimensions as in the first experiment. 9. We have found that the course of the pustules is a little more rapid than in the human subject. 10. The pimple almost always began to appear in the course of the third day, and suppuration was generally set up in the course of the seventh or eighth. 11. The health of the animals exercised a marked influence on the development of the pustules, those who became sick exhibiting them less developed. 12. The eruption was confined to the points of inoculation, no pustule ever being met with on other parts of the skin, or at the commencement of the mucous membranes. 13. Of general reaction there was little or none. In some heifers, dejection, and a little heat of skin were observed. 14. Our experiments at the Academy demonstrate that it would be possible to organise and keep up at a moderate expense, especially in the great centres, sources of animal vaccination. 15. Spontaneous cow-pox is not so rarely met with as is generally supposed. Two instances of this occurred during the course of our experiments. 16. The cow-pox we employed for most of our experiments had an authenticity of origin of which we had no doubt. 17. The number of inoculations which may be made are unlimited. The quantity of virus furnished by each animal is considerable, and more than is required by the exigencies of the most extensive public service. 18. According to our experiments, syphilis is not inoculable in individuals of the ox tribe. 19. In all the instances in which the virus was taken at the proper period, which is now well ascertained, success has been almost always constant, and in all cases as frequent as when human lymph is employed. 20. But wherever it has been taken at too late a period, that is, after the seventh day, the failures have been more frequent, and the number of pustules produced fewer. 21. The results obtained by means of the Neapolitan cow-pox have not been inferior to those furnished by that of Beaugency. 22. It is not uncommon to find after the vaccination of children with this virus, that the period of incubation is prolonged, the eruption not appearing until from the fifth to the twelfth day. 23. Sometimes in the same infant the course of the pustules is very irregular, some being much advanced, while others are scarcely visible. 24. The size of the pustules is considerably greater than that of those derived from human vaccination. 25. There is more general reaction throughout the economy produced, especially at the stage of suppuration, when the local inflammatory symptoms assume a greater intensity. 26. Still, these symptoms have never assumed a serious character in any of our children. 27. With respect to the number of pustules, very nearly the same results have been attained with both kinds of virus. 28. After animal vaccination in a certain number of cases, we have observed two, three, or even four pustules around a single puncture. 29. This phenomenon, though sometimes met with, is observed far seldomer in human vaccination. 30. All the modes of inserting the virus have equally well succeeded. The important point is to take the virus at the proper time. 31. The virus derived from the heifer, like the ordinary vaccine virus, often fails after it has been kept a certain time in glasses or in tubes. 32. Without being able yet to speak positively, the human virus seems to possess some advantage in this respect over the animal virus. 33. Still we have successfully employed the latter after it has been kept a month in tubes; and we have sent it into the country and abroad with good results. 34. Whether its preservative action will prove more complete and durable than the human virus which has passed through several generations, time alone can show. 35. The number of our revaccinations has been too few to admit of any positive conclusion. 36. During the prevalence of an epidemic, affecting a great number of localities, more or less distant from each other, it would be easy to send one or more inoculated heifers into the infected districts, which would furnish the virus necessary for the performance of vaccination and re-vaccination on a large scale." (pp. 51—54.)

This last suggestion seems to us to be the most valuable one of the whole report; and certainly during the experiments in vaccination which were made on the occasion of the cattle-plague, when reliable virus was so difficult to get, such an abundant supply would have been welcome. We may observe that when M. Depaul's report was read at the Academy, it found a severe and able critic in M. Jules Guerin.[1] Although this gentleman, as a member of the Committee, signed the report, in the belief that it was useful as far as it went, yet he thought it desirable to protest against the prevalent desire of seeking substitutes for the present mode of vaccination in place of trying to perfect the conditions of its performance. He believes it no slight matter to shake public faith in so admirable a preservative on the ground that in some very exceptional cases it may have been the vehicle of syphilis. The immediate effects of animal vaccination, he adds, are obvious enough, but its superior preservative operation is a pure hypothesis or utopia; and it may even be open to doubt whether virus which has passed through the human system during successive generations do not possess superior qualities to that obtained from frequent recurrence to the cow. This investigation even would have been much more useful had it been confined to a smaller number of comparative vaccinations, and that the course of *all* of these had been watched over from day to day. It was found that the course of the arm to arm vaccination was much more regular, and that the virulent power was longer retained, and more easily preserved. Experiments should have been tried by vaccinating infant from infant with the virus derived from the heifer; and re-vaccinations ought to have been practised on a large scale. M. Guerin in vain had sought in the committee to extend the experimental field, and had even offered to contribute to the expense. He brought also before the Academy a good deal of evidence of practitioners both in France and Italy who, having tried the practice of animal vaccination had abandoned it from disappointment with the results.

6. *Report on the Epidemics of Cholera that have prevailed from* 1817 *to* 1850. By M. Briquet.—A committee consisting of MM. Bouilland, Barth, Davenne, De Kergaredec, Guérin, Jolly, Melier, Roche, Tardieu, and Briquet, has been sitting for some years past, to which all publications and memoirs received by the Academy relating to the cholera has been referred. Much dissatisfaction has been expressed at the delay it has exhibited in furnishing its report. The present document, bringing down the subject to 1850 (the report on the subsequent epidemics by M. Barth will, it is said, soon follow), in some measure explains its own delay. It occupies more than 200 pages, and exhibits the results of the examination of a vast mass of material. All the works published in India, and the various countries of Europe and America had to be consulted, and for

[1] 'Bulletins de l'Académie,' Aug. 13 and 20, 1867.

France itself, several thousand manuscript documents were besides submitted to the committee by the Government. The result is a very elaborate historical and critical account of the various phases of the disease, which cannot fail to be of great use to all subsequent inquirers, whether they agree or not with the opinions expressed. The treatise is divided into five sections. 1. The history of the cholera in India, with the view of ascertaining whether it always originates there; 2. The history of its appearance in all parts of the world besides France; 3. the history of the epidemics that have occurred in France; 4. The pathology of the disease; and 5. Its treatment. The itinerary of the disease has, perhaps, never been placed before in so clear a light. On the present occasion any critical account of the work is beyond our scope, and we merely quote the general conclusions with which it terminates.

"1. Among the various species of cholera morbus, there is one to which the name of Asiatic or Indian cholera has been given. Its etiological character consists in its faculty of being propagated from one place to another; its anatomical character is *psorentérie*, and its pathological character white stools *sui generis*. 2. It originated in India, where it has probably existed from time immemorial, but its well attested outbreaks only acquired importance about 1760, the period of the contests between the French and English armies in India. 3. From this period down to 1820, epidemics of cholera frequently occurred in India; but beyond its limits they have never been met with, unless it were in the Low Countries in 1665, where the disease assumed the epidemic form with characteristic symptoms in the province of Ghent. Holland was at that time the sole power having very intimate commerce with India. 4. Between 1817 and 1850 three great epidemics were propagated beyond India, all three originating in Bengal. 5. These epidemics have observed two very different modes of propagation. When this has taken place by land, it has generally been through adjoining localities; but when it has extended to countries surrounded by the sea, it has taken place at more or less extensive distances—seaports, and especially mercantile seaports, being generally the first localities attacked. 6. Its propagation is facilitated or embarrassed by various circumstances dependent either upon the atmosphere or upon individuals. The conditions which, according to recognised documents, in general favour the propagation of cholera are, the vicinity of places where it prevails, the proximity of slowly flowing water, inconsiderable altitudes, elevation of temperature, the presence of a large quantity of water or vapour in the air, great atmospheric vicissitudes, defective supply of air, the direction of the winds from infected localities, large assemblages of persons, overcrowding, the presence of great calamities, as war, famine, poverty, low condition of health, debilitating passions, exhausting fatigue, and unsuitable diet. 7. The conditions which tend to place an obstacle to its propagation are in some measure the opposite of those enumerated, as distance from localities where cholera prevails, the absence of water courses, considerable altitudes, low temperature, dryness and freedom from vicissitudes of the atmosphere, the free exposure to rapid currents of air, moderate density of population, general well-being, good health, calmness of mind, moderate exercise, and a suitable diet. Of these various conditions, the former do not necessarily lead to epidemics, nor do the latter offer any absolute obstacle to them. 8. The conditions which predispose, and those which give rise to individual attacks of cholera, are of the same description as those which favour the propagation of epidemics. 9. It has not yet been demonstrated that special pathological conditions are the necessary preludes to epidemics of cholera. 10. The attack of cholera ordinarily commences with disturbance of the alimentary canal, and especially by the so-called premonitory diarrhœa. 11. The disease itself seems to be the result of the influence of an as yet unknown specific agent, the effect of which is to rapidly exhaust vitality at its very sources. 12. The reactionary phenomena are generally the result of phlegmasiæ having a special form. 13. A specific for cholera has still to be sought for, and the most rational treatment consists in combating with a certain amount of reserve, with appropriate agents, the various symptoms as they appear. In his treatment the physician should always bear in mind, that in severe cholera every means which exceeds the bounds of moderation may give rise to very serious accidents. 14. Finally, the mean mortality caused by cholera is generally about one death in every two patients." (pp. 269—271.)

REVIEW VIII.

The Variation of Animals and Plants under Domestication. By CHARLES DARWIN, M.A., F.R.S., &c. Two volumes. With illustrations. 1868. Pp. 843.

WHEN the excitement caused by the publication in 1859 of the celebrated book on "The Origin of Species" had, to some extent, had time to subside, and the true character of Mr. Darwin's brilliant theory was beginning to be more clearly recognised, it was soon perceived that no final decision could be expressed on its merits, until the promised evidence on which the theory was based had been fairly and attentively examined. An interval of two or three years was at first supposed, by the author, to be sufficient to enable him to complete the work, of which he had been urged to publish this now well-known Abstract; but owing, as we notice with regret, to continued ill-health, only half of the anxiously expected and long delayed evidence has been placed before us, and it is further to be regretted that the most important facts are those which are still unavoidably withheld, for "it was the consideration of these facts," writes Mr. Darwin, "which first led me to take up the present subject;" and "I hope," he subsequently adds, "that the reader will pause before coming to any final and hostile conclusion on the theory of natural selection. It is the facts and views to be hereafter given which have convinced me of the truth of the theory." Consequently, the all-important question of the origin of species by means of natural selection, which, strictly speaking, requires for its answer a complete biography of the organic world, can at present be considered only with reference to the variation of animals and plants under domestication.

No date has been, nor perhaps can, consistently with the unfavorable state of Mr. Darwin's health, be fixed for the publication of the remaining portion of this grandly planned work, on which so much of the success of the theory has been admitted to depend. We have for the present been briefly informed that the author proposes in a second work to discuss the variability of organic beings in a state of nature, and it will be shown that variations occurring under these circumstances are greatly dependent on geographical distribution. An attempt will be made also to show that whilst it is the large and flourishing genera which include the greatest number of varying species, those species which are the most variable are also the most widely distributed. But the main subject of this promised work will be the conversion of varieties into species consequent on an ever-recurring struggle for existence. For as the author has already stated in his introductory work, the normal condition of the organic world is war; and as the strongest in the battle of life must, as a rule, ultimately prevail, and the weakest fail, it will, consequently, be found that the establishment of distinctly defined species has been due to the preservation of favored individuals by the gradual extinction of those intermediate varieties which do not possess corresponding advantages in structure and instinct. In a third and concluding work, this principle of natural selection is to be tested by examining how far it will give a fair explanation of the facts adduced. The evidence of geology will be cited to prove that new species have come in gradually one by one, and that "the succession of many distinct species of the same genus throughout the long series of geological formations seems to have been unbroken or continuous." The development of the present from the past and extinct inhabitants of the world will be shown to be in accordance with the theory of descent with modification by means of natural selection; and it will be urged that the facts on which this theory is based "have as yet received no explanation on the theory of independent creations." Whilst the great principle of inheritance at corresponding periods of life; the retention of rudimentary and useless parts; and the remarkable fact revealed in embryonic growth of the similarity of members of the same great class in the earlier stages of development—in consequence of which the embryo, for instance, of a mammal, bird, reptile, and fish, is barely distinguishable—can admit of being satisfactorily explained, according to Mr. Darwin, only by the theory of natural selection.

The first of the two volumes before us is almost exclusively occupied with illustrations of the extent to which variation under domestication has been observed; and not only has Mr. Darwin exhibited rare power and indefatigable zeal in observing and recording facts, but, moreover, the vast accumulation of evidence which he has gathered from almost every available source is much in favour of his assertion that varieties should be regarded as incipient species. It would be impossible within the limits of our proposed review of this valuable work, to notice the successive variations which have been observed in the different races of animals and plants since they were first domesticated by man. Their history is for the most part very defective, and as regards many of those animals which are known to have been domesticated from a very early period, such as horses, dogs, cattle, sheep, and some other quadrupeds, there are no records of the date at which their subjection began, even in those cases in which the genealogy can be traced, with some degree of probability, to animals at present existing in the wild state; whilst in other cases there is evidence which would lead us to infer that the ancestral type, from which the domesticated varieties have been derived, has become extinct. The history of the horse, for example, is lost in antiquity; and the evidence of its domestication, from remains found in the Swiss lake-dwellings, has been traced with that of the sheep, the pig, and some other animals, as far back as the latter part of the stone period; to which it would, perhaps, be difficult at present to assign a correct date.

The observations on the domestic varieties of the dog display an extensive acquaintance with the literature of the subject, and are rich in interesting facts, many of which will probably be new to some of his readers. Although fully persuaded that there has been a large amount of variation under domestication, Mr. Darwin is strongly in favour of the multiple origin of domestic dogs, and he suggests that the larger dogs may be descended from the larger wolves; the smaller and lighter dogs from jackals; and that the slim Abyssinian *canis simensis*, with its elongated muzzle, may be regarded as the origin of the greyhound. It has been very generally observed that the wolf and the dog in some countries are so closely allied, that it is not unfrequently difficult to distinguish between them; and, although climate and the various external conditions of life, which constitute endemic influence, may, by equally affecting both animals, account in some degree for this approximation in their form and character, yet it is impossible to accept such external conditions as a sufficient explanation of all the facts observed. A very close resemblance of this kind has been noted between the more northern Esquimaux dogs and the grey wolves of the Arctic circle; between the Hare Indian dog and the Prairie wolf; and between the black wolf-dog of the Indians in Florida and the wolves of that country. The half domestic dogs of Asia and Egypt are very similar to jackals; and it appears that even the peculiarly offensive odour of the jackal may be imparted to a dog by simply feeding it on raw flesh.

It is equally unknown whether the different breeds of domestic cats, like our domestic dogs, are descended from several distinct species or not. The best authorities on the subject seem to be in favour of a multiple origin, and their opinion is to some extent supported by the fact that distant countries possess distinct races of these household pets. Among the curious varieties which have occurred under domestication we can only stop to notice the tailless cats of the Isle of Man, which occur also elsewhere; a breed of Chinese cats with drooping ears; and the inherited peculiarity of lynx-like tufts of hairs on the ears of some cats in England and also in India.

With respect to the variations of the domestic horse and the domestic ass, the most interesting and suggestive fact which has been noted is the occasional and well-marked tendency in them to the occurrence of stripes. On this subject of striping, Mr. Darwin is well known to have bestowed very close and thoughtful attention; and it is chiefly, as we shall presently have occasion to notice more fully, on the occurrence of cross-stripes or bars in the wings and tail of the domestic pigeon, that he has relied in his argument to prove that our various breeds of pigeons are all descended from the wild rock-pigeon or *columba livia*. The appearance of stripes in the horse does not, however, according to Mr. Darwin,

"Afford nearly such good evidence of their descent from a single primitive stock as in the case of the pigeon, because no certainly wild horse is known as a standard of comparison; because the stripes, when they do appear, are variable in character; because there is far from sufficient evidence of the appearance of the stripes from the crossing of distinct breeds; and lastly, because all the species of the genus Equus have the special stripe, and several have shoulder and leg stripes. Nevertheless, the similarity in the most distinct breeds in their general range of colour, in their dappling, and in the occasional appearance, especially in duns, of leg-stripes and of double or triple shoulder-stripes, taken together, indicate the probability of the descent of all the existing races from a single, dun-coloured, more or less striped, primitive stock, to which our horses still occasionally revert." (vol. i, p. 61.)

The domesticated quadrupeds usually bred for food, including pigs, cattle, sheep, goats, and rabbits, have all received their due share of consideration; and their variations in distant countries have been carefully studied. The pig appears to have been most highly cultivated in China, where it has long been esteemed as a favorite article of food, and where its domestication is believed by an eminent Chinese scholar to go back at least 4900 years from the present time. In this country assiduous attention has been bestowed on the breeding of sheep and cattle; and the development of any valuable character has led, by means of methodical selection, to wonderful improvements in their race. But probably the most satisfactory evidence of the influence of variation in more or less effectually changing the character of any breed of quadrupeds occurs in the case of the rabbit, and Mr. Darwin has in consequence very fully described the variations observed in the several domestic breeds of this animal, all of which he is of opinion may with safety be inferred to have descended from the common wild species. It is a well established fact that the wild rabbit, if taken young, can, though with some difficulty, be domesticated: and that the domestic rabbit, when turned adrift, readily becomes feral and reverts to the ordinary grey colour. In the following account of the Himalayan breed of rabbits, Mr. Darwin has shown us how in accordance with these facts a new species may be readily developed.

"The origin of the Himalayan breed (sometimes called Chinese, or Polish, or Russian) is so curious, both in itself, and as throwing some light on the complex laws of inheritance, that it is worth giving in detail. These pretty rabbits are white, except their ears, nose, all four feet, and the upper side of the tail, which are all brownish-black; but as they have red eyes, they may be considered as albinos. I have received several accounts of their breeding perfectly true. From their symmetrical marks, they were at first ranked as specifically distinct, and were provisionally named *L. nigripes*. Some good observers thought that they could detect a difference in their habits, and stoutly maintained that they formed a new species. Their origin is now well known. A writer, in 1857, stated that he had produced Himalayan rabbits in the following manner. But it is first necessary briefly to describe two other breeds: silver-greys or silver-sprigs generally have black heads and legs, and their fine grey fur is interspersed with numerous black and white long hairs. They breed perfectly true, and have long been kept in warrens. When they escape and cross with common rabbits, the product, as I hear from Mr. Wyrley Birch, of Wretham Hall, is not a mixture of the two colours, but about half take after the one parent, and the other half after the other parent. Secondly, chinchillas or tame silver-greys (I will use the former name) have short, paler, mouse or slate-coloured fur, interspersed with long, blackish, slate-coloured, and white hairs. These rabbits breed perfectly true. Now, the writer above referred to had a breed of chinchillas which had been crossed with the common black rabbit, and their offspring were either blacks or chinchillas. These latter were again crossed with other chinchillas (which had also been crossed with silver-greys), and from this complicated cross Himalayan rabbits were raised. From these and other similar statements, Mr. Bartlett was led to make a careful trial in the Zoological Gardens, and he found that by simply crossing silver-greys with chinchillas he could always produce some few Himalayans; and the latter, notwithstanding their sudden origin, if kept separate, bred perfectly true.'" (vol. i, pp. 108, 109.)

It is useful, moreover, to notice that, although these Himalayans when first born are usually quite white, yet when a single black rabbit is produced in a litter, as sometimes happens, it becomes, before two months elapse, perfectly white. The constancy with which the characteristic markings are subsequently developed in this albino breed of rabbits is considered by Mr. Darwin to be indicative of long inheritance. For it has been observed that characters common to many species of a genus—and a large majority of the species of the genus Lepus have their ears and the upper surface of their tails tinted black, and retain these markings when the rest of the body in winter becomes white—"are found to resist variations, or to re-appear if lost, more persistently than the characters which are confined to the separate species." The account of the Porto Santo rabbits which are the feral descendants of a female rabbit which, with a litter of young, was turned out on the island in 1418 or 1419, is in like manner very suggestive; and Mr. Darwin very truly remarks that most naturalists would, from the well-marked variation in this breed, have ranked them as a distinct species. But far more important than variations in external appearance are the modifications in the osteological characters of these animals, which have been very closely observed. Among these changes in structure, there have been noted decrease in the comparative size and capacity, together with a comparative narrowness of the skull, from disuse of the brain under domestication; a remarkable difference in the form and size of the occipital foramen; an alteration in the size and character of individual vertebræ; and great variation in the shape of certain parts of the scapula and of the terminal sternal bones. These extensive changes, with some other and less notable modifications in their osseous development, exhibit a degree of plasticity which, it must be admitted, we were somewhat unprepared to expect.

But however satisfactory the evidence afforded by the rabbit may appear, the stronghold of Mr. Darwin's argument in the present as in his earlier work is the well-known variability of the pigeon; and it must be frankly acknowledged that his reasoning in favour of the rock pigeon, *columba livia*, being the parent of our several domestic breeds of pigeons is throughout admirably sustained. The wonderful plasticity of the organization under domestication, to which we had occasion to refer in the case of the rabbit, is well illustrated in the varied shape of the domestic pigeon; in the great diversity of its plumage; and still more in those structural changes which affect even the number of the bones, as, for example, of the ribs and the sacral vertebræ. In addition to remarkable peculiarities of structure, there are also to be noticed in some breeds certain inherited movements, presenting singular differences in their habits, and of which the most characteristic and interesting is that of tumbling on the ground, as observed in the *Lotan or Indian ground tumblers.*

The variations of fowls and ducks; of the goose, turkey, and guinea-fowl; of the peacock and canary bird; of gold fish, hive bees, and silk moths; together with the variations of cultivated plants, have all received their share of the author's attention. But we must hasten to notice a very interesting and suggestive chapter on bud-variation, with which the first volume concludes, and which forms, as we shall presently have occasion to show, a fitting introduction to the great subject of inheritance. In this chapter, Mr. Darwin has, though, perhaps, somewhat unintentionally, succeeded in showing how very difficult and sometimes, indeed, impossible it must be, even with the vast supply of carefully observed facts at his command, to account for merely common phenomena in organic life; and, consequently, how important it is that the scientific inquirer should avoid the error of hasty generalisation. There is no portion of the work in which we have felt a deeper interest than in bud-variation; and the importance of this division of the inquiry may be inferred from the fact that the aim of the author in this chapter is to show "in how close and remarkable a manner the germ of a fertilised seed and the small cellular mass forming a bud resemble each other in function, in their powers of inheritance with occasional reversion, and in their capacity for variation of the same general nature, in obedience to the same laws."

The term bud-variation is applied to all those sudden changes in structure or appearance which occasionally occur in full grown plants in their flower-buds or leaf-buds; and they can generally be propagated to any extent by grafting, budding, cuttings, bulbs, &c., and occasionally even by seeds. In speaking of bud-variation it must not, however, be supposed that the term should be limited altogether to plants; for Mr. Darwin is of opinion that if compound animals, such as hydras, corals, &c., had been like plants, which in many respects they closely resemble, subjected to a long course of domestication, they would have varied by buds: and he cites some cases in which varieties of the hydra and of a true coral have been propagated by budding. Among the extensive and valuable series of cases of bud-variation in plants affecting the fruit, which Mr. Darwin, evidently after much labour, has succeeded in collecting, may be cited several instances of peach trees having yielded nectarines, and one instance of a nectarine tree having yielded peaches; the case of a gooseberry-bush, described by the late Dr. Lindley, which bore at the same time four kinds of berries; and some cases of currant-bushes with red and white currants either on the same or on separate branches. Numerous illustrations are given of bud-variations in flowers, leaves, and shoots; and of subterranean bud-variations by suckers, tubers, and bulbs. Among the more noticeable of the latter group of illustrations are varieties of the common potato, produced sometimes by variation in a single bud or eye; or, as occasionally happens, by all the eyes of a tuber varying in the same manner and at the same time, so that the whole tuber assumes a new character; whilst among the variations by bulbs is to be noticed a case in which a blue variety of the hyacinth, for three successive years, gave offsets which produced white flowers with a red centre. In addition to these illustrations of bud-variations, some anomalous and apparently allied cases have been described, of which the most celebrated is that of Adam's laburnum, a form intermediate between the common and the purple laburnum, and which Mr. Darwin seems inclined to regard as a graft-hybrid, which is in accordance with the statement of M. Adam, who first raised the plant. Closely analogous, as reproductive anomalies, to this laburnum, are the cases in which the orange and the citron have been combined; as in the well known case of the Bizzarria orange, which produces at the same time leaves, flowers, and fruit, identical with the *bitter* orange and the citron of Florence; and the case of the trifacial orange of Alexandria and Smyrna, which differs from the Bizzarria "in the *sweet* orange and citron being blended together in the same fruit, and separately produced on the same tree." In commenting on these curious anomalies in the reproduction of the laburnum and the orange, Mr. Darwin remarks that "whatever their origin may have been, the two parent species occur blended together under the form of a sterile hybrid, or reappear with their characters perfect and their reproductive organs effective; and these trees, retaining the same sportive character, can be propagated by buds." With regard to the causes of bud-variation, it is evident that many of the cases referred to are simply due to spontaneous variability; others will admit of being explained by reversion to characters which have, it may be for a considerable length of time, disappeared; and again, some bud-variations are produced by a cross. It should, moreover, be remarked that whilst variation is more commonly the result of sexual generation than of propagation by buds; yet "all the plants which have yielded bud-variations have likewise varied greatly by seed. As it is not desirable, at this stage of the inquiry, to notice more fully the several points of interest connected with this subject, it will be sufficient for the present to state that variability dependent on bud-propagation and variability dependent on sexual generation are the results of closely allied forms of reproduction, which appear to be alike subject to the same laws of inheritance; and that, consequently, the study of bud-variation is to some extent to be regarded as a transition stage in the inquiry, through which we pass from the observation of variations which may be fleeting to the consideration of the influence by which they may become fixed.

But before proceeding to investigate the nature of inheritance, which from its importance may be termed the axis round which the discussion on all other subjects connected with the inquiry revolves, it is necessary to notice an important digression "on the direct or immediate action of the male element on the mother form," which, notwithstanding any supposed relation the subject may have to that of graft-hybrids, seems to be somewhat out of place in the midst of a chapter on bud-variation; and with all due respect for the author, we cannot but express our opinion that it might with advantage be transferred as an appendix to the succeeding chapters on inheritance; since it is apparently far more closely connected with seminal reproduction than with propagation by buds. With regard to this subject, which is one of the most obscure in the physiology of reproduction, it has been shown that in the case of flowering plants when the pollen or male element of one species or variety is applied to fertilise a distinct kind, that a notable effect can by this means be produced on the mother form, in consequence of which the succeeding flowers or fruit occasionally present an altered character. The flowers of an orange, for example, were fertilised with pollen from the lemon, and it was observed that "one fruit thus produced bore a longitudinal stripe of peel having the colour, flavour, and other characters of the lemon." One of the most remarkable, and at the same time best authenticated examples of this effect of crossing is that observed by M. Denis who fertilised the *Chamærops humilis* with pollen from the phœnix or date-palm. In reference to this case, Mr. Darwin remarks that "the fruit or drupe thus produced was twice as large as, and more elongated than that proper to the Chamærops; so that it was intermediate in these respects, as well as in texture, between the fruit of the two parents. The hybridised seeds germinated, and produced young plants likewise intermediate in character. This case is the more remarkable as the Chamærops and phœnix belong not only to distinct genera, but in the estimation of some botanists to distinct sections of the family." In animals, analogous results have been observed, and first-class breeders are so fully aware of this influence of the first male on the subsequent offspring of the same mother by other males, that they are careful to avoid deteriorating the race by any cross with a male of inferior breed. It must, we think, be acknowledged that no satisfactory explanation has at present been given of the abiding effect of a first impregnation on the subsequent progress of reproductive development, although theories of a very opposite description have been advanced to account for the effect produced; and, until more conclusive evidence has been published on the subject, we should not be disposed to agree with Mr. Darwin in ascribing it to the direct action of the male element on the reproductive organs of the female, rather than to any intervention of the crossed embryo.

In passing from the consideration of what Mr. Darwin has to a very great extent succeeded in showing to be probably the origin of many at least of those organic forms which naturalists have hitherto been in the habit of describing as species, we enter a field of inquiry in which the evidence of variation under domestication no longer possesses a corresponding value. For although varieties, as Mr. Darwin states, may be called incipient species,—and all well-instructed observers would, perhaps, without hesitation, be willing to admit that through hereditary influence the otherwise transient effects of variation may often become fixed—yet it is questionable whether sufficient or indeed any conclusive evidence can be derived from this source to prove that variation will lead to higher results, and effect such a transformation, that pigeons, for example, shall cease after a time to be pigeons; or that any of the remoter descendants of rabbits will ultimately be developed into quadrupeds of a higher class than themselves. A vast amount of evidence has been accumulated by Mr. Darwin to prove that varieties probably become species; and he may be considered to have so far succeeded in establishing his position, that variation can now be accepted as one of the chief sources of what have been incorrectly classified as hereditarily independent forms. But when, as already remarked, we attempt to pursue the investigation beyond the origin of the so-called species of naturalists, and apply the same argument to the origin of the higher groups in natural history, our progress, so far as variation under domestication is concerned, becomes simply a leap in the dark. This is chiefly due to the fact that the great principle of inheritance is, in many respects, unfavorable to the suggested extension of the theory, and that hybridism is altogether opposed to it. Consequently, it will be found, as the inquiry proceeds, that although by means of hereditary influence varieties may be raised to the questionable and unsettled rank of species, yet the change thus effected is more apparent than real; for, on the one side, by means of reversion, temporary characters acquired through variation may be superseded by the more permanent characters of the true species; whilst, on the other side, hybridism, by inducing sterility, opposes an impassable barrier to the formation of new, through any intermixture of old and hereditarily distinct, forms.

Before, however, we bestow any special notice on this important division of the inquiry, we have to express our satisfaction at the progress which appears to have been made in the investigation of the "wonderful nature of inheritance," since the publication in 1859 of Mr. Darwin's introductory work on the origin of species, in which it was distinctly though somewhat incorrectly asserted that "the laws governing inheritance are quite unknown." For it must be acknowledged that previous to this date considerable progress had been made in the investigation of hereditary transmission; and that some of the various influences to which an inheritance might be subject, and more especially the influence of prepotency in transmission had been very ably discussed and illustrated by M. Prosper Lucas in his great work on "Natural Inheritance." But it has been due chiefly to the recent publication in this journal of a series of papers by Mr. Sedgwick, in which the influence respectively of sex, age, and atavism on hereditary disease has been fully established, that we possess more definite information on this subject; and it is gratifying therefore to observe, in the following summary by Mr. Darwin, the extent of the change which has been effected by these and other contributions to the literature of inheritance :—

"Finally, though much remains obscure with respect to inheritance, we may look at the following laws as fairly well established. Firstly, a tendency in every character, new and old, to be transmitted by seminal and bud generation, though often counteracted by various known and unknown causes. Secondly, reversion or atavism, which depends on transmission and development being distinct powers: it acts in various degrees and manners through both seminal and bud-variation. Thirdly, prepotency of transmission, which may be confined to one sex, or be common to both sexes of the prepotent form. Fourthly, transmission, limited by sex, generally to the same sex in which the inherited character first appeared. Fifthly, inheritance at corresponding periods of life, with some tendency to the earlier development of the inherited character. In these laws of inheritance, as displayed under domestication, we see an ample provision for the production, through variability and natural selection, of new specific forms." (vol. ii, p. 84.)

It must be freely admitted that the difficult subject of inheritance has been investigated by Mr. Darwin with much care and discrimination, and that he has succeeded in showing the extent to which the variations from the normal type, if we may be permitted to use the term, are capable of being inherited. The illustrations, more particularly of the various inherited malformations and diseases of the eye, with its accessory parts, may be referred to as very useful in assisting to prove that even the most trifling peculiarity or defect may be the heritage of a family for two, three, or more generations, and that the transmission of the inheritance varies greatly in different cases. In addition to such affections, it will be as well also to notice some cases of supernumerary fingers and toes, to which Mr. Darwin has directed special attention, on account of the occasional regrowth of these superfluous parts after amputation. The cases which have been cited in favour of this exceptional power of regrowth are—1st. That of a child with a thumb double from the first joint, and furnished with an additional nail, in which the supernumerary member was removed at the age of three years, but grew again and reproduced a nail. The newly-grown thumb in this case was again wholly removed by its socket-joint, and again grew and reproduced the nail. 2nd. A case, mentioned by Dr. Struthers, of partial regrowth of an additional thumb after amputation in a child three months old. 3rd. A similar case which was observed by the late Dr. Falconer. 4th. The following case, in which the evidence of regrowth after amputation is very complete :—

"A gentleman," writes Mr. Darwin, "who first called my attention to this subject, has given me the following facts which occurred in his own family. He himself, two brothers, and a sister, were born with an extra digit to each extremity. His parents were not affected, and there was no tradition in the family, or in the village in which the family had long resided, of any member having been thus affected. Whilst a child, both additional toes, which were attached by bones, were rudely cut off; but the stump of one grew again, and a second operation was performed in his thirty-third year. He has had fourteen children, of whom three have inherited additional digits; and one of them, when about six weeks old, was operated on by an eminent surgeon. The additional finger, which was attached by bone to the outer side of the hand, was removed at the joint; the wound healed, but immediately the digit began growing, and in about three months' time the stump was removed for the second time by the root. But it has since grown again, and is now fully a third of an inch in length, including a bone, so that it will for the third time have to be operated on." (vol. ii, pp. 14, 15.)

These facts, which have been sufficiently well authenticated, require to be very carefully considered, for they have furnished Mr. Darwin with what he is evidently disposed to think is a very strong argument in favour of the human race being the remote descendants of a very inferior type in organization, far below not only every mammal and bird, but below also every existing reptile; a supernumerary digit being in fact, according to his view of the case, a finger of scorn pointing to our affinity with a fish. For although he has very justly remarked that all that can perhaps safely be said about cases of polydactylism is that they indicate "mere fluctuating monstrosity"; yet he immediately proceeds to suggest,—

"As supernumerary digits in the higher animals, from their power of regrowth, and from the number thus acquired exceeding five, partake of the nature of the digits in the lower animals; as they occur by no means rarely, and are transmitted with remarkable strength, though perhaps not more strongly than some other anomalies; and as with animals which have fewer than five digits, when an additional one appears it is generally due to the development of a visible rudiment; we are led in all cases to suspect, that, although no actual rudiment can be detected, yet that a latent tendency to the formation of an additional digit exists in all mammals, including man. On this view, as we shall more plainly see in the next chapter when discussing latent tendencies, we should have to look at the whole case as one of reversion to an enormously remote, lowly-organised, and multidigitate progenitor." (vol. ii, pp. 16, 17.)

In reply to this overstrained suggestion it should be remarked, in the first place, that the regrowth of supernumerary digits is a very exceptional fact in the human race, for out of an immense number of cases in which an operation for their removal has been performed, and that, too, chiefly, as it is important to notice, at a very early period of life, there are very few examples of any reappearance of these abnormal structures; and, secondly, that in the exceptional cases in which regrowth after amputation has been observed, there is no evidence to show that such regrowth was connected with any exceptionally early period of life at which the operation was performed; and consequently it cannot rightly be regarded as an indication of the power of reproducing lost parts analogous to what has been assumed, but on insufficient evidence, to occur occasionally in the embryonic condition. For although Mr. Darwin is disposed to infer "that supernumerary digits in man retain to a certain extent an embryonic condition, and that they resemble in this respect the normal digits and limbs in the lower vertebrate classes;" yet it is evident that, since these superumerary structures undergo development which is to a great extent parallel with the development of the rest of the body, they must, so far as growth and regrowth are concerned, acquire a less embryonic character as age advances. Consequently, instead of having our attention directed to any indication of the power of reproducing lost parts in connection with the limbs of a fœtus, as contrasted with the fact that "the normal digits in *adult* man and other mammals, in birds, and in true reptiles, have no power of regrowth," evidence should have been forthcoming to show that the regrowth of supernumerary digits, which have been surgically removed, has been more commonly observed when the amputation has been performed immediately after birth than when it has been delayed to a later period of life. The difficulty attending any extension of the theory, such as that suggested by an exceptional power of regrowth in a few cases of polydactylism, so far, indeed, from becoming less, seems rather to increase as we proceed; and a careful study of the comparative effects of variation on analogous structures in different animals will sometimes conclusively prove that, in attempting to explain such variations, it has been found necessary, so to speak, to shift the ground. As an illustration of the difficulty which it is thus often necessary to encounter, let us examine the evidence which has been adduced in favour of the spontaneous origin of webbed feet.

The first and most obvious principle involved in the occurrence through variability of webbed feet is that of utility; and consequently Mr. Darwin has attributed considerable importance to the fact that in some land animals which have become aquatic in their habits, such as the Newfoundland-dog and the English otter-hound, there is a decidedly increased development of skin between the toes. He particularly observed in two Newfoundland-dogs that when the toes were stretched apart and viewed on the under side, that "the skin extended in a nearly straight line between the outer margins of the balls of the toes; whereas in two terriers, of distinct sub-breeds, the skin viewed in the same manner was deeply scooped out;" and a friend, who examined for him the feet of two English otter-hounds, found that the skin in this situation was more developed than in other hounds; and it appears also that there is a dog peculiar to Canada, which has "half-webbed feet, and is fond of water." From the fact that the skin between the toes in these animals is usually more developed than in those dogs which are not accustomed to swim, Mr. Darwin has argued "that as aquatic animals which belong to quite different orders have webbed feet, there can be no doubt that this structure would be serviceable to dogs that frequent the water."

"How inexplicable," exclaims Mr. Darwin in his introductory remarks, "is the similar pattern of the hand of a man, the foot of a dog, the wing of a bat, the flipper of a seal, on the doctrine of independent acts of Creation! how simply explained on the principle of the natural selection of successive slight variations in the diverging descendants from a single progenitor! So it is, if we look at the structure of an individual animal or plant, when we see the fore and hind limbs, the skull and vertebræ, the jaws and legs of a crab, the petals, stamens, and pistils of a flower built on the same type or pattern." (vol. i, p. 11.)

But the occurrence of such a variation as that referred to above in aquatic dogs, even if it could be shown to be permanently established—and at present the evidence in its favour is insufficiently supported by Mr. Darwin's examination of two Newfoundland-dogs, by a friend's examination of two otter-hounds, and by Mr. Greenhow's observations, published in 1833, on the Canadian dog—must not be supposed to be due simply to their acquired habit of frequenting the water, for webbing of the feet is far from being an uncommon variation in animals which never acquire aquatic habits. Numerous cases have been observed of the hereditary occurrence of this variation in the human race; but we are not acquainted with any evidence in favour of its being a more frequent occurrence in the members of those families which have been for centuries aquatic by profession than in others who from constantly living inland have scarcely had the opportunity of even entering the water. On the contrary, it might be urged that, from the frequency with which webbing of the fingers and toes, with other digital variations, prevails as an hereditary peculiarity amongst the inhabitants of inland and especially of mountain districts, quite independent of any aquatic habits, it cannot be regarded as a utilitarian variation in the human race.

The same argument against this supposed origin of webbed feet will apply to the occurrence of the variation in those birds which in like manner have no tendency to become aquatic in their habits; such, for example, as in certain breeds of pigeons, in which it is customarily associated with feathered feet; and it seems to be very important to notice this exceptional fact in the pigeon, as it is opposed not only to the argument founded on utility, but opposed also to the argument which Mr. Darwin has elsewhere employed with much skill in favour of reversion to a primitive type; for as a very large proportion of the feathered races are water birds with webbed feet and bare legs, the conjoined anomaly of webbed and feathered feet in pigeons is opposed to the supposition that they can have descended, through the rock-pigeon, from a webbed footed progenitor. It is well known that, from a very early period in the history of the present inquiry, special attention has been bestowed by Mr. Darwin on a case of hereditary peculiarity in some breeds of pigeons, which have the two outer toes partially connected by skin when their legs are feathered. At first this fact is said to have been utterly inexplicable; but it is now understood to be dependent, not on its utility, as in dogs, for no webbed-footed pigeons have been observed, or have even tried to swim, but on the law of correlated variation of homologous parts. For these two toes in the pigeon, which correspond with the third and fourth toes in man, acquire feathers on becoming partially webbed, because they are, it is urged by Mr. Darwin, homologous to structures in the pigeon's wing representing the third and fourth digits, which are both feathered and completely united by skin. Now, it is important to notice, with reference to this peculiarity in the pigeon, that in other birds, such as marsh- and water-fowl, which possess either a partial or a complete webbing between the toes, as a constant because, as we have hitherto been accustomed to assume, it is in their case a normal development, there is no feathering of the legs or feet, notwithstanding the fact that birds which are thus normally webbed footed, instead of presenting elsewhere any general deficiency of feathers, have usually a very dense plumage. Hence it would not be otherwise than allowable to suppose that the law of homologous affinity, which prevails to a very great extent in normal development, is somewhat exceptionally associated in this case of peculiarity in the pigeon, with the law of correlated variability which occupies a preeminent position in Mr. Darwin's argument on the origin of species; for he has impressively assured us that "of all the laws governing variability, that of correlation is the most important." On proceeding to investigate this subject still further, it will be found that there are many breeds of birds in which, as a result of variability under domestication, the legs and toes become feathered without any corresponding development of skin between the toes. The feather-footed canaries and the feather-legged bantams may be referred to as notable examples of this fact; and with respect more especially to this interesting breed of bantams it may be stated that the leg-feathers, which grow from the outside of the leg, and generally from the two outer toes, have sometimes been observed to exceed even the wing-feathers in length, showing that there may be an excessive development of feathers in this situation apart from any corresponding development of interdigital skin; whilst on the other side there are cases in which exactly the reverse of this has been observed in other breeds of the fowl, such, for example, as occurs in the case of the golden-spangled Polish fowls which are bare legged, and in which the skin between the toes is said, by Mr. Tegetmeyer and other authorities on the subject, to be much developed. It still remains to be noticed that, as in the bird's wing, the second digit is only rudimentary, and the first and fifth digits are wholly aborted, the two remaining digits, which are completely webbed, represent the third and fourth toes in the foot; and that, consequently, the limited development of web-skin in the foot is strictly homologous to that in the wing. Hence "the whole leg tends," says Mr. Darwin, "to assume the structure of the wing." On referring, however, to cases of abnormal development of inter-digital skin in other animals, it will be found that there is occasionally a preference shown for this digital interspace, apart from any abortion of the first, second, and fifth digits in the anterior limbs, and altogether independent of any connection with the development of feathers; and that in cases of inter-digital webbing in man, to which we will now more particularly refer, this preference may be exhibited in the hands and feet, either separately or together. Mr. Canton has recorded a case of symmetrical webbing of the third and fourth toes, in a man who had four sons with precisely the same peculiarity, and four daughters who were exempt from it. Dr. Dickie, of Alloa, has recorded a case of webbing of the corresponding fingers, without any webbing of the toes, which was observed to be hereditary for more than six generations; and it is to be noticed, in this case, that the defect was on many occasions unsymmetrically limited to one and apparently the same hand, and that unlike, also, the preceding case, it occurred "more frequently amongst the females than the males;" whilst a case has come under our own observation of partial webbing of the ring and middle fingers and corresponding toes in some members of a family, in which, for five generations, there had been noticed congenital absence of the terminal phalangeal bones and nails of the little fingers and little toes. With reference to such cases, it should be further remarked that, although a certain degree of preference in abnormal development is thus sometimes exhibited for this digital interspace, yet an equally well-marked limitation to one or more of the other digital interspaces has been occasionally, though, perhaps, less frequently observed in other cases; as, for example, in a case which has been lately brought under our notice, in which the webbing was limited to the interspace between the second and third toes of the right foot in a boy whose maternal great uncle had precisely the same malformation. Many other illustrations of such limitation of inter-digital webbing have been recorded, or have come under our notice; but it will be sufficient to refer at once to the fact that the development of abnormal webbing is not always restricted, even in pigeons, to the third and fourth digital interspace, for Mr. Darwin informs us that he had in his possession "a spot and a nun with the skin extending for a space of a quarter of an inch from the fork between the two *inner* toes" (vol. i, p. 160); and from these birds there might, in accordance with the recognised principles of inheritance, have been bred a race of pigeons with webbing between the two *inner* instead of between the two *outer* toes.

But even if it could have been satisfactorily shown that structural advance in the organization had resulted from variation, it would still be incumbent on the author of this theory to prove that, when from any cause a retrograde change occurs in the development of an animal which has been thus progressively improved, intermediate and newly-formed species and genera are not, as a rule, liable to be altogether passed over when reversion occurs, so as to permit of the degraded descendant being reduced to a rank peculiar to one epoch only, and that often exceptionally remote, in its past history; for if the intermediate species and genera are in any way entitled to their position and their name, there is no need for the reversion to extend to a period always anterior to their development. In the following remarks on the influence of reversion in inheritance we shall endeavour to show that the facts which have been observed by Mr. Darwin are not favorable to the theory which he has proposed.

Reversion, or the principle on which depends the reappearance of characters which have been lost sight of or forgotten through being suppressed for one or more generations, and which occupies a very prominent position in the present inquiry, has often been referred to by many writers as a very curious and a very mysterious phenomenon; but it has not, at least until late years, received much scientific attention. Mr. Darwin, as might be expected, has been fully aware of its great significance in relation to his theory of the origin of species; and he has accordingly investigated very closely the various forms under which it may occur, and the various causes on which it may depend. One of the most common, and, as regards the supposed origin of species, one also of the most important of these forms of reversion, is that resulting from a cross in which the offspring presents the characters proper to either pure parent form. "As a general rule," Mr. Darwin informs us, "crossed offspring in the first generation are nearly intermediate between their parents; but the grandchildren and succeeding generations continually revert, in a greater or less degree, to one or both of their progenitors." This influence of crossing in leading to reversion has become endowed with peculiar interest, in consequence of its effects in the celebrated case of the pigeon having been instrumental in first directing Mr. Darwin's attention to its usefulness in determining the parent forms of our several domesticated breeds of animals; and it is deserving of notice that in the following evidence respecting the origin of the domesticated pigeon, which has been given in detail, he carefully disclaims the merit of having been the first to recognise its effect in causing reversion to the parent rock-pigeon, or *Columba livia*.

"My attention," writes Mr. Darwin, "was first called to this subject, and I was led to make numerous experiments, by MM. Boitard and Corbié having stated that, when they crossed certain breeds, pigeons, coloured like the wild C. livia, or the common dovecot, namely, slaty-blue, with double black wing-bars, sometimes chequered with black, white loins, the tail barred with black, with the outer feathers edged with white, were almost invariably produced. The breeds which I crossed, and the remarkable results attained, have been fully described in the sixth chapter. I selected pigeons, belonging to true and ancient breeds, which had not a trace of blue or any of the above specific marks; but when crossed, and their mongrels recrossed, young birds were continually produced, more or less plainly coloured slaty-blue, with some or all of the proper characteristic marks. I may recall to the reader's memory one case, namely, that of a pigeon, hardly distinguishable from the wild Shetland species, the grandchild of a red-spot, white fantail, and two black barbs, from any of which, when purely-bred, the production of a pigeon coloured like the wild *C. livia* would have been almost a prodigy." (vol. ii, p. 40).

Similar experiments have been made with fowls, ducks, rabbits, cattle, horses, asses, and other animals, and the results obtained have corresponded with those observed in the pigeon; the offspring having exhibited the characteristic colour and markings of what was, in each case, either known or might reasonably be assumed to be the wild species. Even instincts which had been lost were by this means recovered; such as the lost instinct of incubation in those breeds of fowls known as "everlasting layers." Whilst in other animals the primitive wildness of disposition, which had for generations been lost through long-continued domestication, with many other characteristic qualities of the feral state, were by this means restored.

In attempting to account for this reversion to characters which have in many cases been long extinct, Mr. Darwin has assumed that they are capable of remaining latent in the organization for an indefinitely prolonged period, and throughout an almost unlimited succession of generations; and in his remarkable theory of pangenesis,[1] to which we may again have occasion to refer, the marvellous manner in which these alleged latent peculiarities of structure,—for variations both in

[1] In "the provisional hypothesis of pangenesis," which occupies a concluding chapter of the work, Mr. Darwin has assumed that the whole organisation, in the sense of every atom or unit, reproduces itself; and its importance may be inferred from the fact that it has been designed to explain the various forms of reproduction, sexual and asexual; the development and growth of animals and plants; the changes induced in them by variability; and the great principles of inheritance. Mr. Darwin seems to have been led or rather forced to adopt this theory, which appears to be founded on Mr. Herbert Spencer's theory of physiological units, chiefly in consequence of the difficulty or impossibility of otherwise explaining the various forms of inheritance, and especially those which result from the peculiar principle of reversion, which he regards as the most wonderful of all. "In every living creature," he remarks, "a host of lost characters lie ready to be evolved under proper conditions," their evolution being dependent on the awakened action of dormant gemmules; and when such gemmules, derived, it may be, from some remote progenitor, are present in sufficient number to gain the ascendancy, they cause the reappearance of long-lost characters. Each of these gemmules is supposed to represent with exactness the organic unit which was its immediate progenitor, and from which it has been developed by a process analogous to that of budding, and, consequently, analogous to that form of reproduction in which all other forms may, strictly speaking, be merged; and as each individual animal or plant reproduces its kind, so each integral cell or unit, of which the animal or plant is composed, does the same. In like manner also the ordinary distinction between growth and development, in which the former is limited to mere increase in size, and the latter is employed to denote change of structure, is lost, as it were, in the unity of the process by which the work is accomplished. For, in accordance with this theory, every part of the child, as of the adult, generates the same part for the next generation, and consequently "the child, strictly speaking, does not grow into the man, but includes germs which slowly and successively become developed and form the man." Inheritance, which "must be looked upon as merely a form of growth, like the self-division of a lowly-organized unicellular plant," is essentially dependent on a gemmule attaining its full size; and the distinction between the various forms of inheritance, direct, atavic, and collateral, is simply due to the occasionally uncertain and unequal influence of time on their development. In a typical case of direct inheritance the development of gemmules in the offspring would be coincident with the age of a progenitor of the same sex at their birth; and this is probably the

colour and marking are essentially due to structural peculiarities in development—can be accumulated within an inappreciably small compass has been very fully illustrated. It will be sufficient, however, at this stage of the inquiry to state that reversion, according to Mr. Darwin's theory of pangenesis means the evolution of characters which have always been present in a material form, though for a time present in so rudimentary a condition and on so microscopic a scale as to be absolutely beyond our power of detection: and in order that the theory should be made, as far as possible, consistent throughout, it has moreover been assumed that since organic forms may have hereditarily descended from one primeval form, so each of the descendants may be supposed to retain many, if not all of the structural peculiarities which have characterised every stage of their descent; and that although a progressively larger number of these characters become latent in each successive generation, and some may ultimately disappear altogether, and become irrevocably lost, yet in their latent condition they are always ready to be evolved under circumstances favourable to their development. Hence, as there could have been originally no distinction of sex, it has been assumed that even those outward distinctions which now characterise the males and females of

prevailing influence, when associated with that of sex, not only in cases of normal development, in which it would be potent to secure unity of form in the remotest descendants, but also in those cases of abnormal development in which peculiarities and defects are developed in the parents and in the children at a corresponding age. Whilst in a typical case of reversion in which, for example, the inheritance is transmitted from a grandfather, through the medium of a daughter, to a grandson, the gemmules in the intermediate generation are dormant and remain so till they have passed into the fertilized ovum, when their development, as in a typical case of direct inheritance, becomes simply, or perhaps it would be more correct to say chiefly, a question of time. For the gemmules in the grandson continue dormant till he has attained the age at which in his grandfather they were produced, and then their development may be said to begin. In the remoter forms of reversion, in which should be included some at least of the cases of collateral inheritance, the gemmules will continue dormant throughout all the intermediate generations; and it would be impossible to assign any limit to the time during which gemmules might thus remain undeveloped; "but there is no reason to suppose," Mr. Darwin writes, "that all dormant gemmules would be transmitted and propagated for ever," since it is obvious that in the organism which forms their little world they would necessarily have the same difficulty to contend with as that which in the outer world leads to the struggle for existence, and consequently undeveloped gemmules instead of remaining dormant may perish. Finally it may be remarked that whilst, like the occasional and perhaps progressive extinction of race amongst animals and plants, the death of gemmules must be supposed to involve the total loss of any peculiar character in the organization, which had simply disappeared during the time that such gemmules were dormant, since some of the vast number of those which perish leave no descendents, yet there are others which must possess a pedigree of incalculable extent; for if they have been hereditarily derived from the primordial form from which every living thing has been supposed to descend, they are the still surviving representatives of organic units which were coeval with the very earliest dawn of organic life upon the earth.

vertebrate animals must, to a great extent, be looked upon as unreal; for every male, according to Mr. Darwin, possesses in a latent condition all the secondary sexual characters of the female; and every female in like manner, possesses those of the male. When a hen, for example, which has ceased laying, assumes the plumage, the spurs, and the voice of the cock, there is in her case simply the evolution of characters which continued dormant "as long as her ovaria continued to act;" whilst, on the other side, a male bird which has ceased, or has been unable to exercise the reproductive function peculiar to its sex, acquires the secondary sexual characters of the female bird. In accordance, therefore, with this doctrine of latent characters, all vertebrate animals may be said to be unequally developed hermaphrodites, which have lineally descended from a primitively unisexual, or as it would perhaps be more correct to say, asexual form; and that the distinctions of sex, like all other distinctions in organic nature, are merely the result of divergence of character from natural variation in the intermediate generations.

Now if we again turn our attention to the case of the pigeon, we find that it has been very confidently alleged that the combined influence of domestication and methodical selection has had the effect of establishing such complete divergence of character that naturalists would be justified in grouping the various domesticated forms of the pigeon not only as distinct species, but in distinct genera: and it must, we think, be admitted that as the organization of the pigeon under domestication has been wonderfully plastic, that it would be allowable so far as regards structural change, to make such distinctions. But when after thus provisionally assuming that not only species but genera may, in consequence of the anatomical changes which have been effected in their structural relationship to each other, be regarded as the natural results of variation and selection, we pass, as we must do at once, to the consideration of the extent to which such changes are accompanied, as they should be if the argument on the origin of species be sound, by corresponding changes in the physiological relationship of these artificially formed species and genera to each other, we immediately meet with two great obstacles, which seem to be capable of arresting all further progress in the inquiry; their importance being derived from the fact that they are essentially connected with the reproduction of species. One of these obstacles is hybridism associated with sterility from intercrossing in the feral state, and the other is reversion associated with increased fertility from the intercrossing of domestic breeds. It has been already noticed that one of the most interesting, and, at the same time, one of the most common effects of variation under domestication is that observed in the secondary sexual characters which properly belong to the species, and which sometimes either partially or wholly disappear. In some of these cases the masculine characters are transferred to the female, and in others the female acquires the characters and attributes of the male. It is useful to refer again to these variations in secondary sexual characters at this stage of the inquiry, as a frequent effect of domestication for they will, in some degree, prepare us for the results obtained from the crossing of species artificially formed through the influence of variation under domestication, as contrasted with those obtained from the crossing of true species, in relation to hybridism in the latter, and to reversion in the former case.

With reference to the subject of hybridism in general, Mr. Darwin appears to be fully convinced, "that the sterility which almost invariably follows the union of distinct species depends exclusively on difference in their sexual constitution." In the application of this important observation to domestic breeds of animals, which often present differences of structure fully entitling them, it is said, to be grouped as distinct species, and sometimes even as distinct genera, it is evident that before we can proceed any further in the investigation, it has become necessary to inquire why, in the midst of these remarkable variations of structures should the reproductive system in the different breeds, for example, of the domestic pigeon, be specially exempt from any analogous change. For if, as it has been lately urged by one of Mr. Darwin's ablest supporters, the descendants of the wild rock-pigeon have varied so greatly that they ought to be grouped into at least five distinct genera, containing in all 150 distinct species, it must be allowed either that domestication, whilst it promotes variation in general structure, checks in some peculiar manner any tendency to variation in the reproductive organs themselves; or, that the transformation of varieties into species, and of species into genera has not, through the influence of variation under domestication, been really effected. With respect to any special exemption from variation in the reproductive organs of the domesticated breeds of the pigeon, it should not be supposed that they remain altogether unchanged; but, on the contrary, it may be allowed, especially as the secondary sexual characters usually and readily admit of being varied under domestication, that they not only increase or diminish in size simultaneously with any important increase or diminution in the size of the body generally, but that they may also vary in other ways to a greater or less extent; just in the same way that variations of structure amounting to well-marked defects, which are not unfrequently hereditary, occur in the reproductive organs of the human race, without impairing or checking reproduction. Variation of structure, however great it may be in certain cases in which the organisation has been rendered unusually plastic by long continued domestication utterly fails therefore in the domestic pigeon to represent those structural changes in development on which specific and generic distinctions should be based; notwithstanding the fact, of which it would be impossible to overrate the importance, that the apparent variation in the domestic breeds of the pigeon is generally speaking greater than in the several members of the Columbidæ in the feral state. Hence it has been candidly admitted by Mr. Darwin that whilst, on the one side, there is "perfect or increased fertility" in the several domesticated breeds of the pigeon when inter-crossed; that, on the other side, "hardly a single well-ascertained instance is known of hybrids between two true species of pigeons being fertile, *inter se*, or even when crossed with one of their pure parents." It would, therefore, appear reasonable to conclude that as the variation accumulated under domestication disappears very quickly under the influence of reversion developed by intercrossing, the extinction of intermediate varieties, on which great stress has been laid, and which ought effectually to have secured the isolation of these breeds, should be regarded as a very questionable fact, since there is no interruption or arrest in the backward course or reversion to the ancestral type. For if, in methodical selection, there has been any general extinction of intermediate varieties, the hereditary influence of reversion does not appear to have recognised the occurrence; otherwise, the ancestral form, revealed by crossing, instead of being always that of the wild-rock pigeon, would be frequently, if not usually, a later-formed species; and it would, moreover, be allowable to hope that on some occasions, the reversion, like the occurrence of a supernumerary finger exhibiting piscine affinity in our own race, would extend back not only beyond the *C. livia*, but beyond also the ancestral form of all the Columbidæ to that of the first animal which wore feathers. As there is no such evidence of variation in the reversion of the pigeon, either to an older or to a newer species than the *C. livia*, but, on the contrary, a steady determination to stop in the backward course only when this particular species, which represents the feral ancestor of all domesticated pigeons, has been revealed, it must consequently be admitted that the five genera, and the 150 distinct breeds of the domestic pigeon, are not entitled to any higher rank than that of brevet species; and that there is, at present, no sufficient evidence to warrant the supposition that time will confirm their promotion, so as to entitle them hereafter to the rank of true species.

PART SECOND.

Bibliographical Record.

ART. I.—*The Diseases of the Ear, their Nature, Diagnosis, and Treatment.* By JOSEPH TOYNBEE, F.R.S. With a Supplement by JAMES HINTON, M.R.C.S., Aural Surgeon to Guy's Hospital. 8vo. Pp. 466, of which the Supplement occupies 44.

THE care of maintaining this treatise of the late Mr. Toynbee in the high estimation it has acquired, naturally devolves on his friend Mr. Hinton. The author's modes of thought and views on aural surgery were familiar to him, and fall, one might almost say, to his inheritance. He has for some time enjoyed as wide opportunities for independent observations in the same field of study, and has earned for himself a like character for the ardour with which he toils in it. He has presented us with a supplement which runs in the guise of a series of footnotes destined for specified chapters of the original work, and which is so freely drawn from Toynbee's later contributions to the subject, and so much in the same spirit that the volume may be read as the record by one mind of thirty years' experience in the treatment of aural maladies, and of explorations of their causes.

Altogether we have a book redundant in valuable matter, and, we believe, more indispensable to the profession than any other on the same theme. Yet we doubt whether a volume might not be composed from a digest of its contents, and information elsewhere obtainable which might supplant it as a text book. At all events it has one great defect for such purpose: It rather portrays Toynbee's ideas in process of evolution, than incorporates them in the substance of the work; and the notes in the supplement would not always save a docile reader from the imbibition of precepts which are afterwards relinquished as erroneous.

According to the preface, the "domains of anatomy and physiology have only been entered upon, when requisite, for the elucidation of the pathology or treatment," yet in point of fact the work exhibits the writer as pushing to the front as an original investigator in every division of his subject. He even invents several aural instruments. However, we cannot esteem him as an equally safe leader in all of these respects. He might justly plume himself on his unrivalled pathological labours and his aptitude for treatment; he advanced our microscopic knowledge of the structure of the membrana tympani; and, though Wollaston had found that the faucial orifice of the Eustachian tube when made to collapse by exhausting the tympanum of some of its air, was opened by swallowing (an observation not alluded to in the book), and Wharton Jones and Hyrtl had described it as habitually weakly collapsed, it was left for him to show that its usual state is closure and not patency, and to trace out the muscles that open it in the act of deglutition. It is in his attempts to gain an insight into the physiological advantages of this arrangement that he fails in success.

In physiological acoustics he is more remarkable for lingering in untenable positions than for conquests. The announcement of his discovery in 1853 was associated with his adoption of the hypothesis that hearing is effected through the fenestra rotunda and the air in the drum, in virtue of the resonant properties of a *perfectly-closed* chamber. To this fancy he clings even to the middle of the treatise whose preface bears the date of 1860, though he adds as a "second reason" for the closure, "that, as specially pointed out by Dr. Jago, sound may be prevented entering the tympanum from the fauces;" silent as to the facts and arguments with which the latter's paper (published only a few months after his own announcement) was filled, and which were destructive of the theory upheld by himself. However, in the course of the volume he is found to have vanished from the ground he had so long adhered to, and under cover of some acoustic experiments of his own having little new in principle, encamping upon that marked out by Johannes Müller, viz., that the fenestra ovalis and ossicula chiefly conduct sonorous waves to the labyrinth, the other fenestra and air in the drum somewhat helping.

In physiological mechanics he continued to propound as his own observation, that air is forced into the drums through the Eustachian tubes when we swallow with the nostrils closed, although the writer just named had, two years before, called attention to the visible depression of the lachrymal sacs (the alæ of the nose show the same fact) as manifesting that air is, on the contrary, withdrawn. After ten years this correction gains admission to the supplement, where it figures as Politzer's, who merely verifies it by aid of an air-tight "manometer." Thus, there was promise that Dr. Jago's remaining correction, that the opening of the tube is not strictly limited to the act of deglutition, but happens also on the occurrence of eructation, might in due time be allowed.

With similar reluctancy Mr. Toynbee abandons his view that an artificial membrana tympani benefits hearing by converting a drum with a perforate (true) membrane into a closed chamber; being led, he tells us, to do so by witnessing a demonstration by Dr. Julius Erhard, that hearing might be improved by the pressure of cotton wool on an entire membrane, and coming now to the conclusion that it was by restoring contact occasioned by some disconnection or loss in the ossicular chain. Thus, as the supplement points out, he approached the opinion long held by Mr. Yearsley that the cotton wool supported the ossicula.

In turning to other topics, we may glean from the supplement what has been the presumable progress of aural surgery during the last eight years. In diagnosis we are told that Dr. Von Troeltsh's recommendation of employing a mirror to illumine the meatus and membrane so as to set one's hands at liberty is universally adopted. Dr. Lucae's proposal for distinguishing between affections of the nerve and conducting apparatus is found of service. He tries whether the sound of a tuning-fork vibrating on the vortex of the head or forehead grows louder by closing the meatus. However, it is far from new to us that vibrations thus arrested in the meatus are heard through the membrana tympani, and are a test of ordinary hearing power. Siegler's pneumatic speculum for withdrawing air from the meatus whilst the membrane is kept in sight, is said to have great value in determining whether there are bands of adhesion in the drum.

Under "diseases of the meatus," Mr. Hinton gives us an important observation of his own; that, as far as his experience goes, polypi in the meatus invariably spring from the drum, though they may also have attachment to the walls of the meatus; he finds it better to treat the discharges following their removal, as well as most of those from the tympanum, by means of an absorbent powder such as talc, syringing and sending a current of air by Politzer's method through the Eustachian tube and perforated membrane. He adds hints from his own practice on the treatment of boils in the meatus, syphilitic affections of the ear, &c. Also, we have an interesting series of cases from a late paper of Toynbee's on sebaceous tumours in the ear causing death through caries of the petrous bone.

Under Eustachian tube, membrana tympani, tympanum, nervous apparatus, we have :—Politzer's happy method of inflating the drums by a jet of air through a nostril at the instant of swallowing, and a description of his apparatus, Weber's nose-douche, Loewenberg's rhinoscopic observations, not here thought of wide application, though that writer found them useful in syphilitic and scrofulous cases. Dr. Jago's paper in this Journal on the Functions of the Tympanum being commended to the attention of the reader as "containing much suggestive" physiological "matter;" his "description of the symptoms of patency of the Eustachian tube" is instanced as "deserving a careful study." This is a generous allusion on the part of Mr. Hinton, yet it is surely an oversight that in a special treatise of this sort this complaint should remain undescribed, though it may be so rare that he could only add from his own observation one well marked case to the two from which Dr. Jago derived its characteristics; especially as Toynbee himself had latterly spoken of it as "proved" to be "one of the most unendurable of affections."[1] There follows a lucid exposition of Dr. Jago's theory of the functions of the mucus secreted by the lining membrane of the drum in keeping us unconscious of subjective sounds and perfecting the conducting apparatus. Its pathological groundwork is confirmed from Mr. Hinton's practice, and with qualifications there is a leaning in its favour. Dr. Jago's opinion as to the vascular origin of tinnitus is regarded as putting us in the way of solving the mystery in which this has been involved, whilst Mr. Hinton suggests that it may often arise from pressure on the labyrinth caused by irritable muscles of the drum or otherwise. We are told that among the poorer classes hereditary syphilis frequently destroys hearing, and that this disease at Guy's Hospital furnishes one twentieth of the aural cases, and that "evidently it is the disease, or one of the diseases, which Sir W. Wilde described as affecting the ears in early life, subsequently to, or alternating with, an inflammatory affection of the eyes."

Art. II.—*A Report on Amputations at the Hip-joint, in Military Surgery.* Circular No. 7. War Department, Surgeon-General's Office. Washington, July 1, 1867. 4to. Pp. 87. With 9 lithographic plates and 30 woodcuts.

A Contribution to the History of the Hip-joint Operations performed during the late Civil War, being the Statistics of Twenty Cases of Amputation and Thirteen of Resections at this Articulation in the Southern Service By Paul F. Eve, M.D., Professor of Surgery in the University of Nashville, Tenn. Philadelphia, 1867. Pp. 17.

We have had occasion before to call our readers' attention to the interesting and valuable documents relating to the medical and surgical history of the late civil war which have been issued from the Surgeon-General's office at Washington;

[1] 'A Vindication of the Present State of Aural Surgery,' p. 12, 1864.

and we should be indeed wanting in courtesy if we did not take every opportunity of expressing our gratitude and the gratitude of the profession in England generally for the great liberality with which these costly volumes have been distributed not only to the public libraries and journals, but also to private individuals in this country. We observe, with very great pleasure, that the volumes embodying the complete medical history of the whole war, of which these are but detached portions, are advanced in preparation, and the first instalment of them may shortly be expected.

Meanwhile, we turn to the present treatise on one of the most difficult and most contested points of military surgery.

Every hospital surgeon will readily concede that a compound fracture laying open the hip-joint, whether from a gun-shot or from other injury, is as a general rule a fatal accident. The question on which opinions are divided is what rules of treatment does experience suggest whereby the greatest proportion can be saved? By some it is taught that primary amputation is in these injuries universally fatal, and therefore should be banished from surgical practice, while secondary amputation is looked on with favour. Others are in favour of excision, primary or secondary; others prefer, as a general rule, the expectant treatment. The author of the above official report (Assistant-Surgeon Otis, Curator of the Army Medical Museum, U.S.) does not affect to settle these questions, but merely to bring forward materials for their discussion, supplementing these materials by the opinions expressed by those from whose practice they were collected. The pamphlet by Dr. Eve is, as far as the statement of amputation is concerned, embodied in the official report, with some discrepancies which are too trivial to mention.

The report enumerates and tabulates sixty-one cases of amputation for gun-shot wound performed previously to the war under consideration, and gives the references and results of 111 cases of amputation in civil practice. To these are added more or less detailed histories of fifty-three operations performed during the American civil war, all for gun-shot injury, nineteen of which were performed by Southern, or rebel, surgeons, and are included in Dr. Eve's pamphlet. These amputations are divided by the reporter into four classes. 1. *Primary, i. e.* performed before consecutive inflammation had set in, the duration of which period, he says, will very rarely be found to exceed twenty-four hours. 2. *Intermediary,* performed during the persistence of the inflammatory stage, a variable period, usually included between the day after the reception of the injury and some time in the second or third month. 3.

Secondary, or those performed at a period when the inflammation had abated, and the lesions had become, in a measure, local and analogous to chronic disease. And, 4. *Re-amputation,* after previous amputation lower down. We confess that we do not see the advantage of the separation between the second and third classes in cases like these where the notes are so imperfect as to lead to grave doubts whether so difficult a distinction has been at all correctly adhered to. Out of the fifty-three fresh cases included in the report, nineteen were primary. All the patients were known to have died of the direct results of the operation, except three. Out of these three, one was in excellent health at the date of publication of the Report, more than four years after the operation. The other two were known to be alive, and in good condition, in one case two months, and in the other six months after amputation. If all the cases previously published be added to this list, of which not one is known to have survived, and we concede the reality of recovery in the two cases left incomplete,[1] we shall have forty-four primary amputations with three recoveries. Although this is a black list, it certainly proves that recovery is not impossible, and encourages the performance of the operation in cases otherwise hopeless. Such, for instance, was one of the Southern cases which recovered. The surgeon was close to the man when he received his wound. The femoral vessels were lacerated, and the upper part of the femur smashed to pieces by a large fragment of shell. Compression was kept up so that the man did not bleed to death, while hasty preparations were made for amputation literally *sur le champ.* The patient recovered without an unpleasant symptom. But such cases as this can only occur very rarely, and the most that we can say after perusing the report is that primary amputation at the hip is not necessarily fatal, but that it is so so very often that it ought not to be performed when the slightest prospect of recovery exists without it, and this is the general effect of the opinions of the surgeons quoted in the report. In the second, or "intermediary" category (*i. e.* those amputations performed during the presence of inflammatory symptoms) eighteen American cases are included, which were all fatal; of sixteen cases which are added from previously published sources, two recovered, so that the average mortality, if we could trust to the classification, would be shown to be about the same as in primary amputation. In the third class, or that of "secondary" amputations, in the narrow sense here sought to be introduced

[1] The official reporter hesitates to admit these as cases of complete recovery, but the fact is proved from Dr. Eve's pamphlet, for both these cases occurred in the Southern army.

(when the inflammatory phenomena have wholly passed away) only nine fresh cases are reported, two of which recovered, and eighty previously published, five of which recovered. While unsatisfied of the scientific accuracy of the classification, we are quite ready to admit that in this, as in every other kind of amputation for injury, the prospect of survival is better the more completely the surgical fever following the injury has passed away. Finally, eight re-amputations are tabulated (all of which, except one, occurred in this war), and of which one half recovered. As an appendix, fifty-six miscellaneous cases (not American) are briefly referred to, the details of which are imperfect. The total gives 161 cases, with 142 deaths. Of the nineteen who were not known to have died, the reporter classes three as uncertain, but, as we have stated above, if this intends to include the two primary amputations in the Southern army, the reality of recovery seems in those cases completely proved.

The result certainly tends to establish the soundness of the advice given by the best military surgeons of modern times, to avoid the amputation at the hip-joint as a primary proceeding. In desperate cases, it is no doubt justifiable, and a life may here and there be saved by it, but such cases will be of the rarest. The chief interest of the question, however, now is as to the value of the operation as a secondary proceeding. In this inquiry we must distinguish two classes of cases, the one where the mischief is limited to the upper end of the femur, the other where chronic osteo-myelitis has invaded that bone in the whole or a great part of its extent. In the former class it seems probable that excision will be found to give better results than amputation. Thus, in Dr. Eve's pamphlet, we find a table of thirteen cases of "Resections at the Hip-joint," of which four survived entirely, one fell into the enemy's hands, and probably died, and one would, it was thought, have recovered, but for want of nourishment, to which he succumbed two months after the operation. It must be admitted, however, that Dr. Eve's table is not accompanied by sufficient details to render it entirely satisfactory, nor is this surprising, considering the nature of the service to which it refers.[1] As far as it goes,

[1] We cannot refrain from giving the last few lines of Dr. Eve's pamphlet. Whatever our feelings may be as to the political aspects of the war, no surgeon can feel otherwise than proud of the heroic exertions of our colleagues to save life in the dreadful circumstances which surrounded the Southern Army. Dr. Eve thus compares the Southern statistics, as far as he had been able to collect them, with those at that time issued from the Surgeon-General's office of the United States army :—

"In *Circular No.* 6, Oct. 1865, War Department, Surgeon-General's Office, Washington City, is given a table of twenty-one hip-joint amputations with three successful results. One of these is proved to have been erroneous, which reduces however, it shows that the cases of resection are on the whole less fatal than those of amputation, doubtless because they are less often primary. But the most difficult question remains still for future experience to solve. We can hardly doubt that the following opinion is correct, given by Dr. Gilmore of Alabama, who himself amputated at the hip three times, and performed excision three times (and with one success in each) during this war:

"I recollect some six cases, that would have required amputation at this joint, which recovered without an unpleasant symptom. One case wounded at the battle of Fredericksburg, a Mississipian; two cases wounded at Chancellorsville, Georgians, and the remaining cases wounded at Gettysburg, two of them of Kershaw's South Carolina Brigade, and the other of a Georgia Brigade.

"I believe that one third of the cases of gunshot wounds of the femur, usually supposed to require amputation at the hip.joint, will recover if left entirely to the efforts of nature; and I am confident that a much larger percentage will die if subjected to either amputation or resection." (P. 12.)

That this is the correct view of the case is becoming more and more generally admitted; but we have little doubt that the experience of future wars will enable surgeons to contribute more authentic data than seem to exist at present for determining the actual results of the expectant treatment in these formidable injuries. With regard, again, to cases of chronic osteo-myelitis of the femur, its symptoms and the indications for amputation, much remains for future inquirers to supply. Meanwhile, the present volume contains some useful material.

The reader must not imagine that these returns of the Surgeon-General are complete, and comprise all the cases of each kind which occurred; but they bear internal evidence of disinterested accuracy, and we believe that they may be trusted to as unselected and impartial. For statistical purposes a large and unselected return is nearly as good as a complete one.

The Surgeon-General has also been so good as to send us his "annual report," comprising the total of deaths to strength, and such like official details. The only thing which strikes us in this document is the results of the examination of army

the cases of recovery to one in ten, being precisely *double* the mortality that my statistics make it to have been in the Southern service; and of thirty-two cases of resection or excision of the head of the femur only four recovered, being a recovery of only one in eight, a fatality nearly *four times* greater than on the Southern side.

"Thus has the investigation of this interesting subject during the past two months, by the request issued from the office of the Surgeon-General, led the searcher unwittingly to a most favourable result on the side least expected when we consider the destitute and isolated condition of the South during the war."

surgeons, and which we commend to the attention of our own examiners.

"Number of candidates invited, two hundred and seventy-two (272); number fully examined and found qualified, forty-eight (48); withdrawn before their examinations were concluded, twenty-one (21); rejected after full examination, ninety-one (91); failed to appear, one hundred and twelve (112)—of these, forty-seven (47) were recommended and appointed, one (1) declined before appointment, and two (2) declined after appointment."

Thus, out of 160 candidates who appeared to compete, only forty-four ultimately got into the service. Clearly the U.S. army board do not err on the side of laxity.

ART. III.—*Essays on Physiological Subjects.* By GILBERT W. CHILD, M.D., of Exeter College, Oxford, M.R.C.P., &c.

ON first taking up this small volume we were at a loss to understand what had induced Dr. Child to republish the few papers which form it in the shape of a separate volume. The preface, however, explains the matter. It seems that Dr. Child had always intended to collect into one body the various essays which he might publish, but that he had been compelled "by special circumstances" to do so somewhat prematurely, and before his collection had reached any considerable bulk. By these "special circumstances" is, we presume, meant the fact that the author is a candidate for the scientific chair vacated by Dr. Daubeny's death at Oxford; and that he was desirous of showing the electors that he had not stood aloof from the physiological questions which have been afloat of late years, but had taken an active interest in them. This object is, doubtless, a perfectly justifiable one: and we must admit that this volume, so far as it goes, is evidence of the fact which we suppose Dr. Child is desirous of establishing. Although the papers in this volume appeared originally at various times and in various publications, they are not devoid of all connection with each other. The link between them is, indeed, sufficient to give a kind of unity to the whole book, which might as properly have been called "Essays on Subjects connected with Generation" as "Essays on Physiological Subjects," for it is with generation in some aspect or other that each of the essays is concerned. The first deals with the fertilization of orchids, the second with the effects of close inter-breeding, and the remaining two with the vexed question of spontaneous generation.

The first of the four papers is a reprint from the 'Spectator,' and consists of a review of Mr. Darwin's admirable work on the fertilisation of orchids. In his "Origin of Species," Mr. Darwin had stated that he entertained a very strong suspicion that "in no organic beings can self-fertilisation go on for perpetuity." ("Origin of Species," p. 101). In that volume he only gave the general grounds for his belief in the truth of this law; but having been blamed for propounding this doctrine without ample facts, he published his book on orchids a few years later, in order "to show that he had not spoken without having gone into details." In this work he showed that self-fertilization is an excessively rare occurrence with orchids. In the vast majority of these plants, even when they are hermaphrodite, there exist peculiar arrangements (which he describes in detail), by which self-fertilization is made impossible, and the concurrence of two individuals rendered imperative. In one species, and in one only, so far as is yet known, namely in in the Bee ophrys are there any special and perfectly efficient contrivances for self-fertilization; and even in this case, combined with these contrivances are manifest adaptations for the occasional transport by insects of the pollinia from one flower to another. So that in all orchids, hermaphrodite or not, there is a provision for a more or less frequent intercross with a distinct individual. "Nature thus tells us in the most emphatic manner," says Mr. Darwin, "that she abhors self-fertilization." Dr. Child, in his review, admits the correctness of all Mr. Darwin's facts, but objects to the inference which Mr. Darwin draws from them:

"There exist in nature," says Dr. Child, "three forms of the reproductive function, namely, the hermaphrodite and self-fertilising, the hermaphrodite and mutually fertilising, and that in which the sexes are distinct. These three plans may exist in different proportions, and may be variously modified in different classes of organisms, but they all exist in the vegetable kingdom, and they all exist in the animal kingdom. What has been shown in regard to the tribe of orchidaceous plants is that, though the second method is that which they mainly follow, all three are to be found amongst them; in fact, that in respect of this function they form, as it were, a microcosm corresponding to the macrocosm of the whole organic world; but we must confess that we are wholly unable to see that this affords the slightest ground upon which to establish the dictum that 'nature abhors self-fertilisation.'"

The reader will at once see that this is not a fair representation of Mr. Darwin's doctrine. Mr. Darwin has nowhere said that nature abhors self-fertilization. His dictum is "Nature abhors *perpetual* self-fertilization." The omission of this one word makes all the difference. The reader will also notice that though it is perfectly true that in orchidaceous plants all three forms of the reproductive function are to be found, yet that Dr. Child, in the above passage, has passed without notice the fact that when an orchid generates in the first of the three methods—namely, the hermaphrodite and self-fertilising—this mode of generation is invariably interrupted, at more or less frequent intervals, by the adoption of the second method. Thus Dr. Child in his divisions really describes inaccurately the first of the three forms of reproduction in orchids. Instead of saying "*hermaphrodite and self-fertilising,*" he should have said "*hermaphrodite and self-fertilising, with an occasional intercross.*" The insertion of these words would be the admission of Mr. Darwin's dictum, so far as regards this order of plants. How far the dictum may be true when applied to the whole organic world is another question, with which we are not now concerned.

Dr. Child's second essay is a reprint from the 'Westminster Review.' It deals with a subject of serious practical importance, the supposed ill-effects of marriages of consanguinity. There is doubtless a widespread and deep-seated prejudice against such marriages. Few, however, of the many who entertain this feeling would be able to give any rational grounds for it. Mr. Adam is of opinion that one potent cause, which has given rise to the repugnance with which such unions are viewed, is the confusion which would otherwise arise in the descent of property ('Fortnightly Review,' 1865). Dr. Child ascribes the sentiment to superstition. It is, he thinks, a relic of the time which preceded the Marriage Act of Henry VIII, when all blood-marriages were illegal in England, being prohibited by the Church. We cannot, however, but think that the prejudice against all blood-marriages is merely the expansion of the more deeply-felt abhorrence against incestuous unions; and that the origin of this latter feeling is neither legal nor religious, is plain from the fact that it is felt by the savages of Australia and of South America with the same intensity as by ourselves. "Yet these savages," as Mr. Darwin remarks, "have neither property to bequeath nor fine moral feelings to confuse." ("Animals and Plants," &c., ii, 133). More important, however, than the origin of this repugnance is the question whether there are any solid grounds for it. Are the offspring of marriages of consanguinity in any way injuriously affected? Are they more liable to degeneracy than the offspring of other unions? To this practical question Dr. Child does not give any very decided answer, though we infer from his tone that he looks upon the danger to which the offspring of blood unions are exposed as very slight, and as having been ridiculously exaggerated in men's minds. That the danger is a real one however, there can be no possible doubt. The offspring of blood-alliances are at least ten times as liable to congenital idiocy and deaf-dumbness, not to speak of other defects, as the children of other unions. Dr. Mitchell, deputy commissioner of lunacy for Scotland, made careful inquiry into the parentage of every case of idiocy which came under his notice in his official visitations of nine Scotch counties. He ascertained the parentage of 519 idiots who were born in wedlock. In 98 of these cases (that is, in 19 per cent. of the whole) the parents were blood-relations. Now the exact proportion of blood-alliances to other marriages is not known precisely. Dr. Mitchell, however, estimates it as being about one in seventy. If that be the case, and doubtless it is not far of the mark, the proportion of idiots from blood-alliances is in those counties thirteen times as high as it should be. The law of chances would give 1·43 as the due proportion per cent.; whereas there are in reality no less than 19, or more accurately 18·9 per cent. ('Mem. of the Anthrop. Soc., ii, 415). "Of the 98 idiots whose parents were related, the degree of relationship was as follows:

"First cousins in .	42 cases.
Second cousins in	35 ,,
Third cousins in .	21 ,,
	98

"It is probable that more second and third cousins intermarry than first cousins, yet these last produce a larger number of idiots. The closer, in short, that the alliance is the greater appears to be the danger." As regards deaf-mutism Dr. Mitchell obtained a very similar result from inquiry into the data afforded by ten deaf and dumb asylums in England and Scotland.

M. Boudin has made inquiries on a still larger scale in France and found that, while consanguineous marriages are only 2 per cent. of all marriages in that country, the number of deaf mutes born of such marriages is to all deaf-mutes in a proportion, which varies in different parts of France, from 25 per cent. to 30 per cent. He found, moreover, that the danger of deaf-mutism increases with the closeness of kinship in the parents. It will be at once seen how nearly these results tally with those of Dr. Mitchell. The offspring, then, of blood-alliances appear to be more liable to certain congenital defects than other children. The question now arises—Is it the mere consanguinity of the parents which is *in itself* the cause of this liability? or is the explanation to be found in the fact that two cousins or other blood relations are more likely than two non-related persons to be afflicted with the same morbid peculiarity, which, in case of their union, is transmitted in an intensified form to the progeny? The children of two insane, or of two gouty, parents are of course more likely to be insane or gouty than the offspring of a union where only one parent is tainted. It is said, for instance, to have been ascertained that, if a deaf-mute is married to one who hears, the chances of their having a deaf-mute child will be 1 to 135, but if deaf-mute persons intermarry, the chances rise to 1 to 20. There can be no doubt that much of the evil resulting from unions of blood-relations is thus to be explained. Dr. Child insists that this is the explanation of it all. Mere consanguinity in parents, independently of any common taint in their blood, is in his opinion perfectly harmless, and the fear of it is a superstition. Given in fact, a healthy family, one that is without tainted blood, nothing could be better luck for a child than to spring from the union of two of its members. It would probably inherit the tendency to healthy existence in an intensified degree. In support of this view, Dr. Child quotes the "Herdbook" and the "Studbook." He gives the pedigree of the celebrated bull "Comet," and shows that this animal, which in no sense could be called degenerate, was bred with a degree of closeness, any approach to which in the human race would be quite impossible. Similar instances might be given in the case of sheep and pigs. Yet, notwithstanding these undoubted facts, it is the general opinion of those breeders of animals who have had the most experience, and especially of those who breed animals which propagate quickly, that evil does inevitably follow sooner or later on close interbreeding, but that it does not ensue with equal rapidity in the case of all animals. The deterioration consists in the loss of constitutional vigour, size, and fertility, and not in any falling off in the general form of the body. If close interbreeding be really perfectly innocuous, it is the most extraordinary fact that breeders of prize birds, who have every possible interest in keeping their strain pure, should yet be almost unanimous in asserting that it is absolutely necessary, notwithstanding the trouble and expense thus caused, to cross occasionally their much prized birds with individuals of another strain, but belonging, of course, to the same variety. Yet Mr. Darwin assures us that this is the case. Those who have read the evidence collected by this great authority on the effects of close interbreeding, and given in the seventeenth chapter of his recent work, will, we think, agree with him, that it is, if not proved, yet in the highest degree probable, that interbreeding prolonged through many generations is highly injurious. Whether the much more diluted degree of interbreeding, which alone is possible in the case of man, is also injurious, independently of taint in the parents, is a question, in which we agree with Dr. Child, that the evidence is very defective. As to the practical question, however, whether consanguineous marriages are advisable or not, we feel no hesitation. We indorse fully the following remarks of Dr. Mitchell, "The practical question is this, should they or should they not be avoided? And the answer we are led to give is that they should, for the reason that they tend to injure the offspring. It matters not practically whether the injurious influence is the result of some mysterious effect, intrinsic in the *consanguinity* itself, or merely the result of this, that consanguinity increases the risks of finding undesirable or morbid peculiarities transmitted from parents to children in an intensified and dangerous form. If the results are disastrous, they will not be less so on one theory than on another, and the lesson will be the same. If the relations by blood are liable to possess the same morbid tendencies, and if by pairing among themselves for procreation, they are likely to transmit these tendencies in a dangerously increased form to their children, then it is surely their duty to avoid such unions, and to seek among strangers alliances with individuals *more likely* to possess qualities calculated to modify or counteract the morbid peculiarities in question. It may be that there is absolutely nothing whatever in the bare fact of consanguinity, and that a marriage of kinship should be avoided on the same grounds as a marriage between any man and woman *both* predisposed say to insanity. In the case of cousins, though there may be nothing common to them of so marked a character as a declared tendency to insanity, still there may be common to them any one of a hundred transmissible peculiarities, which it would be very undesirable to send down to their children in an aggravated form. Even a strong temperament common to both might thus be intensified into disease in their offspring. It follows, therefore, that as the chances of possessing similar peculiarities are great among relatives, and as intermarriage tends to give a dangerous strength to these in the offspring, that to avoid such risks the prudent will avoid such unions as appear to increase them."

Our space will not permit us to say more than a very few words of the two remaining essays in this book. They both deal with the ancient and still-disputed question of spontaneous generation. In the one, Dr. Child gives an account of the controversy between M.M. Pasteur and Pouchet. With this we need not trouble our readers. They are doubtless well acquainted with the admirable researches of M. Pasteur, which most physiologists have accepted, as giving the *coup de grace* to the hypothesis of spontaneous generation; and no less with the counter experiments of M. Pouchet. They probably know also that what was at first a scientific discussion degenerated in time into almost a personal squabble. In the other and last essay, Dr. Child describes a repetition by himself of a number of Pasteur's experiments. He states that he observed all the precautions which M. Pasteur himself speaks of as "exaggerated," and that notwithstanding this, he obtained results which entirely disagree with those arrived at by M. Pasteur, and now, to a certain extent, vouched for by the Commission of the Academy of Sciences. Dr. Child found that organisms were developed exactly under the circumstances in which M. Pasteur asserts that their production is impossible. For this discrepancy, Dr. Child accounts by the fact that M. Pasteur never examined his substances with any higher power than one of 350 diameters, while it is, according to his experience, impossible to recognise the presence of bacteria with any degree of certainty even with double that magnifying power. Dr. Child himself made use of a glass with a power of 1500 to 1700 diameters. That Dr. Child really found these bacteria in his infusions there can, of course, be no doubt. If any such were possible it would be at once set at rest by the fact that Dr. Beale assisted in the microscopic examination. The question can, therefore, only be whether Dr. Child, in conducting his experiments, inadvertently neglected any precaution. We cannot say that we can see any flaw in his method of proceeding as described by him. We should, therefore, be glad to hear that M. Pasteur had repeated his experiment, using the higher powers, which Dr. Child says are requisite.

It will have been seen that we by no means agree in all respects with Dr. Child in the conclusions at which he arrives in these essays. We must, however, admit that this little volume deals with topics of the highest interest, that it is written in an easy style, and that the author has not blindly surrendered himself to the guidance of authority, but has taken pains to form an independent judgment.

ART. IV.—*Leçons de Clinique Chirurgicale professées à l'Hôtel Dieu de Lyon.* Par M. A. DESGRANGES, &c. &c. Paris, 1867. Pp. 108.

I. *Tumeurs Abdominales.*
II. *Corps Etrangers du Genou.*
III. *Tumeurs du Sein.*

MONSIEUR DESGRANGES' reputation renders any statement of his experience at the celebrated provincial French hospital to which he is surgeon most acceptable. The surgical school of Lyons is looked upon in England as being in many respects in advance even of that of Paris, and particularly in respect of ovariotomy, to which M. Desgranges' first paper in this work chiefly refers. If we are not mistaken, it was from the surgeons of Lyons and Strasbourg that those of Paris first learned to believe in the success of an operation which one of the chief scientific societies of that capital is still only half inclined to admit as being justifiable. M. Desgranges relates two successful cases of ovariotomy, and accompanies them with a commentary in which he lays down rules for the diagnosis of abdominal tumours of various kinds. These diagnostic rules are, no doubt, useful, and in the main correct; but like most French surgeons M. Desgranges affects "netteté" and "précision" in his diagnosis to a degree that nature will not lend herself to. Thus, any student who might imagine that he could diagnose the presence or absence of adhesions in an ovarian tumour by the precepts which M. Desgranges lays down, would we fear be exposed in practice to numerous disappointments. We are glad, however, to see that the operation of ovariotomy is making its way in France, and congratulate M. Desgranges on his acceptable contributions to its progress. One curious circumstance which we have not met with elsewhere occurred in one of M. Desgranges' ovariotomies. "During the first six months after the operation at each menstrual period, a little abscess regularly formed over the pedicle of the tumour," which had been secured outside the wound by a clamp. This circumstance is with great probability attributed by M. Desgranges to the determination of blood to the parts at the menstrual period, and possibly to increased traction on the pedicle by the body of the uterus, increased as it is in volume by congestion.

With respect to loose cartilages in the knee, M. Desgranges' lecture does not seem to us of equal merit with the others. His explanation of the pain which attends such bodies is neither supported by any reasoning nor in itself intelligible. Rejecting the old idea (and as we believe the true one) that this pain depends on the body being caught between the bones (although his own case proved beyond a doubt that such bodies can get between the bones, which Richet and Nélaton deny) and rejecting also the exceedingly improbable explanation advanced by M. Richet, who refers the pain to contusion of the synovial membrane, M. Desgranges asserts that the pain results from stretching and twisting of the ligaments, though there is not a shadow of evidence that any such stretching or twisting occurs, nor does the analogy of sprains lead to the idea that if it did it would produce similar symptoms. Again, as to the treatment, Mr. Desgranges says, at page 53, "that only incomplete, and insecure relief is to be expected from compression and immobilisation, and that, therefore, it is expedient to reject these measures in favour of extraction." On page 60, on the contrary, he says that, "the wise precepts of M. Larrey ought to guide us," the third of which is, that the operation should not be undertaken till attempts to fix the foreign body have been made and failed.

The concluding paper in this pamphlet need not be reviewed at length here. It consists of the history of some characteristic examples of cancer and of sero-cystic (or tubero-cystic) tumour, accompanied as in the case of the abdominal tumours with rules for diagnosis, stated in a tabular form. The paper is an interesting and a practical one, but contains little for detailed remark.

At its conclusion is a statistical table of the results of operation for tumour of the breast at M. Desgranges' Hospital in the ten years 1852 to 1862. The result gives one an insight into the reason why important surgical operations find such slow access to French practice. In fact, the hospitals in France are so unhealthy from their imperfections in ventilation, cleanliness and drainage, that none but unavoidable operations are likely to be performed. As an instance of this, in such a simple and usually harmless operation as removal of the breast, we have here the statement that out of 222 operations, 42, or nearly one fifth died; and if we limit ourselves to the more formidable cases, those of cancers, the proportion is greater still, 39 having died out of 153, or more than one quarter of the cases; and if we turn to the statement of causes, we shall see that they are almost all such as are connected with unhealthy hospital atmosphere; three died of traumatic fever; ten of erysipelas; three of pyæmia; four of hæmorrhage; three of pleurisy; three of pneumonia (it is not said in the latter cases whether pyæmic or no); one of gangrene: two of diphtheritic angina. When will French hospital surgeons be induced to admit that cleanliness and fresh air are even more necessary to success in practice than operative dexterity and scientific acquirements?

ART. V.—*Essai de Pneumatologie Médicale:—Recherches Physiologiques, Cliniques, et Therapeutiques, sur les Gaz.* Par J. N. DEMARQUAY. Paris, 1866. 8vo. Pp. 861.

Essay on Medical Pneumatology, &c. By J. N. DEMARQUAY.

DR. DEMARQUAY here presents us with a work of not far short of a thousand pages octavo, upon medical pneumatology, a subject not familiar, we apprehend, or at least not known under this designation, to many of our readers; for it does not figure among the contents of class-books of medicine or therapeutics. Nevertheless the matters treated of under this heading can claim no novelty. Every old woman has a practical acquaintance, particularly since excessive tea-drinking has been common, with that division of medical pneumatology known as wind in the belly, and every practitioner accustomed to prescribe for hospital or dispensary out-patients, ought to be duly impressed with the grievous ills resulting when the "wind and the water meet," a catastrophe so constantly recounted by his patients. But neither old women nor their doctors will find much to enlighten them on gastro-intestinal accumulations of gas in the pages of the work before us. Not but that the subject is considered. On the contrary the author discusses over a score of pages the questions relating to the sources of the gaseous matters: still he has nothing to tell the interested individuals alluded to how best to cure the windy colic and spasm.

In fact the term medical pneumatology is the peg whereon to suspend dissertations on several topics having no natural affinity either in physiology or pathology. To let the author speak for himself, he says:—"The work is divided into two parts; the first, physiological and pathological; the second, physiological and therapeutical. In the former we treat of, 1. Gases in the blood; 2. Sanguineous or rather vascular pneumatosis; 3. Gastro-intestinal and genito-urinary pneumatosis." These matters he has sought to elucidate by physiological experiments. "In the second part, devoted to a physiological and therapeutical investigation of gases, we have made a particular study of oxygen, carbonic acid, nitrogen, and protoxide of nitrogen and of hydrogen, inasmuch as these fluids enter, almost all, into the composition of the air and of water, and also constitute the nutritive elements in our food." It is this second part which will be read with most interest, although the diffuse style, and the attempt, common in French medical works, to be exhaustive, will make its perusal tiresome.

The author sets out with a general notice, borrowed from several physiological writers, chiefly fellow-countrymen, of the gases found in the blood, and next, as a result of bibliographical researches, gives a collection of cases—many of them reported with little precision, and consequently of as little worth—where gas has been found as a morbid product in the circulating fluid. He remarks on the coincidence of such cases with the existence of paroxysms of dyspnœa, and with the occurrence of profuse hæmorrhage, particularly with that following child-birth. He rejects the supposition of the gaseous bubbles found being post mortem products, and essays, unsuccessfully we think, to discover the cause of their presence in relation to difficulty of respiration and to hæmorrhage.

Gastro-intestinal and genito-urinary pneumatoses next engage attention. Without venturing to deny the possibility of the secretion or exhalation of gas in the bowels, M. Demarquay considers that the facts that some at least of the gases have no existence in the blood, and that different gases are formed in different portions of the intestinal tube, are adverse to the notion of such secretion, and at the same time speak in favour of the derivation of the gases from the contents of the digestive canal. Again as to the seat of the gas in tympanitis or meteorism, he is convinced that it is within the intestines and that no gas enters the peritoneal cavity during life, except there be rupture of the intestinal walls.

To remove meteorism and its disastrous consequences Demarquay sanctions fully the plan of puncturing the distended intestine; urging that the fear of peritonitis is no greater than in tapping for ascites. The puncture may be made by a trocar, at any part of the abdominal aponeurosis, or in the median line, care being taken to choose a point where the tympanitic resonance is very marked. It is well also previously to make a small incision through the skin, preparatory to thrusting in the trocar.

It must, however, be stated that M. Demarquay's personal experience of puncture is not very satisfactory; at times only an insufficient quantity of gas has escaped; at others, the gas has soon re-accumulated.

Above 200 pages are devoted to the subject of traumatic emphysema. He recognises three varieties of the lesion;—1. Partial; 2. Diffused; 3. General. The second form implies the progressive propagation of emphysema from some one part until it finally occupies the entire body. By general emphysema, he means that which has no particular point of origin, but is simultaneously developed at all parts; as, for instance that consequent on septicœmia. After this long disquisition on air permeating the tissues, its mode of entrance, its symptoms and consequences in relation to each organ of the body, M. Demarquay enters upon the second portion of his comprehensive work; the study of gases in their physiological and therapeutical relations. Carbonic acid is first taken in hand, and occupies above 100 pages in its discussion. Much of this space is occupied by historical notices of the first recognition of carbonic acid and its properties, and of the opinions entertained by chemists and physicians respecting its uses and therapeutical properties. But we are also favoured with records of experiments on animals and on man made by M. Demarquay, and would wish that this able surgeon had restricted his book-making chiefly to telling us what he had himself observed, and to what conclusions he had arrived. In dismay at the scores of pages of historic matter and of crude hypotheses of past times, the tendency of the reader of his book is to turn over the pages, at the risk of overlooking something important, to discover the *résumé* or budget of conclusions, which, by a kind dispensation, every Frenchman is fortunately impelled to present his readers. The conclusions respecting the application of carbonic acid and its results in the human economy are found at p. 458; but the necessary shortness of this notice forbids their quotation, and must also be accountable for the remaining brief notice of the contents of this work.

In examining the properties of carbonic acid given internally Demarquay has largely quoted our old English writers, Percival and Beddoes; but it is as a topical application that this gas is of the widest utility, and that the author can speak of its value from extensive personal experience with it in surgical maladies. We would direct our readers' attention to the section here referred to on the topical uses of carbonic acid. (Pp. 499—562.)

Oxygen is the next gas examined. Its medical history, and the notice of its physiological action, of its mode of preparation and administration medicinally, and of its therapeutical action, extends over upwards of 250 pages, and might, therefore, of itself afford ample scope for a review. M. Demarquay is an original investigator of the properties of oxygen on animal life, and has himself resorted to this gas as a medicine, using it both internally and externally. Its curative value in medical cases, in asthma, consumption, and some other diseases, is attested by himself in a few instances, but he chiefly relies, for the demonstration of the fact to the records of others, particularly to those of Dr. Beddoes, published in 1798. The advantages of oxygen in surgical cases are, however, illustrated and enforced by his own experience as a surgeon. He has used the gas in cases of senile gangrene, in phagedenic and cancerous ulceration, and in instances of broken down health and anæmia attendant on caries. In senile gangrene he envelopes the limb in an india-rubber bag filled with oxygen, and allows the action of the gas to proceed for two or three hours. The most striking results of this proceeding are—the cessation of pain, the excitation of the capillary circulation; the decoloration of the limb which rapidly loses its lividity, and lastly the elimination of sloughs, followed by cure.

The third and concluding section of the work is occupied by an investigation of nitrogen, of protoxide of nitrogen, and of hydrogen. The employment of these gases therapeutically has been very limited, and Demarquay can add little to the information presented by older writers and experimenters. A M. Chapelle (of Angoulême) has, we are told, resorted to protoxide of nitrogen as a cure for epilepsy, and detailed his experience in a paper sent to the Academy of Medicine in 1865. The number of cures for epilepsy which have from time to time been vaunted would suggest the inference that epilepsy ought no longer to be met with, or else that each discoverer of a cure has been the victim of delusion. The foregoing observations will suffice to show that the treatise of M. Demarquay is one of great value, especially as a book of reference.

Art. VI.—*An Explanation of the Movements of the Iris*, by Robert J. Lee, M.B. Cantab., M.R.C.P. London, 1867. Pp. 15.

The object of this little work is to show, as expressed in the author's words, "in a brief and simple manner, that the strong analogy which exists between the movements of the heart and the iris is confirmed by the demonstration of similar nervous structures in the two organs." After certain apposite and interesting allusions to the progress of our knowledge of the anatomy and functions of the nervous system; including reference to the discovery by his honoured father of the great system of ganglia and nerves of the uterus and of the heart, and to the connection existing between the spinal and sympathetic nerves, Dr. Lee proceeds to indicate by examples that ganglionic plexuses of nerves "are intended to unite the cerebro-spinal and sympathetic systems." The best example of this given is in the case of the ophthalmic ganglion, "which bears the same relation to the ciliary muscle and the iris as the cardiac plexus bears to the heart, is similarly connected with the cerebral and sympathetic systems, through the branches it derives from the third and fifth pair of nerves, and from the cavernous plexus." The author then proceeds to refute the opinion held by many physiologists, that such ganglia are sources of the nervous power of the organs which they supply with nerves; and are related to the respective organs in the same manner as the brain and spinal cord are related to the voluntary muscles; and he shows in detail how the nerves from the spinal cord differ from those in various organs, such as the heart. After alluding to the difficulty in explaining the movements of the iris, he draws a parallel between the actions of the heart and iris, and points out how "both act to a great degree independently of the brain, and both display power of movement for some period after death. They both derive their nerves from a ganglion or plexus situated closely and externally to them, and they both possess in themselves, when removed from the body and from connection with the external ganglion, the same property they displayed before." He then remarks that, as a result of the view taken of the nervous system of such organs as display in voluntary movements, it was reasonable to expect "that ganglia would be found to exist in the structure of the iris similar to those in the heart and other organs." Dr. Lee then describes the dissections of the ciliary muscles, and iris, and nerves of the eyes of various animals which he has made, and by which, in spite of the difficulty experienced in following the nerves into the ciliary muscle (especially in animals where it is small, dense, and firm), he establishes the fact that the ciliary muscle and iris possess a system of ganglia and nerves similar to those in the heart, and never before demonstrated; and he exemplifies the subject by describing what he found chiefly in dissecting the eye of the bird: representations of the dissected ciliary nerves being well shown by photographic illustrations. He concludes that generally the muscular activity of the iris varies directly with the number and size of the ganglia and nerves connected with it.

Dr. Lee's remarks constitute an important addition to our knowledge of the structure of the eye, and form a very interesting résumé of observations evidently carried on with much industry and carefulness; and in a spirit which we look for in a son of the discoverer of the ganglia of the heart and uterus.

ART. VII.—*Over de Uiteinden der Smaakzenuwen in de Tong van den Kikvorsch.* Door TH. W. ENGELMANN, Assistent bij het Physiologisch Laboratorium te Utrecht. Met Plaat. 'Nederlandsch Archief voor Genees- en Natuurkunde,' Deel III, 3e Aflevering, 1868.

On the Terminations of the Gustatory Nerves in the Tongue of the Frog. By TH. W. ENGELMANN, Assistant in the Physiological Laboratory at Utrecht. Reprinted from the 'Nederlandsch Archief,' Vol. III, Part III, 1868. 8vo. Pp. 26.

IN the investigations detailed in the above paper, the author has had the aid and cooperation of one whose name we are rejoiced to see once more in connexion with physiological research. We trust that the young Heer Schroeder van der Kolk, Med. Cand., is about to walk in the footsteps of his illustrious father, and to maintain the *prestige* conferred by the labours of the latter upon the name which his son now bears.

Billroth had found[1] that only the fungiform papillæ of the tongue are provided with nerves. At the same time he confirmed the fact, already observed by Leydig, that the terminal surface of these papillæ is covered with a peculiar epithelium. From the previous researches of Schultze on the extremities of the olfactory nerves in the mucous membrane of the nose, he thought a connexion between the nerves and the epithelial cells of the terminal surface of the lingual papillæ not improbable, though he could neither trace the nerves into the epithelium, nor find the analogues of the olfactory cells of Schultze.

Fixsen,[2] too, could trace the nerves only into the connective tissue of the papilla. Hoyer[3] denies the existence of a connexion between the epithelial cells and the nerves—

"E. A. Key[4] made an important step in advance. He discovered not only in the epithelium of the papilla peculiar cells, by him called gustatory cells, but he saw also the dark margined nerves continued into delicate varicose fibres, which ascended into the epithelium. Each gustatory cell possesses, according to him, at its central extremity, a thin varicose outrunner, which passes into a fine nerve fibre."

Of Key's researches we gave an abstract in our 29th volume, April, 1862, p. 422. Our notice was, however, taken, not from the German paper above referred to, but from an amplification of the same subsequently published in the form of an academic thesis, by the author, in his native language, the Swedish.

Key's important results, obtained under the guidance of Professor Max Schultze, were subsequently disputed by R. Hartmann,[5] who, unable to find the gustatory cells and the fine nerve fibres of Key, considered them to have been artificially produced. Dr. Engelmann is not aware that any recent investigations have been made respecting the terminations of the gustatory cells in the frog.

The author's own researches relate to adult specimens of the Rana temporaria. We shall endeavour, as briefly as possible, to lay his principal results before our readers.

[1] Müller's 'Archiv,' 1858, p. 159.
[2] Carol. Fixsen, 'De linguæ raninæ texturâ.' Dorpat, 1857.
[3] Hoyer, in 'Arch. f. Anat. u. Physiol.,' 1859, p. 481.
[4] Key, in 'Arch. f. Anat. u. Physiol.,' 1861, p. 329.
[5] Hartmann, in 'Arch. f. Anat. u. Physiol.,' 1863, p. 634

The epithelium covering the circular terminal surface of the papilla consists of three kinds of cells, which he calls cup-cells, cylinder-cells, and fork-cells. All these three varieties are characteristic of the terminal surface of the papilla; they are met with in no other part of the surface of the tongue. They are well defined, having no transition forms among themselves. So long as the cells remain in connexion on the papilla we can, viewing the latter in profile, at most see only two kinds, of which the cup-cells, constituting by far the greatest mass of the epithelium, are the most striking. The author found that he could with glass rods very perfectly isolate the cells in tongues which had lain one or more days in a mixture of equal parts of strong glycerine, and of a four per cent. solution of bichromate of potash.

The *cup-cells*, the "modified epithelial cells" of Key, which are found to the number of several hundreds on the larger papillæ, form the outermost layer of the epithelium investing the terminal surface of the papilla. They consist of cylindrical bodies standing perpendicular to the surface of the papilla, and containing in their lower third a globular, vesicular nucleus, of about ·008mm., in which is a central nucleolus of ·001mm. in diameter. There is no doubt that the cup-cells are not the extremities of the nerves, but only peculiar, indeed essential characteristic epithelial cells of the gustatory papilla.

The *cylinder-cells* consist of an ellipsoidal body, situated in the deepest layer of the epithelium, and continued in a straight cylindrical outrunner reaching to the external surface of the epithelium. The body is almost completely filled with an ellipsoidal vesicle, the nucleus, in the centre of which lies a small nucleolus. Only a very slight layer of protoplasm surrounds the nucleus.

On a review of his observations, the author comes to the conclusion, that neither are the cylinder-cells to be looked upon as extremities of nerves, but as a peculiar kind of epithelial cells, differing in their properties remarkably from other epithelial cells; they are, no doubt, for the most part, the rod-cells of Key. The drawings given by the latter make it probable that he had seen them and taken them for the terminal organs of the nerves. He confounded them, however, with the fork-cells, of which he seems to have observed only injured specimens.

Fork-cells. These remarkable apparatuses, scarcely deserving the name of cells, are, notwithstanding many individual differences, formed after one and the same type. They all consist of a body provided with fibrinous outrunners. The body has the figure of an extended ellipsoid, and is almost completely filled with a nucleus of the form of a vesicle with nearly central nucleolus. The outrunners arise at the two poles of the ellipsoid, which last the author distinguishes as peripheral and central poles.

From the peripheral pole arises an usually forkshaped outrunner, whose free extremities just reach the surface of the epithelium. This outrunner is divisible into two parts, the handle of the fork and the prongs. The longer the former is, the shorter are the latter, and *vice versâ.* The prongs are usually two, less frequently three. All the prongs are extremely slight cylindrical rods. The optical properties of the handle and prongs are those of very fine, pale nerve fibres: they have the homogeneous appearance and the dull lustre found, for example, in the axis-cylinders occurring in the posterior layers of the cornea of the frog. Chemically, too, they almost entirely resemble the latter. They are, moreover, flexible and tolerably elastic.

Outrunners dividing dichotomously arise also from the central pole. These too agree in their physical and chemical properties with minute axis cylinders. They lie in the spaces between the bodies of the cylinder-cells, and their extremities reach the surface of the stratum of connective tissue of the papilla. The latter in this situation exhibits a lamellar thickening, perforated by abundant ramifications of very delicate pale nerve fibres.

The author does not feel in a position to decide whether the central outrunners of different fork-cells pass into one another, or whether each descends separately to the connective tissue stratum of the papilla. In any case these outrunners form with their dichotomous ramifications an extremely dense fibrous network, which almost completely fills the space between the bodies of the cylinder- and of the inferior fork-cells.

While Dr. Engelmann considers the cup- and cylinder-cells to be only epithelial cells of peculiar construction, he looks upon the fork-cells as the extremities of the gustatory nerves, as will appear, he says, more clearly from the description of the nerves yet to be given. As to the epithelium investing the remaining surface of the papilla, it consists of ciliated cells and non-ciliated cylinder epithelial cells. The first of these form a small girdle or garland around the circular layer of the nerve epithelium, a closed ring, as it were, having the breadth of one, or at most of two ciliated cells, and not extending, as stated by earlier observers, over the whole surface of the papilla. The sides of the papilla are covered with ordinary non-ciliated cylinder epithelium.

The nerve fibres of the gustatory papillæ.—The stratum of connective tissue in the papilla consists of a greater under portion formed of loose, and a less upper portion, laminated, composed of dense tissue. The former contains the blood-vessels, the terminations of the divided muscular fibres, and the darkly defined nerve-tubes. The upper part of the stroma of the papilla, called by the author *nerve-cushion,* consists of very dense, nearly homogeneous-looking connective tissue. Inferiorly the nerve-cushion is rather firmly attached to the other connective tissue of the papilla; upwards and outwards towards the epithelium, it is sharply defined. It has neither connective tissue corpuscles, nor nuclei, nor outrunners of muscular fibres, nor blood-vessels, nor elastic fibres, but a surprisingly large quantity of extremely fine, pale nerve fibres. It forms the basis on which the whole nerve-epithelium rests.

"The five to ten medulla containing nerve fibres of the papilla run in the axis of the latter, undivided, to the under surface of the nerve cushion. On entering this, or shortly before, they become more pointed, and suddenly lose their dark contours; their neurilemma, however, coalesces with the dense tissue of the nerve-cushion. Immediately after their entrance the nerve fibres, which have already become very slight and pale by repeated dichotomous subdivision, form a delicate nervous network, which extends horizontally through the whole inferior half of the nerve-cushion, and whence very numerous extremely fine branches, usually again subdividing, ascend in a tolerably straight direction to the free surface of the nerve-cushion. The continuations in the epithelium of these branches, which perforate the nerve-cushion, are the above described central out-runners of the fork-cells."—(P. 22.)

This, which appears to be the principal point in the work, we have stated in the author's own words. Previous observers had seen the nerve-cushion, but had taken a different view of it. Key looked upon it as a colossal extension of the neurilemma, and called it "Nervenschale."

Dr. Engelmann does not profess to have absolutely demonstrated the connexion between the nerve-fibres and the fork-cells above referred to; but he shows that such a connexion is all but certain, though it appears just to escape positive detection through the insufficiency of our optical instruments. "We see delicate, pale nerve-fibres reach in very many points the surface of the nerve-cushion: we see from this surface equally delicate, extremely numerous fibres, having the same properties as pale nerve-fibres, issuing and continued directly into the substance of the fork-cells. If we now assume the existence of a connexion between the former fibres and these latter, we do only what is necessary: we assume what is by far the most probable."

Certain physiological questions suggest themselves, such as, whether each fork-cell is connected with only one or with several dark margined nerve-fibres? The latter view the author believes to be the more probable. In addition to the arguments with which he supports this opinion, we might perhaps suggest, that such an arrangement is more in accordance with what we observe in the nervous system generally, as in the multipolar cells of the spinal cord. This and one or two questions Dr. Engelmann leaves, however, for the present undecided, "Content with the result, that the peripheral extremities of the gustatory nerves are organs of peculiar structure: the fork-cells, which are characteristically distinguished from the peripheral terminal apparatus of other nerves, a fresh proof of the truth of the proposition, that specific functions are connected with specific forms" (p. 26.) Dr. Engelmann's valuable paper is illustrated with a well executed plate from drawings by the author himself.

Art. VIII.—*The Diseases of the Prostate. Their Pathology and Treatment.* By Sir Henry Thompson, &c. &c. Third edition. London, 1868. Pp. 360.

It would be quite unnecessary to give an extended notice of a new edition of a work so well known as this. The professiona public have already marked in the most decisive manner their appreciation of Sir H. Thompson's clearness, fulness, and trustworthiness as a writer upon the class of diseases of which he treats, and we need, therefore, say no more on that subject. The present edition of this work contains some interesting matters not included in the preceding, the valuable results of increased experience and extensive practice. Amongst these one of the most important is the opinion which a very great familiarity with cases of stone has led our author to form of the value of lithotrity, when the prostate is affected with chronic enlargement. Sir H. Thompson claims for himself a personal experience in cases of this nature only second in extent to that of M. Civiale. He points out, in the first place, the great danger, and often formidable difficulty, of a cutting operation in cases of enlarged prostate, and he discusses *seriatim* the various obstacles to the successful performance of lithotrity, when this complication exists. Notwithstanding such obstacles Sir H. Thompson shows that Sir B. Brodie speaks in favour of its performance, and relates successful cases; that Civiale was also in favour of it, providing the enlargement was not enormous (in which case he would propose the high or suprapubic operation), and that Dr. Ivanchich of Vienna has recorded statistics of his own experience, which appear to show a much more favourable result from lithotrity than has ever been obtained by the cutting operation in such cases, and to this testimony he adds his own experience in these words—"I have operated upon numerous cases of calculus of the bladder, in which the prostate was enlarged—certainly in thirty; and I have no hesitation in saying that I cannot consider it as lessening the probabilities of success in skilful hands, or, at all events, to a very small degree" (p. 344). Such an opinion from so experienced an operator is gratifying, since there is no question of the extreme danger of lithotomy in such circumstances. The difficulty of seizing the stone Sir H. Thomson regards with Sir B. Brodie as usually trifling; unless the enlargement is very great and the sinus behind the enlarged gland lodging the stone very narrow, in which case the instrument must be reversed and the stone picked up with it before being crushed, a difficult and dangerous manœuvre. But the common embarrassment is in getting away the fragments. Two recent improvements in the apparatus, however, have made this embarrassment less than formerly. One is the flat-bladed lithotrite, which enables the surgeon to remove a considerable quantity of débris, and is provided with a screw, by which the amount of such débris can be regulated; so that it is less dangerous than the scoop lithotrite formerly in use for this purpose. For details as to this instrument, and the method of employing it, our author refers to a forthcoming new edition of his own work on lithotomy and lithotrity. The second aid in withdrawing fragments is the "exhaustion-syringe" recently introduced into practice by Mr. Clover. Our surgical readers are, of course, all familiar with this instrument, of which Sir H. Thompson speaks in terms of deserved favour. He has used it, he says, nearly 200 times, a convincing proof of its utility. He adds, however, "It is necessary to use all such apparatus with extreme gentleness, and I should prefer to do without it, if possible, as its employment is quite as irritating as a sitting with the lithotrite. Repeated injections, which alter rapidly and considerably the size of the bladder, are always irritating to that organ." Finally, Sir H. Thompson believes that in some cases, assistance may be obtained from the injection of chemical solvents into the bladder, the composition of the stone being previously ascertained.

We have selected the above out of the numerous interesting topics on which this edition of Sir H. Thompson's work bestows new information, partly in order that our readers may judge of the importance and value of the book, and also on account of the interest of the subject. All those who are old enough to have witnessed much of the practice of lithotomy when it was the only means of treating stone in the bladder, agree in stating that it was almost uniformly fatal in elderly people with enlarged prostate. Even in our own limited experience we have witnessed most gratifying instances of the removal of stone by lithrotity in such patients, and it is encouraging to hear on such authority as Sir H. Thompson's that such cases are not exceptional. There are few more real advances in modern surgery than the application of lithotrity as the usual method of treating stone in the adult.

Art. IX.—*Germinal Matter and the Contact Theory: an Essay on the Morbid Poisons, their Nature, Sources, Effects, Migrations, and the means of limiting their noxious Agency.* By James Morris, M.D. London. Second edition. 1867. Pp. 111.

Since we last noticed Dr. Morris's little book he has added considerably to its size, and has adduced numerous instances more or less favorable to his views. His argument is that solid particles may be suspended for some time in the air, and be borne along to considerable distances by currents in it; that minute portions of organic matter are constantly thrown off by animals and men; and that these floating particles are received into the body, some passing into the lungs, so as to reach the blood. Accepting Dr. Beale's nomenclature of germinal matter and formed material, as expressing the two opposite conditions of living active matter and dead inactive material, he proceeds to show how in many instances, if not in all, morbid action is to be regarded as the result of the contact of the affected organ on tissue with germinal matter in a similarly unhealthy state. The cases which are favorable to Dr. Morris's theory are, of course, those in which infection is admitted by all to take place through the medium of the air, as in typhus, diphtheria, smallpox, scarlet fever, and measles; but we are less able to understand why no case of syphilis should ever occur through particles of syphilitic germinal matter floating in the air developing in some favorable spot for their growth and development. Considering the frequency of the disease, and the remarkable vitality which its germinal matter possesses, as indicated by the difficulty with which it is eradicated from a system it has once contaminated, we should yet doubt whether Dr. Morris would admit the validity of his own argument, were a patient with a well-marked chancre to urge that it had arisen from a few floating syphilitic particles which had unfortunately alighted on a pimple. Yet why should this not be so, at least in rare instances, since every one would allow that the direct contact of that same principle with a truly infected surface would almost infallibly induce the disease. However, Dr. Morris's work, notwithstanding that a good many difficulties stand in the way of an unreserved acceptation of his views, is both a thoughtful and suggestive one. It is deficient in positive data.

Art. X.—*Irritability: Popular and Practical Sketches of common Morbid States and Conditions bordering on Disease, with Hints for Management, Alleviation, and Cure.* By James Morris, M.D. London. 1868. Pp. 114.

"Irritability," which, from the ever increasing hurry and bustle of modern life, is constantly on the increase, is, perhaps, one of the few medical subjects that may fairly be discussed in a popular manner. Certainly it is here treated in all its aspects discursively, and, we may add, pleasantly enough—facts or arguments or similes being drawn from the movements of the sensitive plant, the case of Nicolai, the bookseller, a storm on the Alps, the Atlantic Telegraph, the terror of Job at beholding a spirit, and we know not how many others.

We commend the chapter on remedial influences to the perusal of the present and the ex-Chancellor of the Exchequer, though we fear that neither of them would be able to carry out Dr. Morris's recommendation that before attempting to sleep "the last occupation should be as little exciting as possible—anything that requires no thought. If they have been much excited, perhaps there is nothing better than a stroll, with a cigar or without it, under the starry sky, when—

> "'From the cool cisterns of the midnight air
> The spirit drinks repose.'"

Many a man could cure his own "irritability" without professional advice, if he had only time and means at his command, to take amusement or cultivate his love of nature.

Art. XI.—*On the Constituents of Food and their relation to Muscular Work and Animal Heat.* By F. C. Donders. Translated from the 'Nederlandsch Archief,' by W. D. Moore, Esq., M.D. Pamphlet, pp. 415. Reprinted from the 'Dub. Quart. Journ. of Med. Science,' 1866.

In this interesting essay a very admirable summary is given by Donders, of his own and of various recent investigations on the difficult subject of the correlation of physical and vital force, of the relations which exist between the food consumed and the work done, as well as the heat eliminated by the body. He shows that heat can no longer be regarded as developed in the lungs, but that, as proved by Sachs, it is partly directly formed by the combination of oxygen with carbon and hydrogen in the blood, and is partly indirectly produced by electro-motor actions occurring in all parts of the nervous and muscular systems, as well as by transformation of mechanical work in the latter. In this respect, he instances the heart which produces, in twenty-four hours, a work of about 86,000 kilogrammeters, which wholly changed into heat can warm fully 200 kilogrammes by 1°C., and, therefore, all the parts of the body by 3°C. (5°·4 F.). So also, he observes, the movements of respiration and all transitory muscular action must produce heat, not only through the friction which the particles of the muscles exerted mutually undergo, but also owing to the increased activity in the circulatory and respiratory processes to which such exertion gives rise. He then shows what are the changes occurring in a muscle during mechanical work—how this is divided into *statical* when no apparent effect is produced, as in pulling against an insuperable weight; and *dynamical* when movement occurs, and what is the relation existing between the production of heat and of mechanical work. The remainder of the work is chiefly occupied with the consideration of the effect of exertion on the disintegration of the albuminous constituents of muscular tissue; and he introduces a most instructive communication from Dr. Verloren, on the extraordinary muscular energy exerted by insects, and especially by bees, at a period of their existence when their food consists almost exclusively of farinaceous and saccharine material, and which is considered by Dr. Verloren to prove that muscular work is attended with no considerable metamorphosis of albuminous matter. Prof. Donders remarks, however, that the period of the year when bees consume their stored-up honey is in winter, precisely at that period when they are most quiescent; and he states he is somewhat sceptical as to the completely non-nitrogenous nature of the nectar of flowers consumed by bees and butterflies.

The general conclusions at which Prof. Donders arrives are well worthy of earnest consideration, and are opposed to the hasty adoption of the view that much muscular work can be performed on a farinaceous or oleaginous diet. On the whole, he observes, "We come to the conclusion that animals to perform work *constantly* use, not only a larger quantity of food, and, therefore, proportionately more albuminous matters, but that they further require for their labour a more highly albuminous diet. In this way the body attains a definite stationary condition, and that food is thus consumed and used. Consequently, in the excreted matters, the same quantity of nitrogen must occur as in the ingesta. The nullity of the argument derived from the slightness of the increase of the urea excreted during the performance of work is thus made apparent."

On Non-nitrogenized Food in a Physiological point of View. By Messrs. Sella and Stephens. 1867. Pp. 26.

This is a defence of some remarks made in a previously published work, entitled, 'Physiology at the Farm,' against a critical review of their book contained in the 'Field,' accusing them of valuing foods for cattle in proportion to the amount of nitrogenous food they contain. We have not had an opportunity of referring to the original work; but to their reply there is certainly nothing to object, and we fully concur in their opinion, that the speedy death of an animal by starvation, if fed exclusively on non-nitrogenized food, is to some extent incompatible with the recent views of Frankland and others, that the combustion of non-nitrogenized aliment within the body is sufficient to produce that energy on which muscular contraction is dependent; and the same view is most forcibly maintained by Professor Donders in the pamphlet we have just noticed.

On the Function of the Blood in Muscular Work. By C. W. Heaton, F.C.S., Lecturer on Chemistry to Charing Cross Hospital Medical School. Pamphlet reprinted from the 'Philosophical Magazine' for May, 1867. Pp. 6.

The above essay is written in support of the view originally propounded by Mayer, that all the oxidation takes place in the blood, and in opposition to that of Liebig and other later writers, to the effect that it takes place in the tissue of the muscles; and evidence is adduced that the quantity of oxygen which can possibly exude through the walls of the vessels, supposing it to be in solution in the liquor sanguinis, and to be applied to the oxidation of fat or muscle, will not account for one sixth of the work done by the muscles. Mr. Heaton is, therefore, of opinion that all, or nearly all, the force of the body is generated in the blood; and that Mayer was perfectly right in saying "that the muscle produces mechanical effect at the expense of the chemical action expended in its capillaries."

On the Formation of so-called Cells in Animal Bodies. By Edmund Montgomery, M.D., late Demonstrator of Anatomy at St. Thomas's Hospital. London, 1867. Pp. 56.

In this pamphlet Dr. Montgomery has summarised his observations on the mode in which cells, or rather cell-like bodies, may be formed by the imbibition of water from viscid materials extensively distributed through the animal body. His results are interesting as showing how closely it is possible to imitate certain organic forms, and how requisite it is to exercise caution before giving a definite opinion upon the nature of cell structures; but we must altogether dissent from the view that a true cell can thus be produced, since these "counterfeit" cells, however precisely they may resemble true cells at certain periods of their growth, lack their essential characteristics of capability of further development and of reproduction, and are as different in their attributes as are the nerve-like myelin tubes from true nerves.

Art. XII.—*Medical Diagnosis with Special Reference to Practical Medicine: a Guide to the Knowledge and Discrimination of Diseases.* By J. M. Da Costa, M.D., &c. Second edition. Revised. Philadelphia, 1866. Pp. 784.

The uncertainty which has been the opprobrium of medicine since its birth is, we are tempted to congratulate ourselves, becoming less and less an integral part of our science. We look back to the times when it entered into pathology, diagnosis, and prognosis, to an extent which although it may have whetted curiosity and exercised imagination, must have prevented any real feeling of satisfaction being derived from their study. "I am weary of guessing," were the sad words in which the foremost physician of an age not very remote from our own summed up the account of a long professional life. It is true that, in practically the most important department of medicine, we have, perhaps, the fewest landmarks to guide us. We are not agreed as to the degree or manner in which morbid processes may be affected by remedies, and even the question as to the best mode of combating acute inflammations cannot be said to be finally set at rest. But even in therapeutics we have advanced some little way on the road to certainty. Our want of agreement does not depend upon doubt as to the physiological effect of the remedies we employ. The absurd poly-pharmacy of 200 years ago is no longer required as a veil for medical ignorance. We are no longer in doubt as to the action of ordinary drugs. Our uncertainties in therapeutics are probably due, to a great extent, to the theory we unconsciously adopt that the same disease ought to require and yield to the same treatment, at all times and in all persons. Of course this fallacy, which is a very common one, has been more or less protested against by the greatest masters in physic. But if we at once discarded it a large amount of the so-called uncertainties in therapeutics would disappear, whilst experience and skill in choosing and applying a mode of treatment to an individual case would again be thought worth striving to obtain.

In no department has the change from uncertainty to certainty been so marked and decided as in diagnosis. Compared with one hundred years ago, daylight has succeeded the dimmest twilight. Of course the advance strikes the student most in reference to lung, heart, and kidney diseases; but it is true, although in a less degree, of diseases of the nervous system, and of the digestive and assimilative organs. Even in reference to that class of acute diseases which our forefathers saw more frequently than we do—the so-called zymotic class—we have established differences between eruptive and continued fevers which were unknown to the acute physicians of the last century. On the whole we believe we are justified in asserting that the progress of medical diagnosis will bear comparison with that of most other sciences, even in this age of progress.

In the work before us we have undoubtedly a fair and well-laboured view of the present state of the art and science by which disease is discriminated. The author has taken care to bring together all the latest helps and instruments by which information can be gained, and he has given a full and clear description of them. But much more than this, he takes diseases seriatim, according to a simple arrangement, and one by one he gives a full account of their symptoms, physical signs, and differential diagnosis. In almost every instance the information is brought up to the latest date. The utility of such a book is obvious. Diagnosis is, in its very nature, more susceptible of being reduced to rule than prognosis, or therapeutics. The two latter are influenced by peculiarities of idiosyncrasy, constitution, and outward circumstances which may baffle the most sagacious. Diagnosis is far less under such influences; it can be made in many instances with the certainty of exact science, and in every case it deals far more with the objective and less with the subjective, than do the other branches of medicine. It is for this reason that it is fitting that it should be treated separately. It is more capable of being systematised; and something of its principles may be learned from books before they are practised at the bedside with more certainty and success than the other parts of the physician's art. In its present condition it comprises a very large portion of the current absolute knowledge which the student, whilst a student, can hope to acquire.

The present edition of Dr. Da Costa's work follows in quick succession the first. It is enlarged by about ninety pages of letter-press, and it contains twenty-two new woodcuts. The chief additions, the author states in his preface, will be found in the chapters on diseases of the brain, of the larynx, of the blood, on the urine, and on parasites, and in the section on abdominal enlargement. It is very difficult in a brief notice to do justice to a book which contains a large part of all that is known in practical medicine. Truly, it is for the most part only a compilation, but it is a compilation bearing the impress of an accurate student, an observant physician, and a polished writer. The larger portion of the work, excellent as it is, of necessity does not present much that is new to the well-read English practitioner. The sections on physical diagnosis, whether of cardiac, pulmonary, or abdominal diseases, on the urine and kidney diseases, and on diseases of the nervous system present little to need remark, except the fact that they are very good, and represent fairly the teaching of the present day.

We are unwilling, however, to part with Dr. Da Costa so abruptly, and therefore select a chapter from which to give the reader an idea of the scope of the author's information, and of the style in which he conveys it. In the chapter on diseases of the blood, are contained short but not scanty accounts of the conditions known as anæmia, leucocythæmia, pyæmia, septæmia, thrombosis and embolism, scurvy and purpura. The observations on the diagnosis of thrombosis and embolism are a compendium of our knowledge of the subject. After defining thrombosis as the whole process of the formation of clots in vessels or heart, and embolism as the projection onward of the thrombus, or of fragments detached from it, and the phenomena thus occasioned, the author proceeds to review the manifestations by which the presence of thrombi in different situations in the veins and arteries may be diagnosed. In treating of venous embolism he selects phlegmasia dolens, on the supposition that that affection really depends upon coagulum in the venous circulation of the limb, as presenting an example of most of the conditions which result from considerable venous occlusion. In addition, however, he notices that hæmorrhage into the surrounding tissues as in a case recorded by Virchow may result from the stoppage of a vein. From the peripheral or other part of the venous system he follows the transport of the clot into various organs and to the right side of the heart. From the description of the phenomena which accompany embolism of the pulmonary artery, we extract the following as a specimen of the author's manner.

"But the mode of death, and the symptoms preceding it in embolism of the pulmonary artery, are not always the same, and depend very much upon the size of the embolus, and where it is arrested. A large-sized clot, whether it be merely a portion of one occupying the right heart, or be washed at once into the pulmonary artery, will occasion much the same signs as those alluded to as indicative of a large clot in the right side of the heart; the craving for air is particularly intense, and this craving is increased by every movement of the body; the muscular debility, the lowered temperature, the cyanosed look, the turgid veins of the neck and their undulations, the increased, irregular cardiac impulse, though the heart's action is not sufficiently disturbed to account for the disturbed respiration, and disordered general circulation, are also noticed; and, in some cases, a systolic blowing sound, and where the case is at all protracted vertigo, albuminuria, and œdema of the limbs may observable. The intellect is always apt to remain clear. As regards the pulmonary phenomena proper, collapse of the lung, hæmorrhagic effusions, œdema, or capillary bronchitis, are likely to happen, excepting in those instances in which the principal trunks of the pulmonary artery are blocked up, and almost instantaneous asphyxia ensues. If the fragments be very small, the amount of dyspnœa is not of necessity great, nor are the symptoms of asphyxia marked; and inflammations of the parenchyma of the lungs may take place, occasioning often, secondary obstructions and metastatic abscesses in the lungs, from which recovery even possibly may take place. These kind of metastatic abscesses are observable in pyæmia, and are not unusual in puerperal fever" (p. 639).

The account of arterial embolism and its effects on different organs will scarcely bear condensing. Dr. Da Costa especially insists upon the value of pain as a symptom of internal embolism, when taken in connection with the history of the case, the state of the cardiac symptoms, and disordered function of the organ to which the pain is referred.

"The presence of emboli in the splenic, renal, and mesenteric arteries is generally rather to be inferred from the history of the csse, and does not occasion any obvious discernible signs. But tenderness, enlargement of the spleen, and pain in the splenic region in splenic embolism, or disordered secretion of urine and pain in the loins in embolism of the renal artery may be quite marked. The occurrence of pain in these cases of internal embolism must not be overlooked; and in embolism of the arteries of the extremities pain is a symptom of as great or even greater prominence. It may be like a violent neuralgia, or so constant that it is mistaken for rheumatism; and, as happened in a case of embolism of the right iliac artery, under the care of Dr. Hutchinson, and which I saw, it may recur in paroxysms of intense severity, and be referred to the foot, though this be already in a condition of sphacelus" (p. 641.)

In treating of the etiology of embolism as subservient to its diagnosis, Dr. Da Costa remarks upon the effect of malarial fever in giving rise to the accumulations of pigment in the blood, and thus originating a form of capillary embolism which may be the cause of capillary abscesses, or disturbances of the hepatic circulation. Such a pathological sequence can, of course, only be a matter of conjecture based on a close study of the general phenomena and history of a case.

In conclusion we can only say that Dr. Da Costa's book seems to us a good text-book for students, and a good book of reference for practitioners. It is clearly, but not tamely written, in a style which elicits and fixes attention.

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. Second Edition. London, 1868. Pp. 152.

THIS little book is a re-issue of some very interesting and practical lectures, delivered by Mr. H. Smith, on a subject of which he has made a kind of "speciality." We noticed the first edition shortly after its appearance, and need say very little more about the second edition, inasmuch as there seems little in it beyond what appeared in the first. One thing, however, is very important, which is just that there *is* nothing new in this edition. It will be remembered that the main object of Mr. Smith's publication was to recommend a method of operating for piles, not of his own invention, but of Irish origin, and introduced into English practice by Mr. H. Lee. This consists in catching the base of the pile firmly in a clamp, cutting it off, and searing the base with nitric acid or the actual cautery. The advantages claimed for its method are its greater expedition —the slighter annoyance and confinement required in the after treatment, and its supposed immunity from danger of pyæmia, and other secondary complications.

The following is what Mr. Smith's extended experience enables him to say about the new mode of operating:

"Since this was written I have had a very large experience of the use of the clamp in the most severe forms of these diseases. Only thirty-five cases were recorded in the first edition, but I have now operated altogether upon one hundred and twenty cases without any fatality, or even any annoying result whatever. I am so thoroughly conscious of the safety and value of this plan of treatment, that since the last twelve months I have entirely discarded the use of the ligature; and I am glad to say that a large number of surgeons in England and in the colonies are adopting the means of treatment which I so strongly recommend, and against which not one

single objection of any weight has been or can be brought forward."
—*Preface to Second Edition.*

We must leave the respective merits of the new and old methods to the judgment of the profession. Mr. H. Smith's experience, though large, is as yet obviously insufficient to prove more than that the method is one which may be justifiably used. To say that because 120 operations for piles have gone off without a case of pyæmia or tetanus, therefore the method of operating is free from any risk of such an event is obviously premature, since the same thing often enough occurs after the ligature. But to say that 120 cases have been operated on in this way without after-bleeding is certainly sufficient to show that if the cautery be unsparingly used, the method is tolerably safe from risk of hæmorrhage. There ought, however, to be no mistake about thoroughly stanching all the bleeding points, and for this purpose Mr. Smith's clamp is a very convenient one. The operation is not nearly so painful as it looks; and if the patient is timid chloroform can be given. We can testify to the efficiency of Mr. Smith's method, even in severe cases; but we give no opinion as to its superiority.

Art. XIV.—*On Spinal Weakness and Spinal Curvatures.* By W. J. Little, M.D. London. 1868. Pp. 121.

This little treatise on the various curvatures and other unnatural states of the spinal column is well worth the reader's attention, since it embodies the results of many years of practical observation by one who is admitted to be amongst the best authorities on the subject which he treats. We shall be excused from following Dr. Little into all the matters spoken of in this work, but shall attempt to summarise briefly his views with regard to lateral curvature.

Before commencing his subject, Dr. Little makes one remark which should be tested by future observation.

"Many facts have come to my knowledge which tend to show the existence of a more intimate pathological connection between the spinal cord itself and lateral curvature than has been hitherto pointed out . . . . I cannot avoid entertaining the conviction that as pathologists we are accustomed to study the diseases of the osseous framework of the spine as instances of one class of disease, and the diseases of the spinal cord itself as instances of another class of disease, instead of studying them, or looking upon them as jointly dependent upon a common cause" (p. 25).

This is a suggestive observation, and if supported by subsequent experience, will lead possibly to a more accurate theory of spinal curvature. The explanations in common acceptance appear as a rule to be too rigidly mechanical.

The predisposing causes of lateral curvature (rotatory or rotato-lateral curvature as Dr. Little prefers to designate it) are placed by the author chiefly in the unnatural confinement and want of exercise which are enforced by the school education of girls in the upper classes, other causes of constitutional debility, and some special causes of local deformity being, of course enumerated. In this particular Dr. Little is in agreement with most other writers on the subject. In considering the exciting causes, Dr. Little is certainly to our mind less satisfactory than in the other parts of his treatise. He has already laid stress upon the fact that the curve is not merely lateral, but is due to rotation also, that this rotation can only be produced by the action of muscles; that the spinal column is, as it were, passive in the process (in fact, as Dr. Little forcibly puts it, "the affection might with as much propriety be designated bulging of the right or left shoulder, chest, or hip"); and yet that the predisposing causes are all of them those productive of muscular weakness. Again, he has hinted in the passage above quoted at some probable central origin of irritation in the spinal cord in some cases of lateral curvature. Yet when we come to inquire by what mechanism our author supposes the change of shape of the column to be produced, we are met with the unsatisfactory statement that

"The exciting causes of lateral curvature need occupy less space. It is more difficult to define with certainty what these causes are. The predisposing causes already enumerated may indeed suffice, by the intensity of duration of their action, to produce deformity. Thus, I have indicated that simple debility of muscles of the spine, laxity of ligaments from their imperfect nutrition and exercise, probably also a diminished firmness of the osseous tissue itself, enfeeble the spinal column as an active and passive supporting pillar of the frame, render it incapable of sustaining itself erect, and cause it to swerve from the perpendicular" (p. 32).

Surely all this ignores the very observation with which Dr. Little has stated that the deformity is not simply lateral yielding, but *rotation* of the column; which cannot be produced by mere muscular weakness, but must be due to muscular action. The same observation applies to Dr. Little's explanation of the usual direction of the dorsal curve to the right, viz. that it is caused by the weight of the liver, an explanation which seems to us improbable in itself, and utterly inadequate to explain the phenomena. Dr. Little's volume was printed in all probability before he had had an opportunity of reading Mr. Barwell's ingenious papers in the 'Lancet' (Oct. to Dec., 1867). Had he done so, we can hardly think that he would have been satisfied with this meagre and inconclusive treatment of the theoretical part of the subject of lateral or rotatory curvature. In one respect Dr. Little utterly differs from Mr. Barwell, for he teaches that "in the great majority of instances, if not always, the lumbar curve precedes the dorsal one" (p. 33), whereas it is the essence of Mr. Barwell's theory that in ordinary cases the dorsal curve precedes and excites the lumbar, and that it is caused by the action of the serratus magnus on the ribs, and that this traction is exercised towards the right side in consequence of the greater weight of the right arm and the greater volume of the right lung. Whatever weak points there may be in this theory it does at any rate attempt to grapple with the facts, while Dr. Little passes them over.

It is in the practical or therapeutical portion of the work that its chief value seems to us to lie. Here the reader will find an abundance of information, showing the uselessness and worse than uselessness of the cumbrous spinal machines so often employed; the great benefit that may be derived from properly devised exercises and carefully regulated posture, and the best kind of spinal supports in such cases as demand some instrumental assistance. Dr. Little, however, urges upon his reader that such mechanical devices must rather be regarded in the light of supports than of instruments whereby the spine can be mechanically straightened, and we do not doubt that all experienced surgeons would agree with him.

Especially interesting is the chapter in which Dr. Little speaks of the success which he has obtained in the treatment of the simple latural curvature which follows on the deformity of the chest produced by pleurisy.

The treatment which he recommends in these cases consists in swinging the body for several hours daily upon a broad band passing below the sound side of the chest, the head and feet being just supported on the couch. Thus "the weight of the head and shoulders above and of the pelvis and lower extremities below, powerfully, but gently and painlessly, drew asunder the contracted ribs, and replaced the vertebræ" (p. 79). Dr. Little gives three instances of success in the treatment of this deformity in early life; and as it is one which is ordinarily regarded as unavoidable, the suggestion deserves extended recognition and practical trial.

We heartily commend the book to the careful study of practical surgeons.

ART. XV.—*First Principles of Modern Chemistry.* By N. J KAY-SHUTTLEWORTH. London, 1868. Pp. viii, 214.

THIS book is at once simple and philosophical. The introductory considerations, the chapters on heat—specific, latent, and radiant—the notes on ebullition and evaporation, on the barometer and thermometer, and on specific gravity, are all concise and clear. A few only of the non-metallic elements are discussed in this volume, but these elements—hydrogen, chlorine, oxygen, sulphur, nitrogen, and carbon are the most abundant and the most important.

The author adopts most of the opinions of advanced chemists, but he shows a tenderness for the difficulties of the young student, which renders his book easy to read and understand. He popularises and explains the special chemical notation adopted by Dr. Frankland, and his brief, interesting, and clearly written chapters afford an attractive yet serviceable introduction to the completer and more usual text-books of chemical science.

ART. XVI.—*Reflex Paralysis: Its Pathological Anatomy, and Relation to the Sympathetic Nervous System.* By M. G. ECHEVERRIA, M.D. New York. Pp. 80.

IT was our purpose to have included a notice of this work in an article upon the general subject of paralysis; but as circumstances prevent the appearance of such an article for the present we take this opportunity of making our readers acquainted with the nature of the contents of the work. The author was formerly assistant-physician to the National Hospital for the Paralysed and Epileptic in London, and the essay itself (divided into two chapters) is, for the most part, a reprint of observations communicated to the 'New York Medical Journal,' and the 'American Medical Times.' In the first chapter, Dr. Echeverria defines reflex paralysis, and gives its etiology and pathological anatomy. Regarding the causes from which reflex paralysis arises, he describes them as being,

> "Exhaustion of the central nervous incitability—general affections, and a contaminated state of the blood—disturbed nutrition by conditions other than the above—cold, wet, and atmospheric influences, although these latter more properly belong to those causes which act on the blood-lesion of the peripheral nervous system."

Under the term reflex paralysis he ranges those varieties termed functional, idiopathic, asthenic, and peripheral, guarding against the assumption that the name in any way implies

"Contraction in the blood-vessels of the spinal cord as a necessary initial cause of the paralysis, or subsequent absence of structural change in the nervous system as its effect."

The observations in this chapter are copiously supported by quotations and cases culled from the writings of others, as well as by several original cases, and have for their aim the

"Pointing out of the histological changes undergone by the nervous system in reflex paralysis and the facts indicating its relations to the sympathetic system."

This chapter is also illustrated by plates heliographed by Baron Egloffstein's process.

In the second chapter, besides the influence of the sympathetic system on the production of reflex paralysis, the inordinate sensibility, the changes undergone by the muscles, and the condition of the urine peculiar to the disease are considered. The writer shows his reasons, gathered from the recital of pathological facts as well as from physiological and anatomical considerations, for looking upon reflex paralysis as being induced through the agency of the sympathetic, and dwells on the fact that a material alteration in the structure of the spinal cord and ganglia, as well as in the nerves and muscles in fatal cases, has been detected. He believes

"That there are cases such as those of progressive locomoter ataxy, progressive muscular atrophy, &c., which will probably be included in the same class of paralysis due to original lesions of the sympathetic. This classification will be possible when we become more familiar with their etiology, and the morbid changes they occasion in the sympathetic system."

Dr. Echeverria combats the idea that sensibility of the skin is not to any degree impaired in reflex paralysis, a symptom much regarded by Dr. Brown-Séquard as specially characterising myelitis; and insists on the existence of derangement of sensibility in the affection, forasmuch as he looks upon the fact as having an important bearing in relation to the consideration of the part which the sympathetic takes in the production of the paralysis.

The work is one of much interest, as well from the apposite collection of cases and facts in reference to the views of the author, as from the practical hints and suggestions which it contains; and shows what good use Dr. Echeverria has made of the opportunities which he has enjoyed of studying affections of the nervous system.

PART THIRD.

Original Communications.

Art. I.

Notes on the Geographical Diffusion of Epidemic Cholera in 1866 *and* 1867. By Gavin Milroy, M.D., F.R.C.P., Vice-President of the Epidemiological Society.

In the number of this review for last January, I gave a sketch of the spread of epidemic cholera from the shores of the Red Sea over a large portion of Europe, and to two points in the New World, viz., the harbour of New York and the French West India island of Guadaloupe. For the materials I was mainly indebted to Mr. Radcliffe's very able summary, contained in the last report of the medical officer of the Privy Council, of the official intelligence which had been transmitted by our consuls to the Government, in compliance with the recommendation of the Epidemiological Society to the Foreign Office. It is gratifying to know that this source of valuable information has continued to be available ever since, and that, in due time, the profession will again have the opportunity of judging how much may be done by this country in promoting the science of epidemiology by means of the wide-spread machinery of her colonies and consulates, scattered as these are over every region of the globe. It requires but an organised system of continuous record of this kind, based on authentic data furnished by medical men resident in each locality, to lay the foundation of a trustworthy geographical and chronological registration of many epidemic diseases, so indispensable to the accurate knowledge of the natural history and of the physical relations of these maladies, as well as to the elucidation of some important problems in State medicine. And here I would, in passing, allude to the singularly ingenious speculations recently put forth by Inspector-General Lawson in reference to the development and cosmical progress of certain epidemics, especially of fever and cholera—speculations worked out with extraordinary industry over many years of patient research, and animated throughout by an earnest spirit of elaborative induction. Whatever may be the fortune of Dr. Lawson's hypothesis as to the connection of pandemic waves of disease with terrestrial magnetism, his researches will not fail to have contributed to the advance of epidemiological science, were it only by having directed the attention of medical men to the necessity for far more comprehensive and more continuous investigations than they have hitherto been willing to undertake.[1] The greatest difficulty he encountered in his labours has arisen from the want of sufficiently numerous and authentic data over wide areas of the earth's surface, and during many successive years. Until this defect be supplied, no real advance in epidemiology as a branch of true scientific inquiry can be looked for. It is in the hope of exciting more general interest in the profession in respect of geographical and topographical details relating to epidemic cholera, that I now venture to bring before my brethren the following memoranda, meagre and imperfect as they often must be, seeing that they have been gathered chiefly from the journals of public intelligence, unaided by the more ample and authentic instruction to be obtained from the channels of official information.

In the early part of 1866 the pestilence existed, in a more or less developed or active state, in many parts of the European continent, from the western provinces of Russia to the Atlantic shores of France and Spain. But, from the want of trustworthy data, it is impossible to denote the separate districts, or even regions, far less the distinct localities, where the disease had already manifested its development. All that can be said, as far as I am aware, is that it was then present in various parts of Southern Russia, in the direction from the Baltic to St. Petersburg, in several places in Saxony, on the borders of Bavaria with Saxony, and some parts of Rhenish Prussia and northern Rhineland, as well as in Belgium and Holland. Beyond this very meagre statement, little or nothing can be affirmed with confidence respecting the *habitats* of the dormant pestilence throughout Central Europe at the beginning, and in the first month or two, of 1866.

The disease still lingered in Paris, where it had proved so destructive during the previous autumn, and also in several of the north-west districts of France, about Brest, Caen, and other places in the departments of Finisterre, Morbihan, and Cote du Nord. Whether there had been any spread of the malady from the districts in the department of the Vosges, which were infected in 1865, and respecting the origin of which no account has, I believe, been published, seems to be quite uncertain. In Luxembourg, close to the north-east frontiers of France and the adjoining district of Rhenish Prussia, it early began to manifest itself with virulence. In Spain

[1] Dr. Lawson's papers will be found in the 'Reports of the Army Medical Department' for 1861, 1864, and for the present year. See also the 'Transactions of the Epidemiological Society,' vol. ii.

there is reason to believe, notwithstanding the absence of all direct information from Spaniards themselves, that the disease existed, throughout the whole winter, in numerous parts of the peninsula, from Santander on the north to Seville in the south. I am not aware whether there were any manifestations of its presence in Portugal at the same time.

Since the cessation of the outbreak at Gibraltar in November, there had been no reappearance of the disease in that fortress and its environs. Malta, too, was healthy throughout the year, in consequence, many persons alleged, of the vigorous quarantine she maintained. A like immunity seems to have been experienced throughout the Mediterranean sea-board generally, except at some parts of the Italian coast, and probably also of Algeria. Neither in Egypt[1] nor in Turkey did the disease ever become epidemic; and Greece continued, as hitherto, to remain intact. Whether there was any recurrence of it during the winter in the Trieste or other adjoining districts of Austria, does not appear. A reliable history of the epidemic in that part of Europe is very much wanted; it could not fail to be highly instructive in many points of view, and to clear up much of the obscurity that hangs over its development and progress in some regions of the Continent.

Respecting the progress of the disease in the New World, very little can be said. It had, as yet, not extended beyond the quarantine station at New York; and Guadaloupe appears to have been the only place in the West Indies where the disease had appeared. But whether Cuba, where the disease subsequently prevailed as it had done on all previous visitations of epidemic cholera in that region of the world, was entirely exempt at this time, it is not possible to find out, from the notorious unwillingness of the Spanish authorities to publish any information respecting their own territories, although ever most vigilant in their espionage of disease in all foreign countries. Guadaloupe continued to be severely scourged during the early months of 1866. The adjacent British colony of Dominica remained healthy; and all that is known about the French island of Martinique, immediately to the south of Dominica, and which, notwithstanding its great commercial intercourse with Marseilles and other ports of France, had hitherto escaped, is that in April and May of the present year (1866) there was a good deal of

[1] In March, a rumour of an outbreak in Alexandria reached Malta, and forthwith all arrivals from Egypt were subjected to a quarantine of thirty days. In August, a few sporadic cases occurred in Alexandria; and again the same restrictions were imposed. At Mecca, there was a considerable mortality among the pilgrims and soldiery in May, but the disease did not spread beyond Jeddah. Sanitary precautions had been taken by order of the Turkish government. It was stated at the time that a good many cases had occurred also at Massowah on the opposite coast of the Red Sea.

choleraic diarrhœa or cholerine among the inhabitants of Port Royal.

To return to Europe. As was to be anticipated the disease began, as spring advanced, to manifest greater activity wherever it existed during the winter. It was then that it was brought over directly and, so to speak, palpably, to this country, by the landing of sick and infected persons on our shores from various parts of Holland and North Germany. The history of these occurrences and the consideration of their relation to the subsequent development and diffusion of the pestilence through Great Britain are of the highest interest to the epidemiological student; and fortunately he is now not left to mere vague rumours and unascertained statements in following out his inquiries, as he generally is when endeavouring to trace the course of the disease in any other part of the world. It will be remembered that the only places in England where the disease had manifested itself in 1865 were Southampton, and a solitary farm-house near Theydon-bois in Essex.

Notwithstanding the incessant and unobstructed intercourse between France and this country, it is remarkable that, among the many thousands of persons who came over from Paris during the prevalence of the epidemic there in 1865, one solitary instance only of a fatal attack occurred in London, and that was of a female who died in Frith Street, Soho, at the beginning of October. Very many, however, it may be reasonably presumed, must have had diarrhœa upon them on and after arrival. In 1866, cholera poison found its way into England by another channel; and this it did several weeks before its existence in the country was suspected. Streams of poor emigrants from North Germany and Holland had been brought over in March, if not earlier, from Hamburg and Rotterdam to Hull, and thence forwarded on by rail to Liverpool, for the purpose of being shipped over to America. No casualties of any sort to excite suspicion seem to have taken place, either at the port of landing or on the land journey to Liverpool, or yet in that city, among the strangers before their embarkation. The first intelligence of the serious risk that had been incurred was on the 20th of April, when the news arrived at Queenstown that a dreadful outburst of cholera had occurred in the emigrant ship "England," which had sailed from the Mersey on March 28th. The disease had manifested itself six days after leaving port; and so rapidly did it spread among the crowded mass of wretched humanity on board that, within another week, ninety-two souls had perished, and the ship put into Halifax on April 9th for additional medical assistance.[1]

[1] The incidents which occurred at Halifax are so very instructive as to demand special notice. The sick were removed to the "Pyramus" hulk, and the rest of the emigrants were camped out under canvas on an island opposite the southern suburb of the town, the distance across the harbour being about half a mile.

In another emigrant ship, the "Virginia," which sailed from Liverpool on April 4th, deaths from cholera occurred at sea eight days afterwards. When she reached New York, on the 22nd of that month, fifty of the passengers had died.

The earliest death from malignant cholera in England was at Bristol, on April 29th, in a sailor who had come from Rotterdam to London, and had passed on to Bristol. No other case occurred, either there or in the metropolis. Hull also remained quite intact. On May 2nd, two deaths occurred in Liverpool; both were in poor emigrants recently arrived from Rotterdam *viâ* Hull. On the same day the "Helvetia," a sister ship to the "England" and the "Virginia," sailed with 925 steerage passengers, chiefly foreigners, for America. She was to call at Queenstown for her full complement; but, before arriving there, two deaths had occurred on board. "The authorities at Cork refused her any aid, and denied her admittance into the harbour."[1] She was, therefore, obliged to return to

Many deaths occurred in the "England" and the "Pyramus," and a good many attacks, several fatal, in the camp. The epidemic declined in about ten days; the last case, very slight, was on 30th April. None of the thirty-seven saloon passengers had suffered at all; but of the crew, two sailors, one steward, and three firemen died. The history of the casualties among some of the inhabitants of Halifax is of the highest interest. The first attacked were the two pilots who brought the ship into harbour, but without ever, they declared, going on board of her; they remained in their boat all the time till she was moored. They lived at a fishing village about ten miles from Halifax. One (Terence) died of secondary fever on the 19th; the other (Purcell) recovered after several days illness. Of Terence's family, four of the children were attacked and two died. Three of Purcell's children sickened, one severely, the others slightly, and all recovered. Purcell's cottage was altogether very much better than Terence's.

No other cases occurred in the village, nor indeed anywhere else, except in a poor family living in a miserable cottage near the beach at the southern suburbs of Halifax. On the 22nd, one of the children, aged two, was attacked with cholera after having had diarrhœa for nearly a week; it died next day in the City Hospital, to which all the family were then removed. The mother was attacked on the 25th, and died on the 30th. It is stated that a quantity of bedding had been cast overboard from the "England," and had floated ashore near to where the cottage stood, and that the children may have handled it.

Besides the cases now enumerated, the only other casualty was the sad death of Dr. Slayter, the quarantine medical officer, who, after eight days of incessant labour among the sick, was attacked and quickly succumbed. "It was no wonder," he said, "that, in the dreadful state of the between-decks, the disease had been so terribly malignant in the 'England.' "

The above notes are from a valuable paper by Dr. Barrow, in the 'Army Medical Reports,' 1866.

[1] Dr. Trench indignantly protests, and most justly so, against this act of barbarity. "How far the selfish precautions of the officials of Cork were legal is a question well worthy the consideration of the Central Government; but there can be no hesitation in arraigning them as guilty of inhumanity and of a reckless disregard of the duties of charity and hospitality. Apart from the cruelty to the passengers and crew, some weight was due to the fact that, in the neighbourhood of Cork, there are open spaces and even islands, where the emigrants might have been safely lodged without danger to themselves or to the inhabitants of the town, until the ship had been cleansed, disinfected, and prepared again for sea." In 1854, the Cork authorities had been guilty of a similar heartless act. What a

Liverpool, which she reached on the 4th. All the emigrants were at once removed out of the sickly ship—the sick to a vessel set apart for the purpose, and the unattacked to another vessel, or to suitable quarters on shore. A good many cases occurred among the latter, and several proved fatal. Altogether, forty-seven deaths took place at Liverpool, the latest being on May 23rd. Among the victims was the surgeon of the "Helvetia." The vessel, after being thoroughly purified, again sailed with emigrants on May 29th, and reached New York without a single casualty.

In consequence of these events at Liverpool, Spain, Sicily, Malta, and Greece quarantined arrivals from all English ports, without exception.

While these events were taking place in England and on board vessels from Liverpool, the pestilence had been rapidly spreading on the continent. In May, it was widely diffused through North Germany from Pomerania to Rhineland, the movements of large masses of troops in Prussia at that time doubtless contributing to its development and diffusion. Stettin seems to have been the seat of the disease before Berlin was attacked; but how or whence that Baltic port became infected, has never, I believe, been explained.[1] Had the attention of the cholera Conference, which met at Weimar in the spring of 1867, and which consisted chiefly of delegates from all parts of Germany, been directed to the important questions of the topography and chronology of the manifestations of the pestilence throughout Central Europe in 1866 much very valuable information might certainly have been acquired.

Besides North Germany, Holland, Belgium, and some of the northern departments of France were then suffering severely; and the whole of the Italian peninsula from Brindisi to Brescia was, speaking generally, more or less under the pestiferous cloud. The wide-spread and destructive prevalence of the disease throughout most of Central Europe told terribly on the Prussian, Austrian, and Italian armies in the campaign of that summer. Nor were its ravages confined to the theatre of war; for, while it was in June

contrast the conduct of Halifax towards the "England" presented! On neither occasion, too, did Cork save herself from a visitation of the pestilence. Such a bad example is never without mischievous results elsewhere. At Kingstown, a poor sufferer was allowed to die in a cart in the open street, in consequence of being refused admission into any place for relief; and at Holyhead, the whole of the crew deserted a ship in which one of their comrades was attacked, and left him to die without any one near him.

[1] Dr. Goeden of Stettin stated at the Conference that the starting point of the disease in that town in 1866 was in some localities about the mouth of the Oder, and that it sprang up without any discoverable traces of importation from any infected place. In two previous visitations of epidemic cholera, the same thing had been observed; but, on some other occasions, the outbreak of the disease followed on its introduction *ab extra*. The earliest cases in 1866 occurred at the beginning of June; and, about the same time, the disease appeared at one or two points on the Pomeranian coast. Berlin was not infected for a week or two later.

raging at Amiens and other places in Western France, Petersburgh was at the very same time suffering severely. During the autumn, it existed in several parts in Hungary, Bosnia, Albania, and Moldavia. A partial outbreak occurred at Constantinople, in consequence, it was alleged, of importation from Salonica on the coast of Thessaly.[1]

On the other side of the Atlantic, too, the scourge had, about the beginning of summer, begun to appear in New York (the earliest cases occurred about the beginning of May in an extremely filthy locality of the city), notwithstanding the assiduous efforts of the authorities to prevent its extension from the distant quarantine station at the entrance of the harbour, and to which efforts its exemption in the previous year had been ascribed. The Western and South-western States appear to have become deeply infected before or about midsummer; and many of the cities in the interior of the union suffered greatly in the course of the subsequent season. But no attempt to record the progress and course of the pestilence in America has, as far as I am aware, been made. How far south it penetrated in the Southern States, is unknown to me. Towards the close of the year, it had appeared at Greytown, on the Caribbean shores of the isthmus of Panama; and the island of St. Thomas had become infected in November or December, if not sooner. Beyond these simple facts, nothing more can at present be said with confidence.

But we must leave the region of mere surmise and conjecture, and return to our own country to trace the history of the epidemic development in the United Kingdom, subsequently to the events at Liverpool in April and May. "No case of contagious cholera among the townspeople," remarks Dr. Trench in his admirable report, "apart from the emigrants, had been as yet (June 30) recorded. It is true that from May 28, when the 'Helvetia' finally left our shores, no week's registry of deaths, with the exception of the two weeks ending June 2nd and 23rd, had been without a case of cholera; yet, careful inquiry satisfied me either that there had been errors in diagnosis, or that the cases were of the bilious type, and so purely sporadic as neither to be due to contagion, nor to have spread the virus in the families or neighbourhood of the deceased."

[1] Sicily became the seat of a severe and long-continued outbreak in October. It is not possible, from the want of reliable evidence, to determine whether the disease was not in the island previous to the landing of troops from Naples, at the end of September, at Palermo. More than one of its seaports had been quarantined by other Mediterranean ports in the course of the summer; and the extreme rapidity with which the disease appeared, according to report, at Catamia and at other places far distant from Palermo, after the landing of the troops which were accused of having imported the pestilence, is not to be overlooked. Very speedily, nearly the whole of the island seems to have become infected.

That cases had occurred in the autumn in Rome also, there can be little doubt, although the disease did not become epidemic there till next year.

Dr. Trench dates the commencement of the epidemic outbreak in Liverpool from July 2, when the first of a large group of fatal attacks occurred in one of the worst Irish dens, at a distance from the foreign lodging-houses which had been the seat of disease in the spring. No trace of communication or connection with these former cases could be discovered. The epidemic lasted till nearly the end of November, and carried off 1792 victims. The disease "in its epidemic virulence was restricted to the lowest, dirtiest, and most squalid streets of the borough; so much so that, although many fatal cases occurred among respectable tradesmen and artisans, yet these were on the whole so few as to be considered exceptional." Dr. Trench adds that the dissemination of the disease was certainly not due to polluted water supply. Previous to the commencement of the outbreak in Liverpool, the epidemic had begun to manifest itself in the metropolis, and sporadic cases had also occurred in South Wales, in Cheshire, and West Yorkshire, and on board some foreign vessels at Sunderland and Shields.

The earliest undoubted cases in London are supposed to have occurred on June 26, in a labourer and his wife, at Bromley at the extreme eastern limits of the metropolis, in a house on the banks of the river Lea; but, prior to that date, suspicious deaths had taken place in different directions within the metropolitan area.[1] How and whence the morbific poison was brought, or brought itself, it is impossible even to conjecture. The persons first attacked had not recently arrived from, nor had had any discoverable communication with, any infected place or individual. It was not till July 8th that a death occurred at Bermondsey, in a man arrived the day before from Rotterdam (between which place as well as Hamburg and the port of London there had been uninterrupted communication); and the earliest known fatal attack among vessels on the river was not till July 17th, on board a barge at Deptford. "The outbreak in the metropolis," remarks Mr. Radcliffe, "was one of a succession of phenomena which indicated a wide-spread diffusion of cholera infection in the kingdom during June."

The development and degree of prevalence of the disease in different districts of England, during the summer and autumn months of 1866, present many puzzling questions to the inquirer, whether he looks at the late epidemic merely by itself, or compares its topographical history with that of previous epidemic visitations. We seem to be as yet quite in the dark, when we seek to account for the erratic or migratory movements of its course. Human intercourse alone or communication with infected places—whether *internally*

[1] "The mortality returns of the metropolis show that the first deaths from Asiatic cholera in 1866, occurred at Walworth, on May 28th. On June 13th, another such death was recorded at Newington; on June 27th, two deaths at Bromley."—Dr. Letheby, 'Annual Report for 1866-67.'

between different parts of England, or *externally* with foreign countries—does not suffice to explain the irregular and capricious extension or dissemination of the poison. The suburbs of London, and the contiguous districts, were but little affected during the prevalence of the cholera in the metropolis. Manchester remained comparatively intact, while Liverpool suffered so sharply. The "black country" in Staffordshire, which in former visitations was one of the chief seats of the epidemic, nearly escaped, while various places in the coal district of South Wales were severely smitten. Nothing was, perhaps, more remarkable than the comparative mildness of the morbific agency along the whole of the eastern coast of England, notwithstanding the great intercourse there was all the while with the infected ports of North Germany and Holland, and the frequent occurrence of cases of the disease in vessels arriving therefrom. And the fact is the more notable as other parts of the coast, apparently far less exposed to the risk of infection, suffered with unusual severity. While the deaths from cholera, during the quarter ending September 30th, amounted in Hull to only 12, in Sunderland (including Bishopwearmouth and Monkwearmouth) to 26, and in Newcastle and Gateshead to only 8; the deaths in Southampton,[1] in Portsea Island, and in Totness, were respectively 98, 113, and

[1] The history of the *second* outbreak in Southampton deserves special notice. Since the cessation of the *first* visitation in the previous November, no case occurred in the town or suburbs, till the middle of June, 1866. During the spring it was reasonably apprehended that, in consequence of the number of steamers arriving from infected parts on the Continent, the disease would be introduced, especially as the emigrants were allowed to land and go about the town. "But there is no evidence," remarks Dr. Parkes, "that any of these vessels introduced the disease;" moreover, "no emigrant vessels leaving Southampton for America suffered from cholera on the voyage." The earliest reputed fatal cases occurred in the family of a fireman in the "Poonah," one of the Peninsular and Oriental steamers, which arrived from the Mediterranean on the 10th of June, having on the previous day lost one of the crew from cholera. The fireman had diarrhœa on him, when he landed and went straight home. His child, aged three, was suddenly attacked on the 13th, and died the same day. On the morrow the man became worse, and died on the 15th. No other cases in the house, or the immediate neighbourhood of it, occurred, although already several suspicious attacks had taken place in other parts of the town, unconnected with the "Poonah" cases. The epidemic may be said to have begun on July 6, when one death occurred; the next was on the 11th, and the third on the 12th. It lasted till the second week of October. "The outbreak was confined to the low and unhealthy parts of the town. . . . All the upper part of the town, and the suburbs and surrounding villages, remained free." It was "entirely unconnected with the drinking water."

The origin of the fatal case in the "Poonah" on June 9 remains a puzzle. On the voyage from Alexandria and Malta (both healthy at the time), all on board continued quite well till June 8, three days after leaving Gibraltar, where no case of cholera had occurred for full six months; there a supply of water was obtained. This water was found, on examination at Southampton, to contain an undue amount of organic matter; but its use on board had not been confined to the men who sickened. Altogether, this "Poonah" case is as enigmatical as the "Theydon-bois" case of the previous year.

92. The little village of Brixham in Torbay alone had 45 deaths from the disease, in that quarter. Again, whereas Ramsgate suffered severely, Folkstone, Harwich, and Yarmouth (which were in daily communication with the ports of Belgium and Holland) remained all but intact. A similar remark might be made in respect of whole counties. Devonshire was the seat of many fatal outbreaks, while Cornwall on one side, and Dorsetshire and Wilts on the other, were scarcely affected. And if we glance at the course of the disease, during this summer, in Scotland and Ireland, fresh evidence presents itself to show that there must be some occult agency or influence affecting its development and movements, besides the one of palpable and material transmission or communication. "It would be a very difficult thing," remarks Dr. Stark, "to say when cholera first appeared in Scotland in 1866. . . A few scattered cases occurred during May and June, but it was not till towards the close of July[1] that the occurrence of the disease in Scotland was brought under public notice. It then seemed to prevail chiefly in the sea-port towns on the eastern coast, and from thence gradually extended westwards and to the more interior parts of the country. The disease seemed frequently to be carried to these other places by persons who had fled from the towns first seized; but, in numerous instances, no such connection could be traced. The inhabitants of many of the sea-port towns were often very anxious to prove that the disease was imported; but, in very few instances indeed, could such importation be traced to communication with infected districts in England or on the continent. And other parts of Scotland, to which travellers from infected districts on the continent resorted, remained free from the disease." Of 33 counties, including the Orkney and Shetland islands, 15 only were affected. The total number of deaths from the disease to the end of the year, reported to the registrar-general, amounted to 915, and of this number 480 occurred in the eight principal towns as follows:—

Glasgow.	Edinburgh.	Dundee.	Aberdeen.	Paisley.	Greenock.	Leith.	Perth.
53	134	105	62	2	14	95	15

The great mildness of the disease in Glasgow—with its very large population and immense traffic and intercourse—as compared with

[1] One of the earliest undoubted cases was a solitary one in the Perth General Prison, which occurred in a man who had been imprisoned for eight years, and had not been out of the building. No course whatever could be discovered to account "for this remarkable incident," remarks Dr. Christison, who has recorded the case. The disease had not appeared in Perth at the time. What adds to the interest of this event is that no other case occurred in the prison till October 28, when a prisoner (who had been confined for nearly a twelvemonth), occupying the same dormitory as the former patient, was attacked and died. Ten other prisoners were in the dormitory at the time, but none of them, nor any other inmate of the prison, suffered. The epidemic was, however, in the town at the time.—'Edin. Med. Journal,' May, 1867.

Edinburgh and Leith, is a remarkable feature in the visitation of 1866. The stress of the epidemic was felt along the eastern coast from Fraserburgh (a fishing town in Aberdeenshire where forty-six deaths occurred) and the shores of the Firth of Forth. The extraordinarily fatal outbreak at one part of the Fifeshire coast among some collier villages, where more than 140 out of a population of less than 3000 died, while the towns and villages east and west of the district remained free, is one of the most notable events in the history of the late visitation in Scotland.

If we now pass over to Ireland we find many similar events relating to the spread of the disease, which it is equally difficult—if, indeed, it be possible—to explain. The information to be derived from the Irish Registrar-General's returns is much less complete than that which the Scottish returns afford. The epidemic appears to have manifested itself first in Dublin in the last week of July. The earliest fatal case occurred in a girl, who was attacked within a few hours after landing from a Liverpool steamer, and died the same night. This case was followed, during the next four days, by three other fatal attacks among the inmates of the same room which the first patient occupied. Thereupon, other cases began to occur in other unwholesome localities, and soon spread in different directions. The total deaths in Dublin and its suburbs, registered to the close of the year, amounted to 371. Several places within a few miles from the metropolis, and in different directions—as Swords, Cellbridge, Bray, &c.—suffered severely. Along the coast to the south, no town was so heavily smitten as Arklow, where 100 deaths occurred. Its sanitary condition is extremely bad. "The poor people allow pigs to sleep in their cabins at night." To the north, Balbriggan suffered much, while Drogheda nearly escaped; and Belfast, with its large population and busy intercourse, had but comparatively few deaths. The isolated outbreaks at Mallow in the centre of County Cork, and at Westport in county Mayo, during the quarter ending September 30th, are noteworthy. In one part of the town of Carlow seventy-one deaths occurred, and in Limerick there were seventy-seven, whilst Kilkenny lost only nine, and Londonderry appears to have remained scot free. A cholera map of the epidemic visitation in 1866, upon the plan followed out by Sir D. Corrigan in respect of that in 1849-50, would be very suggestive in many points of view.[1] It would, doubtless, show not only that the

[1] Sir D. Corrigan, in republishing his map in 1866, remarks, "It was naturally to be expected that if contagion, promoted by population, free intercourse, and the bringing of numbers together in commerce, trade and manufactures, were an element of much power in propagating the disease, the map would show that the greater number of towns attacked were in those parts of Ireland where trade, manufacturers, and frequent intercourse, brought multitudes together. But the contrary is shown by the map; for in the whole of Connaught, and a considerable portion of Munster (its western portion), not a town escaped; whilst in Ulster,

same areas or regions of the country were not proportionately affected to an equal degree in the two visitations, but also that the morbific cause was much less widely and extensively diffused, at least in virulence or force, in the late epidemic than it was in 1849. The same holds true in respect both of England and Scotland. A good deal may have been due, in respect of certain localities, to sanitary improvements which had been effected in the interval; but this will obviously not suffice to explain the general mitigation and limitation of the pestilence over the length and breadth of the land. In connection with this point, the exemption of Denmark and also of Sweden and Norway in the visitation of 1866 deserves to be noted here.

I now proceed to sketch the geographical history of the epidemic in the following year. It is worthy of notice that, in the spring and early summer of 1867, several severe outbreaks occurred in northern India, more particularly at Benares, in various places in the Punjaub, and in Peshawur, at the north-west corner of the peninsula. A month or two later, it was reported to be raging at Teheran and other localities in Persia, and also in Affghanistan; while several of the Hill stations on the Himalayas, as Murree and Subatoo, suffered about the same time.

Neither Russia, with the exception of some districts of Poland, nor Turkey (proper) appear to have suffered much throughout the year. A partial outbreak at Pera on the Golden Horn, in August, was attributed to importation from Salonica, on the coast of Thessaly. In the early summer, not only the Danubian province of Bulgaria, but also the provinces of Albania, of Montenegro,[1] and Herzogovina (the latter two provinces had been hitherto exempt, it was said), together with the adjacent Austrian provinces of Dalmatia and of Sclavonia, were the seats of the disease. A few cases occurred at Trieste about the same time.

Again, throughout the year, the entire Italian peninsula, from north to south, continued to be in many parts a hotbed of the pestilence. Calabria was still severely ravaged; and the Papal States, if they, indeed, escaped in 1866, were now the scene of most destructive outbreaks. Notwithstanding repeated official contradictions, unmistakeable cases—they were designated by the Pontifical authorities "diarrhœa accompanied with fever"—occurred in Rome in the early summer; as the season advanced, they became so much more numerous and fatal, both in the city and the suburbs, that it was in vain any longer to deny the existence of the disease. In August occurred the terrible outburst at Albano, which was attended with

Leinster, and the eastern parts of Munster, where trade, manufactures, and commerce, brought much greater numbers together, the red dots are considerable in proportion, showing the number of towns that escaped."

[1] Military cordons were drawn round the infected districts in Montenegro, and persons who dared to cross them were shot. The horrors of famine were thus added to those of pestilence.

such fatal effects. At the same time many places in Calabria, as well as Catania and other districts in Sicily, were most disastrously ravaged. Few provinces of the kingdom escaped. Even the island of Sardinia became infected, it was currently believed, towards the latter part of the summer; for, in September or October, arrivals from that island were put in quarantine at Malta and some other of the Mediterranean ports. Altogether, no country in Europe has been more severely scourged, and that, too, during three successive years, than Italy—a convincing proof, coupled with the atrocious barbarities committed by an ignorant people in their insane terror, of the physical and moral debasement that prevails in too many parts of the southern provinces of that fair land.[1]

The distance from the south of Sicily to the opposite coast of Africa is not great; and it is a curious circumstance that Tunis, which it would seem had hitherto very remarkably escaped in 1865 (notwithstanding the arrival of many pilgrims from Mecca *viâ* Alexandria and Malta), and also in 1866, become infected for the first time since 1856, in the course of the present spring. How and whence the disease came, seems to be quite unknown. It manifested itself first, it is believed, in the city; it subsequently extended along the coast, and was especially severe at Susa, south-east of Tunis. It also spread into the interior of the country. Algeria appears to have been suffering, in some degree, about the same time. A still more important coincidence was the reappearance of the pestilence in Malta (which had remained entirely free during 1866) about the beginning of July, notwithstanding the utmost vigilance of the authorities and the extreme rigour of the quarantine restrictions.[2] The earliest fatal cases occurred in the lazaret, in persons who had come from Tunis and Sicily.[3] A woman, who with twenty-nine other passengers from Susa, was taken upon arrival, on June 16th, into the lazaret, was attacked there on July 5th, and died on the 6th; on the 6th another woman, belonging to the same lot, was attacked and died next day.[4] On this day, 7th, the master of a vessel

[1] A letter from Florence states that some official reports have been received there respecting the horrors committed in Calabria from fear of the cholera. It is impossible to imagine anything more frightful. Whole families have been murdered. More than eighty persons have thus perished on suspicion of scattering poison, to which the wretched ignorance of the lower orders in south Italy persists in attributing the cholera.—'Times,' September 17, 1867.

[2] 22 May, 1867.—A quarantine of twenty-one days was imposed on all arrivals from Tunis.

[3] 6 July.—The quarantine on animals from the Roman States, Naples, Calabria, Sicily, and Tunis, has been increased to thirty days.

[4] 10 July.—The lazaret being at present full, several vessels with refugees from Tunis on arrival here have been ordered off the island. With the view of preventing travellers who had passed through infected places in Italy finding their way to Malta *via* Leghorn or Genoa, these ports have been included in the category of ports subject to a quarantine of thirty days. The practice of absolutely refusing admission to ships, having any case of cholera on board, is maintained with rigor.

from Trani, in Sicily, was attacked on board his vessel (which after nine days' passage reached the quarantine harbour on June 24th), and died on the 8th. Sharp choleraic attacks had occurred in some of the villages around Valetta about the same time as the above cases, but none had proved fatal. Valetta itself remained free for several weeks; but, about the middle of August, cases began to multiply there also. Clean bills of health continued, nevertheless, to be issued till the first week in September; but, before that time, Malta had been placed under strict quarantine by most of the Mediterranean ports, and, among others, by Tunis, which, it was alleged, had become quite healthy. During a great part of the summer and early autumn, the British squadron, under Lord C. Paget, was obliged to remain in the harbour of Spezzia, in consequence of the rigorous restrictions in force in Malta.

Gibraltar appears to have remained entirely exempt in 1867, as in 1866.[1] Whether, or to what extent, the same can be affirmed respecting Spain, I am unable to say. Cadiz was declared by the Lisbon authorities to be "suspected" of the disease in the autumn; and there seem to be grounds for believing that it partially existed in the city and its vicinity at that season. Although no reliance can be placed on the statements of the Spanish authorities in regard of the extent or degree in which any pestilential disease may exist in their country, there appears to be no doubt that there was little, if any, epidemic cholera through the Iberian peninsula during 1867. The contrast between Spain and Italy, in respect of the far greater persistance and severity of the disease in the latter than in the former kingdom throughout the present epidemic, is a noteworthy geographical fact; and it is the more interesting as, in some former visitations, the disease is known to have continued in Spain for two or three years in succession.

It has been already noticed that the Austrian provinces of Sclavonia and Dalmatia, as well as the adjacent provinces of Turkey, were more or less deeply infected in the summer. Warsaw, Wilna, and other districts in Poland were, also, more or less, affected about the same time, or a little later. In northern Germany, too, the disease continued to manifest itself in various parts, from Breslau the capital of Silesia, to Cologne and Elberfeld in Rhineland. Two disastrous outbreaks in emigrant ships, which left Hamburg for New York in October, attest its presence in the former city at that period.

[1] The port of Gibraltar having been declared *foul* by the Madrid Board of Health, all arrivals therefore will be refused pratique or communication until they have first performed quarantine at Vigo or Port Mahon. The quarantine upon Gibraltar (both by land and by sea), although the health of the Rock is excellent, is in consequence of Gibraltar not having placed in quarantine arrivals from Morocco, not that there has been any sickness there, but because the Moorish authorities have not imposed sufficiently stringent restrictions on arrivals from Tunis and Algeria.—'Times,' 27th August, 1867.

The most interesting feature in the history of the epidemic in Central Europe in 1867 was its development in Switzerland, which had hitherto remained intact, notwithstanding the prevalence of the pestilence all round its frontiers. In Zurich, which suffered most from the visitation, it seems to have appeared in July. Rumour attributed the first case to importation from Rome, by a child whose mother had died there. At Martigny and other places in the Valais, where several deaths occurred in July and August, the disease was believed to have been imported from north Italy. A few scattered cases were observed in the cantons of Berne, Lucerne, Zug, Schwytz, and Argovia; most of the instances were in persons who, it was said, had come from Zurich. Geneva seems to have remained exempt.

As in the case of Tunis, the retardation of the epidemic visitation in Switzerland for such a long period after the disease had reached adjacent lands is a very curious question, and one of not easy solution.

During 1867, the disease at no time existed with epidemic force in France. Sporadic cases, or small detached groups of cases, occurred in Paris, Marseilles, and also at Chambery, Aix les Bains, and a few other places; but nothing more. In our own country, too, with the exception of an outbreak in the Channel Islands, and also at Port Glasgow on the Clyde, at the early part of the year, the epidemic did not reappear, although a few cases of rapidly fatal cholera occurred in the metropolis, and also on board some vessels sailing from the port of London, during the autumn. Similar attacks probably occurred elsewhere.

Passing now over to the New World, we find that the pestilence, while still continuing its hold on various and distant regions in the United States, had found its way towards the end of 1866, or beginning of 1867, so far to the south of the equator as to have reached the mouth of the La Plata in the 35th degree of southern latitude. In the early part of 1867, it is known to have been prevalent in the city of Buenos Ayres, where it raged with great violence in the course of the spring. The disease, it appears, existed along the coast to the northward, and subsequently extended into the interior along the course of the river Parana to Paraguay.[1] The

[1] By a letter from Dr. Scrivener of Buenos Ayres, dated May 24, 1867, and communicated to me by Dr. Archibald Smith, so well known to epidemiologists by his valuable researches respecting yellow fever in Peru, it appears that the cholera broke out there in the beginning of April. The disease first showed itself in Rosario, a seaport town, sixty leagues distant from Buenos Ayres; it then passed to the neighbouring villages, and ultimately arrived at that city. Dr. Smith states that, according to information from another friend just returned from Buenos Ayres, "the reigning cholera of this year came down the river from the camp (of the belligerents) to the city, and that its greatest havoc was not in Buenos Ayres, but in the remote rural populations."

town of Monte Video, on the opposite shore of the mouth of the great La Plata, was attacked in the course of the early summer, notwithstanding the most stringent embargo on all communication with Buenos Ayres,—in consequence of which the commerce and trade of both towns suffered enormous losses, and the poor classes encountered great privations.

Whether Rio Janeiro or other ports of Brazil had been the seat of the disease previously—and, if so, at what period it first appeared there—I am unable to say. In August, the Lisbon Board of Health declared Rio to be free from cholera; it may, therefore, be presumed that it had been prevailing there for some time previously.

In Nicaragua and other regions of Central America, at some parts in the interior as well as on the Pacific and Atlantic coasts, it is known to have been prevailing during the spring; and, subsequently, Honduras was reported to have become affected towards the end of the year. In the summer, Texas suffered severely both from cholera and from yellow fever. New Orleans, too, was similarly afflicted. At several places in the Southern States of the Union, along the Mississippi and Arkansas rivers, cholera was very fatal. In the northern States, the disease was much less frequent; but it was not utterly extinct.

Among the West India islands, Cuba seems to have been the principal seat of the disease in 1867. So disastrous were its ravages there that special allusion was made to the subject in the Queen of Spain's speech, at the end of the year.

The island of St. Thomas also continued to suffer from cholera, in addition to the terrible calamities of earthquake and hurricanes.

And now, in closing this very imperfect sketch, I have only to add that the epidemic has not yet become entirely extinct even in this present year, 1868. It continues to prevail in the La Plata region, kept up and aggravated, doubtless, by the war which has been going on for a long time past in that country. Moreover, according to a statement in the public journals about three months ago, it appears that several cases of the disease had occurred in Rio Janeiro, and that it still lingered in Honduras, and in the island of St. Thomas.

A rumour also prevailed about the beginning of the year that it had broken out in Tangiers, which had previously escaped notwithstanding its proximity to Gibraltar and Spain, and also to Algeria. If such has been the case, it affords another instance of the curious and perplexing vagaries in the movements of this strange pestilence, while it serves to show more and more the urgent need there is of an authentic record of all such events being from henceforth established, if we expect ever to dissipate the darkness in which so much of its natural history is still involved.

ART. II.

Entacoustics. By JAMES JAGO, M.D. Oxon., Physician to the Cornwall General Infirmary.

THERE are various sonorous movements which originate in the ear itself or adjacent structures. An inquiry into their sources and conditions of audibility may be called entacoustics.[1]

In an essay on the "Functions of the Tympanum," which appeared as an original article in the first two numbers of last year's 'Medico-Chirurgical Review,' I ventured to account for the phenomena in question, as far as they occur in the tympanum, in a peculiar manner. It was beside my purpose to speak of such as have their seat elsewhere in any other than a summary way. On these I would now make a few further remarks, to be read as an appendix to the essay, and to be regarded as another step in quest of a method of study which shall deal with the whole of the phenomena under one title, though the entacoustical problems may not be here shown to admit of like precise solutions as do those in entoptics.

Preliminarily, I must embrace this opportunity of enlarging a little upon certain acoustic phenomena observable in the head or ear, in order to more explicitly connect them than I have yet done with the views I have advanced. The head is made up of many different structures, and is traversable, more or less, by sonorous vibrations in any direction. But these progress best in the compact and most uniform, such as bone (varying, however, in density) and cartilage, constituting the skeleton or frame of the head, and are comparatively subdued by passing through such soft substances as muscle, brain, connective tissue, fat, and skin. The frame receives vibrations best from an external solid body where it is naked, as at the teeth; next best where it is most thinly clad, as at the cartilaginous or bony part of the meatus. It also imparts its own vibrations to a solid body most readily in such parts. The transitions of vibrations between the frame and air may be spoken of in much the same terms, with the exception that wherever there intervenes a watery skin instead of a dry one the interchange is obstructed. To give a few examples to illustrate these statements:—Let a vibrating tuning-fork press the lips against the teeth, and the sound will be heard very weakly compared to that yielded when it is in contact with the teeth; and it is heard somewhat better by way of the under jaw, which carries it close to the labyrinth by a denser medium than the zygoma or

[1] To group certain phenomena for the ear, as the word entoptics has long done for the eye. And as entoptics commonly treats of phenomena contributed by the conjunctival fluids, eyelids, and eyelashes, so entacoustics has been made to embrace phenomena which do not originate strictly in the ear; but it is not meant to be applied to sounds propagated to the head from other parts of the body and within its tissues, and which may be localised by auscultation.

deeper bone that conducts it from the upper teeth. Vibrations imparted to the tragus are loudly audible, their short path to the labyrinth being through uniform cartilage and dense petrous bone; whilst vibrations imparted to the mastoid process, a spongy bone, reach the labyrinth with much less force. In the same manner vibrations falling from the atmosphere upon the head and face (the membrana tympani being excepted from consideration) would more thoroughly penetrate to the said frame at the meatus than elsewhere; (the thinly covered nasal bones have no massive holdfast on the denser bones of the skull). Conversely, vibrations circulating in the frame would more readily pass from it into the air at the auditory canal than elsewhere. This would be found to be so if the radiating vibrations were collected from equal areas of the head or face. Then, again, the whole cylindrical wall of the meatus (an extensive superficies) is lined with mere skin, and our ear placed against another person's, or a stethoscope encircling its orifice, or a tube fitting into it, would catch all the issuing vibrations, so that vibrations circulating in his head would on this account also, if listened for, appear to the auscultator to proceed, *par excellence,* from this source. If the tragus be pressed over the orifice, or this be otherwise stopped, the radiating vibrations will be reflected back into the canal, and will resound in it, so as to strongly affect the tympanic membrane of the person in whose head the vibrations circulate.

But I must remark that the vibrations thus issuing from the meatus are not only those of the person's own laryngeal sounds, or his guttural or oral, such as accompany the acts of swallowing or chewing, but that none are heard better by the person (in mode mentioned) or auscultator than such as enter the frame from the teeth, about its most distant point from his labyrinth and the meatus where the auscultator listens, and more especially those that enter at the tragus, on the distal side, with respect to the labyrinth, of the membrana tympani. Hence we must not imagine that such vibrations as originate in the fauces, or anywhere thereabouts, get to the meatus through the tympanic air and membrana tympani, transitions from medium to medium of widely differing densities, against the best ascertained laws of acoustics, and particularly against my hypothesis of the part played by the fluid on the lining membrane of the drum. None of these vibrations could reach the tympanum from the fauces without bridging through solid substance the length of the shut Eustachian tube, whence they have a highway through compact bone to the walls of the meatus. All of them must necessarily impress themselves upon the walls of the parts where they are generated, and these are in close proximity with the base of the skull, which is ready to conduct them to other portions of the frame. It is because that the highway of such vibrations is the frame that they may be used as a *sure* criterion, with an *intelligent* patient, with respect to the condition of certain aural structures. Where they are distinctly heard by him, when conveyed from his teeth and the stiff portions of the cartilaginous meatus, the acoustic nerve, and probably the labyrinthian fluid, are healthy. If, nevertheless, he is deaf in that ear to external sounds and to the resonance of the said vibrations imparted to the frame in his shut meatus, the conducting apparatus, probably the tympanic structures or cavity, are at fault. No error can result unless there is a flaw in the conducting frame, and this could be ascertained by testing it at different points; though I would advise that the ear should not be stopped by a finger, or by anything held in the hand, for fear of confusion from the muscular rumbling alluded to already in my paper. It may be done by a short stick resting against a solid wall in a still place.[1]

But I must, in connection with the above remarks, again call attention to the fact that I could never perceive that the various Eustachian and tympanic sounds I have heretofore described are intensified by stopping the meatus. A certain portion of the vibrations that pierce the membrana tympani from a tinnitus within it may be supposed to be emitted into the meatus, and the puff of an air-current in the Eustachian tube must be impressed upon its walls, and we might suppose that some of the waves thereof might find a bony road to the parietes of the meatus; but the waves that may be caught in it from either source are not enough to appreciably augment the sound. We should infer from this that an auscultator must have more than an ordinarily acute ear to detect such sounds at the meatus of a patient. Besides, when he listens for the click (dubiously discernible in one's own ear) of the opening of the Eustachian tube whilst the patient swallows, he cannot fail to hear the strong clicking noise of the passage of saliva, or what not, through the fauces. And when air enters or leaves the tube, the coincident smacking or rustling sound of the consequent displacement of the tympanic membrane is loud to the auscultator. If, however, the membrane is perforated, or even (according to my hypothesis as to the action of the mucus on the inner face of the drumhead) dry within, the auscultator's task would be less opposed. I am not aware that any one has pretended to have auscultated a tinnitus aurium; yet this feat, under certain contingencies, seems more realizable than that of discerning the Eustachian sounds, except, perhaps, the *souffle*,[2] where

[1] Much has been published of late on the various foregoing topics; but if the reader will refer to my early aural papers cited in this Journal, he will exonerate me from the suspicion of copying anything in these respects from recent writers; and their views and mine, on most points, have nothing in common.

[2] Toynbee said nothing, in his book, about auscultating the Eustachian "bellows-sound," but afterwards stated that he was in the habit of doing so when inflating the tympanum by Politzer's method; which, however, fills the naso-pharyngeal tract with a loud blast of that type, as it consists of *blowing* forcibly into a nostril at the instant of deglutition.

there is perforation. I am persuaded that auscultators have been deceived by accompanying noises, in most of the cases where they have thought that they have detected such sounds. A coarse noise, like that of air gurgling through a drum filled with mucus or pus, which lies in contact with the drumhead, is not harder to auscultate than the movement of that membrane itself. I wish it to be understood that it is a fact worth noting that such Eustachian and tympanic sounds as are audible to the ear in which they occur are yet, unlike faucial ones, but weakly, if not quite inappreciably, communicable to the textures of the head.

In turning to another subject, I must remind the reader that one of the main positions in my essay was that a series of tympanic sounds are heard through the membrana tympani and ossicula auditus in precisely the same way that sounds entering the meatus are heard. Hence, since I also insisted that the membrane cannot be made tense by any device without impairment of its functions, it follows that this law should apply as much to entacoustical sounds heard through it as to others. I neglected to allude to this phase, however, though I was aware that in such cases as I founded my reasoning upon the tympanic tinnitus might be kept in abeyance, both by condensing and rarefying the air in the drum. I now supply the omission, because every additional test successfully withstood is a corroboration of my central hypothesis. Latterly I have noted, in several like cases of deafness, that distending the membrane in any way, for the time being, muffled the tinnitus.

I have drawn such weighty inferences from personal observations that I cannot refrain from snatching at an opportunity of showing that like ones may be made by others, especially when they come from such a safe and acute observer as Mr. Hinton, and who, it will appear, though usually kind to my views, is not prejudiced in their favour in this instance. Mr. Hinton says[1]—"A little instrument has been devised by Siegler to exhaust the meatus of air while the surgeon keeps his eye upon the membrana tympani and observes the effect produced. It is accordingly called the pneumatic speculum. Its design was to aid in determining the presence of bands of adhesion in the tympanic cavity, by indicating any spots at which the outward movements of the membrane might be impeded. In this respect it has great value. The membrane may be distinctly seen through it to move, sometimes as a whole, at others unevenly and in parts.

"But the little instrument has seemed more useful to me, even in diagnosis, by virtue of its power over some forms of tinnitus. That this affection very often has its source in an excess of muscular irritability and other causes, leading to an increase of pressure on the

[1] "An Outline of the Present Methods of Diagnosis in Aural Surgery." By James Hinton. 'Med. Times and Gaz.,' Aug. 10, 1867.

labyrinth, I cannot doubt; and the more because, in a large number of cases, slight traction on the membrane, by means of this instrument, allays it for the moment."

In another place[1] he mentions a fact as shaking his confidence in this explanation (and which, it will be seen, is agreeable to mine), viz.;—"In some cases conditions which must be supposed to cause great pressure on the stapes, such as an extreme concavity and tension of the membrana tympani, are found without tinnitus;" and any one with healthy ears may satisfy himself, by blowing the breath into the drums, that a "slight" increase of the normal pressure upon the labyrinth is not attended with tinnitus; and in the absence of a statement to the contrary, I presume that the tinnitus would have been equally allayed by condensing the air in the drum, and thus stretching the membrana tympani, or by accomplishing the same end by withdrawing some. Altogether, I look upon the fact ascertained by Mr. Hinton as equivalent to my view, that a tinnitus aurium frequently finds its way to the labyrinth through the membrana tympani; and I cannot help thinking that, had the other symptoms of each case been detailed, I should have derived from them support of my views about the tympanic functions in other particulars, as his description of the constrained movements of the membrane under the use of the speculum confirms my notion of the liability of the tympanic wall to the adherence of tenacious secretion, and is, in truth, but a counterpart of other observations of his own on the changed appearance of the membrane in cases of tympanic catarrh,[2] which he supplies as countenancing my views of the acoustic importance of the tympanic mucus.

Then, again, there were many of the cases in which the tinnitus was not appeased by the speculum. I indicated that constricted vessels in the inner wall of the drum would transmit a tinnitus immediately to the labyrinth; also Mr. Hinton remarks that enlargement of the vessels of the labyrinth was very frequently found on dissection to accompany "even slight inflammatory affections of the tympanum."[3] We may assume that a tinnitus may issue from such vessels, without concluding, with him, that "any considerable amount of tinnitus seldom exists without a somewhat morbidly increased irritability of the auditory nerve," if this means that the irritable nerve causes the perception of a sensation of tinnitus which is not objectively (without respect to itself) produced. If the hearing remain good, notwithstanding the tinnitus, I should believe the nerve to be healthy, and that the membrane and ossicular chain are right, and that the peccant vessels were situated in the inner regions just spoken of. In these cases I believe that the tinnitus rather subsides gradually, on cure, than suddenly, though a pellicle

[1] 'Supplement to Toynbee,' p. 463.
[2] Op. cit., p. 450.
[3] Op. cit., p. 463.

of mucus may drop from the inner wall of the drum, causing sudden cure.

I have expatiated so much in the essay on the phenomena of the tympanum and Eustachian tube, that one of the chief duties that remain to me is to pursue the subject of tinnitus more generally; but in the way of filling up a list of such subjective noises, I may mention that when the attached muscles move the auricle and cartilaginous meatus the rubbing of the displaced structures against one another is heard; and it may happen, when there lies in the meatus a plug of wax, or foreign body, or polypus, that a corresponding friction-sound from the movement of such a thing on the membrane or sides of the meatus, when its cartilage is moved, may be heard. Let also what I have stated about tinnitus that may happen in the meatus be borne in mind.

I casually gave my general views on tinnitus as follows:—"Of tinnitus as a symptom it is somewhat difficult to speak accurately. There may be intracranial noises, and even nervous or mental. These must be diagnosed on other principles; but confining our attention to the ear, we must remember that there are many arteries about it, and that the partial narrowing of the caliber of any one may produce a noise,[1] if the same may not happen in a vein." I would now remark that if a *souffle* be produced in the temporal artery by a due pressure with the finger in front of the tragus, it becomes much louder if the meatus be stopped; and I dare say it may be taken as a safe rule that, when deafness depends upon a cause as deeply seated as the drumhead, a *souffle* or vascular tinnitus from vessels lying external to this is not likely to be heard, and where no deafness exists such a souffle or tinnitus would be augmented in loudness on stopping the meatus; whilst I may adjoin that such a sound issuing from a point more deeply seated than the bottom of the meatus is not likely, in any case, to be rendered *appreciably* louder in this way.

But besides the continuous tinnitus (various in its tones) flowing and ebbing with the pulse, it is a frequent thing for a deeply seated loud puff to assail an ear as the heart beats, and this in cases where there is freedom from deafness. It is generally of a more or less transitory character, and may depend upon posture or sudden movement of the head, or (vaso-motor) nervous excitement. I can pro-

[1] Mr. Hinton says (op. cit., p. 462) of tinnitus, "Scarcely any advance has been made since the publication of this volume (Toynbee's) in the empirical treatment of this symptom, but there seems a gradual progress towards a better understanding of its significance. When of a beating character, and synchronous with the pulse, it is obviously referable to vascular conditions as its exciting cause, and among others sometimes to aneurism of the basilar artery. In some cases pressure over the course of the carotids immediately beneath the ear temporarily arrests it." In the 'Proc. Roy. Soc.,' 1858, I assigned such noises to the circulation, "for they rose and fell as it was quick or otherwise."

duce the like at will by a forcible contraction of the abdominal muscles, and by thus checking the course of the blood through the descending aorta, subject the carotid to unwonted blood-pressure. It has been thought the sound may be occasioned by the internal carotid artery. I am persuaded that it indicates its dilatation beyond the size of the bony ring in the temporal bone by which it enters the cranium close to the labyrinth. Such a souffle or tinnitus, if we may so call it, must, as tried by the tests above suggested, at any rate arise from some arterial branch deeper than the drum, if not from the carotid. There may be rare cases in which it might be constant and aneurismal, or dependent upon some permanent narrowing, compression, or dilatation of the vessel. The very fact of the existence of the sound is a sign of the healthy condition of the labyrinth and nerve.

I will now pass on to the intracranial noises to which I have alluded. It seems to me an interesting question to inquire whether we have any means of distinguishing noises that arise within the cavity of the skull from such as we have above described arising without it or in its wall.

In connection with this question we may note that all the noises we have been discussing are heard on one ear only;—that, though a tuning-fork vibrating on one side of the head may be very faintly heard on the opposite ear if its meatus be stopped, for all practical purposes, we may assert that not one of these sounds can by any device be caught by the other ear;—and that of the ordinary sounds that reach the organs from the circumjacent atmosphere any one which is allowed to enter only one meatus is not transmitted through the head *so as to be heard* upon the other ear also. In a word, sounds that affect one labyrinth from without do not penetrate to the other. Strictly, of course, no sonorous waves can agitate the liquor Cotunnii without also affecting the petrous bone, and tending to circulate through the whole frame of the head, and thus towards the other labyrinth; but if any attain it in a degree that would otherwise be *appreciable*, they must be so strenuous as to produce a too stunning effect through the first ear to be realized in the second.

Now, when we speculate whether any of the numerous vessels which lie within the skull may project a tinnitus upon the labyrinths, we note that many of these are placed between the two, and that the majority of them are separated from either by structures of the same kind, insomuch that, whether they are equidistant from them or not, we should conjecture that a tinnitus arising therein would be likely, if it reached either, to affect the other also. I have a curious observation to relate which illustrates this idea, whatever variety of opinion there may be as to the actual cause of the phenomenon.

I speak of the phenomenon vulgarly called ringing of the head, that falls within everybody's experience. A patient described it to me as an electric spark through the head, attended with noise, causing him to stagger for fear of falling. Where there are good hearing and good health, it will invade the head at intervals, may be, of months, arising without warning and swiftly subsiding. About a year ago I was alive enough at the instant of such occurrence to mark that the phenomenon commenced like an explosion going off close to one ear, and ended by a prolonged ringing in the other. Since then I have had several capital opportunities of verifying the correctness of this observation. This sort of tinnitus often frizzes off, as it were, in contact with one labyrinth, and makes its way *afterwards* to the other, affecting it less strongly, and for more instants. At other times I have heard it hiss or buzz into one, as it were, a little lengthened out, as if from a point appreciably distant, and yet not attain the other until after a still more appreciable interval of time. Again, such an interval may be less and less discernible, or the two may be plainly attained at the same instant. Finally, the ringing, however seizing upon both ears, continues in both for moments before it becomes inaudible.

Such facts indicate that the phenomenon cannot be primarily a nervous affection, for we have no reason for supposing that a sonorous sensation ever passes from one nerve to the other. Nor is it observable that the auditory nerves are exposed to be hit in their paths to the brain by any moving body which assails them separately. And that there should be an actual resonance within the cavity of the skull filled up with brain, or that any bell-like ringing (in accordance with the vulgar idea) in such a bony cavity, if containing nothing but a fluid, could happen, is inconceivable; yet the tinnitus, explosive and transitory, has no resemblance to an arterial souffle, or such a capillary or vascular tinnitus as invades the drum. Nor is it aggravated by closing the meatus, not having sufficient absolute intensity to be conveyed thither in appreciable degree from its point of origin.

To me it seems that anatomy reveals but one apparatus in which the phenomenon could take place, and this one adapted to all its phases. I speak of the chain of large venous tubes and receptacles called sinuses, which connect one petrous bone with the other, and both with other intracranial parts, &c. It surely does no violence to probability to assume that in such a blood-current a bubble or cell may occasionally burst, some fibre snap, some collapsed vein leading to a sinus be forced open, or some coherent things part. This being granted, the explanation is palpable.

The two petrosal and the capacious lateral sinuses are actually supported by the bone that includes the labyrinthian fluid, and the latter, at least, freely connected with this or the drum by veins; and we may infer that a sonorous vibration affecting the blood within them need not be violently strong in order to find some point where it may (whether through solid bone or favoured by the course of an aural vein) penetrate to the labyrinth. Hence should, say, an explosion take place in either of these sinuses, its sonorous waves would traverse the Cotunnian fluid on that side immediately, but would have to circulate all round the tentorium to play upon the other side of the head, and would thus be heard later upon the second ear. Such an event might even occur in the beginning part of one of the internal jugular veins, or, may be, one of the cavernous sinuses, and the waves have to ripple over one labyrinth in their course through the lateral sinus, the torcular Herophili, and the remaining lateral sinus to the seat of the other. If it happens in either lateral sinus there will be a difference in the times of their reaching the two labyrinths, and that difference will be greater as it happens further from the torcular. Whilst should the starting-point be in the torcular, or any one of the sinuses lying in the mesian plane of the head, the two labyrinths will be attained at the same instant. There may be cases, too, in which the undulations may find their way back again to the petrous bone they have already washed, by being reflected, or by winding their way in a circulating manner through one of the small sinuses that help the lateral ones to keep open a communication between the two sides of the head. Lastly, we may imagine that the sounds under consideration may, in certain cases, be increased by resonance; for example, in such a vessel as the torcular.

Hence I should say that a chief office fulfilled by the numerous bands which give a spongy structure to the interior of the cavernous sinus is to intercept sonorous impulses that may, by any chance, be impressed upon its blood-current by the internal carotid artery which traverses it, that they may be cut off from the petrosal sinuses.[1] We may conclude that if, notwithstanding, an arterial souffle should be propagated to these sinuses, so as to affect one ear, it would also pursue its way to the other.

I have no observation to record on the arterial *bruit* from an assigned intracranial cause; but the anastomoses of the intracranial arteries do not furnish a long, broad, tubular communication between the petrous bones, as the veins do. There is the circle of Willis, however, to which the basilar artery contributes, whilst this also sends branches by the side of the auditory nerves to the labyrinths. It seems too limited linearly for a sound arising in it to reach the labyrinths in succession with a recognisable interval. A case of

[1] By placing a finger of one hand on a prominent enlargement of a varicose saphena vein half way up the thigh, and a finger of the other on any one of several large branches half way down the leg, the gentlest tap with either finger was readily felt by the other, so freely are undulations transmitted by the blood in a full vein without valvular breaks.

aneurism of the basilar artery, said not to be infrequent, I have never met with; but I should take for granted that a souffle from such a source would assail both ears at the same instant, unless the aneurism involved the origin of one of the branches to the labyrinth. Beyond this all I can state further is that I regard the brain as such an indifferent medium for the conduction of sonorous vibrations that sounds may prevail in it which never reach the labyrinths, but that, considering the position of the main cerebral arteries, and its general uniform consistence, I should imagine that an intracranial *bruit* or tinnitus is most likely, if audible, to be audible upon both ears, whereas a cranial or intracranial one is only audible on one.

In the everyday cases in which a throbbing is felt, as it were, all through the head, and a beating, or, may be, a blowing sound assaults both ears at each throb, it is palpable that the external arteries take their full share in the fray, but it can only be guessed that the central ones furnish their quota of noise.

In seeking to determine whether a tinnitus or other entacoustical sound is liable to be simulated by a sensation or perception, we must depend a good deal upon analogies. Unless it be that any of the substances that stimulate the gustatory or olfactory nerves at their final distributions may also come in contact with their trunks by being carried along unchanged in the vascular circulation, the acoustic nerve is the only special one any point of whose trunk may directly encounter from without the peculiar stimulus of the nerve. Yet it will have been remarked that I have always assumed that entacoustical vibrations affect the nerve through the labyrinth—that is, where it ends. I have nothing but analogical inferences to justify me in supposing that they are not likely to act upon it elsewhere. In the cases of the two other nerves above cited we have no means of knowing what may happen through the circulation as just suggested, because it is met by it in all its points, and they do not seem to be excitable by mechanical stimulus, either at their expansions or in their trunks. But it has been ascertained that nerves of common sensation, though excitable by pressure or tension at any point of their course, yet are much more sensitive at their tips than elsewhere. Light can not only impinge upon the bacillar layer of the retina, but also on the base of the optic nerve and such portions of its fibres as lie without it. Hence, as it stimulates the bacillar layer only, we may take it for granted that the trunk of the nerve is unfitted to directly receive the stimulus of light. Similarly let us apply a mechanical stimulus to which this nerve responds. Let us turn the eyes quickly in their sockets. Hereupon we have two lucid circles projected by the pinched terminal sentients surrounding the bases of the nerves, but no other lucidities, though the nerve within the base has been stretched, and all its fibres, as they radiate therefrom, have been as sharply bent as the bacillar layer has been. Nor does it appear that the pulsations of the artery, that runs like an axis to the nerve, cause sensations as of light. Thus, without entertaining the question whether extreme violence to the optic trunk (such as its section by a knife) may yield a flash of light, we may safely affirm that it is, at least, difficult to excite the nerve elsewhere than at its terminal expansion. And even here, as I have insisted in treating of entoptics, it is doubtful if any pressure upon the physiological eye produces lucidities but such as creases the retina towards its centre, that is, squeezes the very distal ends of the rods and cones. Such tension of the eyeball as is presumed to arise from excess of the aqueous or vitreous humours does not, I am satisfied, occasion lucidities. And when deep-seated inflammation of the eyeball is introduced by subjective flashes of light (if they be really other than the lucid circles just mentioned), we may well imagine that inflammatory products may irritate or squeeze the said distal ends, as well as that many of these may be compressed by flexure of the retina through uneven swelling of the ocular tissues, or by jerking, impatient actions of the orbital muscles.

In bringing these illustrations to bear on the case of the acoustic nerve, we note that an artery enters the internal auditory canal in actual contact with it, running between it and the facial nerve, and that no rhythmical sounds attend its pulsations, so that the conduct of the acoustic nerve in this particular resembles that of the optic. Such is the sole observation we can make on the trunk of a nerve which is placed out of the reach of experiment; but we can hardly err in presuming from this observation, and the deportments of the other nerves of sensation as above touched upon, that the acoustic nerve is not liable to be excited by sonorous vibrations impinging upon it elsewhere than at its terminal points, and that it is not likely to be exposed to other mechanical pressure or tension capable of exciting it elsewhere.

Then, as to the labyrinthian expansions of the nerves, spread out as they are on the walls of a strong vessel, they are exempt from being pinched by flexure of the wall, nor can the adjoined bristles, otolithes, and Corti's fibres, be thus disturbed. Such steady pressure as we can produce upon the liquor Cotunnii through the fenestræ excites no sensation of sound, and I see no reason to think that a morbid excess of that fluid would provoke any. The vascular supply of the labyrinthian membrane and nervous expansion is very far short of what is furnished to the retina and ocular tunics, and it has never been shown that those structures are so obnoxious to violent inflammations as these are. But should abnormal vascular activity befal them, entacoustical sounds would issue from it, and would impress themselves upon the nerve as long as it and the labyrinthian fluid and structures remain uninjured enough for that purpose. However, we may rest assured that the retina, in its moveable, compressible, and vascular coats, must be far more liable to have its terminal points pinched or irritated, under such circumstances, than are those of the auditory nerve.

Again, through whatever portion of the special nervous apparatus it occurs, very curious lucidities or inequalities of visual power affect our visual vault just as we awake from sleep, whilst nothing comparable to such a phenomenon happens to any other special sense—a phenomenon dependent, possibly, upon the state of the vascular circulation. The following experiment evinces that something like it may ensue from such a cause:—Seated in a dark room, I firmly gripped one external carotid artery, just above the hyoid bone, and almost immediately a strong tingling sensation ran through the opposite arm, and marked variously-hued lucidities overspread the visual field, though the eyes had not been rotated, whilst the head began to feel so dizzy that I dreaded proceeding with the experiment. Still, I cautiously repeated it on two other occasions with characteristic results. I designed it as possibly fitted to modify the vascular circulation about the middle and internal ears enough for me to make an observation upon a very faint tinnitus that at that time occupied one ear, and which I imagined to spring from a very minute vessel. The unpleasant symptoms were so prompt as to afford little chance of modification of the current of blood in the capillaries, nor was the tinnitus either checked or altered. There resulted no unusual acoustic phenomenon whatever, any more than any subjective taste or smell. Thus, as far as this experiment goes, it would appear that when the cerebral circulation is overburdened, as in this instance, by shutting back an extra volume of blood upon the internal carotid, sight is the only special sense very obnoxious to false impressions.

These remarks have regard to such subjective sounds as may afflict us independent of disease in the acoustic nerve or brain. There are cases on record, as to each of the special nerves, wherein it has been apparently the seat of deceptive sensations. In nearly all of these the proper functions of the nerve were badly performed. Associated with such complaints have sometimes been epilepsy, or some cerebral disease. The acoustic nerve-tract may, I by no means gainsay, suffer in this way, though I am persuaded that disease in it by far most commonly produces deafness unconnected with subjective sounds, and that usually the existence of much entacoustical sounds should encourage us to hope that the nerve is still healthy, or that they should be esteemed as a proof of its capability of hearing. The cases which make me doubt the general accuracy of this statement, are those in which tinnitus and deafness co-exist, and in which sounds impressed upon the frame are not heard, and which *possibly* may not be explicable on the supposition of disease of the labyrinth only. I waived suggesting that some of the cases alluded to touching other special nerves may have been instances of depraved secretion coming in contact with the ultimate distribution of the nerve, because it is enough that there is a fair probability that there have been cases free from such suspicion.

In fevers, or when certain narcotic or other drugs are taken into the system, the mind may ramble and noises in the head ensue, or, as in the instance of taking quinine, mental stupidity and deafness. In some such cases, through the influence of the sympathetic and other nerves, the calibers of the arteries may become irregular, to the development of souffles; or the secretion of the mucus in the tympanum may become altered in quality or quantity, and tympanic tinnitus or deafness be hence engendered; or the damaged blood may be less exempt than the healthy from the explosive phenomena ascribed to the venous sinuses. So that it may be hard to divine how far attendant subjective sounds may thus arise. Nevertheless, in cases like these, where the whole of the nervous structures are pervaded by a poison, it is so probable that the auditory part of the apparatus may be somewhere directly teased by it that, in the absence of evidence to the contrary, it would seem well to suspect it might be the seat of illusions of audition which might be mistaken for entacoustical noises. In mania illusions of this sense are no more or less mysterious than those of any other. Still, in any case whatever, a perception ascertained to resemble that from any known entacoustical cause, such as the tinnitus of vessels, should be primarily looked upon as thus originating.

Art. III.

Typhoïd Fever in the West Indies. By William H. Stone, F.R.C.P., &c., late Secretary to the Board of Health in the Island of Trinidad.

It is scarcely possible to find better evidence as to the importance of studying the geographical distribution of disease, than is afforded by the epidemic of which I propose to give some details in the present paper. Typhoïd fever, an old familiar inhabitant of Europe, was allowed on all hands to have made its first decided inroad into the island of Trinidad in the end of the year 1866. It was not merely that it then for the first time obtained recognition. The whole course of the symptoms and the general character of the malady were strikingly different from what had previously prevailed; moreover, the unanimous diagnosis of many able practitioners resident in the chief town, some of them recently arrived from great medical schools in England, was fully borne out by altered indications for treatment, and especially by novel and unexpected results of the administration of quinine, a drug whose peculiar and specific properties endow it with the character of a nosological test, as well as that of a valuable curative agent.

Admitting, what hardly seems possible to doubt, that typhoïd fever, since its precise segregation from kindred types and its admission as an independent entity into the list of diseases, had not before this date been observed in the tropical and malarious climate of Trinidad, it becomes even more interesting to notice what modifications it was liable to assume, and how far its European congener was altered or complicated by the absence or the supervention of familiar or unexpected symptoms. On this point I hope to offer new and trustworthy observations, which tend to show that typhoïd may be blended in several degrees and manners with remittent fever, producing, by the mixture, a form of febrile action seldom if ever described. Something similar has, indeed, been put on record by Casorati, and others of the Italian physicians, though their examples of remittent action hardly equal in virulence what occurs on the isothermal line of greatest heat; and their conceptions of typhoïd, if they existed, fall far short of the precision which now attaches to the term.

It is not my present object to enter on the question of West Indian climate, except in a cursory and subordinate manner. My own meteorological observations, continued steadily through the better part of a year, were founded on those long taken at the Botanic Garden in Port of Spain, and which, by the kindness of the curator, I was several times able to consult. They are being continued by an intelligent resident, and will, I hope, ere long form the nucleus of an independent communication. Proof being long, I must be allowed merely to name such points as seem pertinent to the subject in hand.

The climate of the West Indies, speaking generally, is very different from what is commonly supposed, and still more at variance with its old traditional reputation. Professor Parkes, in his excellent work on hygiene, has already combated ancient prejudices. The West Indies were indeed a pesthouse of Europeans when every sanitary precaution was neglected, when a mode of life was indulged in which could hardly fail to be injurious anywhere, and when, moreover, an emigrant was sent forth with the comforting conviction of his friends that he was committing a virtual suicide. Much of this prejudice, indeed, persists; for instance, in the heavy handicappings with which insurance companies saddle their West Indian policies, although a flourishing office in Barbadoes is doing excellent business at the ordinary European rates. The great majority of Europeans can, with ordinary care, enjoy a state of health in no way inferior to what they have at home. Every now and then, indeed, a person is met with who forms so striking an exception to this rule as by contrast to show it in a stronger light. One such case was under my personal observation. Mr. M—, a gentleman of unexceptionable prudence in his mode of living, so obviously and rapidly wasted and withered under a change from New Brunswick to Trinidad, that it struck even casual observers; at the end of a few months he was obliged to resign a responsible and lucrative Government appointment and evacuate the place, fairly beaten by the unsuitableness of the climate to his constitution.

It is not to be denied that at times the heat is very great. This is a mere truism in regard of a spot lying in 10° of north latitude, and over which twice in each year the sun is absolutely and mathematically vertical. But the amount of cold compatible with these conditions is remarkable, and will probably be new to most readers. On the 13th of February a grass minimum thermometer, verified, like all my other instruments, at Kew, marked 57·7° of Fahrenheit, and the shaded minimum, unaffected by radiation, marked 59·6°. This is, I believe, the lowest recorded temperature for some years, but during the above month I several times recorded quantities not far different. For instance, on February 3rd it was 58·7° on the protected minimum, and, indeed, for the month the minimum was nearly always close upon the 60th degree.

The maxima for both the days named were 89°, omitting decimals, thus giving an extreme daily range of 32° and 31° respectively. It is this large daily range which, in my opinion, prevents the injurious effects of the great heat. After a noontide heat of 98°, the nightly temperature of 58° or 59° feels so cool and fresh that most persons, like myself, were waked up by the cold, and drew their blanket closer over them. My previous impression certainly was that a blanket would be as unknown and useless an article on the heat equator as a pair of skates. To one element of climate I directed very considerable attention, from its comparative novelty, and for the fact that the instruments which I used for its ascertainment were the first imported into the colony. The element in question is the radiation already named. But the minima were not so instructive as the maxima. By means of Sir John Herschel's excellent thermometer, the blackened bulb of which is contained in a globular glass vessel carefully evacuated of air and sealed hermetically, I was able to obtain the actual radiant power of the sun, free from atmospheric influences. On the two days cited above the midday maxima were respectively 147·3° and

149°. On the 18th of January it reached the enormous height of 183·7°, and on the 16th of the same month that of 174°. These large readings were always on bright showery days, and fully bore out the connection stated by Professor Tyndall to exist between the radiation and the amount of watery vapour in the atmosphere. It is somewhat singular that the only reading of the radiation maximum at all approaching to those given above occurred in a far higher latitude. On the deck of the R.M.S. Atrato, when in the harbour of Peter Island, near St. Thomas's, the reading at noon was 179°. The stanchions and wood work of the vessel, where uncovered by awnings, were so hot that they could not be touched by the naked hand. I hope at some future period to collect these latter observations, with additions kindly promised by my friend Mr. Carr, of Port of Spain, into a coherent series. For the present, I only offer them as outlines towards appreciating the "epidemic constitution" of the locality.

Besides being hot, the Island of Trinidad is decidedly malarious. Unlike most of the other windward islands, its geological formation is tertiary, whereas Martinique, Guadaloupe, and others, are, I understand, of volcanic character. In St. Lucia there is an active volcano, the Souffriére. Perhaps, indeed, Trinidad should rather be considered geographically as belonging to the great delta of the Orinoko, as a detached portion of the continent of South America, than as one of the circle of submarine mountain peaks which rise almost equidistant at short intervals from the Virgin Gorda group down to Grenada. Barbadoes, also, has the character of Trinidad in this respect, and stands even more distinctly aloof from the other Antilles. The malarious tendency is due to several large lagoons and marshes, by which the island is intersected. One of the largest of these, the Caroni Savannah, lies directly to windward of the principal town, Port of Spain. It is a marsh of great extent, bordered towards the sea by an elevated belt of mangrove swamp, which holds back the waters of the Caroni river, the largest in the island. This swamp is all slightly, though decidedly, above the level of high water in the Gulf of Paria, as the inland sea is named, and as the tide rises and falls at least four feet there is abundant room for artificial drainage. A simple straightening of the devious river, and a few canals through the fringing swamp, would not only open some hundreds of acres of fine land to profitable cultivation, but would materially improve the sanitary state of the capital town. It is somewhat remarkable that this huge storehouse of miasma, lying just up the prevailing north-easterly wind, does not render Port of Spain as uninhabitable as Aspinwall or Cayenne; but such seems not to be the case. It is generally accounted for by those who have studied the subject as a result of the configuration of the ground about the town itself. Port of Spain lies in a basin of hills, which run close down to the Gulf of Paria on the windward side. These hills, on which are several old forts dating from the Spanish occupation, are agreed on all hands to be deadly even to the acclimatised negro from intermittent and remittent fever, whereas the town at their leeward foot is only moderately affected by these complaints. The vegetable poison is supposed to be diverted by this natural screen and to expend its force elsewhere. The opposite ridge is equally unwholesome, a fact locally explained by the down current of the same miasm settling out of higher regions by its natural specific gravity. It is difficult to give an opinion as to the truth of this hypothesis; a town living in fair health beneath a canopy of poisonous vapours is at least picturesque in idea, and perhaps not more singular than some other caprices of malaria on record. The essential points, however, with a view to the subject in hand, are the Caroni swamp to windward, the very unhealthy circle of hills, and the less dangerous basin of alluvial land which they enclose.

Trinidad has a distinct dry and wet season; but, contrary to the experience of Europe, the summer is wet, the winter dry. The wet season usually commences in April or May, the dry season about October or November. Of course, these limits are subject to considerable variation in different years. It appeared that the wet season of 1867 which I passed in the island was exceptionally dry, and fears were expressed for the following cane crop in consequence, fears which, I believe, were already dissipated in some measure before my departure. But although the quantity of rain falling is considerable, and the showers sudden and torrential, I was not prepared for the amount of fine weather which was experienced even in the wettest months. Fifteen inches of rain were the largest monthly fall during my stay, though I find record of four inches in a single day, and even of larger quantities.[1] The beginning and the termination of the rains have here as elsewhere the reputation of special unhealthiness. I was able myself to substantiate this fact, especially at the latter period, when nearly all my patients in the Belmont Asylum, situated a mile out of the town, had transient but decided attacks of ague.

The epidemic of typhoid fever does not, however, appear to have followed any law of season. The earliest cases seem to have been noticed at the very end of the year 1866, or in

[1] The largest recorded fall of rain in one day is 6·556 inches on May 15th, 1864.

January, 1867, from which date their number slowly rose until the character of the epidemic was recognised. On my arrival in the month of May I found the medical profession fully aware of the visitation, and with one notable exception acknowledging its pathological character. At that time the public at large had not fully taken in the fact, nor was it until the summer months that the increasing mortality, which rose to seventy, five in the month of June, produced a disposition to pantic which was fortunately checked by a plain statement of facs. This the good sense and hearty co-operation of all the resident practitioners enabled me, as Secretary of the Board of Health, to lay officially before that body.

The mortality did not immediately decline, but by the month of September there were evidences of the epidemic wearing out; the usual slight recrudescences occurred; by the end of the year it may fairly be said to have terminated. The occurrence of sporadic cases since then until my departure in March, 1868, struck me as being comparatively infrequent, though on this point I cannot speak from precise statistics. My friend Mr. Knaggs, however, in forwarding me by the last mail (March, 1868), a most valuable report completing the number of 120 cases, remarks—"The above table probably includes the whole period of the epidemic (from January 16th, 1867, to August 28th, 1868), but cases from time to time show themselves in all parts of the town. I am attending one now on the eastern side of the Savannah."

My notes contain a record of about 600 cases, of which some 60 were fatal. This can hardly be considered as representing the whole epidemic. Unfortunately, the indolence and improvidence of the negro character, even when blended with European elements, render it singularly unfit, among other things, for the careful treatment of epidemics. And the absence of any systematic poor-law medical relief for the needy classes undoubtedly caused many cases to slip through unnoted and untreated, especially among children and adolescents, who seem to have suffered most heavily from this disorder.

One other point of a preliminary nature deserves mention at the present stage, namely, the decided preponderance of cases, both in number and severity, in the sewered part of the town. I was in the habit of pricking down each case as it was reported to me into a block-plan. It soon became evident that one district exhibited an unenviable prominence in this respect, and subsequent information showed this district to coincide pretty exactly with a small portion of the town, about a fifth of its whole area, to which a system of pipe-drainage has recently been adapted. It should be admitted, however, that the local configuration of the town, lying on a perfectly flat alluvial savannah, with the only outfall into an almost tideless and stagnant inland sea, is singularly unfavorable for such a method of purification.

I observe since my return to England that an analysis of some cases in this epidemic has been communicated to the Medico-Chirurgical Society by my friend and successor in office Dr. Bakewell. He did not, however, enter into the question of climate, and thus rendered an otherwise forcible history of his own cases somewhat obscure. This was, indeed, noticed in the discussion which followed his paper, and I am, therefore, the more anxious to supply the omission to the best of my power.

In the subsequent reports of cases I shall have the advantage of recording many different opinions from independent observers, and commenting on several lines of treatment which were adopted by practitioners of distinct schools and nationalities.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

REPORT ON SURGERY.

By John Chatto, M.R.C.S.E.

Amputation at the Knee-joint.—Dr. Brinton, in his paper, brings forward evidence and several cases with the object of securing a further adoption of this operation than now prevails. During the late war it was frequently practised by the American surgeons with encouraging results. As many as 211 cases are recorded, and of these 96 recovered, 106 died, one was still under treatment, and in eight the result was undetermined. Of 191 cases, in which dates are given, in 111 the operation was primary. Compared with those derived from amputations of the thigh, in the same army, these results are favourable. Of the latter there were 1597, with 568 recoveries and 1029 deaths.

Dr. Brinton relates seven cases that came under his own care in civil practice, and gives abstracts of 38 others operated upon by other American surgeons, and not hitherto published. He also refers to a great number of cases which have already been published, either at home or abroad. Many of these are, however, so carelessly reported as to be of no use in a statistical point of view, and the author has drawn his conclusions only from such as exhibited exactitude. The most important of these is that which shows that the operation has a very marked superiority over amputation of the thigh, and is even less fatal than that of amputation of the leg. Another advantage it possesses over the other amputations is the favorable stump it gives rise to, the testimony of surgeons, patients, and instrument-makers, being nearly unanimous on this point. The following are the circumstances for which this operation is eligible:—"1. In crushed and compound fractures of the bones of the leg, extending up to or involving the knee. 2. In gunshot fractures of the bones of the leg, in the vicinity of or involving the knee. 3. In gunshot wounds of the knee-joint. 4. In gangrene of the leg, the result of injury to the great vessels or nerves. 5. In chronic and irreparable disease of the bones, or for tumours of the leg. 6. In degeneration and abscess of the knee-joint."—*American Journal of the Medical Sciences*, April.

Dr. Markoe, of New York, has also published a valuable paper on the same subject, in continuation of a former one in 1856. The present paper relates to 51 amputations, performed either in hospital or private practice, and these were attended with 22 deaths. He refers likewise to four other successful cases reported to him verbally. The following are some of the general observations which result from so multiplied an experience. 1. It is no longer doubtful that the ancient prejudice against the operation, and the objections to it on the ground of opening so large a joint are unfounded, at least in a degree, to be of practical importance. 2. There seems good reason to believe that the shock to the system and the demand upon its reparative power is less in the amputation at the knee-joint than in amputation higher up. 3. The condition of the stump during the progress to cure is more favorable and less distressing to the patient than one made through the thigh. 4. The bone being unwounded, it is not subject to be troublesome and dangerous, accidents which sometimes follow the application of the saw, and the exposure of the medullary membrane to the air and to the foul secretions of the suppurating surface. "These considerations, fortified by the observation of a considerable number of cases, have led me to the conviction that the knee-joint amputation is in itself a better and safer one for the patient than amputation of the thigh—a view which statistics, now tolerably extensive, fairly confirm. But if this intrinsic superiority of the operation at the knee be judged 'not proven'—if it be insisted on that it is no better and no safer—it has, in the excellence of the stump which it leaves, a claim for preference which I think nothing should defeat but the ascertained fact of its being more dangerous and more fatal than amputation through the thigh. No one who has ever seen such a stump, and observed its performances, can have a moment's doubt as to its very great superiority over any stump which can be made in an amputation higher up."—*New York Medical Journal*, March.

On Lithotrity for small Calculi in very Young Children.—M. Marjolin, during a discussion at the Société de Chirurgie, observed that he had had under his care several children, from one to four years of age, having very small calculi; and in such cases he believes lithotrity to be the preferable operation. In three such cases, in which he resorted to it, it was attended with complete success. M. Giraldes observed that it was not surprising that such young children should have these calculi, and even be born with them, since, as Martin, of Jena, has shown, their kidneys are loaded with urate of soda. When these calculi are very small, lithotrity is the operation that should be resorted to; but as they usually then cause little inconvenience we have to interfere only rarely. When the calculi have attained a diameter of three or four centimeters, and induce irritation, they should be removed by lithotomy, which in such cases is a more rapid and a safer operation. M. Guersant has often had cases of children of one or two years of age, with calculi the size of a pea, which he has crushed at a single *séance.* After three or four years lithotomy is preferable. He has practised this last 104 times, with eight deaths;

and in forty cases of lithotrity he has also had seven or eight deaths, uusually from intercurrent diseases, contracted in the hospital. M. Giraldés maintained that lithotrity was the more dangerous operation in children. It is long and laborious, as only small and weak instruments can be employed, which break up the calculus into too large fragments. The *seances* have to be multiplied, and, the bladder being a peritoneal organ in children, peritonitis is easily induced, while the neck of the organ is excessively irritated by the fragments. By lithotomy a cure is effected in from ten to fourteen days, and consecutive fistulæ are very rare in children. In thirty-nine operations performed on children, from one to fifteen years, M. Giraldés has met with no instance. In fact, consecutive accidents are exceedingly rare in children.—*L'Union Méd.*, No. 30.

On the Treatment of Anthrax.—M. Richet, in a recent clinical lecture, protested against the opinion of those who regarded this disease as of a malignant nature, and either advocated its entire extirpation, or the application of the cautery. Still, there are two forms of anthrax—the simple and that which is complicated with diffuse phlegmon, this last being a very serious, although not a "malignant" disease. So, too, there are two very opposite conditions of the system under which the disease is developed, one in which the individual is in the extenuated and exhausted state due to extreme misery; and the other where, surrounded by good cheer, he exhibits an exuberance of health. This is why the Academie de Médicine and Société de Chirurgie take such different views of the disease; for while the young surgeons of the latter body have for the most part only seen the disease in hospitals, the members of the Academy, in the enjoyment of large practices, have had full opportunity also of seeing it in civil life.

M. Richet disapproves of the practices which he designates as "ferocious," consisting in the ablation of the tumour, or cauterising it. As to the employment of iodine or the perchloride, it is a detestable practice, giving a varnish to the surface, which prevents the issue of the purulent matters. The subcutaneous incisions advocated by M. Guérin are ineffective, while M. Velpeau's were needlessly enlarged. M. Nélaton's practice of cleaving the tumour into six or seven segments, separated like the petals of a tulip, is a much better one. As long as the anthrax is simple and confined to the skin, M. Rochet does not interfere; but as soon as the subcutaneous cellular tissue is invaded, he incises like Nélaton, taking care to cut through this tissue to the muscles, which should be felt by introducing the end of the finger between the lips of the incision. Sometimes a true hæmorrhage follows; but gentle compression, made by a pledget of charpie introduced into the wound, arrests the bleeding in a few minutes. Afterwards cataplasms and simple baths are ordered. The general treatment is very important: for the enfeebled and wretched wine and bark are indicated; while in the robust a venesection and a purgative, and perhaps an emetic, are called for.—*L'Union Méd.*, No. 38; and *Gaz. des Hôp.*, No. 41.

Gonorrhœal Rheumatism affecting the Hip-joint.—In an interesting case of gonorrhœal rheumatism, M. Richet dwelt upon the diagnostic marks of the hip-joint implication. The pain affected the whole limb, and the knee was exquisitely tender, but M. Richet pointed out as signs of coxalgia being especially present, the fact that the patient could not raise the limb, which remained in a state of forced rotation outwards, the leg being slightly flexed on the thigh. Then the hollow of the groin was effaced, pressure there was very painful, and the pulsations of the vessels, raised by a notable amount of tumefaction, were superficial. This symptom, he observed, although not mentioned by authors, is of the highest importance. It is here that exploration in diseases of this joint should be carried on, for in the region of Scarpa's triangle in the hollow of the groin, the joint is only separated from the skin by a thin muscular layer, a layer of subcutaneous cellular tissue, and the femoral vessels. The remaining soft parts surrounding the joint are also tumified; and such tumefaction can best be ascertained between the ischium and great trochanter in the notch which lodges the great sciatic nerve. This point and the inguinal hollow are the two regions where the articulation is most superficial. What was the cause of the tumefaction observed? It could not arise from effusion into the joint, distending the capsule and the soft parts, for this fibrous capsule offers a considerable resistance, and is not easily distended. The tumefaction was due to the inflammatory congestion of the tissues surrounding the joint, a congestion which explains the elevation of the inguinal hollow, and of the furrow separating the ischium from the trochanter. Such tumefaction would have sufficed for the diagnosis, even without the pain produced during the movements of the joint, especially on rotation outwards and abduction. The limb was measured, and neither elongation or shortening was detected. M. Richet has never in such cases met with elongation due to a propulsion of the head of the femur by fluid effused into the joint."—*L'Union Médicale*, No. 53.

On Forcible Compression of the Knee in Hygroma and Hydrarthrosis.—Professor Volkmann for the last ten years has employed compression in the treatment of hygroma of the patella and chronic effusion into the knee-joint, and has found it of advantage in proportion to the amount of pressure he made. Professor Billroth has also for some years adopted the same procedure, with a like beneficial result. This procedure is the same whether the bursa patella or the knee-joint is concerned. A splint from three quarters to one foot in length, made of very smooth wood, and filled with padding is placed in the ham in order to protect the popliteal vessels from the pressure about to be exerted on the rest of the joint. This splint is bent at its middle, and therefore corresponds to the bend of the knee, to such an extent as to allow of the leg resting on the splint in a condition of moderate flexion in place of complete extension. The patient is much more sensitive to the compression when this is made upon the limb in a state of complete extension. The splint must be very carefully adjusted, the angle in the ham being so rounded off that the longitudinal portion forms a part of a very large circle.

The pad, too, must be so arranged as to prevent all pressure against the bare splint. This done the whole joint is enveloped as far as the splint extends, in a flannel or strong cotton bandage, every turn of the roller being applied with increased force. The bandage is re-applied every second day. The application causes a good deal of pain, and the first, if not the second night, is usually sleepless. The foot becomes swollen and cyanotic, and the patient complains of formication and is much discontented. The arteries of the foot, however, continue to pulsate, although often very feebly, and the inconveniences gradually diminish. How far we may venture to carry this obstruction and compression is a matter of personal experience, and at first most persons will be induced to loosen the bandage for fear of gangrene. But they will soon learn that uninflamed parts can bear an immense amount of pressure without any harm resulting.

As regards the results of his own experience, Professor Volkmann pronounces this means to be an almost unfailing and quickly operating remedy in simple chronic effusion into the bursa patellæ—housemaid's knee, that used to be when housemaids knelt. By simple hygroma he means where there is no thickening of the walls of the bursa, and when it does not contain free bodies. And this simple form is by far the commonest. Of fifty-eight cases of prepatellar or infrapatellar hygromata that he has treated in all but one the effusion was absorbed in from four to seven days, and in the smaller swellings, only two days were required. In no case has relapse occurred, which may be due to slight adhesive inflammation being induced by the compression. In all cases the author continues the forcible compression for at least two days after the total disappearance of the fluid, and orders a roller to be applied for some time after recovery.

The treatment has not been so completely successful in chronic effusion into the joints, the fluid often not completely disappearing, or returning again. Still it is the most efficacious means we possess, except the injection of iodine, which may have to be resorted to in obstinate cases. In some cases, however, it is attended with a remarkably rapid absorption, and even when it is insufficient alone, it is an excellent adjunct to simple puncture or the injection of iodine.—*Berlin Klin. Woch.*, No. 15.

On the Treatment of Rupture of the Ligamentum Patellæ.—M. Sistach of the French army, has founded an elaborate and useful essay on two cases of this accident, which came under his care, and the following are some of the conclusions he arrives at:—1. The efficacy of the inclined plane employed to the exclusion of all other means in these two cases, complicated the one with a transverse fracture of the patella, and the other with a complete detachment of a bony lamella of the tibia, seems to demonstrate the inutility of the various bandages and apparatus usually employed 2. The mode of reparation after these accidents is by a true regeneration of tendon which takes place by the same successive transformations witnessed after subcutaneous sections. 3. A good position of the limb, and its immobility continued until such regeneration is completed are necessary conditions. 4. During the first days after the accident, the gradual diminution of the tumefaction allows of a nearer approach of the ligament to the crest of the tibia, and even when all inflammation has gone, and the patella has resumed its normal position, no apparatus exerts any effect on the retraction of the ligament, while it may keep up or reproduce inflammatory action or induce atrophy of the limb. 5. The duration of the treatment entirely depends upon the amount of consolidation of the tendinous blastema; and any premature movements of the limb may lead to defective formation, elongation, or abnormal adhesions of the ligaments, and may be followed by loss of power of the limb. 6. In transverse fractures of the patella the diminution of the articular tumefaction also primarily induces the progressive approximation of the fragments. At a later period, the fractured surfaces are brought into immediate contact under the influence of retraction, probably produced by the surrounding fibrous tissues. 7. The plan of treatment now indicated obviates most of the causes to which the stiffness of the joint after this accident is attributable. 8. The regular and exact consolidation of the fracture in one of these cases, confirms the success attained by Professor Jarjavy in treating fracture of the patella by position, unaided by any bandage.—April, *Recueil de Mém. de Méd. Mil.*

SUMMARY.

Air Passages.—Bourdillot. Foreign Bodies in the Air Passages. (Gaz. Méd., No. 15. Tabular view of 300 published cases.)

Anchylosis.—Gross. Osseous Anchylosis of the Knee treated by Subcutaneous Intra-Articular Drilling and Disruption. (Amer. Journ. Med. Sci., April.) Details four successful cases, and refers to twenty-six other cases, four of which proved fatal.

Aneurysm.—Gosselin. Case of Diffused Aneurysm. (L'Union Méd., No. 43. From spontaneous rupture of an atheromatous popliteal artery in a patient æt. 68. Digital compression seemed to have effected a cure when the patient died from exhaustion. An interesting autopsy.)—Demarquay. Cirsoid Arterial Tumour. (Gaz. des Hôp., Nos. 30 and 32. Perchloride of iron successfully employed after preliminary ligature of the radial and ulnar.)—Guéniot. Case of Cirsoïd Arterial Tumour successfully removed by Ablation (Ibid., No. 39.)

Bone.—Ranvier. Description and Definition of Osteitis, Caries, and Tubercle of Bone. (Arch. de Physiologie, No. 1.)

Bronchocele.—Delore. *Goitre Suffocant.* (Bull. de Thérap., No. 5. Calls attention to a form of goitre described by Bonnet, which, though small, impedes respiration by being moveable, and passing behind the sternal depression during inspiration. A figure given of the instrument employed for transfixing the tumour before cauterizing it.)

Carotid Artery.—Pilz. Ligature of the Common Carotid. (Langenbeck's Archiv. f. Chir., b. ix, h. 2. A colossal paper, referring to and tabulating 586 published cases.)

Dislocation.—Schinzinger. Cases of Dislocation. (Prag. Viertelj, b. i. States that his plan of reducing dislocation of the humerus by forced rotation outwards has received much additional sanction from his own experience, and that of several distinguished surgeons.)—Demarquay. Case of Dislocation of the Sternum. (L'Union Méd., No. 47. Displacement of the first on the second portion of the sternum through a thrust from the pole of a carriage. Reduction and treatment accomplished by posture alone.)—Huguier. Luxation of the Foot forwards. (Arch. Gen., May).

Ear.—Schwartze. Artificial Perforation of the Membrana Tympani. (Arch. f. Ohren., 1867, No. 4.)—Bonnafont. Case of Exostosis obstructing the Meatus. (L'Union Méd., No. 64).—Grüber. Rare Form of Separation of the Membrana Tympani. (Allg. Wien. Med. Zeit., Nos. 15, 16, and 18.)

Encephalocele.—Ripoli. Congenital Encephalocele. (Bull. de Thérap., No. 7.)

Excision.—Bergmann. Cases of Excision of the Upper Jaw, followed by Plastic Operations. (Petersb. Med. Zeit., 1867, No. 8.)—König. Excision of the Knee. (Langenbeck's Archiv., b. ix, h. 2. Considers the relative advantages of amputation and excision after gunshot injury.)

Exostosis.—Sistach. The Nature of Subungual Exostosis. (Gaz. Méd., No. 15)

Eye.—Berlin. Foreign Bodies in the Vitreous Humour. (Arch. f. Opht., b. xiii, a. 2.)—Bergmann. Extraction with the Capsule. (Ibid.)—Classen. Inflammation of the Cornea. (Ibid.)—Von Graefe. Removal of the Lens in Modified Linear Extraction. (Ibid.)—Knapp. Report on a Second Hundred of Cases of Linear Extraction. (Ibid., b. xiv., a. 1.)—Wecker. Parallel and Criticism of Operations for Cataract. (Annales d'Ocul., March.)—Cohn. Injuries to the Eye among Metal-workers. (Berlin Klin. Woch., No. 8.)—Fano. Case of Living Filaria in the Vitreous Humour. (Gaz. Heb., No. 13.)—Wecker. Emboli of the Vessels of the Retina and Optic Nerve. (Gaz. Heb., No. 19.) Von Hasner. On the Construction of a New Artificial Eye. (Prag. Viertel, b. 2, with Woodcut.)

Fistula in Ano.—Borelli. New Method of Operating in Fistula Ani, when situated high up. (Gaz. Méd. di Torino, No. 17.)

Fracture.—Burlingham. Compound Fracture of the Sacrum. (Amer. Journ. Med. Sci., April. Urine was discharged through the wound. Recovery).—Champenois. Diagnosis of Fractures at the Upper End of the Radius. (Recueil de Méd. Mil., March.)—Volkmann. On the loss of Pronation and Supination after Fracture of the Forearm. (Berlin. Klin. Woch., No. 18.)

Hæmorrhage.—Horteloup. Treatment of Hæmorrhage of the Hand. (Gaz. Hebd., No. 13.)

Hare-lip.—Broca. Application of Osseous Suture in double Harelip complicated with projection of the Intermaxillary Bone. (Bull. de Thérap., No. 10, and Gaz. des Hôp., Nos. 53 and 54.)—Sedillot. On the same subject. (Gaz. des Hop., No. 56.)

Hernia.—Marie. Case of Obturator Hernia easily reduced. (L'Union Méd., No. 59.)—Wimmer. Strangulated Hernia in Children. (Ploss, Zeit. f. Med., No. 2. Analysis of forty-eight cases, chiefly German, already published.)—Doutrelpont. Operation for Hernia without opening the Sac. (Langenbeck's Arch., b. ix, h. 2. Has met with twelve cases, all but one recovering. Gives the history of the operation without mention of the name of the Luke.)

Hospital Gangrene. — Lewandowsky. On Hospital Gangrene. (Deutsche Klin., Nos. 14 and 15. Gives an account of an outbreak in one of the Prussian Military Hospitals. There occurred thirty-seven cases among 400 soldiers, and all recovered.)

Joints.—Volkmann. Treatment of Diseases of the Joints by the aid of Weights. (Berlin Klin. Woch., Nos. 6, 7, and 8. The author confirms, by additional experience, the accounts of the good results from this procedure which he formerly published. Its effects in the relief of pain, and contraction of muscles are very remarkable; and it also prevents the ulcerative process which results from prolonged contact of diseased surfaces.)—Charcot. Arthropathies dependent on Lesions of the Brain and Spinal Cord. (Journ. de Physiologie, Nos. 1 and 2.)

Laryngoscope.—Turck. Laryngoscopic Communications. (Allg. Wien. Med. Zeit., Nos. 1, 3, 4, and 5, with woodcuts. Published not long before his lamented death.)—Hohl. A new Laryngoscope. (Deutsche Klin., No. 1, with woodcuts.)—Tobold. Fixation of the Epiglottis. (Ibid., No. 3.)—Schrötter. Contributions to Laryngoscopic Surgery. (Med. Jahr. d. Wien. Ges., No. 1.)—Fournié. Case of Polypi of the Larynx and Trachea, and Tumour of Pharynx. (Gaz. des Hop., No. 56.)—Rossbach. Constriction of the Larynx from Adhesion relieved by Operation. (Langenbeck's Arch., b. ix, h. 2, with illustrations. States that there are only twelve other cases on record.)

Ovariotomy.—Dittel. Case of Successful Ovariotomy. (Allg. Wien. Méd. Zeit., Nos. 4 and 7.)—Gusserow. Two Cases. (Berlin Klin. Woch., Nos. 12 and 13. One fatal, with autopsy.)—Simon. Two Cases. (Deutsche Klin., Nos. 1 and 3. One recovered. In the other the operation not completed on account of adhesions, and at the autopsy the case was found to be one of enormous hydro-nephrosis.)—Stilling. Three Cases. (Ibid., Nos. 3, 4, 7, and 11. Two fatal.)—Koeberlé. Nine Cases. (Gaz. des Hôp., Nos. 29, 33, 38, 47, 58, 63, 66, and 67. Four fatal.)—Scharlau. Two Cases at Stettin. (Monats. f. Geburt, Feb. One fatal.)—Maslowsky. Case of Double Ovariotomy with Recovery. (Langenbeck's Arch., b. ix, h. 2. Illustrations of the new instruments employed. He gives an historical account of the operation in Russia.)

Penis.—Saurel.—Treatment of Phymosis by Dilatation. (Gaz. des Hôp., No. 31. Woodcut of the dilating forceps employed.)—Bourguet. New Mode of Amputation of the Penis, with prevention of Atresia. (Bull. de Thérap., No. 8, and Gaz. Hebd., No. 15.)

Prothesis.—Hermann. Mechanism of Progression with Artificial

Legs. (Prag. Viertel, B. 2. Description of a new artificial leg, with numerous woodcuts.)

Syphilis.—Sigmund. Report on the Syphilitic Wards of the Vienna Hospital. (Deutsche Klin., Nos. 1, 5, and 6. He insists that syphilis is much on the increase, and that it is very urgent that good syphilitic clinics should be established in all medical schools.)—Sigmund. Primary Syphilis of the Mouth and Lips. (Wien. Med. Woch., Nos. 9 and 19. Sigmund says that cases of this description are increasing in number. During seven years he has met with seventy-three examples among 5551 syphilitic patients.)—Desprès. Phagedænic Chancre of the Anus and Rectum. (Arch. Gen., April. An interesting essay founded on seven cases at the Lourcine.)

Tracheotomy. — Hasse. Twenty-six Cases of Tracheotomy in Diphtheria. (Berlin Klin. Woch., Nos. 1 and 5.)

Tumours.—Forget. On Odontoma, or Dental Tumours. (L'Union Med., Nos. 50 and 60.)—Richet. Diagnosis of Tumour of the Groin. (Ibid., No. 30.)

Urinary Organs.—Liégois. Case of Urethral Calculus. (Gaz. des Hop., No. 37.)—Leudesdorf. On a new Lithotome. (Langenbeck's Arch., b. ix, h. 2.)

REPORT ON MIDWIFERY

By Robert Barnes, M.D. Lond., F.R.C.P.,

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I. Pregnancy.

1. *A Fallopian Gestation cured by Puncture.* By Dr. Edouard Martin.
2. *A Case of Tubo-uterine Gestation.* By Dr. Poppel.
3. *A Case of Abdominal Gestation.* By Dr. Dreessen.
4. *On the Recognition of the Seat of the Placenta before Labour.* By Dr. Pfeiffer.
5. *On the Ætiology of the Normal Position of the Fœtus.* By Dr. Cohnstein.
6. *A Case of Diphtheria of the Mucous Membrane of the Bladder and consequent Discharge of a Portion of the Mucous Membrane; Retroversion of Gravid Womb.* By Dr. Haussmann.

1. A woman, æt. 29, pregnant for the first time in January, 1867. On the 25th of February, after exerting herself in moving furniture she felt a sudden acute pain in the abdomen; a choking sensation and vomiting set in, with straining at stool and urine. The vagina was narrowed, the uterus anteflexed, the body enlarged, the os hard to reach in the hollow of the sacrum. Some days later along with considerable hæmorrhage a piece of decidua came away, and there was felt a small circumscribed swelling behind the abdominal wall projecting over the left horizontal branch of the pubes. The bleeding

returned profusely on the 16th of March, a fresh attack of pain having preceded. On examining now there was decided latero-version of the uterus, the fundus being pushed to the right, the os to the left. In the left side of the pelvis a spindle-shaped swelling was felt through the vaginal wall, and the same swelling was distinguished through the abdominal wall externally. Moving it caused acute pain. These symptoms and the decided growth of the swelling, led to the conclusions that it was an extra-uterine gestation, and that the further development ought to be arrested. Dr. Martin punctured the swelling with a fine trocar. A few drops of watery blood followed. Eight days later it was ascertained that the swelling had not increased; but a sharp rigor set in. Some days later the tumour was smaller, and the uterus had recovered its normal direction. On the 4th of May the patient was considered well.—*Monats. f. Geb.*, February, 1868.

2. Baart de la Faille has collected twenty-three cases of this kind. Dr. Poppel's is as follows; a woman, aged twenty-nine, had borne two living children. She menstruated on the 1st of May, 1867. On the 20th of June, she fell ill with severe abdominal pains. The uterus was found enlarged, and there was a small show of blood, suggesting abortion. Next morning the abdomen was greatly distended, fluctuation was plain. There was acute anæmia and deep collapse, although the hæmorrhage per vaginam had not increased. Fifteen hours after the onset of her illness, death ensued. The diagnosis was acute perforative peritonitis with internal hæmorrhage. *Autopsy.*—Much free fluid blood was found in the abdomen. The uterus was enlarged, of irregular shape; its right side was strongly arched at the fundus, and exhibited on its upper and hinder surface two irregular rents, through which portions of a placenta and of a fœtus protruded. The right tube was inserted a little higher on the body of the uterus than the left. On opening the uterus lengthwise two cavities were exposed. The lower one, which was the uterine cavity proper, was clothed by a thick decidua. The upper cavity was divided from the lower by a partition of muscular nature, but communicating with it by a hole admitting the finger; it contained a fresh dead fœtus corresponding to a development of five months. The cavity was clothed by ovum-membranes and placenta. A corpus luteum was found in the left ovary. It followed that there had been a transmigration of the ovum, but whether extra or intra-uterine could not be determined.—*Ibid.*, February, 1868.

3. A woman, æt. 35, had had three living children. Pregnant again, abdominal gestation was diagnosed by Professor Litzmann. She dated conception from the middle of June, 1860. Pains and hæmorrhage occurred about the middle period of the presumed pregnancy. The abdomen and breasts enlarged, vomiting often attended the pains. Fœtal movements were felt. She then got better; but in March, 1861, pains lasting a whole week, different from labour pains, and fœtal movements recurred. These movements grew more feeble, and ceased on the 30th of March. A sensation of

burning in the stomach remained, with severe vomiting of grass-green matter. Shortly before the death of the fœtus a discharge of blood took place from the genitals, and lasted ten weeks, during two of which it was accompanied with dark coagula, but no membranous shreds. After four weeks, the burning sensation was lost. She was now unable to exert herself. The circumference of the abdomen diminished after the death of the child. Menstruation did not return. The position of the fœtus was made out by external manipulation. At a later period menstruation returned. In October, 1866, the patient had been obliged to keep her bed on account of severe abdominal pain, and menstruation ceased again. In January, 1867, she suffered from frequent mucous diarrhœa, through which she became much emaciated. The encysted fœtus was felt as a hard knobby mass, the size of a four-year old child's-head in the left side of the abdomen quite immoveable. On internal examination the entire brim of the pelvis was found filled with a fixed, hard mass. The os uteri was pushed over towards the right pubic bone—much pain was caused on examination per rectum. She sank on the 1st of February, 1867. *Autopsy.* The cyst was found adherent by numerous points to the omentum. The bladder bounded it and was adherent to it in front. The uterus was not generally enlarged. The mucous membrane looked normal. The left Fallopian tube ran across the wall of the sac. The left ovary could not be discovered. The right tube was found free. The right ovary was also distinct. In the sac was found the remains of the fœtus macerated. The placenta could not be made out. The inner wall of the sac was covered with a smooth serous membrane. At the point of connection with the rectum there was a fistulous opening.—*Ibid.*, February, 1868.

4. Dr. Pfeiffer adverts to the fact that the seat of the uterine or placental souffle may sometimes be felt by the hand, a peculiar vibration or thrill being perceived. He then observes that under favourable circumstances, as thin abdominal and uterine walls, by applying the hands flat to the uterus one may find a smaller segment of a ball, as if seated on a large spheroid, and that this smaller mass has a peculiar stretched elastic consistence differing from that of the parts of the uterus which contain the child. This is the seat of the placenta.

Professor E. Martin, commenting on this, said he had subjected the proposition to repeated trial, and thought it required further observation.—*Ibid.*, Feb., March, 1868.

5. Dr. Cohnstein begins his memoir with a full historical summary of the theories hitherto advanced as to the causes which determine the position of the fœtus in uterô. His own conclusion is that the position is to be sought in the fœtus itself, not in the active or passive movements. It is the circulation-relations in the fœtus which until the end of the sixth month determine the breech-presentation, and after this period causing the substitution of gravity of the upper half of the trunk, cause the head presentation. This proposition

Cohnstein illustrates by the developmental history of the fœtus.—*Ibid.*, Feb., March, 1868.

6. Dr. Haussmann refers to other known cases of casting off of portions of the vesical mucous membrane, and relates the following:—A woman, aged 39, who had borne a child, was seized when three to four months pregnant with retention of urine requiring frequent use of catheter. The uterus was found retroflexed. This was relieved. The temperature and pulse had risen. The urine was at times alkaline, and showed cells of epithelium falling into decomposition, and vibriones. There was at times extreme pressure to void the bladder, which the patient could not satisfy. She several times passed the catheter herself. She recovered. A substance which at one time seemed to have choked the eyes of the catheter was passed, some blood following. This substance corresponded in size to about the fourth part of the area of the bladder. It had several small holes, most probably artificially produced, but one of which possibly answered to the opening of a ureter. One surface was tolerably smooth, the other surface showed numerous beam-like processes, which penetrated the entire membrane. It consisted of fibre and elastic fibres, many epithelial cells exactly like those of the bladder.—*Ibid.*, Feb., March, 1868.

The following contributions are referred to by the title only, on account of want of space:

A Case of Retroflexion of the Gravid Uterus.—Spontaneous Rectification. By Dr. Behm.—*Monats. f. Geb.*, April, 1868.

On the Mechanism of the Obstetric Forceps. By Dr. Dieterich.—*Ibid.*

II. Labour.

1. *Spondylolisthesis in consequence of Lumbo-sacral Caries.* By Dr. Blasius.

2. *On Osteomalacia.* By Dr. Casati.

3. *Pelvic Deformities affecting Labour.* By Dr. Casati.

4. *Obstetric Operations.* By Dr. Casati. Forceps, Craniotomy Cephalotripsy, Cæsarian Section, Induction of Labour.

5. *On the Symptomatology of Incomplete Rupture of the Uterus.* By Dr. C. Hecker.

1. Dr. Blasius describes the pelvis and lumbar vertebræ taken from the body of an insane patient. It was not known whether she had ever borne a child. She died at forty-nine, of phthisis. The pelvis was well formed, but the bones were porous and very fragile. The union of the basis of the sacrum with the last lumbar vertebra had undergone considerable destruction through caries; the upper anterior angle of the first false vertebra was completely destroyed, and in its place was a rough sloping surface. The last lumbar vertebra was in great part destroyed, and had slipped forward on the altered base of the sacrum so as to hang as a projection in the pelvic cavity. A year before death the patient had suffered from sacro-coxalgia, which was followed by psoas abcess; abscesses also appeared

in the vagina; and Dr. Koppe, examining during life, had felt an easily reached prominence in the vagina, which he took to be the lumbar vertebra slipped down.—*Ibid.*, April, 1868.

(This case seems a clear proof that spondylolisthesis is not always of congenital origin. The history of the disease a year before death explains the deformity; and congenital or even infantile anchylosis of the sacral joints invariably induces characteristic pelvic distortions and modifications of development.—R. B.)

2. In the report of the Milan lying-in hospital, for 1866, under the direction of Professor Lazzati, is an interesting case of osteomalacia. This disease appears to be unusually rife in the neighbourhood of Milan, and is attributed to extreme indigence and bad sanitary conditions. A woman, æt. 34, had three natural labours, but had begun to suffer some pains in the pelvic bones in her first pregnancy. These had seriously increased in her fourth pregnancy, and the pelvis was then deformed. Labour set in prematurely, and was ended by turning. The osteomalacia was suspended after delivery. Again pregnant three years later, the disease returned; and again labour came on fortunately at seven months, and ended easily. When pregnant for the seventh time, the pelvic pains and the distortion returned in an aggravated form, the whole skeleton being involved; so that the circulatory and respiratory organs were seriously impeded. There was intense dyspnœa, cough, difficulty of speech, the slightest movements caused excruciating pains, the face became cyanotic and livid, threatening suffocation. Although she was hardly six months gone, Professor Lazzati, moved more by the general condition than the pelvic deformity, punctured the membranes. Uterine action set in immediately, and the thoracic distress seemed aggravated by the effort of labour. A live child was born in thirty-eight hours. The patient died twenty-seven hours after delivery. The thorax was much compressed; the sternum projecting forwards, assumed the shape of a keel; there was passive congestion, and œdema of the lungs, concentric hypertrophy of the heart. The spinal column was distorted. The pelvis was triangular at the brim, and extremely contracted. As proof of the brittleness of the bones, there was a fracture of the right femur, caused by moving the patient in bed.

Dr. Casati points to the purely obstetric aspect from which osteomalacia is commonly regarded, and dwells upon the effects of this disease upon other parts of the skeleton besides the pelvis.—*Annali Universali di Med. Milano*, 1867,

3. During 1866, out of 478 women, 47 exhibited pelvic deformities. These are divided into three degrees. In the first degree the conjugate diameter of the brim measured from 3·3 in. to 2·9 in. This was always determined by the finger, and compared with external measurements with Baudelocque's compas d'épaisseur. The external conjugate almost always corresponded with the internal measurements taken with the fingers. In the women belonging to this series labour was completed in some naturally, in some by instruments, in some by induction of labour.

The defects of the second degree comprised cases in which the conjugate was reduced to 2·8 in. to 2·3 in. Six such cases were observed. In all labour was either instrumental or provoked.

Cases of the third degree included two only, and required Cæsarian section.

All the forty-seven cases, excepting three of osteomalacia, were due to rickets. All remembered to have begun to walk at three, four, five, or six years old; that when they began to walk alone they were seized with the disease called *acute fever*, which rendered them infirm. Most had lived in damp, unhealthy places, and had been badly fed.

It is interesting to observe under what indications and rules the forceps is used in labour. This instrument was resorted to twelve times in 478 labours. In nine to the head, at brim of the pelvis; in three when the head was in cavity. The blades were applied over the temporo-parietal regions of the head, so as to seize in the most reducible diameter, and keeping the pelvic curve directed towards that part of the fœtal head which had to be conducted under the pubic symphysis. The instrument used is that of Professor Lovati, preferred on account of its length, and its power of compressing the head, and thus avoiding turning or craniotomy. The range of pelvic deformity in which the forceps was used was from 2·9 in. to 3·2 in. conjugate diameter.

Three of the children were born dead; of the mothers, four died, namely, three of puerperal fever, one of eclampsia.

The mode of performing craniotomy was by means of Smellie's scissors, applied whilst the head was held in the forceps, which instrument afterwards served for compression and extraction; but sometimes, these slipping, Simpson's cranioclast was resorted to.

Cephalotripsy was performed with Depaul's instrument, being always preceded by craniotomy.

Cæsarian section was performed twice, both times on account of pelvic deformity of the third degree from rickets. Consecutive hæmorrhage occurred, and death the following day.

Induction of labour was performed in twenty-three cases; and it is remarkable, as evincing the care with which the patients are examined, that thirteen of these were primiparæ. In twenty cases the indication was pelvic contraction. The means employed were simple puncture of the membranes, sponge-tents, laminaria, and the elastic syringe. Fourteen children were born alive.

The proportion of still-born children was about eight per cent., which exceeds that which prevails in England.—*Annali Univ. di Med.*, 1867.

Dr. C. Hecker relates two cases of laceration of the uterus during labour, in illustration of a proposition submitted by him that, "When during a labour, which, perhaps, has already exhibited a suspicious character, a quickly increasing smooth elastic swelling forms in the anterior wall of the vagina, and which cannot be regarded as due to prolapsus or cystocele, the existence of an incomplete rupture of the uterus is highly probable." This is due to the collection of blood in the cellular tissue uniting the bladder to the cervix uteri; an extra-peritoneal ante-uterine hæmatocele.

Case 1.—A woman was in labour with her ninth child. The midwife felt no presenting part, but she perceived a peculiar swelling in the anterior vaginal wall. In the evening a copious hæmorrhage took place. The swelling persisted after the bladder was emptied by catheter. On passing the hand into vagina the head was felt; the pains were strong and regular; the countenance calm, but the pulse was over 120. Hecker felt a rent in the left side of the lower segment of the uterus. Turning was readily accomplished, but the head enlarged by hydrocephalus had to be perforated. After labour the pulse rose to 140. Delirium set in, followed by rapid collapse, and death thirty-six hours after labour. No autopsy.

Case 2.—A woman, in her eleventh pregnancy, had frequent hæmorrhages. The posterior lip of the os was carcinomatous. In labour the head presented. When the waters had escaped, a smooth elastic swelling was felt in the anterior vaginal wall. This was not affected by emptying the bladder by catheter. The patient sat upright, her legs hanging over the edge of the bed, and strained forcibly with the quickly-recurring strong pains. These were attended by hæmorrhage. Her countenance was tranquil, breathing but little hurried; but the hands were cold, and the pulse small and 156. She sank undelivered. The child was delivered by turning afterwards.

Autopsy.—The uterus was large and flaccid; a large extravasation of blood was found under the peritoneum, extending upwards to the right kidney. An incomplete rupture of the uterus was seen proceeding from the vagina. In the cellular tissue, between the bladder and uterus, was an extravasation of blood.

Hecker insists upon the occurrence of rapid small pulse as a sign of diagnostic value in incomplete as well as in complete rupture.—*Monats. f. Geb.*, April, 1868.

III.—The Puerperal State.

1. *A new Case of Fatty Degeneration in a Puerperal Woman.* By Dr. C. Hecker.
2. *Puerperal Diseases.* By Dr. Casati.

Referring to a previous case reported in this Journal, Dr. Hecker relates the following:—A woman, æt. 32, had been delivered, easily and without aid, of a mature living child, on the 13th July; was seized, twenty-eight hours afterwards, with shortness of breathing, and died suddenly. The body was well nourished. When examined, twenty-three hours after death, there was no trace of decomposition, of œdema, or of jaundice; but in the skin of the abdominal wall were numerous ecchymoses, which might raise suspicion that there was acute fatty degeneration. In the thoracic cavity was a yellowish transudation in considerable quantity. The lungs were sound, somewhat œdematous, and spotted with numerous sub-pleural blood-extravasations. The muscular structure of the heart was fragile, showing many ecchymoses under the endocardium.

Valves sound. No embolia in the pulmonary arteries. The liver was very yellow, not shrunken, soft, and fatty. The spleen was enlarged, rather hard, looking very fatty. The kidneys were plainly in the second stage of parenchymatous degeneration; the capsule very easily moveable, parenchyma swollen, cortical substance yellow. The uterus was well developed; the entire mucous membrane could be easily scraped off by the knife when its fibres were bared; there was a considerable blood-coagulum on the mucous membrane. In the cervix uteri, all round it was an enormous fresh blood-infiltration without a trace of loss of continuity.

The outer aspect of the peritoneum showed a bluish colour, which answered to a colossal mass of extravasations in the entire mucous membrane of the rectum. There was no blood in the canal of the rectum. The small intestine was empty; its mucous membrane pale, but containing in the duodenum numerous blood-effusions in the mucous membrane. Microscopic examination showed fatty degeneration very plainly in the heart, liver, and kidneys. The significance of this case, Hecker thinks, lies in the fact that its anatomical character shows an obvious transition-stage towards the peculiar acute yellow atrophy of the liver.—*Monats. f. Geb.*, Feb., 1868.

The experience of the Milan lying-in hospital is not such as to redeem the character of similar institutions in respect to the propagation of puerperal fever. Out of 478 labours there were twenty-three cases of puerperal fever, twelve of metritis, and twenty of "miliary fever." Thirteen ended fatally. This result is considered favorable, and credit is taken, and certainly due, to Professor Lazzati for the rigorous care he had exercised in isolating patients on the first appearance of illness, and in enforcing other hygienic measures. Owing to this care the hospital was preserved free from any epidemic during the first eleven months of the year. It is true that several sporadic cases occurred during the year, and that eight ended in death; but before December there was no spreading. In this month twelve cases occurred, of which five died. The rise of the epidemic is ascribed to the admission of a case, at the end of November, of a patient in labour, who had to be delivered by cephalotripsy. She was admitted in a state of extreme prostration with tympanitis. Labour had been in action for forty-three hours; and a dirty-brown, fetid liquid was discharged from the vagina. The fœtus had been dead some time. She died twenty-three hours after the operation, with symptoms of puerperal fever. Suppurative peritonitis was found. The influence of the zymotic or hospital miasm is seen in the history of the diseases attending the prevalence of undoubted puerperal fever. Dr. Casati says puerperal fever represents the prototype of all the zymotic diseases of puerperæ; that is, metritis, not permanent, inflammatory, mostly accompanied by miliaria, essential miliary fever, phlebitis of the lower extremities, represent simply a minor severity of the evil, but depend upon the same zymotic cause. These affections are found to prevail simultaneously with puerperal fever. When puerperal fever breaks out, these affections appear in the hospital. When the fever is driven out the affections described vanish. The same treatment had similar effects. The children born of women affected with puerperal fever, or these cognate diseases, were equally liable to sicken and die, or to be born dead.—*Annali Universali de Medicina, Milano*, 1867.

IV. The New-born Child.

1. *On the Point of Insertion of the Umbilicus in the Different Months of Gestation.* By Dr. C. Hecker.

2. *Asphyxia of a New-born Infant caused by congenital Struma.* By Dr. C. Hecker.

1. Dr. C. Hecker, having measured many fœtuses at different stages of development, draws the following conclusions:—The seat of the umbilicus is never lower than in the lower third of the space between symphysis pubis, and the extremity of the xyphoid cartilage. It is so low as this only in the third month. From this time we find it gradually rise until the sixth or seventh month, from which time there is no remarkable change, the proportion of 1 to 1·6 being maintained.—*Monats. f. Geb.*, Feb., 1868.

2. A child was born normally, and cried several times, when suddenly, after the tying of the cord, it died in spite of energetic restorative means. The thyroid gland was remarkably enlarged, and flattened the trachea against the œsophagus. Its tissue was sound, in state of pure hypertrophy. The lungs were partly emphysematous.—*Ibid.*

CHRONICLE OF PHYSIOLOGY.

By Henry Power, F.R.C.S., M.B. Lond.,
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Digestion.

1. R. Heidenhain. *Essays on the Saliva in the Stodien des Physiologische Instituts zu Breslau.* Heft iv, 1868, pp. 1-125.
2. F. Bidder. *On the Influence of the Sympathetic on the Parotid of the Sheep.* 'Reichert's Archiv. f. Anatomie,' 1867, No. 6.
3. P. Accolas. *Essay on the Origin of the Biliary Ducts.* Thesis. Strasbourg.

The experiments of Heidenhain were chiefly made upon the sub-maxillary glands of the dog, but also on those of the rabbit and sheep. Nearly 120 animals were experimented upon. He considers that considerable differences in structure, corresponding to differences in function, may be traced between different salivary glands in the same animal, and in the same salivary gland of different animals. The sub-maxillary gland of the *rabbit* is more simple in structure than either that of the dog or of the sheep. The cells in the alveoli are soft and ill-defined, darkly granular structures; they possess a nucleus, and this alone becomes coloured with the carmine solution. No difference is observable between those lining and those situated near the centre of the alveoli. The alveoli have no proper investing membrane, but are separated from one another by connective tissue, in the meshes of which are a few lymph-corpuscles. All micro-chemical reactions show that the cell protoplasm is rich in albuminous compounds; but no evidence can be obtained of the presence of mucus. The applications of weak acids renders the protoplasm turbid. Stronger acids, in addition to this, cause it to shrink. Strong acetic acid occasions considerable enlargement, and renders it transparent.

The submaxillary gland of the dog is a *mucus gland*, in which a continuous development of mucus-cells occurs, which break down to form the mucus, whilst their place is supplied by others. On superficial examination, no difference at first appears between the cells contained in the alveoli and those already described as occurring in the rabbit; but on the application of the carmine solution the greater part of each acinus is found to contain large transparent (not granular, as in the rabbit) cells, which remain uncoloured, though the nucleus becomes red; and besides these, at one and sometimes at two spots of the circumference of the alveoli, a semilunar or sickle-shaped mass of darkly granular material may be seen, the presence of which was first described by Giannuzzi. This becomes deeply coloured with carmine, and is composed of cells in close contact with one another. The distinction between the two sets of cells is still more distinct in the *cat*. The clear central cells of the alveoli of the submaxillary gland of the dog are, for the most part, of pear- or club- shape, possess a distinct investing membrane, and one or two highly refractile prolongations, which become tinted with carmine, chloride of gold, or osmic acid. In no instance, even with the greatest care, was Heidenhain able to trace a connection between the nucleus of the cells and these prolongations. The contents of these cells were poor in albuminates, and consisted almost completely of mucus; and in this respect, as well as in their distinct cell-wall, flattened nucleus, and caudate prolongations, formed a marked contrast to the cells in the interior of the alveoli of the same gland in the rabbit. Acetic acid precipitates the contents of the mucus cells in every degree of concentration. Mineral acids, when dilute, precipitate them, but redissolve them when concentrated. The "marginal cells" constituting the semilunar mass of Giannuzzi are ill defined, and contain material rich in albuminous compounds, with a round nucleus.

The submaxillary gland of the *sheep* presents two cell forms in its acini, but the granular albuminous cells are relatively far more numerous than the clear mucus-cells.

The submaxillary gland of *man* agrees with that of the dog in its structure, and is, therefore, essentially a mucus gland.

M. Heidenhain next enters into the consideration of the chemistry of that kind of saliva which is secreted under the influence of the chorda tympani nerve. The exciting agent employed was a magneto-electric instrument of ordinary size, rendered active by a small Groves' element. When the nerve was irritated by this means the secretion never continues to be discharged for any length of time, but after diminishing can, with a short pause for recovery, be again induced to take place with considerable activity.

The chorda-tympani saliva of the rabbit is thin and watery, decidedly alkaline, and no reactions indicative of the presence of mucin can be obtained from it. Its solid residue amounted to 1·239 per cent.

The chorda-tympani saliva of the dog contains mucin, and a very small proportion of albuminous compounds.

In the sheep the amount of proteids, in the same saliva, is much more considerable than in the dog, whilst the proportion of mucin is very variable; and these characters of the saliva in the different animals correspond with the structure of the glands as already mentioned.

An interesting result was obtained from the examination of this saliva in the dog, that the per-centage proportion of solid constituents diminishes with the duration and rises with the strength of the nervous excitation, the difference being chiefly noticeable in the organic constituents; hence it might be said that the latter are formed by the gland during the period of rest, and, at the moment of excretion, are dissolved in the fluids poured forth by the blood; and Ludwig so explained the phenomenon; but Heidenhain shows that the increase is really due to the fact that the rapidity of secretion of the solid constituents rises in a greater ratio than the secretion of the water, that the nature of the solid which really augments is mucin, and that consequently, instead of the diminution on protracted irritation of the nerves being due to an exhaustion of the gland of all its stored-up solids, it is rather due to exhaustion of the nerves, which are no longer able to act as excitors to the glandular elements.

Heidenhain proceeds to give a full and accurate account of the morphological elements of the saliva, amongst which he enumerates mucus-cells of the acini, peculiar transparent spherical bodies of variable size, and proper salivary corpuscles, possessing the well-known characters of amœboid cells. He has not found that the latter occur in greater numbers on excitation of the sympathetic than on irritation of the chorda tympani.

After continuous excitation of the chorda-tympani remarkable changes occur in the substance of the submaxillary gland itself. It becomes softer, its per-centage proportion of water increases, and its solid constituents diminish.

We have not space to follow Heidenhain closely through his experiments with the sympathetic; but some of the more important conclusions which he believes his experiments establish, may here be mentioned. Instead of admitting, with Eckhard, that excitation of the chorda tympani produces a secretion of saliva differing in kind from that secreted when the sympathetic is irritated, he believes that the difference is only one of degree. The saliva in both instances becomes more watery in proportion to the duration of the irritation, the quantity of mucus diminishing.

Both kinds of saliva become richer in salivary corpuscles as the irritation is continued. Both are indebted for their production to impulses which cause a greatly increased pressure of blood in the capillaries. The chemical and morphological changes are, however, greatest in the chorda-tympani saliva. On irritation of either nerve, increased development of heat occurs in the gland. Certain marked differences, however, do exist between the two kinds of saliva. That produced by irritation of the sympathetic is more concentrated, richer in solid constituents, than that obtained by irritation of the chorda-tympani. The morphological elements of the sympathetic saliva only appear for a short time after it is first excited; they then gradually diminish and disappear, and the quantity of saliva thus obtained is also less, whilst it contains more mucus.

Heidenhain was at first disposed to think this was to be explained by the action of the sympathetic on the vessels occasioning their extreme contraction; but this explanation is not satisfactory, since, if the current of blood be checked, and the chorda tympani excited, an abundant flow of blood still occurs. On the whole, he is disposed to admit that the formation of mucus and the secretion of fluid are associated, or simultaneously occurring processes, each of which is induced through the action of a special class of nerve fibres. Both sets of fibres, which for the sake of convenience may be termed respectively mucus-forming fibres and secretory fibres, are contained alike in the sympathetic and in the chorda tympani, but in unequal proportion. The chorda contains very numerous, the sympathetic very few secretory fibres, relatively, to the mucus-forming fibres. Heidenhain considers he has positively shown that, by excitation of certain nerves, processes and metamorphoses may be induced, which occasion lively cell formation.

2. Bidder found that on the application of a galvanic stimulus to the upper part of the cervical sympathetic detached from the vagus causes an immediate increase in the flow of saliva from Steno's duct in the sheep. According to V. Wittich ('Virchow's Archiv.' bd. xxxvii and xxxix), this is due to increased secretory activity of the parotid gland, whilst Eckhard ('Henle's and Pfeuffer's Zeits.,' bd. xxix, and Beiträge, bd. iv, heft ii, Giessin, 1867) considers it to be the result of the expression from the gland of the store of the saliva which has accumulated in the ducts. F. Bidder, however, is disposed to agree with V. Wittich from the results of his experiments on sheep, since he finds that galvanic irritation of the cervical sympathetic occasions not only an increased flow of clear and transparent saliva from Steno's duct, but causes a much greater quantity of blood to traverse the gland, whilst the pressure of the blood in the veins is augmented, and the veins themselves pulsate.

3. Accolas agrees with Morel in believing that the biliary ducts end in blind extremities at the surface of the acini of the liver. The investigations of Turner, Eberth, and Kölliker, however, substantiate Hering's view that the biliary ducts appear as an intercellular network in the acini.

Blood Circulation.

1. Dr. N. Friedreich. *On the Life History of the Blood.* ('Virchow's Archiv.,' 1868, bd., xli, p. 395.)
2. Dr. E. Metschnikow. *On the Development of the Red Corpuscles of the Blood.* ('Virchow's Archiv.,' 1868, p. 523.)
3. Theodore Ainser and Adolphe Lohe. *Researches on the Duration of the Circuit of the Blood during Excitation and after Section of the Vagi,* ('Henle and Meissner's Zeits. f. Rat. Med.,' bd. xxxi, 1868, p. 33.)
4. Dr. Lannelongue. *Researches on the Circulation in the Walls of the Heart.* ('Brown-Sequard's Archives de Physiologie,' 1868, vol. i, p. 22.)
5. Felix Guyon. *On the Arrest of the Circulation in the Carotid Artery during Prolonged Muscular Exertion.* ('Brown-Séquard's Archives de Physiologie,' t. i, 1868, p. 56.)
6. M. C. Legros. *Note on the Epithelium of the Bloodvessels.* ('Robin's Journal de l'Anatomie,' No. iii, 1868.)

1. M. Friedreich observes that the occurrence of contractile movements has of late years been observed in the constantly widening circle of elementary morphological elements, so that it might almost be regarded as a common characteristic of organic substances, yet that the red blood corpuscles in this respect occupy a somewhat exceptional position, since no movements have been observed to occur in them under ordinary circumstances even by the most practised microscopists. He then relates a case apparently of albuminuria in which the red corpuscles discharged with the urine presented at a temp. of 12° R. = 59° Fahr. singular and various forms, underwent division, threw out amœba-like processes, after which the mass of the corpuscles travelled; and which movements persisted for no less than fourteen hours after withdrawal from the body. Preyer ('Virchow's Archiv.,' Bd. xxx, 1864, p. 426), Rollet ('Sitz. d. k. Akad. zu Wien,' 1864), Beale ('Trans. of the Micro. Soc.,' xii, 1864, p. 36) had all observed similar phenomena: Preyer with a moist chamber, Rollet with electrical currents, and Beale with a temperature of 100° Fahr. Kolliker and Kneutinger had seen similar effects on the application of a solution (8—11 per cent.) of urea to blood corpuscles, and to the presence of this substance Friedreich is inclined to attribute the movements he observed. He noticed remarkable deviations from their usual form in the corpuscles of a leukæmic man, that could only be explained on the corpuscles possessing a certain contractility, which after careful examination, he actually detected. On the whole, he thinks that contractile movements, though, perhaps, only slight, will hereafter, on further examination, be found to occur naturally in the red-blood corpuscles. That the red corpuscles of the blood possess a certain amount of contractility is a fact so often proved in England that one wonders what Professor Freidreich is driving at. Many instances of apparent contraction are merely the effect of an altered condition, as of evaporation of the liquor sanguinis, in which the corpuscles swim in the minute drop of blood under the microscope; the corpuscles may be often seen

altering their shapes, as Mr. Gulliver has figured during the examination and particularly at last.

2. No one, says M. Metschnikow, has as yet furnished proof that the so-called nucleus of the red corpuscles in birds and in cold-blooded vertebrata is really a structure analogous to the nucleus of a typical cell, as for instance, an embryonal cell. The necessity for such a proof has become so much the more pressing since a great difference exists between the nuclei of the blood corpuscles and the Furchungskerne of these animals, and secondly, because the nucleus of the adult red corpuscle has scarcely anything analogous with that of the youngest blood-cells of the chick depicted by Remak. In the hope of obtaining sure standpoints for determining the significancy of the several parts of the blood corpuscle, he followed the stages of their development in fowls, which were the animals most at his disposal. The following are the results he obtained:—On the third day of incubation the blood corpuscles possess an irregular form. They are clearly devoid of an investing membrane, but contain in their interior a round perfectly clear and transparent nucleus, with a few very small corpuscles (nucleoli). These refract the light more strongly than the nucleus, but show generally the peculiarities of cell contents. The corpuscles present a faint yellowish coloration. The protoplasma containing only very few of the granules, exhibits contractile movements, sending out pseudopodia at various points.

These blood-cells undergoing movements were first observed by Max Schultz, and increase by division (fission) as was long ago shown by Remak.

On the fourth day of incubation, the movements of the corpuscles are shown, and their form becomes consequently more determinate. At a further stage of development they become more or less oval, presenting a close similarity to the fully formed corpuscles, but he was unable to convince himself of the presence of a cell wall.

On the sixth day of incubation, the blood-corpuscles cease to exhibit any movements at all. The nucleolus increases in size, and sometimes assumes an oval form.

On the twelfth day, if the corpuscles have assumed their usual flattened oval form, the nucleolus almost completely fills the nucleus, and this is still more apparent on the fourteenth day.

On the sixteenth day the previously visible boundary line of the nucleus vanishes, the blood-corpuscles assuming coincidently their characteristic features.

The result of this frequently and easily repeated investigation is that the so-called nucleus of the corpuscles is not a nucleus at all, but an an enlarged and persistent nucleolus, which proves again that the nucleolus may play as important a rôle in the cell formation as the nucleus and weakens the generally received view of the occasional absence and general unimportance of the nucleus.

3. The experiments of MM. Ainser and Lohe were made on dogs, and in Hering's method as modified by Vierordt, which consists in the injection of a small quantity of a 2 per cent. solution of ferrocyanide of

potassium. Each proof quantity of blood taken corresponded to 0·6 of a second in point of time. The total duration of the circuit varied from 14 to 59 seconds. The injection was thrown into one jugular, and the blood taken from the other jugular. Under normal conditions they found the duration of the circuit about 18 or 19 seconds, the heart beating about 24 or 25 times. When the vagus was excited with an interrupted current of electricity, for about 10 or 12 seconds before the injection was made, so that the hydraulic conditions produced by its excitation were established, the pulse fell in one instance from 76 to 50, whilst the duration of the circuit rose from 30·6 seconds to the number of the beats of the heart, being therefore 25·6. In two other cases the time of the circuit rose to 63·9 and 59·6 seconds. Only one experiment was made on the effects of section of the vagi, and it was then found that the normal pulse was 102 and the respirations 28 per minute, whilst 10 minutes after the section the pulse was 231, the respiration 15, and the duration of the blood circuit 17·32 seconds, not, therefore, materially different from its duration in a healthy animal.

4. Dr. Lannelongue commences his essay by remarking that the circulation of the walls of the heart is not accomplished in quite so regular and uniform a manner as is generally supposed. There is a period antecedent to the contraction of the several cavities, during which there is an increased quantity of blood in the vessels of the muscular tissue. When the contraction occurs the vessels are emptied of their contents; it is obviously necessary that no obstacle should exist to the return of the blood, and in order to facilitate this there exist "canals of derivation" proper to the auricles which transmit by several channels at once all the blood they have received from the auricular arteries. The ventricular arteries are deeply situated and tortuous, are covered by the cardiac veins, and often dip into the muscular tissue before their size is much diminished. On the contrary, the auricular arteries are superficially placed beneath the pericardium; they give off only capillary branches to the muscular tissue of the auricles, they run in a straight direction, and are unaccompanied by any vein. The arterial distribution of the heart is very constant, but the venous is singularly variable. That of the ventricles is totally distinct from that of the auricles, and it presents, moreover, some peculiarities that distinguish it from every other part of the economy. As regards the veins of the ventricular walls, all discharge themselves into the right auricle, and all, with the exception of the veins of Galen, which, receiving the blood from the right border of the heart, open by one or two orifices into the right auricular appendix, converge to the great coronary sinus. With the exception of the valve of Thebesius at the mouth of this last, there are no valves in the ventricular venous system. The veins of the auricular walls, with the exception of a few that discharge themselves into the left auricle, open into the right auricle, but they present marked peculiarities in their course, structure, and orifices. On examining the interior of the right auricle, amongst numerous smaller, three larger foramina are conspicuous by the constancy of their position; one of these is situated just below the opening of the superior vena cava, a second exactly opposite the opening

of the coronary vein, and the third is situated in front of the left extremity of the auricle. The orifices of these channels are obliquely placed, and one border is guarded by a kind of valvule formed by the projection of the endocardium, and an incomplete ring of muscular fibres. On passing a probe from the interior of the auricle through the opening it enters at once into a large funnel-shaped cavity, which receives a few small veins, and the outer surface of which is covered by muscular tissue, whilst on its internal face it presents the orifices of certain vessels termed "canals of union of the Foramina," or intermuscular channels, which course through the fleshy fibres, separated from the endo-cardium by one and sometimes two muscular planes; the calibre of these is not everywhere the same, and as they pass along they receive numerous small veins. The object of these "canals of union" appears to be to prevent any hindrance to the circulation of the auricle during its alternate changes from fulness to emptiness, as they receive nearly all the small veins of the auricle. The arrangement of the muscular fibres in their vicinity is peculiar since the fasciculi are at first parallel to them, but subsequently are arranged more and more perpendicularly, so that the contraction of the muscle tends to separate their walls, an arrangement that is only elsewhere found in the uterus. The walls of the canals of union are composed of three layers: an internal epithelial layer; then a layer composed of four or five series of broad flattened nucleated cells, imbedded in a fibrillar matrix; and an external layer of ordinary connective tissue, with much elastic tissue; but it does not appear that any muscular tissue enters into the formation of any of the coats; they are therefore passive. It appears from all this that the venous blood of the auricular walls discharges itself into the canals of the auricles by two methods. A small portion enters it directly by small veins; the greater portion discharges itself into the canals of union of the foramina which are open at both extremities, terminating at both ends in a venous ampulla. Since the contraction of the auricles and ventricles succeed one another it remains to be asked what is their influence on the venous circulation. Muscle in contraction becomes anæmic, less blood entering by the arteries, and more being delivered by the veins: hence, as regards the ventricles, their contraction would effect the repletion of the coronary veins, and the blood which courses through them finding the auricle relaxed, enters it with ease. During the diastole of the auricle the blood coming from the walls of the ventricle will mingle with the venous blood of the body generally, but the venous blood of the auricular walls is placed under conditions that are essentially different in consequence of the peculiar anatomical arrangements of the part. It cannot here enter during the diastole, since at that moment the walls of the auricle are soft and flaccid, effacing the calibre of the vessels, offering a real passive resistance, rapidly augmented by the internal pressure exerted by the rising column of blood which, when the auricles are full flattens the canals, and obstructs their auricles. Now follows the contraction of the auricle, and in consequence of the adherence of the walls of the vessels to the muscular fibres, the contraction of the latter will shorten and dilate the vessels, and cause their orifices to open. Consequently it is during the systole of the auricle that the blood coursing through its parietes will empty itself into the cavity, just as the venous blood of the ventricles is expelled an instant afterwards during the contraction of the ventricular walls. The following table shows the concomitant phenomena:—

Ventricular systole	Anæmia of the ventricular wall. Repletion of the auricular vessels.
Auricular systole .	Anæmia of the auricular wall. Repletion of the ventricular vessels.

5. M. Guyon has made the remarkable observation that during violent and prolonged effort, the circulation in the carotid artery undergoes quite a peculiar arrest; the pulsations in any of its branches, as, for instance, the temporal or facial becoming at first more feeble and more rapid, and finally ceasing altogether; when the effort ceases, and a full inspiration is made, the pulsations of the artery return, but their re-appearance is not immediate, and the first pulsations, though sufficiently full are rapid and a little irregular in rhythm. If, during the same effort, the pulse in the radial be examined, it will be found to be feeble and hurried, but it does not altogether cease. On examining the pulsations in the temporal and radial during parturition, he found that so long as the pains were accompanied by cries little or no effect was produced either as regards the force or the frequency of the pulsations but that as soon as the pains were violent and prolonged, and no cry was uttered, suspension of the pulsations in the temporal occurred in one instance for four seconds, in an effort which lasted ten seconds, and in another for seven seconds, in one that lasted fifteen or sixteen seconds, though during this period the pulsation in the radial was only hurried and feeble. In yet another case when two efforts succeeded one another rapidly, the arrest of the carotidean pulsation occurred almost at the commencement of the second effort, and persisted no less than twelve seconds. In endeavouring to determine the cause of this remarkable phenomenon, his attention was at once directed to the thyroid gland, the anatomical relations of which to the carotid had already been carefully described by M. Maignien, and all his observations tended to refer the arrest to this vascular gland; for, in the first instance, it is in contact with the artery for above half an inch. It is also covered by muscles by which it can be compressed against the artery. Both its arteries and veins are very large; its volume varies greatly under different circumstances, and finally its large size in women, and especially during pregnancy, is well known. Moreover he found that mere cessation of respiration produced no effect on the arterial beats. As regards the utility of this arrangement, he suggests that inasmuch as in all effort the discharge of blood from the head is more or less obstructed, the arrest of the carotid circulation by the repletion of the thyroid gland, and its compression against the artery, is a special provision to cut off the supply of arterial blood to the brain, and thus to prevent what would otherwise certainly occur, the over-distension of the cerebral vessels.

6. M. Legros recommends the use of solution of gelatine, to 400 parts,

of which one part of nitrate of silver has been added in order to demonstrate the epithelium of the blood-vessels; when the blackening effect has been produced the preparation should be subjected to the action of hypo-sulphite of soda, and finally washed out with distilled water. The details are then beautifully seen. The epithelium is of the pavement variety, as is also the case in that lining the air-sacs of the lungs; the cells are very elastic, with sinuous outlines, but of various form, usually more or less elongated; lozenge-shaped, or fusiform in arteries, and shorter and broader, with better-marked angles in veins, and, in both instances, all or nearly all possess a nucleus. In the epithelium lining the lymphatic canals, the cells do not appear to possess a nucleus. He states that in the normal condition there is no desquamation, the old cells disappearing by molecular absorption; but he has not been able to ascertain how the cells are developed.

Nerve.

1. Brown-Séquard. *On the Immediate Arrest of Violent Convulsions by the Influence of Irritation of certain Sensory Nerves.* ('Archives de Physiologie,' t. i, 1868, p. 156.)
2. V. Wittich. *On the Rapidity with which Impressions are Propagated by the Sensory and Motor Nerves in Man.* ('Henle und Meissner's Zeits. f. Rat. Méd.' 1868, bd. xxxi, p. 87.

1. M. Brown-Séquard states that he has met with several cases which presented features in common with those described in the following case. A young American was affected with paraplegia dependent upon an inflammation of the spinal cord at the level of the fifth or sixth dorsal vertebra. The lower extremities were completely destitute of sensibility, and there was no power of voluntary movement in either of them; but the slightest touch applied to any part of the skin of these parts was sufficient to bring on a violent tetanic spasm in the muscles, in which the legs were so rigidly extended that no efforts of M. Brown-Séquard and of his servant were capable of flexing the joints of the ankle, knee, or hip. The servant, however, discovered that if the great toe were grasped, and forcibly flexed, the whole limb became supple, and passed into the condition which follows death, and precedes rigor mortis. In this state the patient remained for a length of time sufficient to enable him to be dressed; but if too long a time were allowed to elapse a fresh attack occurred when the stockings were drawn on, which could again be reduced by forcibly flexing the great toe.

2. V. Wittich describes an apparatus he has constructed for this purpose. For the sensory nerves he found the rapidity of propagation to be on the average 41·3 meters in one second when electrical excitation was used, and 37·56 meters when mechanical excitation was employed, whilst for motor nerves the mean rapidity was 30·3 meters in one second. As regards the physiological time occupied by the eye and ear, *i.e.*, the time that intervenes between the occurrence of a spark or a sound, and the voluntary movement required to open or

close an electrical current he found the mean time through ear to brain and down to hand was 0·179 sec., the maximum being 0·199 and the minimum 0·1625 sec. The mean time occupied for the perception and registration of the occurrence of a spark was 0·194, maximum, 0·223 secs., and minimum 0·163. For the nerves of taste mean time 0·167 sec. The following table gives the results of some other observers, the numbers representing the physiological time in seconds:—

	For Sight.	For Hearing.	For Common Sensation.
Hirsch	0·2	0·149	0·182, hand.
Hankel	0·2057	0·1505	0·1548, hand.
Donders	0·188	0·18	0·154, neck.
Wittich	0·194	0·182	0·1301, forehead.

That the perceptions obtained by means of the auditory should be capable of being registered more quickly than those received through the optic is not surprising, since the course of the former is much shorter than that of the latter, but the great rapidity of the transmission of impulses through the skin or ordinary sensory nerves as compared with the nerves of special sense is remarkable. By subtracting the physiological time for auditory impressions from those for optical impressions, an approximation may be obtained of the rapidity of the passage of impressions through the optic nerves and their amounts:—

In Hirsch's experiments to	0·051 sec.
„ Hankel's „	0·0552 „
„ Donders's „	0·008 „
„ Wittich's „	0·012 „

which gives a rapidity in the optic with—

Hirsch of	1·156 meters per sec.
Hankel „	1·068 „ „
Donders „	7·375 „ „
Wittich „	4·916 „ „

But V. Wittich thinks his estimate and, *à fortiori* Donder's, is too high.

Muscle.

W. Koster. *The Determination of the Maximum of Force in a Living Muscle.* ('Archives Néerlandaises,' 1867, p. 89.)

Whilst Weber in his well-known essay on muscle found the force not to equal more than 2·2 lbs. for a square centimetre of transverse section of a muscle, Knorz ('Henle und Pfeuffer, t. xxiv), found for the flexor muscles of the arm, biceps, brachialis anticus, supinator longus, a force of from 18 to 20 lbs., whilst for the flexors of the foot it amounted only to about 13 lbs. The results of the experiments of M. Koster are as follows:—As a mean of all experiments the absolute muscular force may be estimated at 17·6 lbs. (8 kilog.), for each square centimetre of transverse section; moreover in some individuals the muscles of the calf of the leg are probably stronger than the muscles flexing the forearm or the arm, and these last are a little stronger than the flexors of the foot. After special exercise of one side of the body, any particular group of muscles may acquire a degree of energy very much superior to those of the opposite side. Under any circumstances, however, the absolute muscular force is not estimated, but only the degree of force that a muscle can exert under given conditions.

CHRONICLE OF MICROLOGY.

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Part I.—Physiological Micrology.

On the Presence of Flat Muscular Fibres in the Lung Vesicles of the Vertebrata.—Professor G. Piso-Borme, of Cagliari, who has worked with Moleschott, was induced, the first time in 1845, supported by Moleschott and confirmed by Gerlach, to undertake a fresh proof of the existence of flat muscular fibres in the lung vesicles of mammals, and at the same time also to extend the investigation to the lungs of other vertebrated classes. For this purpose he made use of the means heretofore recommended by Moleschott, namely, the treatment with acetic acid mixture, in order to facilitate the distinction between the connective tissue and the flat muscular fibres, as well as the treatment with solution of potash, so as to isolate the flat muscular fibres.

It is determined that in the lung vesicles of the mammalia, amongst the elastic fibres of the wall of the vesicles are to be found, here and there dispersed, flat muscular fibres, which are remarkable in their rod-like nuclei. That is to say the nuclei of these fibres resemble in their elongated form (= 0·014 – 0·016 mm.) decidedly a little rod, whilst those nuclei which belong to the wall of the capillaries (= 0.007 – 0·009 mm.) are more or less swollen in the centre, and thus are more or less elliptical. The appearance of the nucleus is, therefore, the most characteristic fact, because, on the one hand, by that way of treatment the muscular fibres become sometimes very transparent, so that their outlines among the elastic fibres are hard to be made out, and, on the other hand, because by that treatment the substance of the muscular fibres often becomes unequally attacked by the reagents and a disintegration in the direction lengthwise brought about, whereby the impression of a fibrillary formation is easily made, and a confusion with the fibres of the connective tissue may be induced.

When, with use of reagents, the flat muscular fibres of the lung vesicles have undergone no real changes, then these appear more or less transparent and gelatinous, with fine wavy outline; their ends are generally rounded—at least never so slender and so pointedly lengthened as are the flat muscular fibres of other parts. One meets with fibres which are divided as a fork at one end, whilst less frequently there are such also, in which the furcated arrangement takes place at both ends. Non-nucleated or poly-nucleated fibres never seem to have fallen in the author's way.

The length of these muscular fibres is left behind always after those of the muscular fibres of the trachea and bronchiæ; they, in this respect, approximate to the flat muscular fibres of many mucous membranes. In all animals the number of elastic fibres and the abundance of the muscular fibres stood in inverse proportion to each other.

The disposition of the contractile fibres in the wall of the lung vesicles cannot be reduced to a precise type. Where they are very scanty, passing off separately, to appear dispersed irregularly among the elastic fibres, bent generally towards the wall of the vesicle. One seldom sees them deviate from their original direction, and with the elastic fibres pass over from the wall of a vesicle on to that of another, as in human beings. Where they are somewhat more numerous one finds them in close proximity to the inner surface of the vesicles, then also set near each other, so that they form little bundles of few fibres, with smaller or greater distances between the bundles. Finally, the bundles also end together, so that they appear in the innermost part of the wall of the vesicle as a delicate muscular coat.—*Schmidt's Jahrbücher*, 1868, p. 147.

Sheath of the Optic Nerve.—Professor Sappey, in a paper on the structure of the fibrous envelope of nerves, says, "the outer sheath of the optic nerve is specially remarkable for the multiplicity of elastic fibres which enter into its composition, and for the numerous nervous twigs which it receives. It was, therefore, a great mistake that by the ancients it was considered a mark of union between the dura mater and the sclerotic; that is to say, as if it partook of the intimate texture of both of them. It notably differs from them, 1st, by its elastic fibres, which in both are wanting; 2nd, by its *nervi nervorum*, which are extremely rare in the *dura mater*, and of which, in the sclerotic, one sees nothing whatever. Anatomical examination, far from confirming the analogy which so many anatomists have thought they discovered, shows, on the contrary, that the outer sheath of the optic nerve is distinguished from the two membranes with which it is continuous by characteristics altogether peculiar to itself."—*Robins' Journal de l'Anatomie*, &c., No. 1, 1868, p. 51.

Part II.—Pathological Micrology.

Pus-formation.—Böttcher considers that he has found an explanation of the development of the pus-corpuscles in the uncertain modes of grouping of the discharged manifold cell-forms of an abscess of the gum. Large vesicles, filled with finely granular cells like pus-corpuscles he takes to be mother cells of the pus-corpuscles. As an earlier stage, he points out cells in which were contained coarse-grained globules of various sizes. In the latter is sometimes observed a brighter nucleus, which is considered by the author to be formed probably by a thickening of the protoplasma around the nucleus, which is said to proceed by division of the original primitive nucleus of the pus-corpuscle.—*Zeit. f. Rat. Med.*, 3rd series, vol. xxxii, p. 19.

General Serous Tuberculosis.—Dr. Perrond analyses a paper of M. Humbert Mollière, in which he has recorded two cases of this disease, "in which the lesions were localised in the serous membranes, leaving the parenchymatous parts almost intact." The former author, moreover, refers to a case in which, besides the various serous membranes, the urinary mucous membrane was affected in a similar manner. He writes that "The microscope, in the granulations of the serous tuberculosis, reveals the same structure as in those incontestably tubercular; there are the same connective elements disposed in three consecutive zones, the outermost little differing from the normal connective elements, the middle showing these same elements somewhat increased in volume, furnished with many nuclei, and in course of proliferation, and the innermost, as results of this exaggerated proliferation, presenting masses of nuclei and little incomplete cellular elements, destined to an early death and to that series of retrograde changes which tend to the caseous state, and finally to softening and elimination, and to become cretaceous and encysted."

The disease is called by him only a variety of the parenchymatous tuberculosis. It sometimes finally appears in the tissue of the lungs. —*Gazette Médicale de Lyon*, 1st Dec., 1867, p. 511.

Healing by first intention.—This is a history in detail by Dr. Wywodzoff, *especially as it concerns the blood-vessels in this event.* The author observes in the preliminary notice of the results of his investigations, that he has made his experiments on the tongue of the dog, the rabbit's lip, and the *membrana nictitans* of the frog. According to his experience, the tongue of the dog proved to be better than any other for such researches, for it is a part so rich in capillaries, and, separate from the body, is so easily injected. As the basis of his investigations, the author divides the whole process of healing by first intention into five periods, each of which, in different individuals, is much varied. Indeed, not unfrequently one can, in various spots in the same scar, perceive different phases of the reparative process.

1. *Stagnation* period, lasting twelve hours (in the dog's tongue), twenty-four hours (in the tadpole). This is minutely described by Wywodzoff, as it has been previously by Billroth and others: it is marked by a stagnation of the blood in the vessels all around the edges of the wound, and by thrombus formation in the divided ends themselves. The finest lymphatics are by coagulation or by swelling of the neighbouring tissue also obstructed.

2. *Loop-formation* period, lasting about from the twelfth to the forty-eighth hour after the injury. In those parts of the vessels not thrombosed the circulation has not re-established itself, the force of the blood in them is heightened; whereby the acute and obtuse-angled vascular offshoots have become rounded, they are loops originated, which yet by the same elevated force of the blood broaden and lengthen,

until at last the outermost wall of the arch turned to the edge of the wound yields and gives occasion to the formation of looped processes. At the same time, the edges of the wound have united by a gluey material—coagulated lymph, fibrinous exudation—between them is a great quantity of new-formed cells deposited. The latter either proceed from a division which takes place in the connective tissue corpuscles situated within the edges of the wound or from white blood-cells; possibly they owe their origin to both together. Wywodzoff's observations at least make it highly probable that the blood confined in the wound goes through the same changes as have been described by Billroth as characteristic of the organization of thrombus, *i. e.*, the red corpuscles are gradually dissolved, and blend with the coagulable lymph in the intercellular substance of the recent scar, whilst the white blood-corpuscles multiply, and so at all events give occasion to the formation of those little white round cells which afterwards become spindle-shaped.

3. *Canalization* period, in the between-substance, which, for the most part, consists of newly-formed round cells, channels are opening out from the looped processes, which, without definite arrangement, without uniform law of continuance, are carried on separately in all directions. These are only visible by use of very high powers, as bright streaks bounded by granulation cells; the current of the injection advances only as far as the beginning of these channels in the looped processes: this period ends on the fourth day at the wound.

4. *Vascularizing* period, from the processes of the loops, progressive organization of the channels into blood-vessels, of very large diameter relatively, so that it forms a network of very small meshes. Proceeding from spindle-shaped cells, which, arranged in rows, give intimation of connective tissue disposition. This period generally lasts up to the tenth day.

5. *Consolidation* period, characterised by this, that the between-substance as regards firmness takes on always more the character of proper connective substance, bounds to the vascular dilatation shortly being arranged, but then the lumen of the newly-formed capillaries little by little to about a third diminish. That some of them are again in this way brought wholly to obliteration, as one has generally hitherto assumed, is said not to be the case according to Wywodzoff's observations. The lymphatics are first formed in the scar when the cicatricial tissue is transformed from spindle cells into connective tissue fibres and the vascularisation is come to a stand.—*Schmidt's Jahrbücher*, 1868, p. 39.

The Parent Gland-cells and the causes of Rheumatism.—Professor Salisbury, of Cleveland, Ohio, as the results of investigations begun in 1859, and of which he has already given some account, says—"So long as the parent gland-cells organize normal products, in the normal quantity, no such pathological state as rheumatism can occur. But little control can be exerted over the cell-elements and the filamentous tissues formed by the metamorphoses of the cells after they have been organized and have escaped from under the influence of the parent gland-cells."

The author goes on to describe, of parent gland-cells in the human body, six types—the parent *epithelial*, *fibrin*, *involuntary muscular fibre*, *connective tissue*, *voluntary nerve*, and *sympathetic nerve gland-cells*. And of rheumatism four types—the *lithic*, *oxalic*, *cystinic*, and *phosphatic* types.

In the incubative stage of lithic rheumatism "the blood generally becomes ropy, and the colourless and coloured corpuscles plastic and adhesive, the former tending to adhere together in groups, forming masses too large to circulate freely through the capillary system of the firmer tissues. In it also occur the spores and filaments of a minute algoid vegetation." In the acute stage "the blood is ropy and adhesive, the colourless corpuscles adhering together in little masses. The coloured discs become aggregated more or less in groups and rows, having a tendency to adhere to the meshes of fibrin. Spores and filaments of a minute algoid vegetation are discovered more or less abundantly distributed through the blood. These either float in the blood-stream singly, or in ragged aggregations, in balls or in loose knots or skeins. The filaments have a wavy appearance, and are highly translucent and refractive. These, with their spores, become almost invisible after being a short time between the slides." In the chronic stage, "as the patient recovers, the blood loses its ropy adhesive character, and the algoid vegetation disappears."

"The fibrin-filaments are adhesive, and the fibrin-cells and filaments, and the spores and filaments of the algoid vegetation, have a tendency to form little plastic masses or emboli, which slowly flow along the capillary vessels in the firm, unyielding fibrous tissues. In the blood of this variety occur masses of minute algoid spores, and ropes and knots of algoid filaments. I have designated these minute cryptogams the *Zymotosis translucens*. The spores are very minute, and highly translucent and refractive. The filaments are also highly refractive, and wind in among each other more or less, and occur in all stages of development, from a filament double the length of a spore to three and four inches when magnified 300 diameters. The spores are from slightly larger to two and three times the size of the fibrin-cell-granules. The sudoriparous gland-ducts and plane surfaces of the skin are covered with an algoid vegetation resembling that which is found developing in the blood. The urine also contains similar vegetation."

In oxalic rheumatism "there is a tendency for the fibrin to aggregate and become ropy, and the coloured and colourless corpuscles to take on an adhesiveness which tends to the forming of thrombi and emboli. These, when examined, are found full of either stelline, granular, and crystalline cystine, or oxalate of lime. There is also more or less of a fermentative tendency excited, probably, by the development in the blood, secretions, and excretions, of a minute species of algoid vegetation (*Zymotosis translucens*)."

"In all forms of rheumatism the sweat, urine, and secretions are more or less acid, and the skin and mucous membranes are covered more or less with algoid vegetation. There seems to be a peculiar fermentative state of the excretions and secretions. The blood contains masses of minute spores and bundles and knots of minute algoid filaments."

"This algoid vegetation probably has something to do in giving plasticity to the colourless corpuscles, and causing them to adhere in masses, as described under lithic rheumatism. Two, more, or all of the types of rheumatism may be combined in the same person, in which case the treatment should be varied to suit the conditions present."

"The masses of granules in the blood—of either cystine, of oxalate of lime, or of phosphates—are readily distinguishable from the masses of algoid spores. The granules of phosphates, cystine, and oxalate have a well-defined outline, and do not become less and less visible as the blood stands longer and longer between the slides, as is the case with the algoid spores; besides, the spores are more uniform in size and more highly refractive, and have a fainter outline."—*American Journal of the Medical Sciences*, October, 1867, p. 359.

Pseudo-hypertrophic muscular Paralysis.—Dr. Duchenne, of Boulogne, continues his researches into this large subject, and he has thus summarised his own observations as far as they are anatomical.

1. The hyperplasy of the interstitial connective tissue, and production of a more or less abundant fibroïd tissue is, in this kind of paralysis, the fundamental anatomical lesion of the muscles.

2. It is seated in all the paralysed muscles which have or have not increased in size.

3. This it is which produces the considerable and sometimes monstrous increase of size of the muscles, in exact relation to the amount of hyperplastic connective and fibroïd interstitial tissue.

4. The connective and fibroid interstitial hyperplastic tissue is combined with or joined to a very few or moderate quantity of fatty vesicles; according to observations made in Germany, it is replaced by a considerable quantity of adipose tissue.

This last stage appears, in the pseudo-hypertrophic paralysis, to be the most advanced stage of the alteration of the interstitial muscular tissue.

5. According to observations, the cross-streaking is maintained throughout the length, or in a more or less considerable part of most of the muscular fibres: but it becomes very fine and difficult to be seen. In the parts at which the transverse striation has disappeared, one sees the longitudinal striæ; sometimes even these longitudinal striæ are effaced, the sarcolemmæ seeming then to contain adipose vesicles, which really have their origin in the surrounding interstitial tissue, and which otherwise essentially differ, in their aspect and their confluence, from the fatty granulations which are characteristic of the fatty muscular degeneration.

6. The hyperplasy of the interstitial connective tissue generally only appears in the second stage of the disease; it seems to

be preceded by an inflammatory state of the muscles, which may also produce some slight increase of their size. At this time the cross-streaking of the muscular fibre is already of extreme tenuity.—*Archives Générales de Médecine*, March, 1868, pp. 317-8.

In a former paper Dr. Duchenne has given the conclusions arrived at by M. Ordoñez, to whom he had referred various specimens obtained from a patient during life, by the use of the *emporte pièce-histologique* of the author. This new example of the disease was in the case of a boy; the specimens were got, at long intervals, from various parts of the body where the pseudo-hypertrophic muscles were most marked. M. Ordoñez was referred to, and his observations quoted in reference to the differences of observations that have been made on this subject in Germany as compared with those of the French histologists. M. Ordoñez says, "The muscular fibres were found united in a close network of bundles of fibrillary tissue which made it very difficult to isolate, even partially, the muscular element."

"These fibres, or rather these primitive bundles, did not appear to be sensibly diminished in volume; their margins were somewhat puckered. The cross-streaking of these bundles was not uniform; in some parts it did not exist, and at others a greyish semi-transparent colouring of the substance of the muscular bundle was demonstrable; its margins were puckered; some fatty vesicles and a few molecular granules were to be found in its course. Elsewhere the streaking, which was tolerably distinct, showed a remarkable delicacy, and finally there were parts in which the cross-streaking had been replaced by a longitudinal striation."

"The fibrillary tissue in which the muscular bundles were enveloped was made up of fasciculi for the most part of no great size, delicately wavy, varying as to dimensions from three to eight thousandths of a millimeter; they had very variable course which made isolation of the muscular bundles very difficult."

"The chief characteristic was the great amount of the interstitial hyperplasy of the fibrillary tissue among the primary muscular bundles. After protracted investigation, I have found nothing analogous to it, in the muscles of men or of animals. It is known that the muscular bundles are united by a little amorphous transparent matter, and by fibres of fibrillary tissue, but they are easily isolated, and, above all, they are not, as it were, overwhelmed, in a close network of areolar tissue."

"As to the adipose vesicles pointed out by some Germans in such cases, I must say that I have here and there indeed met with some small collections or groups of them, but I must add that they were disseminated in the network of the fibrillary tissue, and not among the muscular bundles, as they are in cases of fatty transformation of the muscles. This it is which exactly defines the myo-sclerosis of M. Duchenne as contrasted with the fatty and fibrous transformations which are commonly called muscular degeneration."—*Archives Générales de Médecine*, February, 1868, pp. 208-9.

Primary diffused Cutaneous Cancer.—Dr. Vald. Rasmussen reports a case of this nature in a widow, aged sixty-nine. In the first place there was a swelling of the skin over the outer half of the great pectoral muscle. The breast was secondarily affected. There was no ulceration. "In the infiltrated parts of the skin the lardaceous mass was found to consist of alveoli filled with numerous irregularly formed flat cells, with one or two nuclei. In the tubercles situated both outwards towards the epidermis and inwards towards the ribs, the trabeculæ which formed the stroma were thin, the alveoli were large, containing numerous and fresh cells, while the middle, firmer, almost fibrous parts, exhibited the alveoli much limited in extent by great development of the trabeculæ of the stroma, so that in some places there were found only inconsiderable heaps of fat-granules, as remains of former alveoli. No active participation of the elements of the epidermis in the cancerous formation was discoverable. In the places where the infiltration extended out to the epidermis, the papillæ were found rather compressed, and the epidermis itself somewhat attenuated. The tubercles which projected on the surface of the skin consisted only of dense connective tissue. The other deposits had the usual structure of medullary carcinoma."—*Edinburgh Medical Journal*, April, 1868, p. 876.

Phosphorus Poisoning—Changes in the Kidneys.—Dr. Ranvier begins by observing that there are sometimes albuminuriæ, but that in other cases the presence of albumen in the urine has not been proved during life. Most frequently, the kidneys of those poisoned with phosphorus (men or animals) show the characters of complete and generalised steatosis. The twisted tubuli of the cortical substance are then filled with granular matter and fatty droplets close together. The straighter tubes of the cortical substance and those of the medullary substance are often also attacked by the fatty change. But it is irregularly distributed, and mainly consists in a more or less abundant deposit of fatty granules within the epithelial cells. To the fatty granules proteine granules are not added, so that the cells preserve a certain degree of transparency. In the Malpighian pyramids, the different tubes are unequally affected; and in this form, as in that above to be described here, one can ordinarily verify the fact that the reflected (Henle's) tubes are much degenerated, whilst the large straight tubes are much less granular. Thus, in some preparations, one can, aided by this kind of pathological injection, follow exactly the course of these different conduits.

Other times, the kidney tubules, instead of being filled with fatty droplets, are occupied by a sort of exudation composed of fine fatty granules, and an albuminoid substance moulded together; so that the fatty granular matter, instead of being free as in the first kind, and escaping easily from the tubuli which contain it, are bound to each other by an albuminous substance which moulds itself on the uriniferous tubes. The alteration does not invade all the tubes of the cortical substance in a regular manner. In some parts it is so marked that the epithelial cells have entirely disappeared; in others, these cells remain and have become dull by an internal deposition of albumen and fine fatty granular matter; finally, some tubes appear lined with normal epithelium.

The different canals of the Malpighian pyramids are none the more equally changed. The alteration always appears more complete in the tubes of Henle. In fact, in ordinary albuminuria, the fatty casts are composed of a central transparent part covered by granular matter. Here the cylinders have the same composition at all parts of their mass; as one can at once determine, especially when broken ends of these cylinders show themselves successively in all their aspects in the field of the microscope. These cylinders are generally very abundant, the fatty granular matter that they contain is very fine and not very evident without acetic acid. In some cases, besides these casts, one finds hyaline cylinders (said to be fibrinous), but always in small number.—*Robin's Journal de l'Anatomie,* &c., No. 2, 1867, p. 221.

Croupous-diphtheritic Inflammation of the Œsophagus.—E. Wagner, in his contributions to the pathology of this part, having described the normal epithelium, speaks of the uncomplicated diphtheritis, so-called, of the œsophagus. Microscopically, he says, the relations of the so-called diphtheritic deposit, in all its essential attributes, are much the same as those of laryngeal diphtheritis and croup, which I have already depicted. In my case the fibrous network was as delicate as in croup, or at least but little thicker, but never as thick as in an ordinary case of laryngeal diphtheritis, which, moreover, was as clear, homogeneous, and bright, but of somewhat more uniform extent and thickness than these. Their proportionately large spaces contained in greater number the same corpuscular elements, especially numerous, large, one-nucleated pus-corpuscles. On the surface one sees either the same network, but which there shows abundant irregularities—sometimes interspersed with numerous fungous filaments, which mostly resembled the *Oïdium albicans. Or, the surface showed a one- to three-fold layer of very smooth, indistinctly nucleated epithelium,* to which, in opposition to the following statements of other authors, I particularly draw attention. As soon as these are separated (a fibrinous degeneration seems to be not possible) there probably soon begins the irregular destruction of the deposit. On its under surface the diphtheritic network in recent cases generally still shows a manifold layer of epithelial cells, which show no particular change, so that for the most part a two to fourfold layer is to be met with over the points of the papillæ, and manifold where it lies between them. Between these cases and those in which a fibrous network is formed in the place of the whole epithelium, all possible transitions are to be found. These were formerly described by me as cases of laryngeal diphtheritis and croup of a primary kind, hardly ever occurring, whilst subsequently I found the same circumstances in secondary cases observed. Moreover, the boundary between normal and fibrous, degenerate epithelium is generally clear enough, not only in the direction of the perpendicular, but also of the horizontal diameter.

Here also especially I often found thicker depositions of the network lie at its edge over normal or little changed epithelium.

Of the *œsophageal diphtheritis complicated with epithelial suppuration* the author says that, microscopically, investigation showed a *combination of suppuration in the epithelium and in the mucous membrane, with diphtheritic change of the former.* The yellow longitudinal streaks, visible with the naked eye, appeared on cross section as more or less regular wedges of a very delicate network, of the same nature as in ordinary croup, containing in its little spaces almost always a round nucleus or a pus-corpuscle. These wedges for the most part extended through the whole thickness of the epithelial layer through it even to the outer surface of the mucous membrane, their small side turned to the latter. Laterally, also, they were sharply outlined; they seemed, as already remarked, to become most conspicuous with the cataract needle. Near these microscopic diphtheritic wedges the epithelium, which was inconsiderable, and only in the upper parts, is found to be more extensively distributed. The highest of all the epithelium was much flattened when it was as usual present, not diphtheritically degenerate. In the situation of the non-fibrous-transformed parts the epithelial cells were found medium sized, free nuclei and pus-corpuscles lying so closely together that no other structure thereabouts was visible in most places. Although the mass was so firm, it could be employed so well in the hardened preparations to the finest sections, and the latter were so very fine to separate that one must perhaps assume a connective substance to be present between those elements. Only here and there between the above-named bodies were the elements visible, which appeared as non-nucleated, altogether flattened epithelial cells. At all separate points unaltered epithelial cells were visible. The formation of pus-corpuscles I did not see in them. The uppermost epithelial cells were unchanged, not engaged in suppuration, but easily separable in fine cross sections.

The tissue of the mucous membrane was in most parts so thickly set with pus-corpuscles that its outer limit (towards the epithelium) could not be seen. It was found that where the diphtheritic degeneration of the epithelial cells affected the whole thickness thereof, as in finely fibrillated preparations, in these but few connective-tissue-fibres were visible; only in few places could one still see the origination of the pus-corpuscles from the connective-tissue-cells. The muscular coat of the mucous membrane showed here and there series of pus-corpuscles between its elements. The submucous coat was interspersed with numerous pus-corpuscles, lying almost always but sparsely and singly, exposed at the edge of the ulceration. The epithelial cells within the enlarged acini of the scanty mucous glands of the œsophagus seemed in part only increased, in part engaged in a process of endogenous cell formation. The proper muscular coat was normal.

The ulcers were situated, as could be seen with the naked eye, on the inner surface of the circular fibre layer of the muscular coat, and even with it. Everywhere remains of the submucous coat were found, especially of the elastic fibres.

So far as a conclusion is deducible from the anatomical investigation of the morbid process in the œsophagus, the result is the following. The epithelium showed in every respect the greatest and most extensive change. This was transformed for the most part into pus, in a less degree into diphtheritic false membrane. Next the proper mucous membrane seemed to be attacked. It showed only purulent infiltration. Later this showed itself in the scanty connective tissue between the contractile cells of the muscular coat of the mucous membrane. Last of all, the submucous coat became involved. —*Archiv der Heilkunde*, 1867, No. 5, pp. 449—68.

Congenital Sclerotico-corneal Tumour containing Hairs.—Drs. Lainati and Visconti have examined minutely a tumour, small, rounded, reddish, firm, covered with a quantity of fine, curved, short hair, closely connected with the subjacent tissues (which were found to be perfectly healthy), and removed from a girl of fourteen years of age. "The mass of the tumour was composed of connective tissue, in the meshes of which were found groups of fatty cells. Elastic fibres in it were very rare; on the other hand, nuclei were in almost all parts of it very numerous. The tumour was covered by a pretty thick envelope, formed of layers of epidermoidal cells, with a fine nucleus of reddish-yellow colour, slightly reflecting light. Some layers of conjunctival epithelium, found at the borders of the tumour, were the only elements of conjunctivitis of which one could assert the existence. A vertical section showed distinctly the bulbs of the hairs with which the surface was covered. On specially observing these hairs with their bulbs they were found to resemble in all points those of the skin. Beside the hairs were found fine sebaceous glands; in one of the preparations especially was found a hair with, on one side, a compound grape-like sebaceous gland, and on the other a simple utriculated gland. No vestige of sudoriparous glands could be found"—('Italian Journal of Ophthalmology.') *Archives Générales de Médecine*, September, 1867, p. 350.

Corneal tumours.—Molluscum.—At the Berlin Medical Society M. Graefe spoke of some very extraordinary tumours formed on the two corneæ of a patient. These tumours had commenced as yellowish, elongated patches: at the base of one of these patches was situated a prominent tumour, of a dirty yellow, which covered nearly all the cornea. The skin of the patient was strewn with a number of little tumours (molluscum), very probably analogous to those of the cornea. These tumours were of a dark brown colour.

The little corneal tumour was excised—it was composed, in the centre, of embryo-cells and partly of large cells, ramified and having a considerable nucleus. It was probably a sarcomatous production. The base of the tumour was infiltrated with fatty granulations.

M. Graefe has twice observed fatty tumours developed on the cornea, but the patient showed no alteration of the cutaneous surface.—*Archives Générales de Médecine*, March, 1868, pp. 378-9.

Fungi in the Kidneys.—Dr. Morris Tonge reports this in a case of phthisis. No microscopical examination of the urine had been made, and the mouth was not examined.

"The pelvis of the left kidney was very nearly filled with a yellowish-white pultaceous substance, adherent to the apices of many of the pyramids." "Microscopical examination of the pulpy matter proved it to consist of the sporules and mycelium of a microscopic fungus, apparently a species of oïdium, possessing the following characters:

"1. Round or oval vesicles, single or grouped, containing one or more globules (probably oil-globules or minute sporules), and sometimes granular matter.

"2. Elongated vesicles, united at their ends, so as to form continuous cylindrical tubes, branching dichotomously, sometimes terminated by strings or groups of round or oval cells, lateral development of such cells being also not uncommon. The tubes contained oil-globules and granular material; here and there the contents had escaped, and an empty tube was seen. The white portions of the medullary cones were penetrated by the fungi for a varying depth, generally one twelfth to one sixth of an inch."—*Archives of Medicine*, April, 1867, p. 314.

New Cryptogamic Skin Diseases.—Dr. J. H. Salisbury of Ohio, U. S. describes two skin diseases (1) *Trichosis felinis*, and (2) *caninis*, which he distinguishes from *Trichosis furfuraceæ.* Both are readily transmissible to the human subject. Of *T. felinis* the author says it is produced by a species of fungus that develops in the fermentation of cat's milk—first around the lips, nose, face, and eyes, and spreads to the head and body. It forms, with the epidermic cells, circular patches of thin rusty scurf on the face, nose, lips, and head. The hair soon sickens, curls up, dies, and crumbles away. On infants and young children it spreads rapidly, attacking all parts of the body alike. The cells of the hair-follicles and of the epidermic layer between them are shrunken and shrivelled, and the hairs, diminished in size, become brittle, and break off and crumble away. The deeper parts of these follicles become enlarged often, and the hairs die, shrink, and fall out. The capillary vessels in the papillary layer of the skin beneath the diseased surface, become congested and enlarged, producing a reddening of the skin and a slight elevation of the diseased surface.

In ordinary ringworm the fungoïd cause exists mostly in the spore state. The plant does not advance beyond its cell condition. Its growth seems to be confined simply to cell multiplication by pullulation. In this disease the plant-cells multiply by pullulation, and these advance to the filamentous stage of growth. These filaments are found running through among the cells of the epidermic layer.

T. caninis, the author says, differs from the former in that the fungus is more luxuriant, large, and more confined to its filamentous stage of development. It attacks less the hair-follicles than the *felinis*, and extends more generally to all parts of the epidermic cell surfaces.—*American Journal of the Medical Sciences*, April, 1867, pp. 379-83.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1868.

PART FIRST.

Analytical and Critical Reviews.

Review I.

A System of Medicine. Edited by J. Russell Reynolds, M.D., F.R.C.P. London, Professor of the Principles and Practice of Medicine in the University College, Physician to University College Hospital, &c. Vol. II, containing Local Diseases. London, 1868. 8vo. Pp. 990.

In a former number, reviewing the first volume of this work, we pointed out the characters by which it is intentionally distinguished from other collections of medical knowledge. Of these the most valuable is that it presents a real life-like sketch of the current opinions of the most vigorous portion of the present generation, so that we may not only know the present, but we may predict from it what will be the future opinions, for the next twenty years or so, of those whose talents and inclinations are imitative rather than creative, that is to say, of the most numerous, useful, and generally popular part of the profession. It is not so much that Dr. Reynolds' book will be a guide, as that the men whom he has selected as his ministers (including himself) will certainly be the guides of English practitioners; and we are glad to be able to learn how they are to be guided. The *personnel* of this new staff of contributors to literature is smaller, indeed, than was exhibited in the first list, but certainly not less weighty: the fresh names are Dr. Anstie, Dr. Bastian, Dr. Chambers, Dr. Wilson Fox, Dr. Gull, Dr. Hughlings Jackson, Dr. Maudsley, Dr. Radcliffe, Mr. J. N. Radcliffe, Dr. Ramskill, Dr. Roberts, Dr. Sanders, Dr. Sutton; while Dr. Gee, Dr. Begbie, Inspector-General Maclean, and the editor himself, may be mentioned as contributors on both occasions. The reason of the large influx of new names is that an entirely new class of diseases is taken in hand, those, namely, which are capable of a classification following in some degree that of the historians of the healthy body, the physiologists and the anatomists; maladies with respect to which we can say at once that they are diseases of the nervous system, of the digestive apparatus, and so on. Now, each of these subjects is best handled by men who have had leisure to collect all the physiological knowledge extant on one or two subjects, as well as to take that general view of all physiology as demanded by the subjects of the first volume. Special investigators, though not special practitioners, have been selected by the editor to set before us the state of knowledge on the matters each one's inclinations and opportunities have led him to know most.

The principle of division thus established in regard to all diseases, viz., that of limitation or localisation, is again applied by Dr. Reynolds as a means to be employed for the primary subdivision of the large group of diseases occupying the greater part of this volume; "Diseases of the Nervous System" are ranged under two headings, "General Nervous Diseases" and "Partial Nervous Diseases." Under the first, for example, come epilepsy, alcoholism, insanity, hysteria, diseases exhibiting themselves in altered functions of the brain, cord, and nerves; while as a transition to those in the second heading we find wasting palsy, writer's cramp, &c., which, although they exhibit the maximum of their obvious symptoms in particular parts of the nervous system, are yet of such uncertain pathology that we shrink from describing them as diseases of either brain, cord, or nerves exclusively. It may be that some readers of the hyper-anatomical school will object to this, and wish to see greater prominence given to certain favourite hypotheses, which refer to more accurately defined localities the origin of these diseases. But we think on the whole the editor has exercised a wise discretion in demanding a multiplication and verification of observations, both in death and life, before ascribing these diseases to particular kinds or localizations of tissue change. Scientific accuracy might be speculated for at a risk of possible loss of truth, by ranking them under more anatomical headings.

In the second group of diseases of the nervous system, the principle of arrangement is sufficiently obvious to need but little elucidation. In the first place, a subdivision is made upon simply anatomical grounds into affections of the cranium, the spinal cord, and the nerves; and each of these is again subdivided on an anatomical basis according as the various tissues constituting them exhibit the primary lesions. The next principle of division is that determined by the nature of the anatomical change which these tissues undergo respectively. Here an attempt has been made to place in close proximity those diseases which have the most highly marked clinical similitude —such, for example, as getting together concussion of the spine, compression of the cord, caries of the vertebral column, &c. —an attempt which certainly would save a certain amount of repetition of diagnostic differences, were it fully carried out, but which is too generally found inconsistent with the demands of the other principles of classification. Thus we find "Ecstasy," "Catalepsy," and "Somnambulism," though declared to be of the same nature as "Hysteria," separated from it by a gulf wide enough to include "Wasting Palsy," "Mercurial Tremor," and "Writer's Cramp;" while before we get to "Hysterical Paraplegia" we have to pass through "Cerebral Hæmorrhage" and several hundred pages of other tissue lesions. We think the editor had better not have attempted this element of arrangement, or at least have concealed his attempt, which then probably would not have been found out.

We have been particular in describing Dr. Reynolds' classification of nerve-diseases, because he has evidently bestowed much time and thought upon it, and certainly with good effect. We trust he intends to do the same with affections of the digestive, respiratory, and circulatory system, but this volume only takes us as far as diseases of the stomach, commencing with the functional disorders and ending with the tissue lesions of that organ.

Dr. Maudsley's article on "Insanity" begins the volume, and gives, within the compass, we are glad to say, of a very moderate number of pages, a very broad view of the subject. For a specialist to accomplish this task involves an unsparing amount of the *labor limæ.* But Dr. Maudsley's mind is apparently one eminently fitted to exercise the literary pruning-knife; he has what Aristotle so much values in the philosopher—the faculty of seeing resemblances stronger than that of seeing differences; and, consequently, instead of trying to distinguish himself, as so many specialists do, by magnifying varieties into species, and giving a distinctive name to the disease of almost every patient, he applies himself to showing how the variations are the outcome of a common principle. Thus, while he recognises the two well-marked groups of insane persons—those in whom the *feeling* or *affective* life is chiefly perverted, and those in whom the *ideational* or *intellectual* derangement predominates

—he points out that moral alienation is the fundamental fact, and that it precedes hallucination, both in order of time and logical sequence. "To insist," he says, "upon the existence of a delusion as a criterion of insanity, is to ignore some of the gravest and most dangerous forms of mental disease." Again, he points out that the delusion is not the cause of the impression of misery in melancholia, or of the exaltation of self-feeling in mania, or of the silly acts of craziness (dementia), but is simply its expression, determined by accidental circumstances of disposition, education, and worldly estate. This leads him to a most valuable and practical guide to prognosis; namely, that insanity, however abnormal or however slight its manifestations may be, is curable in an inverse ratio to the fixedness of a definite type of morbid action of a chronic nature. Hence in melancholia, where there is a fixed idea that the cause of misery is in some external agency, the prognosis is unfavorable; and hence monomania, which takes its name from the unvarying monotony of the direction of the morbid ideas, is far less curable than mania. The expression of this leading idea contributes a most useful page of illustrative prognosis.

Dr. Maudsley's treatment of insanity rests on the principle of removing the causes—the physical causes by calming physical excitement with opium, henbane, and bromide of potassium, and the moral causes by taking a patient away from the sights, sounds, and associations of the home where the disease has come on. This last is the most painful part of the business for all those whose daily life has not inured them to the idea; for though the exceptional wealthy may be able to take Dr. Maudsley's advice of travelling in the early stages, or removal to another residence, yet for the great majority of our patients, we can attain the desired object only by sending them to an asylum.

Now, it is a severe struggle to a non-specialist, not only to risk losing sight of his patient, but also to affix to the family the stigma of having a relative in a madhouse, especially when it is an incipient, curable case, capable of easy concealment from all except the attendant physician. We will, therefore, in the interests of non-specialists, go a little out of our way to quote, from a lecture on an allied subject, a scheme which strikes us as capable of being adapted to the treatment of early insanity:

"One good plan that can sometimes be adopted, to the saving of pride and pocket together, is to negotiate an exchange of patients, where two families of about the same social standing are simultaneously afflicted with an hysterical member. The relative of A can take charge of B, and the friends of B repay the debt by their care of A. Mere kindness even may induce people to receive on a visit such inmates, when it is pointed out how valuable the kindness really is, and what a high office of Christian charity is thus fulfilled, when a sick person, incurable at home, is rendered curable by removal."[1]

The adoption of some snch arrangement would be most advantageous to that large class lying between hospital patients and those in completely easy circumstances.

The next article, on "Alcoholism," is by Dr. Anstie, whose long devoted attention to the physiological action of so-called "stimulants" is well known.[2] From that physiological action (which, he points out, in the way they are generally used, is that of a depressant), he deduces the pathology of the condition named:

"The exciting causes of alcoholism may be understood, then, to be the repeated direct action of blood *strongly* impregnated with alcohol on the tissues of the nervous centres and branches, rendering them physically incapable of the due performance of their functions, and the influence of an insufficiently oxygenated blood-supply consequent on a morbid condition of the blood-corpuscles."

He therefore treats it by a conjunction of moderate elimination and continuous ample nutrition; and he entirely dispenses with the employment of alcohol, finding no ultimate harm to accrue from its immediate disuse.

Dr. Anstie, however, strongly dissuades from the administration of violent eliminants, such as, for example, tartar emetic. He says that, though it is possible that sudden improvement may in some cases have followed its powerful effect, yet that it is perilous in the extreme to the forces of circulation. There is some dry humour in the suggestion by which the author would restrict its employment. He thinks it should not be used till a diagnosis has been made between the vigour of a really strong pulse and the false shadow of it exhibited in the glaring eyes and delirious jactitation—a diagnosis to be accomplished by the calm application of M. Marey's delicate sphygmograph to the raving patient. In most cases we should prefer to forego the advantage, though Dr. Anstie prints a tracing thus obtained.

The three next short articles by Dr. Chambers are on the rare, and, therefore, less important, diseases, "Ecstasy," "Catalepsy," and "Somnambulism." They endeavour, however, to give a practical turn to the subjects by tracing their connection with hysteria, mesmerism, nocturnal incontinence of urine, seminal emissions, and other defects of semi-voluntary power.

[1] 'Chambers' Clinical Lectures,' page, 385 (fourth edition); "Hysteria."
[2] 'Stimulants and Narcotics.' Anstie. 1864.

"Chorea" is treated of by Dr. Radcliffe as a disease of debility, whose natural tendency is towards recovery. He gives a full description of the ordinary and exceptional features of it and of the means of diagnosis, and ends with a list of about thirty remedies, between which he leaves the reader to take his choice. We should have been glad of a more critical analysis of these remedies, and of some opinion as to how far their great number is to be accounted for by the fact of all the moderately severe cases getting well of their own accord, while the fatal examples seem quite unaffected by any measures. A philosophical examination of the history would also have been interesting, to trace, if possible, how a disease, originally remarkable for its hysterical and ecstatic phenomena, should have become so changed in character, that we find chorea and hysteria less frequently united in the same patient than the doctrine of chances would lead us to expect, when two such common ailments are concerned. Such critical inquiries should be suggested by the writer, not left to the reader.

Dr. Maclean's article on "Sunstroke" is interesting, from the great mortality which has at sundry times and places occurred from that agency to British armies under the old *régime*, and because we believe this is the first occasion on which the successful treatment has been inserted into a systematic work on medicine. He traces its pathology to the depressing effects of excessive heat, aided by other corroborative circumstances, such as dampness, tight clothes, &c., on the circulation separately, or the nervous centres separately, or on both together (Morehead's cardiac, cerebro-spinal, and mixed varieties). In the first, sudden death takes place by instantaneous syncope; in the second, the patient passes through a stage of raving fever into coma; in the third, there is a combination of minor degrees in each.

We should have been glad to have seen, as an appendix to this article, some remarks on the local effect of other kinds of heat than that of the sun on the nerve-centres. We have witnessed phenomena exactly the same as those of insolation, from the exposure to a hot gas-burner of a bald head engaged in making up railway accounts: and we have heard of the same results happening to steel puddlers, from the great heat evolved by the furnace. We are sorry to say that neither Dr. Reynolds' System, nor even the Nomenclature of the College of Physicians, enable us to give a name to this disease, which clearly is not "congestive apoplexy," because there is no congestion. Would not "heat-stroke" be a more inclusively correct term for both together?

In the article on "Wasting Palsy" the author adds to the full account of the disease which he published in a monographical form in 1858,[1] the subsequent researches of Gull, Lockhart Clarke, and Luys; and he announces the important result that they have changed his opinion as to the morbid anatomy of the disease. He now considers it proved that the primary lesion exists in the spinal cord, or at least some part of the nervous system, and not in the muscle itself; and he thinks that clinical facts seem to designate as the special parts affected a special set of organic nerves, having upward connections with the sympathetic ganglia and the cerebro-spinal axis, by no means identical with the central connections of the motor nerve-fibres, and which act as nutritive centres to the groups of muscles to which they lead. For our own part we should have preferred classing this variety of disease amongst the forms of progressive paralysis.

The name "metallic tremor" is preferred by Dr. Sanders to "mercurial tremor," from an account derived from Dr. Brockmann, a practitioner in the Hartz mountains, of a similar infliction befalling the lead-miners of that district. Professor Schönlein also attributes the disease to the poisonous action of zinc, arsenic, lead, and bismuth, as well as to mercury. He differs from the latter pathologist in assigning the nerve-centres as the seats of lesion instead of the terminal fibrils and the muscles themselves, which on post-mortem evidence seem to be accepted in Germany as the locality of the morbid action.

Such articles as that of Dr. Jackson on "Convulsions," and that of Dr. Ramskill on "Vertigo," we look upon as particularly useful in the present day. A symptom common to many anatomical lesions and morbid states is taken as the subject, and its bearing upon each discussed, pointing out how far it may be treated in one common way, however diverse may be the exciting internal causes, and how far the diversity of the causes may modify the treatment. We say they are particularly useful in the present day, and we should be glad to have more of them, because there is prevalent an over-strong reaction against the mere symptomatic therapeutics of our forefathers, and often a diagnostic differentiation is made to involve an unnecessary variation of treatment. We should be glad to see a treatise on *similia similibus curantur*, in the sense of diseases which resemble one another being usually best treated by remedies which resemble one another. Dr. Jackson is several times unnecessarily apologetic, as if he were going to be tried by a packed jury of pure morbid anatomists. Let him take heart; those excellent pioneers of science are in a minority,

[1] 'An Essay on Wasting Palsy,' by William Roberts, M.D.

even among the critics. We think, for example, that he is by no means "sacrificing correctness to convenience," when he divides the subject into (1) fits under seven years of age, and (2) fits above seven years. The line which separates the two classes seems dim and roughly defined, simply because the classification is natural, and arrived at by grouping, rather than by logical and artificial division. The sets of diseases which originate convulsions are different at the different periods of life, and arrange themselves according to their primary and secondary characters, as the author has done. The differences and likenesses of those falling together are essential, not accidental.

By "Epilepsy" Dr. Reynolds means a sudden temporary loss of consciousness, sometimes with, sometimes without spasm, originating in the brain as the first material cause. He thinks it of great importance to restrict the use of the word to that natural group of cases, with the joint goal of attaining clear ideas on their rational treatment, and a correct view of their anatomical pathology. On the latter point the author takes stock of our knowledge in eleven propositions. Our investigations into the material cause do not seem to have advanced further than a sufficiently probable conjecture that the primary and essential spasm is in the vessels of the pia mater of the medulla oblongata and upper part of spinal cord. The treatment recommended is purely empirical—the specific bromide of potassium in large doses. We are glad to hear from Dr. Reynolds, with regard to this fashionable remedy for everything, that in his large experience extending to "many hundreds" of cases, he has "witnessed *no* ill effects from its administration." Among ill effects he does not include a few minor phenomena, scarcely sufficient to induce the omission of the drug if otherwise beneficial.

There are two methods of trying to make variations of temperature available for preventing fits at night—a bag of ice down the back and a hot bottle to the feet. Dr. Reynolds has found the latter the most effectual, the former having proved, after fair trials, quite useless. Though not epileptic ourselves, we still feel heartily glad of this comfortable result of experience.

The article on "Writers' Cramp" we should have been glad to have seen united to that on "Torticollis," 700 pages later, and a general essay on the whole subject of local spasms, including nervous strabismus, stomach cough, asthma, and stuttering, treated physiologically as a whole.

"Hypochondriasis" is elegantly and philosophically described by Dr. Gull as "one of the transformed neuroses which descend from a parent stock strongly tainted with insanity." The most important point in the management of the disorder is, in the author's view, to distinguish its phantom subjectivities from the signs of tissue lesions in the parts indicated. To that he contributes valuable aid. One observation we would take leave to add, which has often assisted us much in this common dilemma, namely, the existence of a peculiar suspiciousness in the hypochondriac. Not only is he extremely anxious that you should believe him, but he has always a shrewd notion that you do not. The needless tone of advocacy adopted by the patient has often given us the first hint that his tale was an ignis fatuus.

The article on "Hysteria" of Dr. Reynolds follows next, in which the author consistently deduces all he says of the natural history, causes, phenomena, and treatment of the disease from its pathology as a condition of the nervous system, essentially characterised by deficient volition. He divides the symptoms to which this deficiency gives rise into the interparoxysmal and paroxysmal; and in the former distinguishes graphically the mental, sensorial, and motorial condition; where the morbid phenomenon are all evidently unbridled vagaries of nervous functions normally kept under control. With this there is often joined as a powerful ally, often, but not always, as a precursor, some disorder of the general health, especially in the direction of defective nutrition. The treatment, then, may rationally address itself to rectifying digestive and other imperfections by means of medicines, but the essentially therapeutical and preventive management of hysteria must be moral.

When a street has got rightly or wrongly an evil reputation, the introduction of respectable tenants is facilitated by a change of name. This seems to be the reason for the substitution of "Locomotor ataxy" for the old-fashioned *tabes dorsalis,* which was supposed to impute to the patient previous abuse of the sexual organs. In a good half of the cases the accusation would be misplaced; and where it is true, probably points to a symptom rather than a cause of the disease. On this ground we are quite willing to accede to the new nomenclature, and also to the omission of Dr. Duchenne's prefix of 'Progressive,' which is a needless aggravation of the patient's sorrows, a "*lasciate ogni speranza, che intrate*" of the consulting room. Dr. Radcliffe's paper seems, however, to show that, though superfluous, it is unfortunately only too correct an epithet.

The PARTIAL DISEASES OF THE NERVOUS SYSTEM are of course the section in which the aid afforded by morbid anatomy to medical knowledge begins to be conspicuous. The first instance of this is in the essay by Dr. Ramskill, on "Simple Meningitis," which is made to include the acute hydrocephalus of older authors, that is to say, all those cases where tubercles are not the starting-point of the inflammation. These are proportionately so numerous, and present so many individual peculiarities, that they form the subject of the next article, contributed by Dr. Gee. We are happy to see that in both simple and tubercular meningitis the inflammation, as it occurs in infancy, youth, adult, and old age, is treated of as one, and not discussed in the usual way, which makes medical students reckon it up as two, if not four, diseases. Pathology suffers as much by being split up artificially into that of children, and men, and women, as anatomy would by being taught in the same fashion.

Dr. Gee very properly makes the most elaborate part of his paper that upon diagnosis, for upon our being happily able to exclude our patient from the category of a case of tubercular meningitis rests our only possibility of a favorable prognosis, our only opportunity for expecting any advantage from therapeutics. The latter consists in keeping up the strength on the chance of our diagnosis being wrong, preventing violent measures being adopted, and if the corneæ begin to ulcerate, keeping the eyelids closed with a piece of sticking plaster.

"Congestion of the brain" is a very important subject, and is treated of by the editor himself, with the assistance of Dr. Bastian. Congestion of the brain is the first stage of two pathological processes, hæmorrhage and inflammation, each of which is represented in this volume; yet we are glad it has an article to itself, because it is the first stage also of a third process, more important than either, namely, that of recovery, and it is peculiarly as the curable stage of acute cerebral affections that it claims notice. In opposition to the views of Kellie, Abercrombie, and Reid, Dr. Bastian believes that observation and experiment alike show the amount of blood existing at one time in the cranium to be liable to variation, and he considers that too much stress has been laid on the mechanical peculiarities of the cerebral circulation. He holds, therefore, that its hyperæmia is a reality, and that it may arise, like hyperæmia elsewhere, from two causes, mechanical impediments to the due return of blood and vital irritation. In accordance with this pathology, Dr. Reynolds divides the symptoms into those constituting an apoplectic, convulsive, delirious, and febrile form of the disease, and apportions the treatment accordingly. His unprejudiced observations about detraction of blood are most judicious, as are those about other therapeutical measures. One hint about bedding we have found so valuable we cannot forbear from extracting it:—

"Much relief may be obtained by ensuring a position during sleep which shall prevent not only the head, but the head and shoulders from sinking down to the level of the body. This may be easily obtained by a simple contrivance placed under the bed or mattress upon which the patient lies; such an arrangement being much better than a mass of pillows, which shift their places, and often maintain the head in a condition of undue heat."

The same authors handle "cerebritis," "softening of the brain," and "adventitious products." The intention of the first-named paper is apparently to recognise inflammation of the substance as arising from meningitis, and to exclude it as necessarily the pathological condition in "red softening." In accordance with this pathology we find here described an "apoplectic, convulsive, and delirious" form of symptoms in acute softening, but not a "febrile," as in cerebral congestion. Of chronic softening, the symptoms are described as paralytic, either sudden or gradual. As to causes, while it is allowed that alterations in quality of blood and diminished nutritive activity of tissue elements may be looked upon as accessory causes of no unfrequent occurrence in the production of cerebral softening, especially in old people, yet practically the etiology of the disease is to be sought in impediments to the circulation of the blood. These are classified in the following manner:—

Morbid conditions of cerebral vessels.	Obstructing circulation.	Arteries . . .	Embolism. Thrombosis.
		Capillaries . .	Embolism.
		Veins and sinuses .	Thrombosis.
	Preventing osmosis and nutritive exudation.	Diseases of coats of capillaries and small arteries.	

The explanation of non-traumatic red softening as a consequence of mechanical obstructions to the circulation, and the consensus of evidence to the production of redness and swelling by this process from the analytical experiments of Cohn, Vulpian, Prevost, Cotard, from the application by Marey and Weber of the laws of hydrodynamics to physiology, and from the anatomical observations of Rokitansky, is clearly placed before the reader in abstract.

The remarks on prognosis are extremely judicious. It is pointed out that *cæteris paribus* the lesion is in proportion to the extent, rather than to the violence or abnormal character, of the phenomena; and therefore the prognosis is worse when the mind, sensation, and motion, are all slightly impaired, than when either one of them alone is profoundly affected; also that if there is evidence of much collapse or congestion, the disease may quickly pass away, however severe the symptoms; whereas the same amount of symptoms without congestion or collapse, would be of extremely grave import. Altogether, this is more encouraging than the usual way of treating the same subject.

"Adventitious Products in the Brain," shortly records what is known concerning such morbid matters as are not of sufficiently frequent occurrence as to have an article to themselves, such as cysts, hydatids, nodes, &c. It contains a great many references, which we trust are all correct.

In "Apoplexy and Cerebral Hæmorrhage," Dr. Hughlings Jackson always keeps in view the two parts of his subject as designated by the ingeniously constructed title. He confounds them neither in pathology, prognosis, nor treatment, as is too often done by both writers and practitioners. We are sorry for the necessity, we feel sure it was a stern necessity, which forced the editor to shear this essay down to half its length by the omission of the illustrative cases. It is worthy of notice that Dr. Jackson has never seen but one person bled for cerebral hæmorrhage, an innocence from blood-guiltiness which many of his readers will envy.

Dr. Gull has taken advantage of "Abscess of the Brain" being comparatively a rare disease, to treat the subject exhaustively by a collation of all the cases he could find sufficiently well recorded. We are so glad to get such a good crop of these that we will not grumble at giving up the ground (thirty-four pages) to its growth. It will be a classical *Memoire pour servir.*

Diseases of the Columna Vertebralis fall into the hands of Dr. Radcliffe. Their separate consideration is prefaced by some excellent preliminary remarks on the physiology of the part, and on what may be called its physiological pathology, that is to say an inquiry into the true significance of pain, spasm, and certain symptoms analogous to pain and spasm which figure conspicuously in the histories of spinal maladies. Thus is saved a good deal of digression or possible repetition in what follows. The revolution which Dr. Brown-Séquard's observations have made in nerve-physiology, the deposition of the spinal cord from the throne upon which Marshall Hall had placed it as the centre of a system, and its re-institution as a conductor, the reconcilement of the apparently contradictory results of lateral and vertical sections of the cord, and other results of the researches of the above-named physiologist, and of Mr. Lockhart Clarke, have made it needful for us all to go back to school during the last two years, if we would do more than prescribe by routine. And we think it probable that the great majority of the readers of the 'System,' will be thankful to have these results, albeit not medical, put before them in a clearer form than an original discoverer can attain to. We would especially commend the simplification of Dr. Brown Séquard's diagram.

In the latter part of his preliminary remarks the author reiterates the conclusions which he placed before the College of Physicians in 1862, namely, that pain and fever are antagonistic to one another, and that this warning is evidence of a present state, which is diametrically opposed to inflammation, though it often passes into it, which is in fact the swing of the pendulum in the opposite direction, and also that spasm has the same significance as pain, and is equally opposed to an inflammatory condition.

It is to be remarked that "inflammation" is here held to mean increased quickness of circulation, combined with an increased amount of blood in the capillaries. Dr. Radcliffe does not define it so, but it is evident from the context that he narrows the word to that sense.

The cases which illustrate the articles on "Spinal Meningitis, Myelitis, and Congestion," are very good evidence of Dr. Radcliffe's propositions; pain and spasm are conspicuous by their inconspicuousness. We think this more striking than would have been their entire absence; for in persons so very ill as these patients, the predominant anæsthesia and paralysis must, of course, by mere reaction, elicit a certain amount of the opposite state, during the acute stages of the illness. It is the predominance of symptoms, rather than their exclusive presence, which is the test of their being the real outcome of the lesion they are associated with. We give vent to this suggestion with some reserve, but still we think it applicable to more diseases than that immediately before us.

In the cases above referred to the post-mortem appearances in the spinal cord were clearly enough relics of acute inflammation. "Tetanus," with its absence of special morbid changes in that viscus, and its exhibition of pain and spasm, comes next in marked contrast. We think Dr. Radcliffe's way of taking a typical case as his text and appending to it what he has to say on the subject contributes to condensation, and incontestably adds interest to the matter. In discussing the etiology of this frightful and fatal complaint, the author does not attempt to trace any connection between the acknowledged causes, cold and damp and wounds, and the production of such a fearful consequence in only a few cases. He simply quotes the usual enumeration of the prominent circumstances, and the period of the manifestation of the results. He does not either point out its relations to hydrophobia, except as regards external symptoms under the head of diagnosis. We are surprised at this, because in the therapeutical part of his essay an analogy has suggested itself to his mind which we have often thought might be worked out into fertility, we mean that of snake-bites. He points to the cases of recovery after poisoning by the cobra and rattlesnake when excessive quantities of stimulants, such as alcohol and *eau-de-luce*, have been administered, as an encouragement to drench with the same agents, in the same excess, our tetanic patients. Ought not this suggestion to lead further? May not the connecting link between chilled wounds and spasmodic paroxysms be an animal poison generated in the wound during the process of healing? And, being an animal poison, therefore poisonous in extremely minute doses? And, being an animal poison, therefore latent in the system for long periods? And, being an animal poison, therefore specially fatal to the nervous system? The greater tendency of punctured and closed wounds to cause tetanus is very suggestive of the needlelike serpent's fang, and the frequent triviality of the dog's bite, which are the more deadly the less blood flows. The inoculation of animals with the matter from the wound might be an experiment worth trying in deductive pathology.

The article on "Spinal Irritation," we fear, is a somewhat dangerous one, as tending to erect into a separate class cases culled from hysteria, hypochondriasis, and neuralgia, and capable of being successfully treated on the principles already enunciated under the headings in this volume. Dr. Radcliffe does not give a single symptom pathognomonic of his disease which is not included in the description of those above named by Drs. Reynolds, Gull, and Anstie. Equally with them, he "pours in oil and wine," and uses blisters to the painful spots; though why, when recommending this treatment, he quotes without reprobation Mr. Teale's proposal to employ local depletion by cupping and leeches, we cannot imagine.

The most interesting of the remaining subjects treated by the same author is reflex paraplegia. Of this our knowledge is almost entirely derived from that form in which lesions of the urinary organs cause incomplete loss of power in the legs. Its diagnosis from the paraplegia dependent on spinal myelitis is deeply interesting, and important to both patient and pathologist, for it is an eminently curable complaint, whereas in the last-named recovery is the exception. The hints given by Dr. Radcliffe will be exceedingly useful to the practitioner, for upon his diagnosis depends whether he shall apply his remedies to the bladder or to the spinal cord, and whether he shall lead the sick to anticipate restoration of health or make up their minds to a gradual augmentation of the infirmity. Dr. Radcliffe differs from Dr. Brown-Séquard in the pathology of this paralysis; that physiologist is well known to have considered it as a phenomenon of "irritation" of the vaso-motor nerves of the exciting organ, say of the bladder, and to have connected it with a state of capillary contraction and comparative bloodlessness; whereas our author traces it to an inflammatory condition, especially suppurative, especially chronic. Certainly it would seem a strong objection to Dr. Séquard's view, that in states where the whole nervous system is in a state of great apparent activity, as in tetanus, where it may be presumed that the vaso-motor nerves participate in this state of irritation, and produce vascular contraction and anæmia of the spinal canal, paralysis is precisely the symptom which is *not* present. Whereas it is long continued conditions of the bladder, or prostate, or kidneys which notoriously originate paraplegia.

The article of Mr. Netten Radcliffe, on the pestilential nervous fever which has lately attracted so much attention from appearing as an epidemic in Dublin, ought doubtless to have accompanied the other zymotic diseases in the first volume. But it does not appear to have been ready at the date of that publication; indeed some of the valuable information which it puts before us is on facts too recent to have been then collected. So it has been placed among "the affections of the nervous system, to which it bears the closest relationship" under the name of "Epidemic Cerebro-spinal Meningitis." We think it would have been better to have put it in an appendix altogether, for it has no more business here than typhus would have among diseases of the lungs. The recent recognition of this as a distinct fever, and the number of active men who are in various parts of Europe working at the subject, would have been an additional reason for delay, for our information is gaining, if not in quantity, yet at least in accuracy, daily. The laws of its spread and diffusion are deeply interesting to us in this island, for hitherto England and Scotland have escaped the development of the disease into an epidemic, in spite of several sporadic instances having occurred, and we should be glad to learn how to retain our purity. The suggestions of Dr. Richardson as to the possible generation of the disease by an organic poisonous growth in decayed corn urgently demand investigation; for they will certainly create a panic, which, if justifiable, should be encouraged, but, if groundless, must be instantly arrested by correct information, as it would raise the price of food.

Of the articles on diseases of the nerves the most important is that by Dr. Anstie, on "Neuralgia." The graphic character of the clinical history of the disease as here given is unconsciously added to by the fact of the describer being himself a sufferer. We think too that such a misfortune makes a man's opinions as to the pathology more valuable; for he must be conscious of minor changes and slight symptoms, which would not be recorded by a non-medical patient, or not observed in another person, and is thus able to speak with great weight of those early modifications of function which are the clue to the real nature of diseases. We should be glad to have a collection of accounts of their own cases by medical physiologists. As to treatment we are not so sure that it is an advantage, for the effect of special remedies on a man's own constitution is apt to over prejudice him in their favour in spite of the different constitutions of those who consult him. We must have all observed how a physician of large stomach and appetite will stuff his patients, how the hard ascetic will starve them, how liberally a lover of the gifts of Bacchus will dispense them to him that is ready to perish, how one that has found water agree best with himself will expect the sick as well as the sound to be teetotallers.

To illustrate what we mean, as to pathology, we would especially point to the author's observation of the previously *anæsthetic* condition of the parts about to become painful—to his distinction between the *point of tenderness* and the seat of spontaneous pain, to his notice of the modifications of vegetative life, such as the atrophy of the hair, and the local epithelial coating of the tongue in the track of the affected nerves.

Dr. Anstie adds his testimony to the opinion that pain is an evidence of lowered vitality, not an exaltation, and that painful parts feel less, in respect of answering the purposes of feeling, than when in a normal condition. He advocates, also, the doctrine of an organic change in the *centres* of innervation, rather than the peripheries, as the cause of neuralgia; though he liberally gives the reader an opportunity by copious references of seeing the opposite advocated. And he views this organic change as of an atrophic nature.

On the anæsthesiæ and spasms which depend on local lesions of the nerves there is not much to be said in the medical treatise, but what there is seems well said by Dr. Warburton Begbie, except a few pages on "Wry-neck" by Dr. Reynolds.

The rest of the volume is occupied by Dr. Wilson Fox, who writes on diseases of the stomach. Of this, about half is a revised edition which has already been before the public in a monograph entitled "Diagnosis and Treatment of Dyspepsia," which we reviewed in the 'Medico-Chirurgical Review' for

October last (No. LXXX). We then described it as highly creditable to its author, and as giving a comprehensive sketch of the different views which have been taken from time to time by the highest authorities in medicine relative to the nature of the disease, and which are taken at the present of the many problems on doctrinal questions involved in this dark subject. The first three chapters of that work, viz., "On the Nosological Classification of Dyspepsia;" "On the General Symptomatology of the Stomach;" "On the General Symptoms and Causes of Dyspepsia," are here condensed into one article on the "Disorders of Function," with advantage, we think, to the vividness of the impression produced; for we do not seem to miss anything, and certainly remember the points of the argument better in the later work. Then comes an article on "Atonic Dyspepsia," in which we see sufficient additions to show that the author's mind is continually engaged in the practical part of the subject, which in this most common of all the diseases of digestion has principal play.

Under atonic dyspepsia Dr. Fox classes all those cases both where the loss of functional power is an expression in the stomach of an universal loss of functional power, a generally depressed vitality, and also where it is the result of local atrophy or degeneration; for, as he truly says, "the vital phenomena exhibited are frequently clinically undistinguishable."

The next article "On the Neuroses," or nervous affections of the stomach, is principally remarkable for the copiousness of the therapeutical suggestions contained therein; showing that they are an eminently curable class of disorders, which while they reward investigation in that direction, yet are apt to fix a false value on the means employed. Much more important than this long list of remedies is the remark by Dr. Fox, that they may be treated advantageously by modifications of the tonic and stimulant plan recommended by him for atonic dyspepsia, and that their cure under this system affords valuable proof of their true nature.

Under "Acute Gastric Catarrh," the author includes a large range of cases extending from the *embarras gastrique* of French practitioners to the *febris mucosa* of Frank, and actual erythematic gastritis, where suppuration takes place in the cellular tissue of the parietes of the organs. It seems to be the degree rather than the extent of the morbid process, which constitutes the case as one almost absolutely fatal, or so slight as hardly to require medical attendance. Yet that these slight "bilious attacks" are of the nature of inflammation, seems sufficiently evidenced by their being so often the commencement of that congested thickened condition of the gastric secreting membrane, which is here described and illustrated by woodcuts as "chronic catarrh."

The remainder of Dr. Wilson Fox's contribution has not been before published. It describes the various organic lesions found in the stomach after death, and allots to them the various pathognomonic symptoms by which they may be detected clinically. The headings are "Chronic Ulcer of the Stomach and Duodenum;" "Cancer of the Stomach;" "Hæmorrhage;" "Hypertrophy of the Walls;" "Stricture and Obstruction of the Cardiac Orifice;" "Obstruction of the Pylorus with Dilatation;" "Softening;" "Perforation;" "Rupture;" "Tubercle." Of these, the most elaborate is the first, to which that on hæmorrhage may be considered as an appendix; indeed, for an interesting portion of it (the treatment) the reader is told to "see Ulcer of the Stomach." And rightly is it made the chief object of attention, for the lesion that it describes is the only one that admits of cure, and upon our knowledge of the true nature of the morbid processes involved must rest our hopes of success in that direction. The next in length is that on "Cancer," in which the great interest turns upon diagnosis, as in all the rest of the articles which follow. We think this should have been more borne in mind by the author, and a due perspective observed in selecting the amount of information laid before the reader. For the sake of a few more references to authoritative observations on the diagnosis of cancer, a matter always of intense interest to the patient, we would gladly have spared some pages of statistics. And, on the other hand, on the question of ulcer, we should have expected to see brought more prominently forward a good many of the original researches on the pathogenesis of the disease to which reference is given in a note.

Before we conclude, some notices are due upon the general conduct of the volume, and we cannot but congratulate ourselves upon an improvement being manifested in some particulars which we previously ventured to criticise. The foppery of enumerating synonyms of diseases which are not synonymous, in a number of languages which nobody ever reads, is given up. The references are fuller, and we are not so often startled by the bracketed quotation of some surname, familiar, perhaps, to the writer, but unknown to us and to our successors. We are not quite satisfied with the binding of the volume; it is scarcely strong enough for its size, and two of our sheets have become loosened from the stitches in the act of reviewing. And we shall be afraid of having it bound, lest the valuable tables in Dr. Gull's article on "Abscess of the Brain," should get sewn into the back. Not only in these tables but throughout, the margin is very narrow.

To continue our grumbles—with reference not only to this, but to almost all publications—why cannot the edges be cut? Who would not willingly pay threepence a volume extra, and sixpence extra if the tops were gilt in the Fonthill fashion, to be saved this constant annoyance? They manage these things better in America.

REVIEW II.

1. *Researches upon the Venom of the Rattlesnake, with an Investigation of the Anatomy and Physiology of the Organs Concerned.* By S. WEIR MITCHELL, M.D., Lecturer on Physiology in the Philadelphia Medical Association. Published as one of the Smithsonian Contributions to Knowledge. Acccpted for publication, July, 1860.

2. *On the Treatment of Rattlesnake-bites, with Experimental Criticisms upon the various Remedies now in Use.* By S. WEIR MITCHELL, M.D., Lecturer on Physiology, &c., Philadelphia. J. B. Lippincott and Co. 1861.

3. *Experimental Contributions to the Toxicology of Rattlesnake Venom.* By S. WEIR MITCHELL, M.D., Member of the National Academy of Sciences, &c., New York. Moorhead, Simpson, and Bond. 1868.

"ANIMAL POISON." This is a term very generally made use of to indicate certain kinds of animal matter in some septic stage of decomposition; but there is, evidently, another sense in which the term may be applied, and that too with even a more rigid and appropriate accuracy.

The alarming, the sometimes fatal effects, resulting from the bites of venomous animals, would seem to require the designate of "*animal poisoning,*" whilst the material producing such consequences is, in the truest and broadest sense, an "*animal poison.*"

That the investigation of this poison, or more correctly speaking, that the investigation of these poisons (for, at present, we have no proof of the poison of the various venomous serpents being identically the same) should prove a subject of high interest, and that an experimental inquiry into their chemical composition, as well as into the mode by which their terrible effects are accomplished, should serve a very useful purpose can very readily be understood, for independently of the knowledge to be acquired by these means, as limited to the bites of venomous reptiles, and the best method of treating such injuries, it is very far from being improbable that researches of this description may aid, both directly and indirectly, in solving some of those problems connected with "*blood diseases*" which have hitherto defied the questionings of our most astute pathologists.

In Great Britain, fortunately, our personal experience in the matter of venomous serpents has been of the most circumscribed character, and hence it has happened, it may be supposed, that those of our countrymen who have written about them have confined their observations pretty much to the natural history and ordinary descriptive anatomy of these creatures; the *general* bibliography of the subject is, nevertheless, a tolerably extensive one, and includes the names of many of the most distinguished scientific men of different countries and of very ancient dates. Notwithstanding this, it is worthy of remark that a *complete* and, in every way reliable account of serpent venom has been (until within the last few years) nowhere to be found in the writings of any single author, nor, indeed, from the writings of all those who have contributed to the literature of the subject up to a recent date, collectively, can a full, comprehensive, and satisfactory account be gleaned. It is not meant by this that serpent venom, and the effect it is capable of producing in men and in animals, has not been studied and in written about by many able men, but simply that the crude and imperfect investigations made in years gone by require the corrections to be obtained from advanced science—errors to be erased—facts to be supported, and cohesion to be perfected between such facts, as being of a past period, and the additional facts and information arrived at in this our time, acquired by modern means applied to modern research.

Upon the subject-matter before us, the writings of Charas, of Redi, of Mead, and many others during the seventeenth century, added no really important knowledge to that furnished and handed down to us by the Greek and Roman fathers of medicine; or if these authors added anything to such knowledge, it is not going too far to say the information communicated was purely conjectural in its character; but in 1767 an admirable and vigorous effort was made by the Abbé Fontana to exhaust the subject of viper venom, and a record of his three thousand experiments served to impress the mind with an idea that he could scarcely have failed in the accomplishment of his object.

It is nearly impossible to over-estimate the value of Fontana's Essays "On the Venom of Serpents;" they must be considered, beyond all question, as an indelible record both of his industry and sagacity, and that they served to clear the way for subsequent research, and removed many existing absurd notions and vain conceits, is past all denial; still, with their many excellencies, they contain not a few errors, which, however, have only been proved to be such by the light and advantages gained (and guided) by, and through, an improved and more perfected science.

Beyond some desultory communications on the toxicology of venom, and frequent but unsatisfactory statements as to what may, perhaps, be called the therapeutics of snake-bites, nothing of noteworthy interest occurred until 1817, when Mangili asserted, *and proved,* that serpent venom when taken by the mouth is perfectly innocuous: later on (1843), Prince Lucien Bonaparte pronounced his opinion, and subsequently verified it by experimental analysis, that the venom of serpents is of an albuminous nature. From this date there has been no modern work devoted to the consideration of serpent venom, and the anatomy and physiology of the venom apparatus, until 1860, when the first of the works of Professor Mitchell, at the head of this notice appeared. This contribution was published for and through the Smithsonian Institution: as all works so published are submitted to the examination of a highly competent committee before publication, there is in this circumstance alone a guarantee that it is a book of no ordinary merit. It is a large quarto volume of 117 pages, and contains some excellent woodcut illustrations. The contents have special reference to the venom of the rattlesnake, and the anatomy and physiology of the various organs concerned in the secretion and emission of the poison.

The work is divided into eight chapters. The first chapter is occupied with attentively made observations on the habits of the "Crotalus" (the particular species of rattlesnake on which all the professor's observations and experiments were made) when in captivity; the inactivity of the creature, the difficulty of getting it to take food, the necessity for artificial feeding and the best method of effecting this somewhat ticklish proceeding. It appears that although unwilling to eat when in captivity, the creature displays no similar indisposition as regards taking water; a plentiful supply of fresh water, every day, is absolutely necessary to preserve the health of the "Crotalus" when in confinement.

The process of changing the skin is fully entered into, and also the question of the loss of the fangs; our author being of opinion that such loss takes place during the change of skin, and, as with some fishes that change their teeth, the loss of fangs occurs several times in the course of the year. Professor Mitchell considers that, as the outer layer of the cornea is shed with the skin, it is true that serpents are, at any rate to some extent, blind during the shedding of the skin.

Lastly, the supposed power of fascination possessed by serpents is discussed: on this subject Dr. Mitchell has formed a very decided negative opinion, and gives an amusing account of a variety of instances tending to conclusively refute this, at one time, very generally believed in faculty.

The second chapter contains a most admirable and carefully detailed account of the anatomy (general and microscopic) of the venom apparatus; the various structures, and even the weights, particular and relative, of all the parts concerned, are minutely entered into; whilst some original observations on the peculiar formation and functions of the gland duct give an additional value, and lend an increased interest to this part of the work.

The third chapter treats of "The Physiological Mechanism of the Bite of the Crotalus." The writer commences the chapter by remarking that—

"Of the many authors who have treated of the anatomy and physiology of the rattlesnake, and other venomous serpents, no one has entered fully into the subject of the mechanism of the movements which inflict the bite and inject the poison. Redi, Fontana, Tyson, Ranby, Smith, Home, Duvernoy, Soubeiran, and others, have nearly all in turn contributed something to this subject, but I find nowhere a full and complete account of the part played by the various muscles, and of the exact uses of many of the peculiar arrangements of tissue which characterise the poison apparatus. Nothing, in fact, can be more admirable than the mode in which the motions in question are effected, and yet, while they interest the physiologist, from the wonderful example they afford of a series of complex acts following one upon another in ordered sequence, to effect a certain end, they are not less interesting to the physician, who may learn from their study how he may be deceived as to the occurrence of poisoned wounds, and how the snake which appears to strike may really fail in its object, even though seeming to have inflicted a wound."

He then proceeds to give a very graphic narrative of all the elaborate movements made, from the period of the snake preparing to strike, up to the actual infliction of the wounds, and the withdrawal of the fangs; with regard to this last act, *i. e.* the withdrawal of the fangs, Professor Mitchell observes:

"It happens not unfrequently, that the teeth of the lower jaw catch in the skin of the bitten animal, and thus prevent the snake from retreating at once. When this takes place, the serpent shakes its head from side to side, with a motion which so nearly resembles the shake a dog gives his prey, that it has been mistaken by at least one observer for an expression of rage. It is really an attempt to escape; nor is it always successful, since a large animal will often drag a snake until the fangs themselves break loose, and are left in, or on, the bitten part."

The following sentence concludes the chapter:

"So far as I am aware, it is the only full account of the mode in which the bite is given, and of the parts played by the different organs and tissues concerned."

The fourth chapter is devoted to a consideration of "The Physical and Chemical Characters of the Venom;" whilst these characters are fully and ably descanted on in the body of the chapter, some very lengthy and interesting foot notes afford the necessary information as to how the venom is procured—the best method of securing the snakes, and of obtaining the poisonous secretion. Dr. Mitchell, after enumerating the various plans adopted by others for securing, without personal danger, a supply of venom, enters into a description of the course he himself adopts for the accomplishment of this purpose. He stupefies the animal with chloroform; about twenty minutes being the time necessary to effect this. He says if, at the expiration of this time,—

"The lower jaw hangs relaxed when opened, the neck is seized firmly, the fang caught on a saucer edge, and the glands stripped from behind forwards by pressure with the thumb and forefinger. The venom usually escapes alongside of the fang, from under the mucous cloak."

The operation is, no doubt, an effectual one; but from the danger (real or imaginary) attending its performance, we cannot look upon it in the light of a very inviting proceeding.

The quantity of venom obtainable depends upon two or three circumstances: the size of the animal, its healthiness, and the length of time it has been without using its fangs. Fifteen drops appear to have been the largest amount Dr. Mitchell ever saw ejected by a natural process, and twenty-nine drops the greatest amount he ever found a single venom gland capable of *holding*.

The colour of serpent venom varies from "a pale emerald green to orange and straw colour;" and when it has remained any length of time in the gland it is "of a darker hue than when its ejection follows rapidly upon its formation." No experimentalist has ever attempted to obtain the specific gravity of serpent venom; indeed, the difficulty of getting a sufficient supply for such a purpose is so great as to offer an apparently insurmountable obstacle. Our author, however, has adopted an ingenious process by which he is enabled to offer an approximation to this desirable object. He thinks the venom may become somewhat concentrated by remaining long in the gland, and fixes the mean specific gravity at about 1035.

The venom of the Crotalus when fresh, is described as being devoid of either taste or smell, whether in the fluid or dried state. Some authors, as Mead, and Brainard, speak of the poison as having "a peculiar and disagreeable odour;" and Mead asserts that it has an acrid and caustic taste. Jeter describes it as tasteless, but having the power to benumb the tongue when placed on that organ. Professor Mitchell tasted the venom of the Crotalus upon several occasions but could not recognise any such action.

Most authorities have represented the chemical reaction of rattlesnake venom as being acid, and Dr. Mitchell's experience is confirmatory of this; but he pronounces the reaction of the mucous membrane of the mouth of the Crotalus to be alkaline; in fact, so much so, that litmus paper reddened by the action of the venom became blue when left for a short time in contact with the serpent's jaws. Although the poison is subject to decomposition, and when kept long in the moist state smells most abominably, it, nevertheless, still retains its fatal qualities. It is not a little singular that whilst rattlesnake poison in a state of decomposition, loses none of its deadly virulence; vibriones, rotiferæ, and other forms of minute animalcular life, are found to appear in it when in this condition. Professor Mitchell is not satisfied with the analysis of viper venom given by Prince Charles Lucien Bonaparte in his essay, and after stating the grounds on which he bases his objection, explains the process he has himself adopted, and gives as the result the following as the composition of serpent poison.

"1. An albuminoid body. *Crotaline*, not coagulable by heat of 212° Far."

"2. An albuminoid compound coagulable by a temperature of 212° Far.

"3. A colouring matter, and an undetermined substance, both soluble in alcohol.

"4. A trace of fatty matter.

"5. Salts, chlorides, and phosphates."

In reference to the point which has sometimes been raised as to whether the poison gland of the serpent tribe is to be regarded as a true salivary gland, or not, it is remarked:

"The argument from anatomy alone would certainly teach us to respect this view as correct, and to consider the poison gland as a true salivary organ. Its position and general structure all favour this idea, just as the appearance and minute anatomy of the pancreas were once believed to authorise us in placing that organ among the salivary bodies, and in giving to it the name of the abdominal salivary gland. But in this case, as in the one before us, the broader light of physiological inquiry has revealed the truth, that anatomical resemblance, even to the minutest details, does not of necessity involve physiological likeness." * * * *

And then as to the secretion itself, the writer proceeds to say—

"Lastly, its singular nature as a ferment, poisonous to other animals as well as to its owner, constitutes a distinction, which, with the other points of difference already considered, forbid the physiologist to regard it, in any true sense, a salivary secretion, or its forming organ as a salivary gland."

With regard to the effects of various temperatures, and the influence produced by certain chemical agents on the activity of the venom, we have in this chapter some interesting and really valuable information, a considerable amount of such information being the produce of original research. The experiments described are very numerous, and Dr. Mitchell says that he planned and executed them for the purpose of increasing

"Our knowledge of the influence of physical and chemical agents upon the noxious properties of venom."

And he adds,

"They clear the ground for more just conceptions of the real value and therapeutic possibilities of antidotes."

From the experiments referred to

"It seems that neither *freezing*, nor prolonged *boiling*, deprives the poison of its life-destroying capability."

When the venom was treated with alcohol, oil of turpentine, solutions of nitrate of silver, ammonia, soda, and potassa (short of caustic strength), the action of the poison was neither altered nor delayed; even when mixed with strong nitric, sulphuric, and muriatic acid, with ammonia, chlorine water, iodine, &c., the potency of the venom was not interfered with.

The fifth chapter relates to the "Toxicology of the Venom of the Crotalus," and the first observations are addressed to a consideration of the effects produced on living vegetable matter, both in the lower and higher vegetable existences; the evidence as to serpent venom being capable or not of producing any pernicious effect upon vegetable life appears to be very conflicting; for, whilst Dr. Salisbury in the 'New York Journal of Medicine,' vol. xiii, new series, 1854, p. 337, gives a circumstantial and methodical account of having succeeded in poisoning four shoots of young lilac, a small horse-chestnut of one year's growth, a corn plant, a sunflower plant, and a wild cucumber, Professor Mitchell has signally failed in producing any such effect upon such plants as he has experimented upon. The following account appears in a foot note:

"An amusing story, which passed through three persons, reached the 'Philosophical Transactions,' vol. xxxviii, p. 321, in the following form: Sir Hans Sloane learned from Col. Beverley ('Hist. of Va.,' 2nd ed., p. 266), that Col. James Taylor, of Metapony, had stated to him that, having found a rattlesnake, he cut off his head, with three inches of his body. A green stick, the bark being peeled off, was put to the head. It bit it, when small green streaks were observed to rise up along the stick towards the hand. At this juncture, the colonel wisely dropped the stick, which, in a quarter of an hour, of its own accord, split into several pieces, and fell asunder from end to end."

Two sets of experiments made by our author to see whether the venom of the "Crotalus" would, or would not, prevent the germination of seeds, afforded evidence of the poison preventing germination; the seeds used were canary and mignonette.

Turning now to the action of venom on animal life, Dr. Mitchell makes the following appropriate commencement:

"In place of doubtfully deciding as to the cause of death, we are summoned to witness the operations of a substance which sometimes acts with a potency so swift as to defy observation, and which has a power to alter the blood and tissues in a moment, and with a celerity which is a source of unending wonder, even to one who, by daily repetitions, has become familiar with the changes thus produced."

We have then a lengthened series of experiments on cold blooded animals and on the animal itself; the whole observed facts of each experiment are thoughtfully and lucidly related and the results compared with those arrived at by other observers and experimentalists.

The sixth chapter is occupied with an examination of the "Toxicological Action of the Venom upon Warm-blooded Animals." A long list of experiments upon pigeons, rabbits, and dogs, is given, and a table of symptoms is appended to the experiments made upon the pigeons and rabbits. In every instance in which death occurred, a post-mortem examination was carefully made, and a very ample and clear account of the pathological condition of each part is given.

The "Action of the Venom on the Tissues and Fluids," forms the subject of the seventh chapter. The power of the stomach to absorb the venom of serpents is the most important question here brought forward. The experiments of Harlem, Mangili, Russell, Davy, and others, are all referred to and their evidence taken as conclusively establishing that the unbroken mucous surface of the stomach is incapable of absorbing this poison, "or of admitting it into the system in any form possessing noxious properties."

The effects of the venom on the pulmonary tissue, on muscle, on the heart, on the capillary system, upon the intestinal movements, upon ciliary movements, upon the nervous system, the sensory and motor nerves, and the nerve-centres themselves, upon the calorifacient functions, and upon the blood, are all duly considered and discussed through the medium of well devised and adroitly carried out experiments.

The eighth and last chapter refers to "Crotalus Poisoning in Man." Dr. Mitchell begins this chapter in a somewhat apologetic strain; and as his own words will best convey an idea of the difficulties under which he has laboured, when endeavouring to make this part of the work as complete and accurate as he wished, we proceed to quote them.

"The cases of rattlesnake poisoning in man have been separated from the rest of this paper, owing to the difficulty of grouping the phenomena of human poisoning with those observed in animals. This difficulty arose from the imperfect reports of such cases as have been recorded, and from the fact that, in man, the symptoms were possibly modified, in some instances, by the remedies used, and were thus no longer comparable with such as had been seen to exist in animals submitted to no modifying treatment. * * * * * Unfortunately, although I have collected at least fifty cases of crotalus bite, the most of these scarcely deserve the name of medical reports; and among the whole number, I have been able to select but sixteen, which were sufficiently rich in details to be of the slightest value. The numerous gaps in the accompanying table show but too well the want of full medical statements of the order and character of the symptoms, even in these select cases; and it is humiliating to observe that, of the four post-mortem examinations of the lesions in this mode of poisoning, but two were made in this country.

"If, then, in the table of symptoms in man, and in the following remarks upon them, such a lack of detail is met with as would disgrace the most ordinary report of 'an interesting case,' the blame must rest where it belongs, with the physicians of our own country, who have failed thus much in their duty as medical observers."

After these remarks, the writer proceeds to say that whatever may be the amount of difference in the virulence of the poison secreted by the different kinds of venomous snakes, the mode of affecting the system varies but little whether the injury be inflicted by the viper, the copperhead, the rattlesnake, or the dreaded, but not more deadly, cobra. He then gives the tables referred to in the above quotation, and subsequently dwells upon the following points: the sex of those injured; the situation of the wound; the local symptoms; amount of hæmorrhage from the wounds; local results; the constitutional symptoms; and the post-mortem examinations he has previously expressed so much dissatisfaction with.

The remaining part of the chapter is occupied with some brief allusions to the various antidotes which have from time to time enjoyed a short-lived reputation. Professor Mitchell divides these in the following manner:

"1st. Those which remove the poison and the poisoned part—excision and amputation.

"2nd. Those which partially remove the venom, and more or less detain it in the wounded part:—ligature; scarifications; suctions; caustics.

"3rd. Those agents which, being injected *into* the wound, or wounded part, are supposed to destroy the venom, or to render it innocuous, as injections of iodine.

"4th. Local applications of various substances, as alcohol, ammonia, indigo, olive oil, &c."

Of all the above the author seems to think disparagingly, saving and except amputation, excision, and caustics, *when not too long delayed;* he approves of the administration of stimulants.

At the end of the work there is an appendix (A), which gives "An Enumeration of the Genera and Species of Rattlesnake, with synonymy and references," by E. D. Cope.

There is also another appendix (B), giving a most ample bibliography.

The second work at the head of this article is a reprint from the 'North American Medico-Chirurgical Review' for March, 1861, and may be considered as a supplement to the first; it displays the same painstaking and accurate experimental examination of all the various proposed remedies for rattlesnake bite that marks the former work, and is, moreover, suggestive on many topics not mentioned in the larger book. The third contribution of Professor Mitchell is also a reprint, in this instance from the 'New York Medical Journal,' for January, 1868. The following extracted paragraphs may serve to show, perhaps, the motive which gave origin to this publication, and they may also indicate to those who have read the two first published communications, the desirability of not neglecting a perusal of this one, containing as it does the most important of this distinguished physiologist's researches and discoveries in connection with the ultimate mode in which the rattlesnake venom "affects the economy of animals."

"In the year 1860 I published, through the Smithsonian Institution, a paper of 117 quarto pages, upon the 'Anatomy, Physiology, and Toxicology, of the venomous organs of the Rattlesnake.' From the days of Fontana, 1781, no researches of any moment had added to our knowledge of the poison of serpents; and I had, therefore, the pleasure of contributing a large amount of completely new information to the modern history of animal poisons.

"Since the date of my 'Smithsonian Essay,' and of a paper on the treatment of snake bites which I published in the 'North Amer. Med.-Chir. Rev.,' March, 1861, I have suspected that in at least one point I was partly, and in another, altogether wrong, so that it became an imperative duty to correct my former experiments by a second, and more careful examination of the dubious conclusions which I had committed to print.

"As usually happens with those who question nature by the fertile method of experiment, I was gradually led aside into by-paths, which proved to be of the utmost interest, so that, besides the questions with which I started, I have found myself able to answer many others of equal, and some of far greater interest."

The subjects dealt with are classed as follows:

1. What is the dose fatal to pigeons?
2. Why is venom harmless when ingested by animals?
3. What surfaces have the power to absorb venom?
4. How does it act directly on the tissues?
5. Does it alter the blood, and how?
6. Is the venom poisonous to the serpent itself?
7. Is it capable of being physiologically neutralized by any of the agents introduced into practice since 1861, such as the sulphites and carbolic acid?

This notice has already been extended beyond the usual limit; we cannot, therefore, venture into a *seriatim* account of the conclusions arrived at upon the various questions raised above, but would direct especial attention to the experiments carried out to determine the manner in which the poison of the "Crotalus" affects the various tissues as being of the first importance, and of the highest interest.

Dr. S. Weir Mitchell's fame as an original investigator and physiologist, already places him in the foremost rank; and we are confident that these contributions to the literature of a confessedly very difficult subject, cannot fail to add another leaf to his well earned laurels.

Review III.

St. Bartholomew's Hospital Reports. Vol. III. pp. 486.

This volume of 'St. Bartholomew's Hospital Reports' contains twenty-five papers, and is prefaced by a memoir of the late Dr. Jeaffreson, physician to the hospital, who died at the end of the year 1866. The memoir does no more than justice to the memory of a most accomplished physician and estimable man, who, although he never lectured on medicine, and, unfortunately for his contemporaries and for posterity, never placed upon record the results of his extensive experience, was nevertheless held in the highest esteem by the students of the hospital, was consulted by a large circle of patients, and was beloved by a host of friends. It is satisfactory to learn that, although Dr. Jeaffreson had barely passed the middle period of life, his practice had been so successful that he amassed a very considerable fortune, and was about to retire, at least partially, from his professional pursuits, when death put an end to his distinguished career. Although his prospects, on starting in life, were not brilliant, he never had struggled with adversity: on the contrary, he was favoured in his youth by the friendship and active assistance of many persons of influence, and the advantages thus gained he turned to the very best account, and fully justified the estimate which had been formed of his character by those who were best qualified to form a judgment.

I. *Some Statistics of Pyæmia.* By William S. Savory, F.R.S.—In this paper Mr. Savory has collected the particulars of what seem to be the main features of 133 cases of pyæmia, ninety-five of them having been taken from the pages of the Medical Journals and the remaining thirty-eight having occurred in the wards of the hospital during three years. All the cases mentioned occurred in British practice, and no doubtful or questionable cases were included in the tables. Mr. Savory thinks that in some important facts the records of pyæmia are deficient, as, for instance, in omitting to mention whether there is any evidence of infection from without, and whether there has been proximity to other cases. Although pyæmia is often spoken of as arising from contagion, there is no evidence to show that it does so in the same manner as scarlatina or smallpox. But pyæmia commonly supervenes on foul wounds, and the emanations from these wounds may impregnate the atmosphere, and may be carried to adjacent healthy wounds in other persons, and in them create morbid action. It is generally thought that this disease is specially prevalent in hospitals, but this opinion may be partly due to the facts that patients are often admitted into hospitals who are already the victims of the disease, that cases in hospitals are more carefully recorded than those which occur in the houses of the poor, and that the great majority of persons who are the subjects of injuries or operations likely to be followed by pyæmia are admitted into hospitals. Still there can be no doubt that certain conditions sometimes prevailing in the best hospitals may predispose to pyæmia, because it is almost impracticable to avoid having an undue number of open wounds or sores in a ward, and then the atmosphere is likely to become tainted, in spite of the utmost care. The tables yield little support to the view that phlebitis or even thrombosis is a necessary antecedent of pyæmia. The earliest symptom of this disease, according to nearly all the tables, was rigor or shivering, and this symptom is the more diagnostic when, as is almost invariably the case in pyæmia, it is immediately or very rapidly followed by profuse sweating. The rigors appear to Mr. Savory to stamp the relationship of pyæmia to the specific fevers, and they may be regarded as indicating the operation of a poison in the blood in both cases.

II. *Practical Observations on the Nature and Medical Treatment of Obstruction of the Bowels and upon Constipation.* By Thomas Head, M.D.—Dr. Head draws a distinction between obstruction of the bowels, or an entire interruption of their ordinary functions, and constipation, which is a less serious affection. The former most commonly arises from intussusception and from the lodgment of particles of undigested food in or near the ileo-cæcal valve, and more rarely from hernia and from malignant disease of the bowels; the latter has its seat almost exclusively in the large intestine. In one of the cases related, which was that of a child five months old, there was obstinate obstruction of the bowels, and the treatment adopted by Dr. Head was to inject warm oil and quicksilver into the bowels, and then to hold the child up by the legs so as to allow the quicksilver to traverse the intestinal tube by gravitation. This practice, although novel, is described as being successful. In cases of malignant diseases nothing can be expected from treatment except the alleviation of the sufferings of the patient. Constipation of the bowels is a common affection, and it is almost incredible to what an extent the large intestines may be loaded with scybalous concretions without causing the patient any remarkable inconvenience. Married women, of middle age, have sometimes regarded the sense of fulness and weight of the abdomen as indicative of pregnancy. For the removal of such scybalous masses, the retention of which in the higher portions of the colon is likely sometimes to produce relaxation resembling diarrhœa, Dr. Head recommends small doses of blue pill and podophyllin, with a little acetate of lead, with about a quarter or a sixth of a grain of opium, and a drop of creosote at bedtime, for several successive nights, followed in the morning by a draught consisting of castor oil, liquor potassæ, a little chloroform, and a few drops of laudanum. Dr. Head notices that a laceration of the mucous membrane covering the sphincter ani is sometimes caused by the passage of hard scybalous fæces and by the forcible expulsive actions of the abdominal and intestinal muscles.

III. *On Convulsions in Children.* By SAMUEL GEE, M.D. —In this paper, Dr. Gee gives a summary account of 102 cases of epileptiform convulsions in children, and he divides them into three classes according as their causes are local, general, or uncertain. In twenty-four cases the convulsions were apparently symptomatic of local disease in or near the cerebrum, such as tumours, abscesses, disease of the vertebræ, &c. In 73 cases 1 was caused by anæmia, 1 by uræmia, 12 by acute specific diseases, as scarlet fever, measles, &c.; 1 by syphilis, 1 by exhaustion from a large ulcer, and one from chronic Bright's disease, but 56 were unaccounted for, and they are classed under the head of convulsions depending upon the general condition of the child. Dr. Gee considers that many cases of essential convulsions (eclampsia) are associated with a constitution tending to rickets. After giving a brief abstract of the 56 cases, Dr. Gee remarks that hypertrophy of the brain sometimes co-exists with rickets; that sometimes enlargement of the cranial cavity occurs in rickets, without the brain being enlarged at the same time; that in reference to dentition, the backwardness of the teeth and the tendency to convulsions are sometimes concomitants of the ricketty diathesis, and that in several cases the occurrence of measles did not produce fits, although the children had been subject to them previously. The cases of convulsions of uncertain origin were only five in number. The treatment adopted in most of the cases consisted in the administration of the bromide of potassium or ammonium (for a child, for instance, of a year old), in doses of four grains three or four times a day, and when the fits had ceased, of cod-liver oil and vinum ferri.

IV. *On the Relation of Life to other Forces.* By W. Morrant Baker.—In this paper, which is altogether speculative and philosophical, Mr. Baker draws a comparison between the forces exercised under the influence of life and those which are displayed by inorganic matter. He shows that the actions of life consist in a great measure of utilising force, and making it subservient to special purposes, as when the heat of the sun under the influence of vegetation is made to assist in the decomposition of carbonic acid, and the formation of woody fibre. Vegetation exercises a chemical force in the evolution of oxygen, and the fixation of carbon, and when vegetable matters are used, as they constantly are, as the food of animals, the latter exercise another force in the act of assimilation. Again, it has been found that certain organic products have already been prepared artificially by chemical processes, and it is not illogical or unreasonable to hope that even such substances as gelatine and sugar may be formed in a similar manner. But Mr. Baker also draws distinctions between the powers of life and those of inorganic matter; for instance, no chemical process can form a cell, and no machine of human invention possesses the power of self-development or of generation. The whole tendency of Mr. Baker's paper appears to be to show that the forces excited in living bodies are analogous to other forces, such as those denominated galvanic, chemical, &c., and that the belief in the doctrine of the mutual convertibility of all forces, vital and physical, and their unity and imperishability, is by no means inconsistent with a belief in the existence of an all-powerful Creator.

V. *An illustration of extensive Cancer, traceable to dispersion from the primary Tumour, as distinguished from its constitutional reproduction.* By Charles H. Moore.—After describing the different modes in which cancer is disseminated, Mr. Moore relates the case which forms the subject of his paper, and in which the communication between the cancer and the surrounding parts was maintained by the agency of a creamy liquid, and the continuity of distant tissues with the original disease was maintained.

The patient was a woman, aged 48, who suffered from a rapidly-increasing tumour of the left breast, which soon formed an ulcer. There was a cluster of enlarged and moveable glands in the axilla, and a firm and globular gland was felt above the clavicle and behind the sterno-mastoïd muscle. An operation was performed with the view of relieving local suffering and temporarily arresting the growth of the tumour, and the knife was employed in the usual way, care being taken to remove with the tumour all the thickened integument as well as the diseased axillary glands. In about two months and a half the wound was entirely healed, but two small, hard nodules were perceived below the cicatrix; and in about two months more six tumours were observable under the skin below the scar. But Mr. Moore specially draws attention to the fact that above the scar the integuments were pale, flat, and supple, and the supra-clavicular gland behind the sterno-mastoïd was not larger than before the operation. The disease again spread rapidly in all directions, but the contrast of the appearances above and below the scar was still very striking, the tumefaction and discoloration being much more marked below than above. Death ensued in a few months more, and it was then found that the body was not emaciated, and the muscles were well nourished. The diseased mass presented a creamy whiteness, and the surface, when cut or squeezed, yielded abundantly a perfectly white juice resembling cream. The disease was found upon microscopical examination to be cancerous, and the milky or creamy fluid contained numerous oil-globules. From the examination of the organs affected with the disease, Mr. Moore infers that after the operation was performed, and a tough, transverse scar was established across the left side of the chest, the lymphatic current was temporarily arrested in its course upwards, but was conducted downwards into the lower part of the left side of the thorax, and subsequently to the bronchial glands, diaphragm and liver, which last, however, presented only one cancerous nodule. The mesenteric glands were not affected, and hence the patient was maintained in a state of general good nutrition. Mr. Moore considers that in this case the disease was nourished from healthy blood, and that the whole source of the cancerous impregnation was derived from the left mammary region, whence alone the oily products overflowed the tissues.

VI. *On the Examination of Patients suffering from Deafness.* By Thomas Smith.—In this paper Mr. Thomas Smith, who disclaims the character of a specialist in diseases of the ear, describes generally the affections to which that organ is liable, and the methods by which they are most readily investigated. He draws a comparison between the eye and the ear to prove the essential difficulty attendant upon the investigation of diseases of the latter as compared with the former, the one organ presenting a number of transparent structures which are easily seen, and the other having nearly all its structures concealed in bony cavities. Mr. Smith describes the instruments necessary for investigating diseases of the ear, and shows that they need be only few in number and simple in construction, and he gives directions as to the inquiries which should be made of the patient when examining into the history of the disease. The use of an ordinary watch is available for ascertaining generally the existence and the degree of deafness; but in exploring the condition of the external meatus, a small concave mirror may be used, together with the simple tubular speculum of Sir W. Wilde. To ascertain the patency of the Eustachian tube, the patient should blow air into the tympanum while the surgeon listens, by means of the otoscope, to the effect produced. The Eustachian catheter is also described by Mr. Smith, and also the mode of introducing it, which is a matter of some difficulty to be overcome only by practice upon the dead subject, or on an anatomical preparation.

VII. *On Disease of the Mitral Valve.* By J. Andrew, M.D. —Dr. Andrew commences his paper by relating a case in which it would appear that a disease of the mitral valve was cured. The patient was a girl, aged 9, in whom the physical signs and some of the rational symptoms very clearly indicated mitral regurgitation, but after treatment for some months the physical and other signs began to improve, and eventually, but not until about three years from the date of her first attendance, the characteristic murmur disappeared, and she became quite well. The improvement was much greater than in any other that Dr. Andrew has ever observed, but he has notes of several cases in which the progress made was so considerable that he hopes the results in the present case may be equally favorable. In considering the question of the possibility of recovering from mitral disease, it is necessary to distinguish between the several forms of lesion which give rise to regurgitation, for some of these appear to be remediable; but of mitral constriction Dr. Andrew says nothing, believing it to be all but a hopeless affection. Incompetence, however, depends upon causes which are sometimes removable. In rheumatic fever, for instance, it may happen that only the surface of the valve is inflamed, and the murmur may disappear, and no trace of the disease will be left, but in other cases the substance of the valve is inflamed, and the murmur is developed more slowly, and the disease is far more permanent. Supposing that mitral disease is remediable in certain cases, Dr. Andrew suggests that the curative measures should have for their object to diminish the sum-total of the blood in the body; to maintain the nutrition of the heart and its muscular power, and to diminish the frequency and energy of the heart's action. The first object is promoted by prescribing a somewhat restricted diet; the second by the employment of some preparation of iron,—preferably the tincture of the perchloride,—or quinine; and the third by the avoidance of all excitement and by the use of digitalis.

VIII. *Report on the Cases of Cholera treated in the Wards of St. Bartholomew's Hospital during the epidemic of* 1866. By WILLIAM CHURCH, M.B.—The number of cases admitted into the cholera wards of the hospital in the epidemic of 1866 was 136, being a smaller number than in former epidemics. Mr. Church arranges in tables the history of the cases admitted, giving the names, ages, and previous residences of the patients, together with the nature of the premonitory symptoms, if any existed, the date of death or of discharge, and other particulars. The proportion of deaths to admissions was 33·08 per cent., being a higher proportion than in the epidemic of 1854, but a lower one than in that of 1848. Premonitory symptoms, by which Mr. Church means those which existed at least twelve hours before the commencement of the algide state, were absent in 43·1 per cent. of fatal cases, and in 21·6 per cent. of non-fatal cases. The temperature was noted in thirty-one cases, twenty-two of which were fatal, and nine recovered. As a result of his observations, Mr. Church considers that the fall of temperature below 94·5° was an indication that the case was severe, and the chance of recovery small. The microscopical examination of the discharges did not lead, apparently, to any very important results, the chief microscopical characters of the stools being the presence of large cellular bodies resembling mucous corpuscles, and a quantity of faintly granular material, with numerous bodies resembling free nucleoli. The stools were sometimes of a brickdusty appearance, due in many instances to the presence of blood-corpuscles; and this appearance seemed to be invariably a fatal symptom. The treatment was of the most varied character, but it was uniformly unsatisfactory. In only one case, under Mr. Church's care, was the injection of saline fluid into the veins attempted. The measure was attended, apparently, with some success; but the patient died soon after the injection. In eighteen cases a post-mortem examination was made, and the appearances observed are arranged in a tabular form.

IX. *Note on Dr. Roberts's method of estimating Diabetic*

Sugar. By PHILIP J. HENSLEY, M.A., M.B.—Dr. Roberts's rule for estimating the amount of sugar in a specimen of diabetic urine is to ferment the urine by means of yeast, having first taken the specific gravity, and then, in twenty-four hours, taking the specific gravity again, when the fermentation has ceased, and the scum has subsided. The density after fermentation is subtracted from the density before fermentation, when the "density lost" is ascertained, and the number of degrees of "density lost" indicates as many grains of sugar per fluid ounce. This method is virtually to estimate the amount of sugar by determining the weight of carbonic acid lost, instead of by actually collecting and measuring the carbonic acid, as is usually done. Mr. Hensley points out that the plan pursued by Dr. Roberts is open to several objections, and he shows, by a series of mathematical formulæ, the errors which it involves, but which, we may remark, are not very great, and do not much invalidate Dr. Roberts's rule, which, as Mr. Hensley admits, is extremely useful for ordinary purposes.

X. *Observations on the Passage of certain Substances into the Urine in Healthy and Diseased States of the Kidneys.* By DYCE DUCKWORTH, M.D.—Dr. Duckworth, in this paper, embodies a series of experiments conducted by him during the last two years. The first series of experiments refers to the excretion of some pigmentary and odorous substances by the healthy kidney, including the iodide of ethyl, iodide of potassium, bromide of potassium, indigo, aniline, logwood, turmeric. and santonine. It is known that most of these substances can be detected in the urine when the kidneys are healthy, but in opposition to assertions to the contrary, Dr. Duckworth shows that the pigments of indigo and logwood are excreted by the healthy kidneys. The second series of experiments was made upon patients suffering from disease of the kidneys, and the question was, to determine whether medicinal and odoriferous substances passed through the unhealthy as well as the healthy organs. The general opinion was that they did not; but Dr. Duckworth shows that this opinion is not altogether supported by facts. In his examinations he employed chiefly santonine, iodide of potassium, and turpentine; and he found that, in a certain number of cases, where the patients were suffering from diseased kidneys, the above-mentioned substances were found in the urine. Santonine, Dr. Duckworth believes, has not been previously employed in the manner described in his experiments; but he observes that it is easily exhibited, is the least unpleasant of all the substances he has used, and also yields its reaction in a very marked manner.

XI. *Ligature of the External Iliac Artery.* By Augustin Prichard.—Although ligature of the external iliac artery is a rare operation, it happened that the two cases recorded by Mr. Prichard occurred at the same period in the Bristol Infirmary. In the first case there was aneurysm in both thighs, and the femoral artery on the one side and the external iliac on the other were successively tied, though at an interval of four years. The first operation, which consisted in tying the femoral artery, was quite successful, and the recovery was rapid; but the second was attended with considerable difficulty, owing to constitutional and other complications. The aneurysm in this latter case was immediately below Poupart's ligament, and evidently extended a little upwards, and the ligature of the external iliac was effected with some little difficulty, and was followed by local inflammation and great constitutional disturbance. But the ligature came away on the twenty-second day, and the patient seemed to be recovering, when he was seized with a fresh set of symptoms, partly neuralgic and partly inflammatory, and the limb was stiff, hard, and painful. Eventually, however, the man entirely recovered, and resumed his usual employment, which was that of a butcher. The second case, which occurred at the same time as the first, was one of femoral aneurysm, in which ligature of the external iliac was also performed; but in this instance the operation was performed without any difficulty, and the patient recovered slowly, and without any remarkably unfavorable complications.

XII. *Remarks on all the Principal Cases of Injury of the Head admitted into the Hull General Infirmary during the Six Years from* 1858 *to* 1863. By Charles Jewel Evans.—Mr. Jewel Evans in this paper arranges in a tabular form the history of forty cases of injury of the head, placing the simpler cases first, and afterwards the more serious and the fatal ones: but no cases of scalp-wound, unless complicated with serious injury, are admitted. More than half the cases were from falls into the hold of a vessel, or into a dry dock, and of the whole number more than half, namely twenty-two, ended fatally. In five instances the operation of trephining was employed, but the result was fatal in all but one, and Mr. Evans agrees in the opinion now generally entertained that the operation of trephining is not one from which a hopeful result may be often expected. In many cases in which the trephine is used the injury is so serious that no favorable termination can be even hoped for, and many cases which recover without trephining would, in all probability, be seriously endangered by the operation. When recovery takes place after the use of the trephine, it is a great question whether the instrument deserves the credit of the cure. Venesection was employed in two cases both of which were fatal, and this operation is now comparatively little employed in injuries of the head, but Mr. Evans thinks that local bleeding, especially in mild cases, is sometimes decidedly attended with benefit. Mr. Evans confirms the general opinion that the discharge of thin watery fluid from the interior of the cranium, although rare, is pathognomonic of fracture of the base of the skull. Facial paralysis, on the same side as the injury, is quite as much pathognomonic of fracture of the base of the skull as discharge of serous fluid, and is even more pathognomonic of fracture of the petrous bone, the facial nerve being injured in its course through that bone. Very little reliance can be placed upon the state of the pupils in injuries of the head, but the most common condition is that of dilatation. The respiration, in the cases where it was noted, was generally slow and laboured, and the pulse was usually slow.

XIII. *On the Value of the Thermometer as an Aid to the Physician.* By W. Ainslie Hollis, M.B.—The investigations which form the groundwork of this paper included an examination of more than sixty cases of phthisis, and a still larger number of other cases as diabetes, disease of the heart, aneurysm, typhoïd and typhus fevers, pneumonia, bronchitis, pleuritis, erysipelas, scarlatina, phlebitis, &c., and Mr. Hollis establishes the following proposition from his researches, namely, that "all local disorders, whether organic or functional, have a tendency to modify the thermometric ranges of the body, either by producing local variations of temperature, or by affecting those of the system at large." He then draws up in two tables the causes which tend to lower, and those which tend to raise the temperature of the body. Among the causes leading to diminished temperature are evacuations of various kinds, as for instance, diarrhœa, and looseness of the bowels, but when this diarrhœa is caused by ulceration of the intestines, as in typhoïd fever, the temperature is not diminished, but is raised. Perspiration and hæmorrhage also tend to diminish the temperature, and the same effect is produced by blisters and other counter-irritants. It is stated that the favorable action of blisters in acute rheumatism may be explained in this view. Deficient aëration of the blood, from whatever cause, will also diminish the temperature of the body. On the other hand, the temperature is raised in many acute and febrile diseases, but still the height of the thermometer in a case of fever is no criterion, *per se*, of the severity of the attack, for the temperature may be very high in a favorable case, and much lower than that point in a fatal one. The rapid degeneration or destruction of tissue such as occurs in certain inflammations, and in tuberculosis, carcinoma, and ulceration, also tends to increase the temperature. On the whole, Mr. Hollis arrives at the conclusion that thermometric registrations, although valuable in themselves, do not supersede or lessen the value of careful observations of the general symptoms of disease, and that the real value to be assigned to any given temperature must depend upon a due consideration of the processes which have caused it.

XIV. *Extraordinary anomalous Affection of the Nervous System in a Boy.* By LUTHER HOLDEN.—The subject of this curious case is a boy, a patient in the hospital, between twelve and thirteen years old, whose only morbid affection appears to be a small swelling of the neck about the size of a hen's egg, situated on the right side of the neck, and who usually seems to enjoy very good health. But when this swelling is touched, however slightly, a series of extraordinary phenomena immediately present themselves, the boy becomes deaf, dumb, and blind, insensible to pain, and tetanic. After remaining in this state some forty or fifty seconds, he recovers after drawing a deep sigh. The physiological and pathological nature of this affection has been investigated hitherto in vain, and all therapeutical appliances have been fruitless. A proposition has been made to remove the tumour, which seems to be of a fatty nature, but there is a division of opinion as to the probable result of such a proceeding. Mr. Holden's own opinion is that its removal would not be attended with any serious results, but the further history of the case is promised in a future number of the 'Reports.'

XV. *Surgical Cases in the Devon and Exeter Hospital.* By PHILIP CHILWELL DELAGARDE.—This paper contains the record of thirty cases taken from the surgical practice of the hospital, and includes surgical affections of the hand and of the head and neck, together with several cases of cancer. In a note on scirrhus and other forms of cancer, Mr. Delagarde expresses his opinion in favour of operating in certain cases of this disease, as he inclines to the belief that cancer commences as a local affection and afterwards affects distant parts, in contradistinction to the circumstance that the local phenomena of scrofula and syphilis are manifestations resulting from constitutional taint. Even when little hope of an absolute cure can be entertained in cancer of the breast, Mr. Delagarde does not refuse to operate, because such a proceeding alleviates the symptoms of the disease and does not involve any risk to life in well-selected cases. The instances in which Mr. Delagarde thinks an operation advisable are those of scirrhus and epithelioma, but in encephaloïd cancer there is not much chance of cure by operation. He prefers the knife to the use of caustic pastes, and he is unacquainted with the effect of acid injections either in primary or secondary scirrhus.

XVI. *On Tracheotomy in Children; its Method, its Dangers, and its Difficulties.* By F. Howard Marsh.—In this paper Mr. Marsh records the conclusions at which he has arrived chiefly from his experience as House Surgeon at the Hospital for Sick Children. In the first place, he combats the objections which have sometimes been made against the operation, and he concludes that it can be as deliberately performed as any other surgical proceeding, that the struggles of the patient may be obviated by chloroform, and that the apprehension of dangerous hæmorrhage has been exaggerated. After a brief but careful description of the structures concerned in the operations for opening the windpipe, he draws a comparison between laryngotomy and tracheotomy, declaring his opinion to be in favour of the latter as being on the whole more efficacious, although laryngotomy is most easily performed. But laryngotomy affords but little space for the introduction of the tube, it injures the integrity of the larynx and the vocal apparatus, and the tube causes great irritation and distress. In tracheotomy, few structures of any very great importance are involved, the tube is easily introduced, and the risk of hæmorrhage is but slight. Mr. Marsh then points out the spot most suitable for the operation, which should not be too low down on the one hand, where the trachea is very deep seated, nor too high, where the thyroid gland is placed, and which it is expedient not to wound. The tube to be employed is then described, and the different steps of the operation are minutely related, together with the after-treatment, so far as surgical ministrations are concerned. Some cases are then recorded in which the operation was performed, and in some the proceeding was successful; in others, where the result was less fortunate, the causes of the failure are pointed out. The patient may die from the constitutional disease for the relief of which the operation was performed, or from the local effects of the operation itself, and sometimes in cases which promise well, some accidental circumstance produces a fatal result, as where a portion of false membrane becomes entangled in the canula. Ulceration about the wound, or of the trachea around the canula, is also a more common result and a more frequent source of danger than is generally supposed, and Mr. Marsh has notes of nine cases where this complication occurred.

XVII. *Case of Anchylosis of the Atlas to the Occipital Bone and of Dislocation and subsequent Anchylosis of the Atlas and Axis.* By Professor TURNER.—The case was taken from a specimen in the Anatomical Museum of the University of Edinburgh, but nothing was known of the history during life. From the appearances observed it seemed that the articular surfaces of the bones had been diseased in the first instance, the ligaments relaxed and destroyed, and the bones subsequently displaced and finally anchylosed. The size of the spinal canal was diminished in the locality where the disease existed, and it is inferred that the spinal cord must have been considerably atrophied or much compressed in the antero-posterior direction.

XVIII. *A Discussion of the Mechanical Theories which have been advanced to account for the origin of Pulmonary Emphysema.* By PHILIP J. HENSLEY, M.A., M.B.—Mr. Hensley first considers three modes of explanation which have been adopted in accounting for the existence of emphysema. One offered by Laennec supposed that the lesion was caused by obstruction of the bronchial tubes, and by the action of the inspiratory muscles, which, being more powerful than the expiratory, would force in more air than expiration was able to eliminate, and thus the air would become imprisoned. The second explanation attributes emphysema to violent efforts in coughing and other expiratory acts, which are supposed to cause rupture of the air vesicles. A third view is that pulmonary emphysema is "a secondary mechanical lesion depending upon some condition of the respiratory apparatus which leads to partially diminished bulk of the pulmonary tissue, and consequently disturbs the balance of the air in inspiration." To all these views Mr. Hensley takes exception, and although he is not himself prepared to offer any very satisfactory explanation of the cause of emphysema, he explains some circumstances which he thinks may give rise to stretching of the lung tissue, and thus cause dilatation of the vesicles. In almost all cases emphysema is preceded by frequent attacks of bronchitis, which disease prevents the free passage of air into the bronchi; and Mr. Hensley thinks that if the bronchi leading to some parts of the lungs are impassably blocked up, the tension in inspiration will be borne unduly by neighbouring portions, and thus dilatation of some of the vesicles be established. But he regards the stretching of the lung tissue only as an accessory to the lesion, and he believes that there is in the first instance a degenerative process in the tissues as well as an inflammatory condition of the bronchial mucous membrane, and that emphysema is caused therefore not only by mechanical, but also by vital causes.

XIX. *Respecting the Treatment of Fractures of the Lower Extremities in the Wards under the care of Mr. Paget.* By J. Astley Bloxam.—Before describing the new forms of apparatus employed at St. Bartholomew's in the treatment of fractures of the lower extremities, Mr. Bloxam alludes to the circumstance that no apparatus is now employed in fractures of the femur in children and in fractures of the patella. It is found that splints, bandages, and other such applications, are attended with great inconvenience in young children, and that the fractures may be safely treated by rest and the maintenance of a suitable position of the limb. Fractures of the patella are also treated without any apparatus, the patient being confined to bed with the injured limb extended on a level, with sand-bags placed on each side, and with a cradle to keep off the weight of the bed-clothes from the limb. In a few days from the time of the accident, the extensors cease to act, and the fragments approximate themselves, the union, however, being usually ligamentous. The description of the apparatus employed in the treatment of other fractures of the lower extremities is illustrated by diagrams, without the aid of which a description would be impossible. It should be mentioned, however, that in fractures of the thigh, the inconveniences of the long splint are obviated by some novel contrivances, and that the new apparatus employed in these cases is both useful and convenient.

XX. *Case in which a large Hydatid Cyst was removed from the Chest, with ultimate complete recovery.* By Reginald Southey, M.D.—The subject of this interesting case was a female, aged thirty-one, who suffered from a variety of anomalous symptoms, somewhat resembling neuralgia and hysteria, from cough, though without expectoration, pain in the right side of the chest, and dyspepsia. No benefit followed from the remedial measures adopted, and change of air seemed to make her worse than before. The local indications and the physical signs were very obscure, but at last a marked bulging made its appearance upon the right side of the spine, extending into the interscapular region. An exploratory incision was then made by means of a trocar, and a large quantity of puriform serous fluid was evacuated, and some relief was afforded. But eventually a free incision was made between the sixth and seventh ribs, and a soft jelly-like mass protruded which was recognised as part of an hydatid cyst, which was fortunately entirely removed, its size being about that of a pig's bladder, and it was perfectly unattached. It appeared to be formed in the sac of the pleura above the diaphragm, and was wholly unconnected with the liver. The patient was restored to health in less than three months after the operation, and although she suffered from occasional pain in the right side, and the lung on that side was probably permanently altered in structure, she appeared stouter and better looking, and actually increased in weight more than a stone.

XXI. *Some Cases of Disease of the Brain.* By John Crockett Fish, M.B.—This paper contains a brief history of five cases of cerebral disease, three being cases of tubercular meningitis, one of acute meningitis, and one of abscess of the cerebellum. All the cases were fatal, and in all there was considerable difficulty in the diagnosis, owing to the anomalous nature of the symptoms. In the last case, where disease of the cerebellum existed, there was also disease of the petrous portion of the temporal bone, and it is remarked that the danger in cases of chronic otorrhœa (which had been the first symptom in the case), may be staved off almost indefinitely by attempts to solicit the return of the discharge, and by other appropriate means.

XXII. *On Senile Scrofula.* By James Paget, F.R S.—Mr. Paget remarks that scrofula is much more common in advanced life than is generally supposed, and that instances of the kind are to be found with equal frequency in private and hospital practice. The diagnosis between scrofula and gout in the old is often difficult, and it must be made by observing the co-existing constitutional characters, or other marks of the disease; and in distinguishing scrofula from cancer in advanced life, it is to be remembered that the hard cancer is more common in the old than the young, and that cancer is painful and increases rapidly.

XXIII. *The Anatomy of Brain Shocks.* Part I. By George W. Callender.—This paper, which is the first of a series, contains forty-eight cases of injury or disease of the brain. The whole series is to include a hundred cases of the same kind, all fatal, which occurred and were recorded by the late Dr. Kirkes and by Mr. Callender, during the years 1849-1859 inclusive. Before describing the series of fatal cases, Mr. Callender refers to a few instances of concussion or contusion of the brain, which show that various subjects are affected in a very different manner by these accidents, partly from the varying nature of the injury, but in some measure by the constitutional peculiarities of the individual. The fatal cases are arranged in three distinct groups, the first containing those which were fatal from pressure on the surface of the brain or the walls of the ventricles; the second containing cases in which portions of the brain were destroyed by disease; and the third comprising cases in which there was bleeding into various parts of the substance of the brain.

XXIV. *A Case of Traumatic Aneurysm.* By Holmes Coote.—In this case there was a wound of the femoral artery and vein, with effusion of blood into the limb. The case seemed to be doing well under pressure, but during the treatment the patient became excited in conversation, and from this and other causes, profuse hæmorrhage occurred, and although the limb was promptly amputated a fatal result ensued.

The reports conclude with an extract from the "Statistical Report of the Hospital for the year 1866."

Review IV.

Structure and Function of the Retina.

The microscopic examination of the retina is beset with such formidable difficulties that even those who are most conversant with the subject scarcely succeed in mastering all its details, or in reconciling their own observations with the conflicting statements of others. And since the interest which attaches to researches of this kind is seldom felt beyond the circle of those personally engaged in them, it cannot be expected that the medical practitioner already overtasked by the demands of a many-sided science should follow very closely the laborious advance of retinal anatomy.

But looking beyond this drudgery of anatomical detail to the fair prospect which it offers of a final explanation of the phenomena of retinal vision; looking also to the extreme interest and importance of the scientific problems therein involved, we cannot but regard the numerous contributions recently made to retinal anatomy with a satisfaction proportionate to their present and prospective significance; for whether we measure their value by the amount of time and pains bestowed on them, or by their own proper merits, we have equal reason to congratulate ourselves on the great progress made towards a satisfactory settlement of the anatomical basis on which the future physiology of vision may rest.

The retrospect, however, becomes more perplexing, as our scrutiny of facts must be closer and our breadth of view wider. Amidst an *embarras des richesses* arising from the researches of so many independent observers, a clear insight into our present position is to be obtained only by free criticism of each disputed point. Meanwhile discoveries multiply, and the reader, whose point of view is that of a past period of fact and opinion, and who is dependent on such chance literature as may fall in his way, finds himself in the midst of a transformation scene, of which the first masks are all he can recognise. Standing *super antiquas vias* he is more disposed to remain *laudator temporis acti* than to join in any forward movement. So much of fiction has withal intertwined itself with the growth of scientific fact as to necessitate a careful sifting of the abundant material now collected. For this, however, leisure and opportunity too often fail, although the inquiry might otherwise possess sufficient attraction. We propose, therefore, in the present article, to attempt a brief summary of the results hitherto achieved, in the hope of supplying thereby a want which may be felt by many of our readers.

For the greater part of our knowledge we are indebted to foreign anatomists, whose persevering efforts attest alike their skill and patience in microscopic investigation, and their courageous faith in the ultimate triumph of scalpel and lens over the mysteries of organic structure. Without disparagement of British anatomy, we have but to contrast the circumstances which favour the prosecution of such studies abroad with the slight inducement held out to those desirous of pursuing a similar career in England to account for any seeming indifference or inferiority of research. The fact, nevertheless, remains that we have to draw largely on Continental sources for the material of our physiological anatomy, as may be seen at a glance when we compare our handbooks and journals with those of Germany. One of the ill consequences of this dependence on foreign labour appears, in the absence of any continuous effort on our part, to place our knowledge of the microscopic anatomy of the retina (and other organs of sense) on a par with that which we possess of the numerous organs whose diseased states daily force themselves upon our observation.

Micrology has, however, won its own undisputed place and authority in physiological science; and those who cannot rely on their own experience must be content, if not to swear to the words of a master, yet at least to accept the labours of those who have devoted themselves to microscopic studies; more especially since physiological anatomy has become the very corner-stone of pathology and rational medicine, are we bound to give the fullest consideration to those new aspects of organic life and function opened to us by comparative histology. The larger features of comparative anatomy are now supplemented by the minutest details which organic matter presents to the scrutiny of the microscopist armed with magnifying powers yearly improved and increased, and it would be indeed passing strange if no practical results should be gathered therefrom. Again, the study of embryonic development, besides yielding its interpretative clue to the obscurer facts of general anatomy, reveals also the *genetic* relations between the several elements and tissues, and places in our hands an intellectual pass-key wherewith to open and explore the secret passages of nature's labyrinth.

But all this microscopic research demands a practised eye and hand and a power of interpretation acquired only by experience. The instrument is efficient only in the hands of an expert. And herein lies the advantage of the continental system of academic instruction. Under a master's eye, and encouraged by the words and example of illustrious teachers, the student, emulous of fame, seeks to distinguish himself as a candidate for professorial honours and office, by diligent re-examination of past discoveries, or by striking out new paths of inquiry. The academic teaching which made the use of the microscope, needle, and chemical reagent a second nature, expresses itself in after life by continued devotion to scientific investigation. The utterance of an Ehrenberg, "*das wissen wird sich im suchen entfalten*" (knowledge will unfold itself in the seeking), was not spoken in waste places.

Nor has such labour, undertaken in the true spirit of science —that of seeking and finding without bias of preconception or prejudgment—been in vain. The schools of Germany have created for us the modern science of histology, built upon an accumulation of observations which nothing but the zeal of knowledge could have accomplished; and histologic studies have, by imparting to general anatomy precision of detail and accuracy of method, greatly facilitated our study of the functions pertaining to differently organized structures. True it is that anatomical analysis supplies but a portion of the data required to enable us to penetrate the obscure profound of physiology. But from whatever other sources additions to our knowledge may come, a certain correlation between structure and function must be established before any physiological doctrine founded on external circumstantial evidence can be unreservedly accepted.

On the other hand, it is equally true that the phenomena of sensation, perception, consciousness, lie outside and beyond the circle of pure physical science. Notwithstanding that our sensory organs are supplied with special apparatus constructed to meet the circumstances and conditions of an external world; notwithstanding that demonstrable physical changes accompany the proper function of nerve matter, still the essential nature of that function remains, and perhaps ever will remain, wholly inscrutable, a thing "*sui generis.*"

Thus it has happened that the subjective (sensorial) phenomena of vision have been studied separately from their physiological basis, and without reference to the anatomy of the brain and the eye considered as the organ of sight. A wide chasm yawns between the physical and metaphysical sides of the inquiry, the filling-up or bridging over of which demands a knowledge of things which even the most sanguine investigators fail to see their way to. In so far as vision depends on certain arrangements of transparent media possessing special refractive powers, it is easy to explain on physical principles, and illustrate with the help of physical appliances, the *action* of this dioptric apparatus. But to account for the conversion of material into psychical impression, to explain the *sensation* of light or colour, is another thing. We know not what happens in a nerve when it perceives blue or red, and if we should ever discover that the perception was always induced by a certain change in the condition of the nerve, we have not thereby explained what sensation is.

The psychical phenomena of vision as learnt from our own perceptions occur in point of time subsequent to the processes going on in the eye itself. The consciousness of objects in an external world, and the manifold relations of this consciousness to other sensorial functions—in brief, all that is understood by the term "cerebral vision," forms a subject of metaphysical inquiry. On the other hand, the mode of action of the dioptric apparatus of the eye forms the subject of physical and mathematical demonstration. We have then a cerebral apparatus whose function is purely sensorial, and an apparatus of sight in the front part of the eye whose function is purely physical. Between these two extreme ends of the complex whole we find in the retina an intermediate organ which serves as the connecting link, and combines in itself the material instruments of physical and psychical action. It would appear in fact to share the double function of refraction and sensation. Its anatomical elements differentiate as they diverge in opposite directions, but yet preserve their continuity. If it be asked where is the proper physiological part of our subject, and what is its basis? the answer is implied in the following question—How is the *act of seeing* accomplished, and what is the *organ of sight?* In the presumption that this act of seeing is a retinal function, and the retina itself the organ of sight, we employ the phrase "structure and function of the retina" as defining the subject and object of our present inquiry.

And since experience teaches us that the function of a part or organ is intimately bound up with the particular structure of that part, we have here to inquire what are the anatomical elements of the retina: in what connection and relation do they stand to each other separately and as a whole; and, lastly, what share is contributed by each element towards the function (if divisible into parts), and in what manner do they combine to one common purpose? An inquiry into the structure and function of the retina resolves itself thus into two main branches:—1. What anatomical substratum exists for our physiological conception of the act of seeing? 2. What is the actual performance of the retinal structure as declared to us by our own sensation, or taught us by observation of the phenomena of vision?

The remarkable *speciality* which characterises the structure of all sensory organs, affords *primâ facie* evidence of an essential connection between the particular arrangement and constitution of elements observed in these organs, and the functions respectively performed by them. The inference is unimpugnable that the complex organization of the retina is *necessary* to the conversion of certain material impulses into corresponding sensations. And conversely the multiplicity and variety of sensations which concur to an act of vision, *e. g.*, the recognition of so many distinct points, lines, spaces, figures, surfaces, &c., or of various light and shade, or of pure and mixed colour; the combination of each and all with a separate psychical consciousness and intelligence; the instant and constant interchange of light, sight, and thought, lead us equally to the conclusion that the material instrument must be both complex and peculiar. The physiological conception of vision as accomplished by material mechanism postulates, at the least, the following anatomical provisions:—1. Lines of isolated communication by which material impressions may travel towards a central organ. 2. Structural elements capable of being affected by such material impressions, and of transmitting them in a new or modified form to a central organ. 3. Intercommunications between the separate elements themselves, either singly or in groups. 4. Communications of a second or still higher order between these combined groups and the central organ. Finally, where a duplicate apparatus exists (two eyes, two optic tracts, and doubled cerebral ganglia, &c.), communications between the corresponding parts of each eye, nerve, and brain mass. A number of additional arrangements are moreover necessary to the physiological conception of an apparatus adequate to bring into play the numerous co-ordinated movements of the whole body, so as to complete the harmony of visual function with the other sensory functions, and with the general sensation and motion. Such, for example, as 1. Nerve communication between the central organ and the nerve-centres which direct the motions of the muscles of the eye-ball, and those of "accommodation." 2. Communications between the central organ, and the nerve-centres which direct the organs of hearing, smell, touch, &c. 3. Communications of the central organ with the nerve-centres which control the action of the limbs. 4. Communications with the centres of common sensation, and, finally, with the nerve-centres which preside over involuntary movements related, however remotely, with the function of vision.

From this brief indication of a complete anatomical basis for the whole physiology of vision it will be at once obvious that by far the largest portion of the subject is referable to the general physiology of the brain and nerve-system, which does not further concern us here. The significance of the retinal function will be, however, sufficiently marked if the first series of anatomical postulates should be substantiated as material facts, as well as physiological assumptions. The fundamental office of the retina is the reception of material impressions which are transmitted in modified form to the brain ganglia. The *excitation* of the retinal nerves by whatever means effected, results in the *sensation* of light. But it is here necessary to draw a distinction between sensibility to a material impression and conscious sensation. Thus, the undulations of light passing through dioptric media reach the columnar bodies of Jacob's membrane, producing a definite but yet unexplained effect on their substance. This effect is then transmitted from the columnar stratum to the ganglionic layers of the retina, and thence to the optic nerve. Now, whatever be the change or induced condition thus brought about, it may be taken for granted as one that is essential to the perception of light when this perception is excited by the impulse of light undulations. But numerous pathological observations prove that the sensation of light may be excited through other channels. The excitation of the retina is therefore neither the perception itself, nor is it always necessary to this perception. On the other hand the occurrence of separate and independent sensation of light caused by stimulation of the optic nerve or the corpora quadrigemina does not prove that the retina is *not* capable of light *sensations* of some kind, or that its function is limited to the exercise of a more or less specialised " sensibility." As, however, " conscious sensation " involves a psychical operation, it must be admitted that the operation of a retinal sense is not likely to be extended so as to combine single into compound sensations, or to *take cognisance* of either. Such acts form more probably a part of " cerebral vision," though it must be borne in mind that the retina by its genetic relation to the brain, of which it was originally an integral part, stands on an equality of endowment in a physiological sense.

Besides the retinal action, which informs us of the existence of external objects, and which is due to the peculiar excitation of retinal nerves by light from without, there is an internal activity of the retina optic nerve and ganglia (corp. quadrig.) excited by various stimuli (electricity, blood stimulus, pressure, and irritations of nerve, &c.) which are capable of causing sensations of light, colour, form, &c. These *subjective* sensations, whether of retinal or cerebral origin, are interpreted according to the individual experience or fancy. The share taken by the retina in this subjective vision forms a branch of inquiry supplementary to that of the principal and ordinary function of this organ.

Having thus sketched the general outline of a physiological anatomy of the retina, and indicated its purport and limits, we must now address ourselves to the tedious but necessary labour of reviewing the history of anatomical discovery, and extracting from a voluminous literature the facts that seem to be confirmed by general assent, or that may be accepted provisionally as being in harmony with that which is known. We require, at least, to know what are the actual and probable facts and conditions of this organ of sight; what we may affirm or reject; and what surmise, as pointing in the direction of future discovery. The truth or probability of our facts must be sifted before we draw conclusions. In the absence of a positive basis, *no* conclusion is better than a false one. Nor will it advance science to assign arbitrarily this or that function to any given structure in order to render an hypothesis plausible, or to give undue force to an argument which may be just in itself.

The pertinence of the foregoing remark will be admitted when we consider the many errors of fact and interpretation which are inseparable from the study of such perishable elements as those of which the retina is composed. The natural bias of the observer is to suspect that which he has not seen, and to overestimate that which he has seen. His natural ambition is to solve each problem that comes before him in a manner agreeable to his own observations and conclusions. But (as is equally natural) his method of procedure is one-sided, and the end, seen only in part, is but partially attained. Thus the anatomist follows to its vanishing point the mechanism of structure. The physicist watches the equilibrium of forces and the sequence of changes consequent upon the first disturbance. The physiologist tests the action and reaction of living organs. The metaphysician analyses phenomena presumptively beyond the reach of material actions and influences. Each tasks his utmost powers and perhaps gains a step here and there, yet finds ample verge and space for speculation on points which his instruments of research cannot reach. The struggle is not always progressive, nor do the several lines of inquiry proceed *pari passu.* But on the whole minute anatomy, which has so often fallen behind the requirements of the physiologists, has in later years gained most ground, and bids fair to supply such a basis of fact as may serve for a closer and safer analysis of functional phenomena than was heretofore possible.

In the history of retinal anatomy it so happens that the earliest and latest discoveries relate to the same part, namely, that known formerly as Jacob's membrane; and the changes of opinion entertained concerning its structure and function are not a little curious. As each successive examination revealed new and unexpected facts, the attention of anatomists concentrated itself more and more upon this marvellous structure. The interest first excited by the discovery of a direct connection between the optic nerve-fibres and this columnar stratum has been further enhanced by the proof adduced (many years later) of its physiological significance as the probable percipient portion of the retinal apparatus. Quite recently an hypothesis has been revived by the discovery of facts which yield presumptive evidence in its favour; namely, that the rods and cones perform a catoptric function, in arresting and inflecting the undulations of light by virtue of a peculiar molecular arrangement, and high refractive power of the substance of which these elements are composed. Such a catoptric function had been formerly assigned to the columnar stratum on physical grounds by Brücke, but the hypothesis was rejected by those who first propounded the notion that both rods and cones were either actual nerve papillæ or modified nerve ends, and consequently endowed solely with the properties of nerve matter. New observations respecting the nature of the organic substance of which these cones and rods are composed have again brought into prominence the hypothesis of Brücke. Schultze has in his endeavours to explain the function of the columnar stratum laid great stress on the physical character and special morphology of the rods and cones, and brought fresh evidence in support of the theory of Brücke, modified, however, so as to include the theory of nerve action. To this much vexed question we shall recur when treating of the physiology of vision, mention having been here made of it for the purpose chiefly of explaining the frequent allusion to anatomical points bearing on the discussion, which will be found in our historical notice.

Fifty years ago the retina was supposed to consist of a "medullary expansion" of the optic nerve, supported on its *inner* surface by a "vascular" coat, which again rested against and was adherent to the "hyaloid" membrane. This "vascular coat" was demonstrated by macerating and scraping away the layer of "medullary" fibres, and obviously corresponds with the capillary network accompanying the ganglionic layer of nerve-cells supported by the connective tissue of the *membrana limitans interna.*

Exclusive of these two layers, says Jacob ('Cycl. Anat. and Phys.,' article "Eye"),

"I find that the retina is covered, in its external surface, by a delicate transparent membrane, united to it by cellular substance and vessels."

After describing his method of demonstration he further says—

"That it is not the nervous layer (medullary expansion) which I detach is proved first by the impossibility of separating that part of the retina so as to present the appearance I mention; and secondly, because I leave the retina uninjured and presenting the appearance described by anatomists, especially the yellow spot of Sömmering, which is never seen to advantage until this membrane is removed. (!) . . .

"Besides being connected to the retina I find that the membrane is also attached to the choroid coat apparently by fine cellular tissue and vessels, but its connection with the retina being stronger it generally remains attached to that membrane, though sometimes small portions are pulled off with the choroid coat. . . .

"The appearance of this part I find to vary in different classes of animals and in man according to age and circumstance. In sheep, ox, horse, and mammalia generally it presents the same character as in man, but it is not so much tinged by the choroid pigment, and adheres more firmly to the retina. In the bird it presents a rich yellow brown tint, and when raised the blue retina shows beneath. In fishes the structure is peculiar and curious. It has been already described as the 'medullary layer' of the retina by Haller and Cuvier, but I think incorrectly, as it does not present any of the characters of nervous structure, and the retina is found perfect beneath it."

The foregoing extracts from Jacob's communication to the

'Phil. Trans.,' 1819, fairly represent what the author knew and what he did not know respecting the structure to which his name was given in honour of the discovery.

Dalrymple, led astray in his account of the "*Tunica Jacobi*," by the current doctrines respecting serous membranes, described it as a double layer. Jacobs, however, denies this:

"If" (says he in his article "Eye," 'Cycl. Anat. and Phys.') "the retina be merely in contact with the vitreous humour and choroid, we argue from analogy that a cavity lined by serous membrane exists both on its external and internal surface; *but this is not the fact.* In the eye a distinction of parts was necessary, but to accomplish this a serous membrane was not required, a single membrane with the interposition of cellular substance answers here."

And in another place—

"My observations lead me to conclude that wherever the different parts of the eye are in contact they are *connected to each other by cellular substance,* and consequently by vessels," &c. &c.

The chief interest in these extracts lies, firstly, in this—that they show how theoretical notions interrupted the observation of facts, and, secondly, how the idea of a *connective* tissue grew out of the discussion.

In the introduction to Mackenzie's 'Treatise on the Eye' (2nd edit., 1835) a representation is given by W. Jones of the "tunica Jacobi" as seen under the microscope, which leaves not the slightest doubt of his having had the columnar stratum under observation.

The yellow spot of Sömmering received about this time much attention. The following is the anatomist's account of it:

"In the very centre of the retina is found an actual deficiency of the 'medullary layer,' *or* a real hole perfectly round with a defined margin. The transparent vitreous humour and black pigment are so clearly seen through this hole that there can be no doubt that it is a real aperture. Surrounding this '*foramen centrale*' the remarkable yellow colour is so disposed that it appears much deeper towards the margin, and totally disappears at the distance of a line."

Jacobs concluded from his examination of the yellow spot that no "foramen" existed, but that a fold of retinal substance normally existed at this spot. He erroneously states that the yellow spot was a *projection* of retinal substance inwards towards the vitreous humour. Whilst examining the internal surface of a fresh retina under strong sunlight illumination, he satisfied himself of the prominence of the fold by holding a needle opposite to it, and observing that the shadow deviated from the straight line, when passing over the situation of the fold. Had the "*fovea centralis*" not been distorted by this fold, Jacobs' ingenious observation would have demonstrated a depression instead of a prominence. Sömmering correctly interpreted this fold as an accidental puckering. With respect to the anterior termination of the retina, opinions varied; some anatomists asserting that it extended to the lens, and even behind it; others that the "vascular layer" only extended to the margin of the lens, whilst others again believed that this vascular layer lined its posterior surface. Jacobs fixes correctly the limit of the "nervous layer" at the "*ora serrata,*" and admitting the *appearance* of a continuation of the vascular layer (*membr. limitans*) as far as the lens, concludes finally against the existence of a *pars ciliaris retinæ.*

The next series of observations was made by German anatomists. In 1834 Gottsche demonstrated the filamentous structure of the "nervous expansion," previously held by all anatomists to be "medullary." Behind this he found a "compact layer" (the granule layers of modern authors) from the external surface of which "staff-like bodies" were seen projecting which presented an appearance like a thatched roof (figures representing this are to be found in many modern works, *e. g.*, 'Carpenter's Human Phys.,' 1854). Gottsche macerated and scraped off this retinal structure in order to demonstrate what appeared to him of greater import, namely, the nervous expansion of fibrils.

In 1835, Huschke, simultaneously with Treviranus, noticed the staff-like bodies, and Treviranus, after long examination, came to the following conclusions respecting the relations which the layers of the retina bore to each other: 1. The optic nerve-fibres change in some part of their course from the meridional to the radial direction.

"After the optic nerve has penetrated through the sclerotica and chorioidea its cylinders (nerves) spread out singly, or in bundles, on the *outer* (*!*) surface of the retina in all directions. Each individual cylinder or each bundle consisting of several cylinders at a certain part of its course, *bends inwards* towards the inner surface of the retina. Immediately after this it passes through openings in a vascular network which springs from the central vein of the optic nerve. Before it arrives at the inner surface of the retina it penetrates through a second vascular network formed by the twigs of the central artery of the optic nerve. Having passed the latter it is received by a sheathlike continuation of the vascular layer, and covered by this it terminates *behind the vitreous body* in the form of a papilla."

Allowing for the mistake by which the position of the optic nerve-fibres is represented as outside instead of inside the retinal layer in which they are supposed to terminate as nerve "papillæ," Treviranus seems to have had a distinct notion of a radial set of nerve-fibres. And if the "papillæ" described by him were really the cones of the columnar layer, his assertion of a direct connection between the optic nerve-fibres and these cones (or rods), though then unproved, must be accepted as an anticipation of later discoveries. The comments of Jôh. Müller, in his 'Jahresbericht,' 1837, shows how the statement of Treviranus was understood by his contemporaries. Müller says—

"The termination of each separate fibre of the nerve layer, in a staff-like body, seems still a postulate rather than an ascertained fact."

And he adds—

"If every nerve extremity correspond to a fibre of the optic nerve the thickness of the retina ought to diminish progressively from the point of entrance of the optic nerve to the border of the ciliary ligament independently of the varying thickness of the coats of the retina."

And, again, after discussing the thickness of the nerve-fibres and the relative fineness and number of elements of the columnar stratum, he remarks that—

"It is not easy to understand how so many fibres as are necessary to furnish the staff-like bodies can be compressed into the narrow compass of the optic nerve."

These acute remarks of J. Müller bear out the assertion that the physiological significance of Treviranus's discovery was fully understood, and point to two important facts not made out till a much later date, namely, 1, the regular thinning off of the optic nerve expansion towards the *ora serrata*; and, 2, the greater number of the *radial* nerve-fibres as compared with that of the meridianally disposed *optic fibres*.

In 1836, Langenbeck described the retina as consisting of a cortical (external granule) layer; a filamentous (nerve) layer; and a vascular layer. He observed, also, that the granule layer was at the yellow spot circumscribed by a sharpe edge (in consequence of the separation of the optic nerve-fibres). Besides this, he notices that the granule layer ceased at the *ora serrata*, and that a *pars ciliaris retinæ* lined the posterior surface of the *corpus ciliare* terminating at the junction of the ciliary processes with the urea.

In 1837, Valentin ('Repertorium,' vol. ii), in his account of the retinal structures, demonstrated the following noteworthy points :—1. The primitive fibres of the optic nerve do not simply run alongside each other, but interweave in a plexiform manner, leaving elongated fusiform spaces or meshes, in which "overlaying globules" were seen. These overlaying globules (Belegungskugeln) were the *ganglionic nerve-cells* discovered, or mentioned, for the first time, by Valentin, who first, also, mentions the plexiform arrangement of optic fibres. 2. The granule layer he describes as consisting of a mass of granules lying close together, but no connecting fibres were seen. 3. Valentin lays due stress on the absence of retinal layers at the entrance of the optic nerve, in consequence of which he remarks that "this point being *still pure optic nerve* (only with numerous "overlaying granules"—ganglionic nerve cells) *is merely a light conductor, not an organ impressible by light.*

Michaelis (1837) describes four retinal layers: 1, external serous layer (tunica Jacobi); 2, a granule layer; 3, a nerve and vessel layer; 4, an internal serous layer. In the first layer he recognises the cone and rod structure, and notices the red and yellow-coloured globules found in the bird's retina. The first accurate description of the bending of the optic nerve-fibres round the "yellow spot" is given by Michaelis. They are arranged, he says—

"In a peculiar manner round this spot, for whilst in other situations the nerves radiate in straight lines, around the *macula lutea* they are arranged in the form of arches, of which one part meet in the '*foramen centrale*,' the next in succession curve in regular arcs on each side of it towards a line which stretches outwards from the *macula lutea* towards the peripheral portion of the retinal expansion."

The *thin transparent* spot, which has obtained the name of *foramen centrale*, is elongated. Further, he observes that at this *macula luten* the granule layer is very thin in the centre, but increases in thickness towards its circumference. The "*foramen*" is formed by a single layer of little globules—in fact, Michaelis believed the *foramen* to be simply a "fovea," as it is now called. He also found that the nerve-fibres which run to the "yellow spot" ended on its surface. The internal "serous" membrane of Michaelis (membrana limitans interna) is described by him as containing many globules furnished with fine threads, which he took for nerve-fibres, but which Kölliker considers to have been threads of the radiary system of fibres (connective tissue) running to the *membr. lim. ext.*, and mistaken for nerve-cells and their prolongations.

In Joh. Müller's classical work on physiology (1840) the chapters on "sight" are occupied with physical and metaphysical expositions of the general phenomena of vision. Nothing is added in his account of the human retina to the researches of the authors quoted. J. Muller's own observations on the constructive details of the retina relate chiefly to the eyes of invertebrata. What may be termed the first period of retinal anatomy closes at this date, and the observations of the next ten years form a middle epoch.

In 'Müller's Archiv,' 1839, Henle argues in favour of the theory of Treviranus; and in his 'Allg. Anat.' the whole previous history is clearly and succinctly given. A close study of the columnar stratum led him afterwards to the discovery of threadlike prolongations from the inner ends of the rods, which he likened to nerve-fibres. He then also describes the various changes which they undergo during maceration and decomposition. In the rods of the retina in reptiles and fishes he notices a striation of their substance. Michaelis had already shown that the columnar stratum lay external to the nerve layer; and in 1840 Bidder and Hannover demonstrated this fact in all vertebrata. The latter discovered several new facts—the existence of double or twin cones (*coni gemini*); also of coloured globules in the retina of amphibia and birds. In respect to the cones he distinguished a flask-shaped body surmounted by a tapering rod-shaped outer portion, which showed a cross striation of its substance, and a disposition to break across in the lines of striation. This observation is of prior date to Henle's notice of the same fact. Hannover asserts that the cones are solid, and denies the nerve character attributed to their substance. The prolongations from their *inner* ends he mistakes for filamentary attachments to the choroid pigment membrane, and this confounding of the inner with the outer end of the rods and cones led him into the error of asserting that the columnar stratum was intimately adherent to the choroid, but simply in contact with the "true retina." He therefore viewed it not as nerve structure, but as a reflecting surface, whose function was to throw back the light penetrating the globe of the eye on the transparent nerve-cells and fibres through which it had passed. This opinion he maintained controversially in later essays ('Recherches,' &c., 1844, and 'das Auge,' 1852).

Brücke, 1844 (Müller's 'Arch.'), describes the cones as thickened rods, and assigned to them a catoptric function, the object of which was to arrest and isolate the lines of light projected on the retina by the dioptric apparatus in front, and to reflect them back on the nerve-cells and fibres, producing thereby single impressions of light intensified by this mode of reflection (as from mirrors). To understand Brücke's theory we must refer to the constructive detail of the invertebrate eye. In the "compound eye" of the invertebrate the separation of rays of light entering it is effected by means of numerous small corneal facets, with lenses behind them, to each of which is apportioned a separate bundle of nerve-fibres, which run straight-forward to the front of the eye, ending immediately behind the cornea. In the vertebrate type the light passes in lines, determined by the dioptric apparatus in front, through the globe of the eye and transparent retina, till it falls on the closely packed rods and cones of the *tunica Jacobi.* These have their long axes directed radially to the centre of the eye, and the light reflected by them is thrown in isolated lines upon the nerve-fibrils (or prolongations of their inner ends), whose direction is also radial, until they meet the nerve-cells of the ganglionic layer. In the invertebrate compound eye the nerves run forwards in a cone-like expansion, filling the globe of the eye; whilst in the vertebrate eye the optic expansion is spread out on the inside of a hollow sphere, and the nerve-fibrils turn backwards through the thickness of the retina upon the outer layer of cones and rods. Instead of a multitude of separate images, produced by the corneal structure of the invertebrate compound eye, a more perfect camera picture of external objects is formed in the columnar stratum, which Brücke looked upon as a close-set series of small mirrors, formed by the ends of the rods and cones which effect the same isolation of points in the picture that is accomplished by the corneal facets of the invertebrate eye, and the reflection in the radial direction brings back the picture upon the separate fibres of the optic nerve. Thus the columnar stratum at the back of the retina in vertebrate eyes performs a function comparable with that of the corneal structure at the front of the invertebrate eye. A more perfect camera picture is first produced, and then the "mosaic" of cones and rods enables this picture to be recognised in detail. In the latter scheme vision is effected by a backward view on the concave of the *tunica Jacobi;* in the invertebrate plan vision is directly forwards on the convex cornea.

We now continue our historical sketch. In 1845, Pacini described the retina as consisting of five layers, thus counted in order from without inwards: 1, T. Jacobi; 2, layer of nucleated granules; 3, grey nerve layer with fibres; 4, ganglionic layer; 5, optic expansion. The cones of Jacob's membrane he describes as single and double (*coni gemini* of Hannover), some being furnished with an outer narrow rod-like portion. The inner ends of both cones and rods enclose a granule with nucleus in it with a thread-like prolongation. Pacini contends for the true nerve character of the whole. The granule layer he figures in one thick, undivided mass made up of numerous rows of granules containing nuclei, and giving off fine threads of communication. He also gives the true position of the ganglionic layer and saw nerve prolongations from these ganglia, which he conjectured to be continuous with the optic nerve-fibres. A fine granular mass (" fibre grigie") lying immediately behind the ganglionic layer is correctly described by him as consisting of a minute network of delicate fibres in a finely granular matrix.

Bowman (" Lectures," 1846, ' Med. Gazette,' and ' Todd and Bowman's Phys. Anat.') describes the following layers of the retina, counting from within outwards. 1. Fibrous grey layer—

" Apparently consisting of the tubular fibres of the optic nerve deprived of their white medullary substance that is being no longer tubular and white but solid and grey, and united more or less into a membrane. The bundles of fibres anastomose in a close plexiform manner, and finally constitute a thin sheet becoming thinner and less fibrous as we trace it forwards."

This fibrous layer is united to the hyaloid membrane by a layer of nucleated cells, almost transparent, and difficult of discovery on that account (fig. 117, ' Phys. Anat.'). 2. Outside the fibrous layer follows a vesicular grey layer (the fibre gris and ganglionic layer of Pacini), resembling the grey substance of the brain. In this layer is distributed the network of capillaries (the vascular layer of older authors). Bowman describes ("Lectures," 1846) pale nerve-threads similar to those of the cerebral nerve-ganglia which proceed from the cells of the ganglionic layer, but were not traced to the optic nerve-fibres. 3. Next outside to this ganglionic layer the granule layer is described and figured as divisible into two separate layers. The granules he compares with nuclei of cells. 4. The tunica Jacobi is described as consisting of club-shaped rods, whose outer ends "are seen to be formed by a sudden bending back of the stem like a crook." Cones, as well as rods, are distinguished by Bowman, and the layer " forms a connecting medium between the retina and choroidal epithelium." The " yellow spot " is stated to be formed by a *projection* of the retina towards the vitreous humour with a minute aperture at its summit (see *ante*, T. Jacobi). The expansion of optic fibres cannot be traced over the yellow spot, but sweeps in an arch round it—

" Nucleated cells occupy the elongated meshes of the fibrous plexus, until at length the fibres disappear and the closely set cells seem to cover the whole surface of the spot. The gradual subsidence of the fibres in the intestines of the cells we have distinctly seen."

The colouring matter of the yellow spot is not deposited in grains of pigment, but is diffused through the tissue. Bowman concludes his account with the observation that the use of the yellow spot is unknown ('Phys. Anat.')

Hassall ('Micr. Anat.,' 1849) gives a description which is, in many points, erroneous and retrograde. The T. Jacobi is a single stratum of cells, whose thickened ends lie against the choroidal epithelium, and their ends towards the granule layer:

> "Although these cells adhere together with sufficient firmness to constitute a distinct membrane, it would appear that they possess a certain power of movement (!) upon each other, for it is only on such a supposition that we can explain satisfactorily the fibrous appearance which the membrane frequently presents when viewed *in extenso*."

Hassall's figure, instead of demonstrating this, simply shows the decomposed state of the rods, long before explained by Henle as a result of maceration. Hassall maintains that the T. Jacobi is "certainly not a nervous structure." Among the *facetiæ* of retinal anatomy may be placed his observation that each cell of the tunica Jacobi "has not an inexact resemblance to a human spermatozoon! than which it is, however, less considerable in size." In his account of the granule layer he follows Bowman. The ganglionic layer (hitherto overlooked! says Hassall) is an exceedingly thin and delicate structure, consisting of "caudate ganglionic globules," and hardly to be considered a distinct stratum. Immediately outside the fibrous layer Hassall describes "a vesicular layer:" the cells composing it are several times larger than the nuclei of the granular layer. His figure delineates "clear transparent globules without nuclei," but no other anatomist has found such globules in this part of the retina. The fibrous grey layer (optic nerve expansion) "is made up of grey gelatinous fibres without any tubular sheath." A *vascular* layer is supposed by him to exist in the *inner* surface of this nerve layer, an error into which Pacini also fell.

In 1850 Corti (Müller's 'Archiv.') traced the course of the offsets from the cells of the ganglionic layer, and found them to be continuous with the optic nerve-fibres. His observations were made on many mammalia. In the retina of the elephant he found the best examples, and figures the communications between the ganglionic cells themselves, as well as with the optic nerve-fibres ('Zeitschr. f. Wiss. Zool.,' vol. v.)

In 1852 Henle ('Zeitschr. f. Rat. Med.') confirms with Dittrich, Gerlach, Herz, Kölliker, and Virchow, the fact that the "yellow spot" is visible immediately after death, but that *no fold* of the retina exists at that spot. The transparency of the retina allows the parts behind to be seen through, especially at this part. A surface view of the T. Jacobi shows a mass of clearly defined but very small circles (end view of the rods) with somewhat larger circles interspersed (cones). The proportion of rods to cones varies at different parts of the retina. In the middle of the yellow spot *cones alone* are seen closely packed together. At the edges of the spot a single circle of rods surrounds the centrally placed cone; towards the *equator oculi* a double or triple circle of rods is grouped round each cone. The somewhat larger circle which represents the greatest width or thickness of the cone contains a small circle within it, which indicates the smaller rod-like outer half. Sometimes instead of this circle the rod itself, looking like a small nail ("Stiftchen") within the outer circle, indicates its accidental breaking off or curve, so as to be seen in oblique position. The action of solution of iodine on rods and cones is different, the substance of the rod becoming stained while that of the cone does not. The colouring matter natural to the yellow spot is diffused. Granule and ganglionic layers were seen, but no fibres of communication. A transparent, tough gelatinous cement unites the whole in one firm mass. The rods appeared to project into the pigment layer of the choroid.

We now come to a turning-point in the history of retinal anatomy. Hitherto the presence and alternation of the concentrically disposed layers of the retina had received almost undivided attention. Although several observers had noticed fibres which were supposed to run from one element to another, and had surmised a direct continuity of the columnar stratum with the optic nerve-fibres which, however, was not yet proved, nothing approaching to a correct statement of their true relations had been put forward, though many new details had been ascertained during the years 1840-51, which may be called the middle period of retinal anatomy. The third and last period commences with Heinrich Müller's researches, since which the anatomy of the retina has made continuous progress on the new basis afforded by H. Müller's discovery of the radial system of fibres. In 1851 H. Müller described this system of fibres, which, in contradistinction to the "meridional" course of the fibres of the optic nerve expansion, was named "radial" by their discoverer ('Zeitsch. f. Wiss. Zool.'). These fibres he found to extend from the inner ends of the rods and cones to the outer granule layer, then forming a solid mass between the outer and inner granule layer and finally penetrating the ganglionic layer, to be inserted on the outer surface of the internal limiting membrane. The existence of a constant and distinct anatomical continuity between the outer and inner layers of the retina by means of this radial system was the first result of H. Müller's investigations; and the same anatomical disposition was proved for all classes of vertebrate animals but man. The inquiry was soon afterwards taken up by Kölliker, with whom H. Müller then conjointly carried on his researches on the human retina. In his microscopic anatomy (1854), Kölliker gives the further results of their joint examination, and of his own studies and opinions respecting the nature and function of the several parts. Kölliker re-discovered, or rather confirmed, most of the facts already mentioned, added a number of carefully observed facts, and based on the whole a new physiology of vision.

The anatomical peculiarities of the cones and rods receive much attention and elucidation. The cones are described, much as Hannover had described them, as consisting of a flask-shaped inner portion and a rod-like outer portion, the two being separated by a fine cross-line. But the statement of Hannover that the cones are solid bodies is refuted, Kölliker affirming them to be long tubular nucleated cells, the outer prolongations of which formed the tapering cone-rods resting against the choroid pigment layer, and the inner prolongation (the nucleus placed at the inner end of the cone) being continuous with Müller's "radial fibre." The whole structure he declares to be a modification of ordinary nerve matter. He confirms Henle's statement that cones *alone* exist at the "yellow spot," and that this spot is bare of nerve-fibre layer. He agrees with Müller in representing the cones of the "yellow spot" as longer and narrower than elsewhere. The rods also he describes as having a fine nucleus towards their inner ends, and an extremely fine nerve thread proceeding from this end. The cone fibres he distinguishes as being much thicker than the rod-fibres, but considers rods as well as cones to be modified *nerve*-cells and fibres. The fibres of Müller proceeding from these elements he at first believed to run to the outer and inner layers of granules respectively, but this first description of their destination he afterwards recalled.

Two layers of granules with an interspace filled by the fibres of Müller running from without inwards are described by Kölliker. The granules are recognised as small cells filled with a large nucleus, and interpreted to be bipolar ganglia. These granule cells with their communicating fibres are likened by Müller to "currants on their stalk." The inter-granule space is entirely occupied by the fibres of Müller running in close parallel lines in a finely granular cement. The ganglionic layer of Kölliker includes the grey substance of Pacini, with its minute plexus of delicate fibres (see Ecker, plate xix, fig. 2), and the large multipolar cells described by Bowman, and Corti, and others. Kölliker succeeded in demonstrating the fibre connections between the ganglia themselves, and between the ganglia and optic nerve-fibres. Through this ganglionic layer the system of radial fibres is seen to penetrate in bundles collected together at close intervals, and finally attaching themselves by a broadened triangular foot, or pencil of fibres, to the outer surface of a membrane described for the first time accurately as an integral and independent layer on the inside of the retina, the membrana limitans interna (Ecker, pl. xix, fig. 7). Many important particulars of measurements and details of the several layers and elements are given, for which we must refer to the various papers by Kölliker ('Wurzburg Transactions'). The optic nerve at its entrance is composed of ordinary bundles of nerve-fibres with enclosing sheaths or neurilemma. In its passage through the sclerotic the fibres are still tubes having dark outlines and filled with white nerve substance. But immediately after they appear as yellow-grey strongly refractive fibres, consisting no longer of ordinary medulla; they have no nuclei on their outside, are markedly varicose, and devoid of any axial fibre. The expansion of fibres is thick at the commencement, where the fibres overlie each other forty to sixty deep, but rapidly diminishes in thickness as it spreads out, becomes very thin round the "yellow spot," and ends with a single intercepted layer towards the *ora serrata*. The anastomosis of the fibres in bundles, with interspaces filled out by the deeper seated ganglionic cells and the termination of fibres in these cells, is demonstrated. The arching of the fibres round the "yellow spot," as described by Pacini, is confirmed. A few nerve-fibres round its margin seem to drop into the depressed surface and, as surmised by Remak, are traced to the ganglia of the "yellow spot." Kölliker extends this conclusion to the whole retinal surface, affirming that each single optic nerve-fibre runs to its ganglion or perhaps several fibres to one ganglion.

Shortly after Müller's discovery of a radial system of fibres, doubts were entertained as to its right interpretation. Some histologists maintained that the whole system was to be considered as a framework of fine connective-tissue fibres, for the support of the delicate elements of the several layers. Müller himself came, after repeated examination, to the following conclusions:—1. The fibres proceeding from the inner ends of the rods and cones, and ending in the granules of the outer layer, are unquestionably nerve-fibres; 2. The fibres passing in bundles through the ganglionic layer, and inserted into the memb. lim. interna, are *not* the same as those which have been traced from the nerve-cells to the optic expansion: on the contrary, their connection with the membr. lim. interna indicates their histological character as connective-tissue fibres. From the position of this inner system the hypothesis of their being concerned in the perception of light is of itself disproved; whilst the fact of the connection of the optic nerve-fibres with the ganglionic cells receives repeated confirmation. 3. The distribution of the inner radiary system varies in different parts of the retina; through the thick mass of optic fibres at the back of the eye the radial fibres pass in strongly defined bundles or pillars, which run direct to the membr. lim. interna, and are inserted by distinct but delicate filaments on its outer surface; but it is just at this part of the retina that the nerve-cells are few in number. At the equator oculi the inner radial fibres are also strongly developed, while the nerve-cells are at this part relatively less numerous than at and around the yellow spot. At the yellow spot, when the nerve-cells are found in mass, the inner radial fibres are entirely wanting, and this again corresponds with the fact that at the "yellow spot" there are no optic fibres, and therefore no support for them required; the membr. lim. interna lying here close against the ganglionic cells. Towards the front of the retina the radial fibres are present in much greater proportion than the ganglionic and granule cells; and here the connection of the radial fibres with the membr. lim. interna is most distinct and least liable to be confused with ganglionic offsets to the optic nerve expansion. Thus, throughout the retina the disproportion between the number of *inner* radial fibres and the nerve-cells of the ganglionic layer goes to prove that these fibres are *not* nerves but connective-tissue framework. But Müller maintained rightly that *all* radial fibres are not to be confounded together, and that the outer set (running from the inner ends of the cones and rods to the outer granule layer) are different in character as in distribution.

Other details given by H. Müller deserve notice, as they bear upon the investigations of later anatomists. Of the rods, he says that they extend through the whole depth of the columnar stratum, and appear divided into an outer and inner half by a cross line in the middle, at which point they readily break into two portions, each half reacting differently to chemical agents. At its inner end the rod contains a nucleus, and then suddenly tapers into a fine thread which runs to the granule layer ending in one of the granules. The cones are also separable into two halves—an outer tapering rodlike part (Henle's 'Stiftchen'), which reaches to the choroid pigment; and an inner flask-shaped part, which occupies the inner half of the depth of the columnar stratum. The two halves are defined by a cross line, as is the case with the rods. The inner half (the conical part) contains at its inner end (that is, just at the boundary line between the columnar stratum and the outer granule layer) a nucleated granule, and then tapers into a thread considerably thicker than the rod thread. This cone thread ends with a triangular button or triangular-shaped enlargement at the inner border of the granule layer. These threads, given off by the cones and rods, are the radial *nerve*-fibres.

With respect to the granule layers, Müller observes that their mass (thickness) varies, not only in different animals, but also in the different parts of the same retina (see 'Ecker. Icones. Phys.,' table 19). Thus at the "yellow spot," the *inner* granule layer is thick, the *outer* thin, and the inter-granule space occupied by radial fibres is deeper in proportion as the outer granule layer thins off. Towards the equator oculi the outer granule layer increases in thickness, and the inner is relatively thin, whilst the inter-granular space for radial fibres likewise diminishes. Both granule layers run thin as they spread towards the ora serrata, and almost disappear at the ora itself, and the inter-granular space regularly decreases as the thickness of the granular layer diminishes.

The ganglionic layer also varies in mass at different parts of the retina. Reduced to a single layer of cells at the entrance of the optic nerve, where the nerve-fibre mass is thickest, it becomes a deep aggregation of cells at the "yellow spot," which grows less and less as the layer spreads over the equator oculi, and from thence forwards through the ora where the cells no longer form a continuous layer.

The figures and text of Kölliker's 'Micr. Anat.' (figs. 404-5-6-7 and 411) sufficiently indicate the uncertainty then prevailing (1853) respecting the mode of connection of the outer layer of radial fibres (namely, the prolonged ends of the rods and cones) with the inner system of fibres whose bundles are seen running from the inner border of the granule layer through the stratum of optic nerve-fibres to the outer surface of the membr. lim. interna. In fact, Kölliker draws a scheme of communication which does not really exist in the form delineated by him in his 'Microscopic Anatomy.' In discussing the question of the nature of the inner radial fibres, he endeavours to show that these fibres are allied morphologically and chemically with the substance of which the tissue of the vitreous body is composed, rather than with that of ordinary connective tissue. The objection that the membr. limitans in which the fibres terminate is obviously not a nerve structure, he disposes of by affirming that the connection is one only of contact, not of intermixture of tissue. The conically expanded ends, ranged close together, or crossing each other as they join the limiting membrane, give rise, he adds, to an appearance of a clear border between the layer of optic fibres and the membrane, which is produced by the swelling of the delicate fibres into a gelatinous mass; and this liability to swell by imbibition of water has led to an erroneous conclusion that a layer of transparent vesicles exists here, which in section shows a row of clear globules. This appearance he figured in the first edition of his 'Handbuch.' Bowman describes the same as an epithelial layer. It is possible that the papillæ of Treviranus may in reality have been these altered conical ends of the radial fibres. Michaelis (see ante) described the membr. limitans as a "serous" layer. According to Kölliker, the substance of this membrane, which is exceedingly thin and delicate, differs chemically and morphologically from that of the radial fibres attached to it; these latter, as already mentioned, he likens with the tissue of the vitreous humour, the membrane with the hyaloid coat. Yet he noticed in the fibres and the membrane *nuclei*, a circumstance which favours the opinion that the tissue is of the kind known as connective.

Some anatomists have compared the membrane, with its attached fibres, to that which lines the ventricles of the brain, being *genetically* identical; and in the fœtal brain this lining basement-membrane of the ventricles shows the same kind of delicate fibrous tissue passing from its under surface in contact with the cerebral substance, and penetrating its mass. Henle calls the membrane "hyaloidea limitans," on account of its being so often found inseparable from the hyaloid membrane. Schultze objects that they are genetically distinct, the membrana limitans belonging to the retinal system, the hyaloid to the vitreous body. This anatomist also contends (against Kölliker) for the intimate connection of the radial fibres with the limitans, and considers both to be integral parts of the connective tissue of the retina.

Nunneley's account of the retina, 1858, is extremely imperfect. He separates the layer of rods from that of the cones, making the first external to the latter. The granule layer is not divided into two, and the intergranular fibre layer receives no notice. Neither is there any account of the radiary system of connective-tissue fibres, or of the rod and cone threads. The cells of the "nucleated vesicular layer" are spoken of as having no communicating fibres with the granule layers or with the optic nerve layer. The optic nerve pierces a single narrow aperture of the choroid. Its fibres, of different lengths, terminate as they pass forward by being lost or continuated into the true retinal elements; the granules being the connecting medium between the nerve-fibres and the rods. A "vascular layer" is needlessly retained, being, in fact, no *layer*. A layer of hyaloidal cells is described (see Kölliker's explanation). The account of the yellow spot is confined to the question of a "foramen," which he correctly decides as abnormal; the yellow colouring matter he attributes to "minute choroidal globules," and not to any colouring matter diffused in the T. Jacobi. The rods of this T. Jacobi he correctly describes as continuous on the outer surface. But, on the whole, "inclines to regard this much debated spot as a vertigiform remains of the spot where a large blood-vessel has passed through the retina in the development of the eye! and carried with it some of the choroidal colouring matter." No wonder that he says "it is very difficult to offer any satisfactory opinion of the use of this peculiar spot."

The details given of the several retinal elements are somewhat contradictory. He notices, however, a fine transverse line dividing the cones of fishes into an outer rod-like and an inner bulbous portion.

So many researches by different observers have followed the first publication of Kölliker and Müller's discoveries that it is impossible in any article like the present to give separate abstracts of them. Besides that the repetition of similar observations is as unnecessary as it would be tedious. We refer to our bibliography of a list of the more important researches, and some of the results will be found incorporated in the hasty abstract with which we must conclude.

We give here the enumeration of retinal elements as counted by Kölliker (see Ecker's 'Icones Phys.,' pl. xix) in 1853; and also the tabular arrangement devised by Henle ('Handbuche'). By a comparison of the two, the corrections and additions made during the last fifteen years may be gathered at a glance. We confine our explanatory remarks to the points of difference observed in the two schemes.

Kölliker and Müller.	*Henle.*	
1. Layer of rods and cones.		1. Rods and cones.
2. Outer granule layer.	1. "Mosaic layers."	2. External limiting membrane.
3. Intergranule layer.		
4. Inner granule layer.		3. Granule layer.
5. Fine molecular layer.	2. Fibre layer.	4. Outer fibre layer.
6. Nerve-cell layer (ganglionic).		5. External granular layer.
		6. ,, ganglion layer.
7. Optic nerve expansion.	3. Nerve layers.	7. Internal granule layer.
8. Ends of radial fibres inserted into.		8. ,, ganglion layer.
		9. Optic nerve expansion.
9. Membrana limitans.	4. Limiting membrane.	10. Limitans hyaloidea.

On comparing these tables we see that the earlier one is a simple enumeration of layers, whilst the second is a *classification* of elements based on physiological distinctions. The physiological argument we must defer to a future opportunity, and content ourselves at present with stating the facts which determined Henle in his arrangement.

The "mosaic layers" Henle separates from the rest because firstly, the retina naturally separates into two portions, the outer of which includes all that belong to the "mosaic" structure; secondly, because the blood-vessels of the retina are confined to the "nerve layers" of the inner portion, and are wholly absent in the "mosaic layers;" thirdly, because Henle denies that the continuity of the cones and rods with undoubted nerve structures is as yet absolutely demonstrated, although he does not *exclude* them from the series of nerve formations by the use of the term "mosaic layer." And, similarly, the "granule layer" (No. 3), counted as belonging to the mosaic layers is separated from the "nerve layers, firstly, because Henle considers the granules of this layer differ in substance as well as position from the granules of the nerve layers. He finds them to be striated as though made up of molecules possessing different refractive power, and showing other differences which distinguish them from all other known nerve-cells, whilst he allows the ganglionic character to the granules of the inner layer.

"The "outer fibre layer" of Henle is thus named on account of its position and unmistakeable character: *denominatio fit a potiori.* Schultze (de ret. struct.) declared this fibrous layer to be composed of connective tissue, and held also the rods and cones from which they spring to be modified connective tissue. But in his later writings he emphatically expresses his conviction that the rods and cones with their thread-like prolongations are nerve structures, and the granules in the middle course of these threads to be bipolar ganglia. Henle agrees that the outer fibre layer is composed of nerve fibres. This "outer fibre layer" constituted by the thread-like prolongations of the granules (outer granule layer of Müller and Kölliker) obtains, according to Schultze, its distinctive character when the granules are few in number, and is, in short, the inner half of the outer granule layer devoid of granules.

The "nerve-layers" of Henle include several of the layers of Kölliker's table differently arranged. The name "intergranule layer" Henle omits, having substituted for it his "outer fibre layer" and "external granular layer." In comparing the two tables layer for layer we find that Kölliker's first (rod and cone stratum) receives as an addition or new layer, *a membrana limitans externa*, and that Kölliker's second layer (outer granule layer) is Henle's third layer. Kölliker's third layer Henle divides into two (outer fibre layer and external granular layer

4 and 5). The fourth layer of Kölliker corresponds with Henle's sixth. His fifth, sixth, and seventh with Henle's seventh, eighth, and ninth. The eighth and ninth of Kölliker make up Henle's tenth. We proceed to explain the reason of these changes.

The difficulty first experienced in interpreting the true significance of the radial systems of fibres has been resolved by repeated examination of them. It is now believed that a complete framework of connective tissue exists for the support of the delicate nerve-fibres and cell elements of the different retinal layers. A line drawn through the stratum of rods and cones just *above* the slight swelling at their inner ends which indicates the position of the "outer granule layer," marks the external limit of this connective-tissue framework, and this boundary line (H. Muller distinguished it by the name "rod and cone granule line") represents what is called the external limiting membrane. It is *not* a membrane (Schultze), but simply the outer surface of the connective-tissue mass, pierced with openings for the passage of the rods and cones through it. This surface, if the rods and cones could be removed would appear as a sieve-like expansion, but the rods and cones in passing through it fill up the openings. Immediately underneath it are the rod- and cone-granules (or cell nuclei). The name limitans externa is accepted by anatomists as indicating the exact *outer* boundary of the connective-tissue framework, as the *inner* boundary has long been recognised by the name "limitans interna." The latter is a true limiting membrane, as it is not pierced by any retinal element, and in fact completes the retina on its inner face. Henle's granule layer (No. 3) (Kölliker's outer granule layer, No. 2) is formed by the mass of granules which Kölliker, Müller, Schultz, and others, hold to be bipolar ganglia (striated granules of Henle). Schultze contends that they are imbedded in a sponge-like mass of connective tissue, whose outer limit is the membrana l. externa, and which is continuous with the connective-tissue fibres that accompany the rest of the retinal elements until they reach the limitans interna. This connective tissue is at one place finely reticulate, at another brought into a large meshed network, at a third gathered into bundles of fibres supporting or isolating the nerve-fibres and cells, according to the disposition of the several layers. But the radial disposition predominates, and thus attracted Müller's attention. As we have seen, Müller found both the rod- and cone-threads which he considered nerve fibres, *and* the inner system of radial fibres whose difference of position and histological character he recognised. Kölliker and Müller failed to prove the continuity between these two systems of fibres, because, as later researches have shown, they are not continuous in the sense supposed by these authors at the time of their first publication.

In the inter-granular layer of Kölliker the intermixture of connective-tissue fibres with rod- and cone-threads is, according to Schultze, so intimate that neither can be isolated readily or for more than very short distances. In the following inner granule layer of Kölliker the granules (also bipolar or multipolar cells) form the chief mass, but is supported, says Schultze, by a radial areolar tissue in which connective-tissue nuclei may be seen. In the fine molecular layer of Kölliker (Henle's seventh) a minute plexus of exceedingly delicate fibres, both of nerve- and connective tissue (Pacini's grey layer), gives its distinctive character. In the ganglionic layer the large nerve-cells are retained in position by the sponge-like areolar tissue (Schultze; see also figures in 'Phil. Trans.,' vol. cli, May, 1866, section of fovea centralis by Hulke), which encloses the cells in partitions. Finally, the optic nerve fibre-layer is supported by the relatively massive system of connective-tissue fibres, whose bundles have been already frequently mentioned as inserted into the internal limiting membrane.

It is worthy of notice that blood-capillaries spread through all the inner nerve-layers of the human retina, penetrating outwards as far as the intergranular layer. These capillaries are supported by the connective tissue. He (1865) fancied that he had detected perivascular lymph canals accompanying the blood-vessels, such as he has found and described in the grey cerebral substance. In some mammals only a few capillaries are found, chiefly in the neighbourhood of the entrance of the optic nerve. In birds, reptiles, amphibia, and fishes, no blood-vessels capable of being injected have been found. In the human retina the capillaries are abundant.

Course of the radial nerve-fibres.—The course of the rod- and cone-fibres has never been followed in unbroken continuity to the ganglionic layer. Schultze explains this by affirming that the radial fibres enter into a plexiform anastomosis in the intergranular layer, and also in the grey molecular layer, changing at each place their radial into horizontal direction. The ends of the rods taper into exceedingly fine threads, which run to one of the outer granules, and thence from its opposite pole to the inner border of the outer granule layer, ending there, to all appearance, in a small knob. The ends of the cones likewise taper into cone-threads, which are much thicker than the rod-threads. Schultze and others believe this thick cone-thread to be a strand containing two or more nerve-threads. At the inner border of the granule layer these cone-threads end apparently like the rod-threads in an expanded button-shaped knob. Henle observed two fibres given off, one from each corner of this knob. These fibres turned off in opposite directions, and ran horizontally in his outer fibre layer. Hasse, a third fibre proceeding from the under side of the knob. Schultze, a great number of fine fibres all given off from the inner side or base of the knob. These observations are supposed to afford an anatomical basis for the theory of colour perception first propounded by Young and carried out by Helmholtz.

The now horizontal course of the rod- and cone-threads, and their anastomosis with each other, precludes any further isolation so as to follow their continuous course. A narrow band running concentric with the granule layers is thus formed (Henle's layer 5). On the hypothesis that the cone- and rod-threads transmit separate single impressions of light (effected in the substance of the cones and rods), this first plexus of horizontal fibres offers an anatomical basis for the possible combination or grouping of single impressions. On the inner side of this band spring the fibres, which again take a radial course to the granules (nerve ganglia) of the inner layer (Kölliker 4, Henle 6). Through the granule layer the direction of the fibres is radial. Next on the inner border of these nerve-granules the fibres again form a minute plexus, where the continuity is a second time lost in consequence of the horizontal direction and constant anastomosis of the fibres, and the intermixture of connective-tissue fibres. From the border of this plexus (Kölliker 5, Henle 7), which is contiguous with the large nerve-cells of the ganglionic layer (Kölliker 7, Henle 8), fibres are readily traced till they join the nerve-cells. Most authors agree in describing these as devoid of any cell-membrane, and in every respect similar to the ganglia of brain substance. Offsets from them, joining the optic nerve-fibres as well as connecting the cells together laterally, may be considered equivalent to the axial cylinders of ordinary tubular nerves.

We must here conclude our summary of the retinal structure, which is, from want of space, incomplete in many details. Sufficient, however, has been said to give some general idea of the complicated relations of the several elements. Before we can enter into an analysis of the function of these elements; before we can apply our anatomical facts to the physiology of vision, a number of observations respecting the intimate structure and material condition of the cones and rods have still to be collected. We are at present, so to speak, but at the beginning of the end. On a future occasion we hope to lay before our readers many points of interest already made out, especially regarding the comparative anatomy of the columnar stratum.

Recent investigations offer a prospect of great promise, and there is every reason to believe that retinal anatomy will ere long disclose to us a safe anatomical basis for the explanation of many questions relating to the physiology of vision, for the solution of which there have hitherto been no satisfactory data. Meanwhile we cannot but offer our meed of praise to the many distinguished anatomists who have carried us thus far on our way. In the wide range of microscopic anatomy no subject offers more formidable difficulties than the examination of the retinal structures, and none has been met with greater determination and ingenuity of research.

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Review V.

1. *Spiritual Wives.* By William H. Dixon. In two volumes, 8vo. London, 1868. Pp. 675.

2. *Grace abounding to the Chief of Sinners; or, a brief relation of the exceeding mercy of God in Christ, to his poor servant, John Bunyan.* London, 1666.

We are not admirers of Mr. Dixon's style of dressing his goods for market, of expanding into two handsome volumes, decorated with all that costly bravery which will require a great sale, and entails heavy advertising, four cases of the epidemic development of a certain mental diseased state. But we must allow that his flowing and brilliant periods render the reading easy, and the highly coloured descriptions carry us onwards, like the thrilling lithographs in travellers' tales, so that we arrive at the end incredibly quicker than we had expected from the size and weight of the book. Also we thus gain an abundance of detail, interesting to the morbid anatomist of the mind, and perhaps not without value to him whose office is the endeavour to cure and prevent its diseases. It is with a view to the last-named end that we have studied the strange confessions and exposures here embodied, and have joined to them an autobiography, which we found it a wholesome exercise to re-read after them, for the purpose of purging our contempt and renewing our hopes for human nature. The recent publication placed first in the heading of this article records the victory of the disease; the other work, the victory of the patient. This more fortunate event is, we believe, by far the most common; but it does not so often come before the public, as the sufferers are only too glad to wipe away the memory of that which seems shameful, and even to their medical confidants are reticent of their hazy recollections. The conjunction of pleasure in telling the truth, vivid power of word painting, and clear language, such as we find in Bunyan, is unique.

The morbid condition of which we speak consists essentially in taking the idiopathic suggestions of the patient's own mind for the external stimulus of a separate intelligence.

"As I was in the midst of a game of cat," writes Bunyan, "and having struck it one blow from the hole, just as I was about to strike it the second time, a voice did suddenly dart from heaven into my soul, which said, 'Wilt thou leave thy sins and go to heaven, or have thy sins and go to hell?' At this I was put to an exceeding maze," &c.[1]

Again, at a later period of his life, he tells us,—

"I was much followed by this scripture: 'Simon, Simon, behold Satan hath desired to have you;' and sometimes it would sound so loud within me, that once, above all the rest, I turned my head over my shoulder, thinking verily that some man had behind me called me; being at a great distance, methought he called so loud."[2]

In Mr. Dixon's book an adventure of one Miss Mary Lincoln records the same feeling,—

"During the afternoon she heard the voice of God warning her to flee—escape for her life, for the judgments of God awaited the place. Her voice changed, and she was filled with power. She waited in Little Rest (a small village in Brimfield) until evening, when another dear sister felt drawn to follow her—Flavilla Howard. . . . She felt that the clothes she had with her and those she had on were a burden. She laid them all aside. They then escaped to the west mountain," &c.[3]

This inexplicable terror, this hearing of voices unheard by the world, is exceedingly contagious; as it is worded by the last writer, others are "drawn to follow" by sympathy; and times and circumstances concurring, there ensues what is technically called "a revival," sometimes limited to small infected districts, sometimes widespread enough to be historical.

Why does not this oftener take place? It is very usual for patients to complain to us of voices and warnings and threatenings; why are they not irresistibly driven to obey and to communicate their feelings? Simply because they question them, bring them to the test of common opinion, exercise free will upon them, and so regain the use of reason. If the suggestions are in accordance with reason they may be obeyed, if not they are cast out and forgotten with all speed. These wise persons are in fact their own keepers and mad-doctors. Whereas the foolish by an initial yielding find their will weaker day by day, till at last recovery by their own efforts, however wished for, becomes impossible.

Compare the conduct of the two persons whose cases we have

[1] 'Grace Abounding,' § 22. [2] Ibid., § 93.
[3] 'Maria Brown's 'Letter to John H. Noyes,' Discon. ii, 38.

quoted. The impression on the senses of Miss Lincoln does not appear to have been very vivid, or she would have repeated the sentence as she heard it; yet she sets off and defies decency without a pause. Definite words are burnt in upon the nerves of John Bunyan, yet after a short argument with himself he goes on with his game of cat; and the warning which he really allows to have a practical influence is reasonable enough, namely, the rebuke he received from an old woman for profane swearing, a month after the notable game of cat. Again, the "Simon, Simon," which came rattling in his ears so distinctly that he looked back to see if somebody was not calling out behind him, he thinks afterwards was designed to stir him up to prayer and watchfulness; but he never thought of screaming it out to others like a ranter, or shaping his actions in accordance with it: indeed, he is struck with the fact that Simon was not his name, and set himself to "muse and wonder, what should be the *reason* of this Scripture"—there was his safeguard, he at once brought his *reason* to bear. Nor was his ear the only sense affected; at another place he mentions having felt something pulling at his clothes when alone in prayer. Constantly assailed in this way, living the life of an enthusiast, an excited preacher and writer and martyr, in excited times, he yet preserves his sanity to the end. On the other hand the well-educated lady first mentioned yields at once to the lightest force, becomes contagious, infects others, and is soon the centre of a band of ecstatics. We are not surprised to find them next giving way to the instinctive promptings of the reproductive organs; and the singing of "Woe, woe, to Babylon" was followed by what Noyes (one of their temporary prophets) calls "a bold self-sacrifice for the purpose of killing shame and defying public opinion." This consisted in nocturnal visits to the bedrooms of their male leaders, for the purpose of spiritual bundling.

It is doubtless singular, as Mr. Dixon points out, that in such various countries as Germany, America, and England, epidemic disturbances of the established relation between the sexes should have originated always in religious revivals. But we think the explanation lies close at hand for the psychologist. The aim of these revivals is to inject, by the agency of sympathy, certain active panics into the mind. The dogmas inculcated are mostly true and harmless in themselves, in fact, mere platitudes. The danger consists in teaching the patients to obey instantly and without question; to obey the emotion excited in their own souls, which they are led to consider the voice of God. To that they surrender their volition. But when will it cease to be the voice of God? Who is to draw the line at which thoughts that arise in the man are to be examined and questioned again? It is so pleasant to feel safe under the infallible guidance of an omnipotent spirit within, that we are not surprised at those affected being loath to suspect the re-assertion of their rights by the animal and intellectual propensities. The man "*has God* in him! the man *is* God!" Archdeacon Ebel allows his more intimate followers to look upon him as a secret manifestation of the Deity, Schönherr is the Holy Ghost, Prince is "The Beloved," the Rev. Abram C. Smith claims to be inspired when he invades the once happy home of the Cragins and makes Mary Cragin his concubine before the weeping eyes of her overawed husband.

We must pause a moment to notice the exceeding pathos of the last-quoted episode, described very much in the words of the actors of the tragedy. The wills of a loving couple are slowly and inextricably inwound with the coils of spiritual influence. They never cease to love one another, as human creatures ought to love; but with the husband's sad consent, the poor woman is unwillingly yielded to the arms of the representative of the divine. The only thing in literature like it is Homer's picture of the heralds, in the strength of their sacred office, leading off the lingering Briseis from Achilles' tent—

'Η δ' ἀέκουσ' ἅμα τοῖσι γυνὴ κίεν κ. τ. λ.

while her master sits down and weeps with his gaze fixed on the dark-blue horizon of the Ægean. The situation is too harrowing for the poet to dwell on, as we here find when it is acted in real life.

But it is only because we are not furnished with the details that we feel less in other instances the horrible slow torture to the affections which the preachers of spiritual matrimony do not scruple to inflict by wholesale. With one exception (a plump beauty in middle life) Mr. Dixon describes all the female inmates of the Agapemone as having a look of ill health, in spite of the favorable external physical circumstances under which they are placed.. The unnatural degradation of the mind has reacted on the bodily aspect.

What is meant by spiritual matrimony? Some readers may exclaim—"Is not all this mere hypocrisy and lust, or vanity, taking advantage of weak intellects for its selfish gratification?" It is impossible to think so in regard to the persons whose biographies are related by Mr. Dixon. Archdeacon Ebel was from his youth up occupied wholly with religion; Mr. Prince, at Lampeter, as one of the "Praying brethren," was a daily living protest against the prevalent worldliness of the place; Noyes, Smith, Worden, and all the rest whose names come before us, were characterised by zeal, perseverance, and power in the service of God; their daily walk was convincing to their neighbours that they held a holier faith, and lived better lives, than common men. Neither will it do to set the matter aside as "mere insanity," and so end it. Often would the relations of Ebelians, Free-lovers, Princeites, have been glad to demonstrate insanity sufficient for a certificate, but cannot do it. No doubt it is a morbid condition of mind, or we should have no excuse for reviewing it in this periodical, and it also sometimes ends in insanity (as in the case of Miss Lincoln), but even then it is a cause, rather than an effect, of that condition which the law is able to recognise as incapacitating a person for self-guidance. The confession of M. L. Worden marks its position in psychical nosology. He says of the leaders of the movement,

> "They believed in salvation from sin; that whosoever is born of God does not sin and cannot sin, and has no disposition to sin . . . they believed they were led by the Spirit. They rejoiced in deliverance from what they called Babylonish captivity, or the legality of the churches, and no doubt this sentiment finally affected their feelings and practice in various ways, and especially was applied to domestic and social relations."

Then in nauseously pious, and tediously exculpatory language, the narrator goes on to tell how the marriage tie was discarded, and temporary comminglings of male and female souls indulged, till "the relation became so far carnal as to lay just foundations for scandal." It is clear that the aberration dates from the moment when the voice within is held to be the voice of God, and *ex vi termini* deserving of immediate obedience. We are quite ready to believe that when the Rev. Mr. Prince entered the room to perform the blasphemous rite by which he selected poor Miss Paterson as his concubine, with his own lawful wife standing by as an assistant, he did not know what he was going to do. Sister Ellen, who was also present, assured Mr. Dixon that from close (shall we say "jealous"?) observation she was certain of the fact:[1] and to all these people the mere entrance of some notion into their heads is sufficient, and indeed the only, evidence of its truth. When once a man has determined that the infection of nature has been expelled from his body, all the rest logically follows. The restraints of the higher life being thrown off, the degenerate man reverts into the promiscuous intercourse of the inferior animals, and joins a herd of "Free-lovers." The reason why revivalism leads to adultery and fornication more than to other sins is simply the universal possession of generative instincts.

Mark how shrewdly John Bunyan scents out the old Adam

[1] 'Spiritual Wives,' vol. ii, chap. xxxiii.

lurking still, probably unconsciously, in the proud, spiritually led prophet. He is speaking about those salutations to which Mr. Dixon applies the German student's term of "Seraphim kisses." "Some indeed," says he, "have urged the holy kiss; but then I have asked why they made baulks, why then did they salute the most handsome, and let the ill-favoured go?" By such habitual questioning of the spirit and bringing it under the yoke of reason, "God's poor servant, John Bunyan," pestered with voices, haunted with horrible despairs, twitched by the devil while praying, terrified with visions, surrounded with persecuted enthusiasts when out of prison, and when in companioned by his own morbid thoughts, not only kept himself sane enough for all the needful purposes of life, but turned his sorrows into an everlasting treasury of charming allegory for all ages and nations. And herein lies the moral we would draw: let those who confide to us their delusions be taught that they can and ought to restrain themselves by their own free will; that the free will grows by exercise; and that if they judiciously exert it, no one need ever know anything about the morbid conditions of their minds till they publish an autobiography.

It is remarkable what an effect a single lesson of this kind will have sometimes. We once went to live for a few months with a lunatic of no great compass of mind, and rather spoiled by an idle dilettante life, but still with the feelings of a man of honour. He was haunted with the conviction that all meat brought to table was human flesh, and had other equally nauseous impressions regarding most articles of food. The first day at dinner he exhibited his delusions, and we then told him positively that he could restrain himself, if he liked, from such disgusting thoughts, and that a repetition of them would lead to a breach of our engagement, and that therefore such repetition would be not the conduct of a gentleman. During three months' companionship there was not only no display to us of the special delusions, but all others much abated.

If every one will act thus as his own keeper from the first, lunatic asylums will be needed in much fewer numbers; let a man once yield to a morbid impression, say he cannot help it, or place his will under the dominion of another's, and the most serious step of all has been taken in the direction of a madhouse.

Review VI.

1. *Epidemic Meningitis, or Cerebro-spinal Meningitis.* By Alfred Stillé, M.D., Professor of the Theory and Practice of Medicine, University of Pennsylvania, &c. Philadelphia, 1867.

2. *Eighth Report of the Medical Officer of the Privy Council,* 1865. London, 1866.

3. *A Biennial Retrospect of Medicine and Surgery for* 1865-6. (New Sydenham Society, 1867.)

4. *Report of the Proceedings of the Medical Society of the King and Queen's College of Physicians of Ireland.* (In 'Medical Press and Circular,' 1867.)

5. *Klinische Beobachtungen über Meningitis Cerebro-spinalis epidemica.* Von Prof. Ziemssen und Friedrich Hess, in Erlangen.

Clinical Observations on Epidemic Cerebro-spinal Meningitis. By Professors Ziemssen and Friedrich Hess, of Erlangen. From the 'Deutscher Archiv für Klinische Medicin,' 1866. Bd. I, pp. 72 *et seq.*, and pp. 346 *et seq.* ('German Archives of Clinical Medicine.')

6. *Four Cases of Cerebro-spinal Meningitis in Shorncliffe Camp.* 'Medical Times and Gazette,' April 4th, 1868.

7. *A System of Medicine.* Edited by J. Russell Reynolds, M.D., &c. London, 1868. Art. *Epidemic Cerebro-spinal Meningitis.* By J. N. Radcliffe, M.D.

8. *Zur Pathologie der Epidemischen Meningitis.* Von Dr. Klebs, in Berlin. 'Virchow's Archiv,' 1865. Band XXXIV, pp. 327 *et seq.*

On the Pathology of Epidemic Meningitis. By Dr. Klebs, of Berlin.

9. *Publications of the Massachusetts Medical Society.* Vol. II, No. 1. *Spotted Fever, or Cerebro-spinal Meningitis in the State of Massachusetts.* Report, &c. Boston, 1867.

10. *Proceedings of the Pathological Society of London.* Vol. XVIII. 1867.

11. *A Report upon Epidemic Cerebro-spinal Fever.* By Ed. W. Collins, M.D., &c. 'Dublin Quarterly Journal of Medical Science,' August, 1868.

Dr. Stillé speaks of epidemic meningitis as having ravaged the United States for the last ten or eleven years, and as now appearing to approach the end of its career (1867). One hundred and twenty cases were treated in the Philadelphia Hospital in the first quarter of that year. He regards the disease as distinct from typhus fever, and as presenting "a surprising variety of morbid phenomenon by virtue of its double character as a blood disease, and an inflammation of the cerebro-spinal membranes."

He considers that morbid anatomy has of late enabled us to distinguish the disease from forms of fever, with which in the last and in previous centuries it was confounded. It was not recognised as a distinct affection until the beginning of the present century.

Unconnected with animal or other putrefaction, Dr. Stillé says it is pandemic, and uninfluenced by any "miasmatic, cryptogamic, or analogous agent." Nor is it due "to a special poison like cholera, small-pox, or measles.

"Its outbreaks have occurred almost simultaneously in regions as widely separated as Europe is from America, and annually it has made a mid-winter attack upon towns and rural districts, the salubrious and unhealthy alike, completing the cycle of its progress in a period varying between ten and fifteen years. Three such periods, at least, have occurred during the present century. The first of eleven years began in 1805, and terminated in 1816; the second, of thirteen years, occurred between 1837 and 1850; and the third extends from 1856 to the present time, and has already lasted for eleven years, during which the disease has been almost constantly present in Europe, but absent during four years from the United States."

Vieusseux described epidemic meningitis in 1805 as a disease new to himself and his colleagues; a petechial eruption and engorgement of the brain marked the course of this "malignant non-contagious fever." Mathey in one of the fatal cases found a gelatinous exudation on the convex surface of the brain, and a yellow puriform matter upon its posterior aspect, upon the optic commissure, the inferior surface of the cerebellum, and the medulla oblongata. Prussia, Holland, Rhenish Germany, Bavaria, or the east of France, had the disease prevalent each year from 1805 until 1816.

It prevailed in America from 1806 to 1816. In 1822 it appeared in France, in 1823 in Connecticut, in 1828 in Ohio. Sunderland, in England, was visited in 1830, and Naples in 1833.

From 1837 to 1850 it visited France, Italy, Algeria, Gibraltar, England, Ireland, Denmark. America was again visited from 1842 until 1850. Between 1850 and 1854 we did not hear of the disease; in the latter year, however, it broke out violently in Sweden, where it continued till 1860. Sporadic cases now occurred in Britain, and it again appeared in several parts of the United States.

Holland was attacked in 1860, Portugal in 1861, and Germany in 1863; in the latter country it was severely felt also in 1864 and 1865.

It occurred in Dublin also in 1865, and also in the United States from 1861 up to the commencement of 1867. To this account by Dr. Stillé we regret to add that the disorder has now[1] (January, 1868) reappeared in Dublin.

The author dwells upon the great diversity in signs, both during life and after death; referrible, as he considers, to the double nature of the disease, namely, one of the blood and of the nervous system.

Meningitis, sometimes sudden, at other times gradual in its development, presents generally the former character in the earlier period of its prevalence.

Chilliness, prostration, vomiting, and headache, are commonly the earliest symptoms, and may continue from an hour or two to several days. In general, the longer these symptoms continue the milder will be the attacks.

Feverishness and pains in the back and limbs occur, and as the case advances, the symptoms already named become alarmingly severe either with or without the occurrence of a heavy chill. The pain in the head becomes excruciating, the face livid or pale and sunken, and extreme restlessness comes on. The pulse is as often slow as frequent, and the skin little if at all warmer than natural.

The pains, which at first were "vague," are now concentrated, and seem to dart in every direction from the spine, which is also, especially at its upper part, the seat of severe aching; and, in many cases, its muscles become more or less rigidly contracted, so that the head is drawn backwards, or the whole trunk is arched as in tetanus. Trismus is not uncommon, and clonic spasms frequently affect the extremities; even general convulsions are occasionally observed. Wandering and hallucinations, delirium, violent maniacal ravings, incoherent mutterings, and stertorous coma, may often be present in greater or less severity. The pulse now increases in frequency, sometimes becoming extremely rapid; the skin, although it grows warmer, does not acquire the temperature of idiopathic fevers, nor sustain it as equally as in them. Eruptions often are observed. In some epidemics only herpes labialis is seen; in others, the state of the skin may resemble roseola, measles, or the mulberry rash of typhus, or, from the first, it consists of petechiæ, vibices, or

[1] Happily, however, the disease did not exceed the sporadic form this year in Ireland. August, 1868.—Reviewer.

extensive ecchymoses. The tongue generally is like that of typhoïd fever; at first moist, then coated with "mucus," then red and shining, or brown and fuliginous.

The appetite fails, thirst is not often urgent. One or two liquid stools at first are generally followed by constipation, although in very grave and protracted cases diarrhœa may even become colliquative.

A soporose condition, with muscular relaxation, debility, and tremulousness, with paralysis of the sphincters and of other muscles generally, but by no means always, attends the fatal issue of the attack; rigid opisthotonos may be present. When recovery occurs, the cases have seldom if ever been so grave as depicted above, "especially the typhoïdal symptoms."

If the nervous symptoms have been very serious, and life spared, the return to health is long delayed, or, it may be, is never perfectly attained.

Dr. Stillé with much reason speaks of the almost endless variety in the symptoms of this Protean disorder.

Our space does not permit of our giving the detailed observations of Dr. Stillé, upon the several signs and symptoms noticed in various parts of the world, by the numerous authors whom he quotes, as well as those made in the cases that came under his own notice. He treats of those affecting the nervous system under the headings of—"Headache,"—which presents great variety in those cases which are not at once smitten down with "lightning speed" (méningite foudroyante). It is variously described as "acute," "violent," "heavy pain," "distress amounting to torture, particularly through the temples," "lancinating," "crushing," "boring," "as if nails were forced into the brain," resembling rather what is caused in inflammation by tuberculous or other deposits or by wounds, than the pain of typhoid or typhus.

Vertigo,—this symptom, with anxiety at the stomach, quick and irregular pulse, nausea, and even fainting, was commonly observed early in the disorder.

Debility, "great, surprising, and sudden," "a death-like sinking sensation in the epigastrium," "syncope,"—these symptoms exceeded both in uniformity of occurrence and in degree, anything of the kind met with in typhus or typhoïd fevers.

Delirium sometimes ushers in an attack of the disease, and although not constantly present in severe degree, it not unfrequently occurs in greater or less severity in most outbreaks of meningitis.

Coma, almost always present in fatal cases, most usually occurs in a marked degree only towards the closing scene.

Complete loss of memory, especially as regarded the circumstances attending the commencement of their illness, very frequently occurred.

The facies, or expression of countenance in this disease, is also peculiar; the suffering, whether paroxysmal or persistent, being represented by the features. As the case advances, the expression becomes "fixed and stupid," differing, however, from that of a drunken person in the absence of turgidity or purplishness, as well as from the dark swollen and flushed face of typhus, or the languid expression and the circumscribed flush on the cheek of typhoid fever. The face is generally pale and sunken from the beginning; in not a few cases it bears resemblance to that seen in cholera.

Hyperæsthesia of the skin is a frequent symptom; it may, however, as Dr. Stillé observes, be sometimes confounded with reflex irritation. It is often followed by cutaneous numbness or insensibility.

Pain in the spine and limbs like the last-mentioned symptom is referrible, as Niemeyer considers, to pressure upon the roots of the spinal nerves by exudation matter.

The severity and rapidity of these pains which are described as pricking, stinging, benumbing, often causing blindness, faintings, sickness at the stomach, præcordial distress, partial loss of motion in one or both limbs on one side, with great prostration of strength, have been fully noticed in the records of the disease from all countries where it has been observed. It may occur in any stage of the disease and vary in duration, being sometimes limited to its early period, in other cases continuing throughout the whole course of the disease, and even after convalescence. The author puts confidence in cupping the nape of the neck and along the cervical vertebræ in sthenic cases, and in the use of dry cups followed by vesication in cases accompanied by debility for the relief of the neuralgic pains. The latter appear generally to be aggravated by pressure.

Tetanoïd phenomena are even more characteristic of epidemic meningitis than pain of the spine and limbs; but this symptom may be absent as well as the petechial spots. In some cases the sterno-mastoïd muscles of one or both sides may only be affected, in others the muscles of the neck more generally, and in some those of the abdomen as well as of the neck, jaws, and superior extremities may be firmly and rigidly contracted. Cases have been recorded by Stokes, Kendall, and others in which the head was drawn back to an extreme degree. So variable have been the symptoms of this disorder that, both in America and in Europe, thoughtful and able physicians have been divided as to the real nature of the disease, some regarding it with Dr. Stokes as a disease of the blood of an essential nature, many others as cerebro-spinal meningitis. A reason for this latter opinion, which the writer of this review, in common with Dr. Stillé, holds, is the fact that during the prevalence of the disease many cases of fever assume to a greater or less extent the tetanoïd or other cerebro-spinal character. Opisthotonos of the muscles of the neck attended cases that were ushered in by furious delirium as recorded by Love (1847).

Dr. Parks found in the records of 261 cases in Massachusetts that "severe opisthotonos existed in 107, slight in 80, and in 'nearly all' of 26 cases; so that the symptom was absent in only 48, or in less than one fifth of the whole number."

Tremors and twitchings of tendons are much less frequent than in typhus, observes Dr. Stillé; we have observed in 1867 in Ireland tossing of the arms occurring in a child of seven years of age in the advanced stage of a fatal case.

Clonic spasms or convulsions have been frequently observed, especially in children. One half of the body has been convulsed while the other half was paralysed.

Paralysis, or loss of muscular power in different degrees, and more frequently still, abnormal muscular contraction has often been met with in the limbs, and as observed by Jackson, paralysis of the muscles of deglutition. This latter symptom was seen by us in at least one case, and lately we had under our care a lad, of eleven years, with muscular contractions of both legs at the knees; this case has dilated pupil and ptosis of the left eye, and vomiting of greenish matter often in the morning as well as convulsions at night.[1]

Dr. Stillé is disposed to refer the prostration which has obtained for this affection the name of "sinking typhus," to the effects of the congestion, effusion, or exudation upon the origin of the pneumo-gastric nerve, which associates the actions of the lungs, the heart, and the stomach.

Symptoms furnished by the Organs of the Senses.—The Eyes.—Differing from the dark or dusky, almost purplish colour of the eye in typhus, and the rather striated redness of the conjunctiva in typhoïd fever, the eye in meningitis generally presents a reddish or pinkish colour without distinct vessels being to be seen; sometimes conjunctivitis, with profuse[2] purulent discharge is present. Double and triple vision was noticed by North, Banks, Armstrong, Gilkrest, and Jenks. Banks, Burdon Sanderson, and many others observed strabismus; we have observed this sign in meningitis and also in a case of fever

[1] After an illness of upwards of three months this boy died, having become attenuated to the last degree. No autopsy was allowed.

[2] Gordon.

which relapsed into meningitis; in some epidemics it is not a common symptom.

Blindness was often observed in America as the earliest deviation from health. In some cases sight returned in a few hours, in others in a few days; and although restoration of sight was the rule, permanent loss of sight by amaurosis occurred in one case reported by Jenks; cataract, injected cornea, soft and shrunken globe were also seen in America. Cases of rapid destruction have been observed.

Purulent ophthalmia, softening of the cornea, hypopion, opacity of the vitreous humour, and synechia posterior have been met with. The affections of the eyes have been[1] attributed to inflammation of the neurilemma of the trunks of the nerves which supply the eyes.

Some writers attribute ulceration of the cornea to its exposure in consequence of paralysis of the orbicularis.

The pupil varies much in shape and size in meningitis; sudden contractions and alternate dilatations, permanent dilatation, one pupil contracted and the other dilated, contraction, photophobia, and spasmodic movements of the ball of the eye have all been witnessed in the disease.

Loss of hearing in some cases, and of the sense of smell in others, and instances of purulent discharges from the ears and nose have been met with.

Though the physiognomy varies from that indicating maniacal fury to that of profound stupor, yet as Dr. Stillé observes, "its average condition does not indicate either excitement or coma, or even a greater degree of dulness. The patient moves his eyes more briskly than in the two forms of fever several times referred to, and winks their lids in a manner quite unknown in those affections."

Symptoms presented by the Digestive Organs.—The general state of the tongue was "moist, whitish in the centre and at the tips and edges." The appearance was not constant, being in some cases dry, and more or less brown. A "bloodless appearance" was regarded by some as indicative of approaching death.

Nausea and vomiting have been observed as characteristic of the disease, evidently traceable to the cerebral lesions.

The appearance of the matters vomited has generally been bluish or greenish, though in some cases it was whitish and viscid. Faintness, or coldness, or a deadly feeling has been experienced in the stomach by many of the sick, indicating, as Stillé observes, the cerebral origin of the symptoms.

Constipation appears rather to attend the disease than diarrhœa,

[1] Niemeyer.

though the latter has been occasionally seen; in many persons the bowels continued to act regularly.

Contrary to what is seen in typhus and typhoïd fevers, the appetite in meningitis very early returns, and even becomes craving almost as soon as the "painful stage of the disease is passed." This more especially is the case with children. Thirst is not characteristic of epidemic meningitis, at least "cool and acid" drinks were very rarely grateful; if a desire was evinced for liquids it was for such as were "warm and aromatic," as they relieved the depression so constantly felt at the stomach. This was not always the case, however, as Stillé observes that at Philadelphia, recently, the patients were "clamorous for fluids." A form of sore throat resembling cynanche maligna—minus the swelling of the tonsils—was seen in some of the American epidemics. Hale observed that sometimes the gums and fauces were swelled and inflamed, and that there was accompanying partial salivation. Only one European observer—Levy, Stillé thinks, noted a similar condition; the latter writer speaks of a thin pearl-coloured or whitish band upon the gums, which in one case invaded the fauces.

Aphthæ have been seen. Swelling of the cervical parotid and sub-maxillary glands were met with, and not infrequently in fatal cases. To impaired muscular power, and in some to blunted perception in the nervous centres, as well as in other cases to the use of fly-blisters, may be attributed the occurrence of retention, of incontinence of urine, and in other cases of dysury.

The chemical characters of the urine also varied in one case, albumen, granular casts, and pus-cells were found, together or singly, in the former case without urea, in others phosphates with diminution of the chlorides occurred.

Pain and swellings, often of a purple colour, of the joints, not unlike gout, pointed to the predominance of the blood element in some cases. "Synovitis," "inflammatory effusions in the joints," and effusion without redness, were observed by different physicians in Europe and America.

Respiration was generally difficult, in some cases sighing, laboured and interrupted (Tourdes). Burdon Sanderson mentions cases in which "its embarrassment was marked by a slow laboured inspiration, followed by a quick inspiration and a long pause." Serous effusion in the air-tubes causing gurgling rhonchi often preceded death. Pneumonia pretty often occurred. The suspirious and interrupted respiration resembled that seen in tubercular meningitis. It is most often seen in cases presenting tetanic symptoms, paralysis, and other evidences of central cerebral compression.

The state of the pulse varied much in the records of meningitis compiled from very many American, British, French, and other authors, and in the cases observed by Stillé himself, and corroborates the observations made by Ziemssen and Hess, recorded in another portion of this review. Diminished force and volume, and a tone so much impaired that slight causes produce extreme variations in its rate and rhythm are, therefore, the characteristic qualities of the pulse in this disease, and those by which it is distinguished from the fevers to which it bears a superficial resemblance.

The state of the skin varied greatly from dryness, usually in the earlier stages, to "profuse sweats," which latter were apt to continue if once excited.

As to temperature great variety was observed, although frequently below the normal standard it not uncommonly rose, especially in fatal cases, as the disease advanced. Thus Wunderlick noted 107°, 108°, and 110° respectively in three cases at the point of death. These observations extend to cases recorded in several parts of the world, by many physicians, agreeing with what we have seen, and what we have had occasion to note from other observers. As Dr. Stillé observes, this irregularity of temperature serves to distinguish epidemic meningitis from typhoid and typhus fever. Eruptions of the skin did not by any means invariably occur, according to the observations collated by Dr. Stillé; however they were present sufficiently often in the various American epidemics to give the name of spotted fever to the disease, and in Europe, though not occurring as frequently as do the characteristic eruptions of typhus, still our experience and reading lead us to agree with Ziemssen and Hess in regarding the disease as very rich in cutaneous affections.

Petechiæ, though noticed as occurring rarely by some, were considered by the two last-named authors, as the most frequent form of cutaneous affection.

The authors quoted by Stillé mention the following as occurring in the disease:—efflorescences, carbuncles, pustules, buboes, bright red eruptions, some like measles and some like erysipelas, miliary eruptions, nettle-rash, rash like scarlatina, bullæ, herpes labiales, ecchymoses, roseolous and erythematous affections.

The disease in Ireland in 1846 has not been described as presenting cutaneous affections, but the epidemic of 1866-7 presented a considerable variety, viz., herpes, urticaria, bullæ, petechiæ, and large spots resembling purpura hæmorrhagica; large mortifying portions of integument and subjacent soft parts also occurred in some cases.

The fatal end appeared in some cases to depend upon asphyxia from pressure of the serous effusion upon the medulla oblongata and spinal cord; coma, gradual asthenia, convulsions, paralysis, and in other cases delirium were varieties presented by the closing scene of this remarkable malady. Some individuals gradually recovered, a few lingered for months, reduced to skin and bone, others recovered with impaired vision or hearing; such is the tenour of the records of the recent Irish epidemic.

Extreme variety in duration of epidemic meningitis has characterised the disease in all countries. Tourdes well observed that it is "distinguished by the slowness of its cure, and the rapidity of its fatal issue," and Hirsch asserts that "its duration is between a few hours and several months."

The return to health is generally slow, irregular and uncertain, rapid and extreme prostration and emaciation which attend the attack being followed by tardy and irregular convalescence, as described by Fish and Gallup in America, and Tourdes and Gillkerst, among European authors. Hale and others, however, record several cases that were followed by rapid convalescence. "Still the general statement continues to be true" (as Stillé observes), "as it is expressed by Hirsch, 'convalescence is irregular and protracted; in spite of good food and regular digestion, emaciation and debility are sometimes of long duration.'"

"Persistent headache, neuralgia, convulsions, stiffness of the neck, or pain in moving it, morbid sensibility of some portions of the skin, palpitation of the heart, dyspepsia, &c., embarrass the return to health." "It is highly probable," as Gallup remarks, "that the internal membranous inflammation is always present more or less."

Deafness more or less perfect; impaired vision; paralysis of one or more limbs, and general impairment of muscular power; loss of memory and even insanity are consequences of the disease, deafness or dulness of hearing and affections of the eyes being more frequent than paralysis, and far more common than the affections of the mind.

"Apathy or apparent stupor" is, however, far from uncommon, and continues for a considerable period in some cases, resulting, if the cases recover, in deafness not infrequently. Sanderson, Gordon, Mayne, and others, have mentioned this condition. Recurrence of the initial symptoms (Sanderson) frequently occur. Hale, Gallup, Parks, Jackson, also state that relapses are very far from being uncommon. Stillé observes that the earlier authors considered "cure" common in relapsed cases, but that the more recent writers do not coincide in this observation.

The mortality varies much: thus, Hirsch found it, in the greater number of epidemics of meningitis between 1838 and 1865, to vary from 75 per cent. to 20 per cent. Stillé found that it varied from about 6 per cent. in Massachusetts to but 35 per cent. in the Philadelphia Hospital. And in ten epidemics occurring in various places between 1838 and 1848, the last-named author states the rate to have been 70 per cent., and in the decade 1855 to 1865 it was but 30 per cent.

Niemeyer found the mortality in the Baden epidemic to be 30 per cent., whereas in West Prussia it was probably not much less on the whole than 50 per cent.[1]

Murchison advocates the idea that the cerebro-spinal epidemic of Germany was but a variety of typhus.[2] Stillé regards this as a serious error, especially in respect of the treatment of each disorder.

Stokes, Banks, M'Dowell, Gordon, and other able Irish physicians consider the epidemic as quite different from typhus. H. Kennedy records what we have ourselves also seen, namely, cerebro-spinal meningitis complicating typhus and typhoïd fevers, and he advocates local bleeding and mercury. M'Dowell in 1846, Burns and others in America also observed the tendency to spinal irritation in ordinary fever cases, and in persons in health, when cerebro-spinal disease was epidemic.

Dr. Banks regards the epidemic as it appeared in Ireland as appearing in two forms, one bearing all the characters of a blood disease, but not presenting the cerebro-spinal symptoms , in the second, on the other hand, were found all the symptoms and post-mortem appearances of cerebro-spinal meningitis.[3]

Dr. Stokes spoke of the occurrence of two forms of *essential disease* in Dublin in 1866, the first being that which he proposed to call malignant purpuric fever, and which preceded the other, namely, the cholera. He pointed out the great rapidity of some, especially of the early cases of the former disease, which became much less frequent during the pressure of the cholera, to reappear again on the subsidence of the latter disease. Dr. Stokes observed from the fact of the occurrence of these two diseases, almost at the same time it was hard to avoid the conclusion that some relation existed[4] between them. Be-

[1] 'Biennial Retrospect, New Sydenham Society,' 1867.

[2] 'Lancet,' April, 1865, and 'Sydenham Society's Retrospect,' 1865-6.

[3] Report of the Medical Society, College of Physicians of Ireland, 'Med. Press and Circular,' June 19, 1867.

[4] The apparent relationship between cholera and meningitis epidemica was alluded to in the articles "Cholera Authorities" in this 'REVIEW.'

tween the epidemic under consideration and typhus, existed the following difference, the great rapidity of the course of the former, the eruption, its mode of appearance, character, and duration, the frequency of cerebral and spinal lesions. Again, the temperature was generally low in the purpuric cases. Some very rare cases have undoubtedly been recorded in which a temperature of 107° and even 110° Fahr. was attained: these latter observations have been made since Dr. Stokes recorded his experience as above related.

Another point to which Dr. Stokes alluded, was the idea held by some that an affinity existed between the epidemic and measles, owing to the great prevalence of the latter in a very fatal form at the same time as the former. "All these facts would strongly bear out the view that this disease was a blood-poisoning, and that the cerebro-spinal affection was a secondary disease in this form of fever."

Dr. Stokes remarked that in a few cases there were grounds for supposing it to be contagious. It is worthy of remark that an outbreak of purpuric measles occurred also in 1867, in an epidemic form at Sydney,[1] New South Wales, and it was considered by Mr. Carroll to bear considerable analogy to the epidemic as it was observed in Dublin. The cases were frequently ushered in by convulsions, and nearly every fatal case was terminated by them.

Arnes and Hirsch have observed the intermittent type of the epidemic in malarial districts, but Stillé says it is by no means confined to such localities.

Authors have made several classifications of the forms of meningitis as seen in different epidemics. The abortive form seen as a complication of ordinary fever has been already mentioned as observed by H. Kennedy. M'Dowell, Sargent, Stillé, Gauné, Burns, and Kempf, have all mentioned the liability of persons resident in affected localities to headache, neuralgic pains in the nape of the neck, and in various parts of the body. Wunderlich classifies the disease according to its degrees of severity; other authors mention the congestive and inflammatory, the malignant and mild forms of the disease.

Stillé observes that "according to its type and duration there never fail to be found some of those changes in the membranes or in the substance of the great nervous centres, which denote the existence of inflammation. Congestion of the blood-vessels and exudation of serum, fibrin, or pus, beneath the meninges, and different degrees of alteration in the nervous pulp attest the nature of the process." He continues to say that we are not always to expect to meet with positive changes post-mortem,

[1] 'Medical Press and Circular,' 15th April, 1868.

owing to death at an early stage of the inflammatory process, or to a constitutional element, a morbid condition of the blood, which underlies all the phenomena of the disease, and modifies more or less its features.

While Burdon Sanderson states that post-mortem rigidity did not affect any of the muscles which had been contracted during life, Gordon thus speaks of a girl of fifteen that died of the disease after a week's illness:

"The body, after death, presented a very frightful appearance. It was still prominently arched forward; it was of a dusky blue colour, and with a copious eruption of black spots of various sizes from that of a small pea to a crown piece; some small and circular others large and irregular in form. One or two of those near the knee had taken on a gangrenous action, and appeared to have been rapidly spreading; several of the smaller spots were effusions into the layers of the skin—very prominent, hard, black, and circumscribed, like the minute spots of apoplexy of the lung. There were, moreover, various patches of hepatic eruption on different parts of the body, and several bullæ containing dark-coloured serum. In this case the brain and spinal cord presented all the appearances of intense purulent arachnites. The blood in all the cavities was very fluid and dark-coloured. The lungs were 'intensely congested.'" [1]

This author points out the absence, in his experience, of the almost paralytic condition of severe cerebro-spinal cases in the debility of malignant measles, which has often co-existed epidemically with cerebro-spinal meningitis; he also says,

"Although there may be many spots of petechiæ, there are never found those ecchymoses from decomposed blood in the substance of the true skin which are so characteristic of the epidemic; and while the rubeolar eruption is dark-coloured in the extreme, it retains its characteristic of being an elevated eruption, and affecting more or less the crescentic form."

We must observe that the eruption of epidemic meningitis is in some cases raised. Gordon well observes the difference between the coryza of measles and the pulmonary affections of the epidemic, those of the latter being œdema of the lungs or diffuse pulmonary apoplexy, and these combined with a more or less emphysematous condition of the anterior portions. The last writer also speaks of the necessity of distinguishing between meningitis and hysteria, and of the occurrence of albuminous urine in the epidemic disease. For ourselves, we do not regard cerebro-spinal meningitis as being likely to be confounded with hysteria.

The purplish spots on the anterior surfaces, the congestion of

[1] 'Proceedings of the Pathological Society of Dublin,' 1866-67.

the eyes, &c., disappear or grow paler after death, while (Stillé) states that large patches of a livid colour, or a uniform discoloration of the same hue may sometimes be observed along the posterior parts of the neck, back, nates, and thighs.

Klebs states that to atrophy of the muscles and connective tissue is due the rapid emaciation of protracted cases.

American authors generally mention considerable congestion existing in the cerebral veins and arteries, in meningeal cases. The same has been observed also in the earlier stages by European writers. The transparency of the arachnoïd becomes impaired if the cases pass the congestive stage, and the pia mater often is found adherent to the brain.

The "arachnoïd cavity and ventricles" often contain serum which is sometimes of a red colour. The microscope showed pus-globules in the serum. Occasionally (Tourdes) the arachnoid cavity was dry or marked by flakes of pus.

The ventricles often contained serum, which assumed a milky hue in some cases from exuded fibrine.

Stillé quotes many writers to show the inflammatory nature of meningitis, where the disease had lasted long enough to form fibro-purulent, pus-like, semi-purulent deposits in the coverings of the brain and spinal cord.

The stages of congestion and of exudation are exhibited in the post-mortem examinations of fatal cases. In malignant cases death occurs before time has sufficed for the full development of inflammatory lesions; in those of longer course may be found, first of all, turbid serum, then a more or less pasty and fibrinous deposit with some admixture of pus, and, finally, in cases of long duration, the exudation becomes tougher, more adherent, and shrivelled.

As we find deviation from the state of health evident from the post-mortem appearances of the meninges in proportion to the duration of the case, so are the results of the disease upon the substance of the brain and spinal cord.

Softening of the spinal cord appears less frequent than that of the brain, but the former does not very infrequently occur. Partial and superficial softening of the cervical portion is repeatedly seen.

Examination of the blood drawn from those affected with epidemic meningitis bears evidence of the[1] inflammatory nature of the disease. Some observed firm fibrinous clots in the heart, but "perhaps (Stillé) the greater number of observers have reported it to be dark and liquid." "A shrivelled or crenated appearance of the edges of the blood-discs, and an irregular

[1] These observations of Dr. Stillé's are qualified by comparison with those of others—Dr. Klebs, for example.

distribution of these bodies in the field of the microscope, instead of the ordinary arrangement in rouleaux or piles," was noted by many.

Evidence is thus afforded to show that inflammatory[1] action exists in the early stage of the disease, and that in a large number of fatal cases disorganisation of the blood is one of the conditions producing death. But in many other cases death is evidently caused by agencies independent of such a change, and results from direct interference with the functions of the nervous centres which are necessary to life. Stillé does not regard morbid changes in the lungs, heart, stomach, intestines, liver, kidneys, and spleen, in fatal cases, as properly belonging to the pathological history of the disease. We think it well, however, to call attention to the state of the lungs, spleen, and heart, as worthy of being always noted when a post-mortem examination of a fatal case of epidemic meningitis is procurable, and as affording characteristic lesions generally in the disease; indeed, it would be well to note the condition of all the organs as far as possible in this peculiar malady.

Pus has in some cases been found in the joints.

The cause of deafness Klebs has traced, in some cases, to suppuration of the internal ear; in others it was probably owing to alteration in the structure of the brain or medulla oblongata.

Stillé regards the disorder as pandemic in the temperate zone, as more prevalent in winter than in summer, and as visiting all localities, whether rural or urban, preferring the former rather than the latter. Youth and early manhood, rather than age, suffer by the disorder.

Debilitating agencies, as over-fatigue, fear, grief, nursing, abuse of stimuli, wet, cold, and previous illness, as measles, influenza, chincough, and fever, as has been noted in Ireland, predispose or even excite this disease.

Some in America have thought war had a bearing on the matter also. On this latter point, however, evidence is rather against its correctness. Stillé argues, too, that as London, Liverpool, and New York have escaped the disease in an epidemic form, very little importance can attach to the influence of dirt and over-crowding.

We purpose reverting to this subject in connection with the consideration of the writings of other authors; another point to which we also hope to refer again is the question of the contagiousness or non-contagiousness of the affection. The author whose work we now more especially have under consideration thinks the great preponderance of testimony is in favour of the

[1] This again might deceive those who have not witnessed the disease, and lead to severe antiphlogistic treatment. See, however, p. 380 for Dr. Stillé's treatment.

latter idea; however, when speaking of the writings of Boudin, he says it only shows "the existence of some local cause capable of determining the development of the disease in all who came within its influence; a cause susceptible of being transported by healthy men, or by their camp equipage, from one place where the disease existed to another where it had not previously occurred. The views of Boudin have failed to satisfy and convince all judges who have examined them. They fail most where most they need strength, in the proof that the disease is ever communicated from man to man; and without that proof there is no evidence of contagion," &c.

It appears to us that Stillé allows his own opinion to sway him, when he says that "a cause susceptible of being transported by healthy men, or by their camp equipage," &c., is not contagion.

Varying extremely in the mortality of different epidemics, meningitis is, for the most part, of greater gravity in winter than in spring, and in childhood and advanced life than in those between those periods.

When the attack comes on with great suddenness and severity the prognosis is unfavorable, especially if coma appears early.

Though the danger is greatest in the first few days of the illness, and the prognosis becomes more favorable as the case advances, fatal relapse may occur, even in the convalescent stage.

Of unfavorable symptoms we may enumerate the following:—delirium, and, still more so, persistent coma; unconcern about the patient's own self; a slow and compressible pulse; lively jactitation, rigid retraction of the head, spasms of other than the spinal muscles, general convulsions, extensive hyperæsthesia, dilatation and insensibility of the pupil, retention or incontinence of urine, and all cerebral paralyses (Mannkopf). To these Stillé adds—deep coma, paralysis of the muscles of deglutition, and a rapid change of the pupil from a dilated to a contracted condition. Coolness of the surface is very significant of danger, especially when the skin grows purplish by the diffusion of blood beneath it, or even by venous stasis. Petechiæ are less unfavorable than the last-named symptom, and light-coloured eruptions, incidental to the disease, are rather favorable than otherwise. Rapidly developed, dark-coloured discoloration or eruption, profuse sweats, with coma, bullæ, and gangrenous spots, bronchial obstruction, with serum or mucus, pneumonia, and pericarditis, are all signs of danger. A dry, shrivelled, or fissured pale tongue, a fuliginous condition of the mouth, obstinate vomiting and diarrhœa, and persistent albuminuria, form also grounds for a most unfavorable prognosis. Whereas mildness of symptoms, slight loss of strength, moderate pain and stiffness, absence of petechiæ or vibices, a desire for food, and power to assimilate it, are, it need hardly be said, favorable indications.

Prognosis is, however, very uncertain, owing to the possibility of a sudden accession of nervous symptoms, even when the hour of danger seems to have passed away; and, on the other hand, patients have recovered after all hope seemed to have passed away.

Acute pain in the head, neck, spine, and limbs, faintness, vomiting, stiffness, or spasm of the cervical or spinal muscles, hyperæsthesia of the skin, delirium alternating with intelligence, and merging afterwards into dulness or coma, occasionally convulsive spasms, paralysis of the face or of one side of the body, constitute the symptoms more especially indicative of inflammation of the cerebro-spinal meninges. The epidemic prevalence of the disease, the cutaneous eruptions, ecchymoses, debility, redness of the eyes, foulness of the mouth and tongue, and more or less of other typhoïd conditions, are among the evidences afforded by epidemic meningitis that it partakes of the nature of a blood poison. To these must be added its great mortality to complete the diagnosis.

Stillé gives a tabular summary, in parallel columns, of the diagnosis between epidemic meningitis and typhus. Most of the points enumerated have been already quoted by us.

He thus concludes a chapter on the nature of epidemic meningitis:—"The inflammatory element and the septic element are both necessary to constitute the disease; either may be in excess and overshadow the other. According to the relative predominance of one or the other, the disease assumes more of a typhoid or more of an inflammatory type; and it is this diversity in its physiognomy which has led to such opposite doctrines in regard to its nature and its nosological affinities."

The treatment has varied with the epidemic, as when regarded as "sinking typhus" it was actively stimulating and tonic, if it assumed an inflammatory type, antiphlogistic.

Many highly intelligent practitioners used opium largely on account of the severe pain, and the spasmodic phenomena.

Our modern experience would not warrant our returning to the use of tartar emetic as used by Vieusseux and mentioned by Stillé, nor can we for a moment suppose that any one would now advocate purgatives. As to depletion, if only local, Stillé thinks it may be practised in some sthenic cases. But in young persons, especially in children, he states, the least abstraction of blood may be followed by dangerous exhaustion. He has seen cupping the nape of the neck, and along the cervical vertebræ of essential service. If scarifying cupping is contra-indicated dry cups give much relief and aid the effects of vesication.

Cold to the head and spine has long been used in America as well as in Europe with effect in the forming stage of the disease and while the pain in the head is most intense: in many cases, however, benefit from its use has not followed. Confidence has generally been placed, especially in America, in the use of blisters applied to the vertex, and upper part of the spine in some cases. It must, however, be borne in mind that Tourdes and some other authors quite disapprove of their use. Warmth to the surface was the practice formerly in America, and is now advocated by Gordon. Nutritious diet, hot aromatic infusions, claret, port, madeira, or even brandy, and other stimulants have been generally regarded in America in former epidemics as essentials in the treatment. Recent experience, however, has led to the rule that alcohol, as Dr. Stillé says, should not be included in "the ordinary and systematic treatment of epidemic meningitis, but as a cordial to be held in reserve against those signs of failure in the power of the nervous system, which call for its administration in diseases of whatever name. In this, as in so many other respects, epidemic meningitis presents a striking contrast to typhus fever."

Though in America the evidence is not unanimous, still enough remains to justify the use of opium in epidemic meningitis in free and often repeated doses. Stillé says that sixty years ago it was freely used in the epidemic of 1808, and he quotes several authors to show that even in coma, large doses of opium produced the best effects. The tolerance of the drug he states to be remarkable—the doses he himself used were a grain of opium every hour, in very severe, and every two hours, in moderately severe cases. Under this treatment "the pain and spasm subsided, the skin grew warmer and the pulse fuller, and the entire condition of the patient more hopeful." The early stage of the attack, however, seemed that in which greatest benefit from the opium arose. Quinine is not considered efficient by Stillé in epidemic meningitis. Mercury has been used by practitioners in the various epidemics with but very doubtful benefit.

Arsenite of potassa, iodide of potassium,[1] belladonna, and ergot are other remedies mentioned in Stillé's work. He recommends nutritious diet and great care that no exertion, such as assuming the erect or even the sitting posture be made before convalescence is fully established, pointing out the danger from the singular debility characteristic of the disease,

[1] Bromide of potassium has been much recommended by Trayer.—'Medical Times and Gazette,' 1867.

expressed in the title *typhus syncopalis,* which was conferred upon it.

To the rapid extension of epidemic cerebro-spinal meningitis over a great part of Germany, as well as to its peculiar and varied symptoms, the frequent malignity and rapidity of its course, so often inexplicable by post-mortem appearances, the insufficiency of atmospheric data, the conflict of opinions as to its most suitable treatment may be attributed the interest taken by German physicians in the inquiry into its nature, pathology, and treatment.

Having traversed much of Western and Northern Europe, as well as North America, cerebro-spinal meningitis visited Northern Germany in 1863. In the succeeding year the disease was first recognised in Southern Germany, at Erlangen, by Professors Ziemssen and Hess. They record forty-three cases, with twenty post-mortem examinations.

As to the etiology of the disease, telluric conditions appeared to be entirely without influence upon the spread of the affection.

The effect of atmospheric influences appeared to be very limited. In general, however, most patients were placed under unfavourable hygienic conditions, only five belonging to the wealthy classes.

The epidemic afforded no grounds for assuming the existence of contagion. This statement differs, as our readers may remember, from the observations of Boudin, and from some particulars recorded in Britain and Ireland where contagion could not be excluded from consideration.

With respect to age the disease was most frequent and most severe in childhood; at that age, too, it proved most frequently fatal.

From 1 to 16 years, 24 were attacked.
„ 17 „ 65 „ 18 „

42

of the former 16 died, and of the latter 6 died. The sexes were tolerably equally affected—of the above 22 were males and 20 females.

As to the previous condition of the patients the majority were in good health, and only three had been exposed to any directly injurious influences.

The existence of a premonitory stage could only be traced in five instances.

Headache, languor, nausea, loss of appetite, and flying pains, constituted the precursors of the attacks, and were usually followed by a completely free interval, which lasted for several hours, during which the patients felt perfectly well and continued apparently so until the outbreak of the disease, which was always sudden.

The initial symptoms were rigors, intense headache, vomiting; not infrequently from the commencement loss of consciousness, coma or delirium, convulsions, rigidity of the back of the neck. When the attack issued in hydrocephalus, the return or increase of the headache with vomiting constituted the most important symptom. Even in the slighter attacks the restlessness of the patients was characteristic. Except in the hydrocephalic cases convulsions were not observed. Of spinal symptoms contraction of the deep muscles of the neck was the most constant, and it was entirely absent only in six cases.

Retraction of the head was for the most part so considerable that the occiput stood at a right angle to the thoracic vertebræ, and swallowing was rendered extremely difficult.

The contraction of the neck occurred on the first day of the illness, only in five instances; in all other cases it first appeared between the second and fifth days.

General hyperæsthesia, for the most part confined to the trunk and lower extremities, was met with in sixteen cases.

The pulse was liable to great and sudden changes, and the course of the temperature was irregular.

The hearing was affected in eight cases, and the eyes were in some instances implicated, but not severely. Bronchitic, atelectatic, and broncho-pneumonic conditions in the lower lobes of the lungs and their dependence on the spinal rigidity were frequently noted. The frequency of respiration was, in general, not essentially altered; in particular the striking arhythmia of respiration, characteristic of tubercular basilar meningitis was, in accordance with Niemeyer's experience, not observed. Cutaneous affections were frequent, especially facial herpes. Erythema, roseola, urticaria, sudamina, and petechiæ, the latter being relatively the most frequent, were observed. They were usually symmetrical on both sides of the body.

"The frequency of the occurrence of exanthemata, but especially their frequently symmetrical arrangement in a severe affection of the central nervous system, might contribute to confirm the assumption of a direct dependence of these eruptions on irritations of the cutaneous nerves of nutrition, a dependence which, as is well known, Daniellsen and others endeavoured to establish for herpes zoster, as Von Baerensprung has recently done for herpes in general, so far as it does not depend upon fungus formation."

The digestive organs on the whole suffered but little.

The duration of the 42 cases varied between 12 hours and 30 weeks.

52·3 per cent. of the cases terminated fatally; 4 of the 22 deaths being due to chronic hydrocephalus. Three cases ended in imperfect recovery, viz., 1 in chronic hydrocephalus, 1 in complete deafness, and 1 in hardness of hearing; the other 17 recovered completely.

Among the post-mortem appearances, almost all the cases presented exudation accumulated beneath the cerebral arachnoid; in eleven it was purely purulent or gelatino-purulent, in five, it was only slightly turbid serous fluid; in two, purulent and turbid gelatinous exudation alternated in different places. In one case the dilatation of the central canal of the spinal cord, and the filling of the same with pure pus was very remarkable. The authors never observed any injurious effect to attend the even frequent employment of morphia; on the contrary, its effect was so remarkably palliative that it appeared to them to be, with the application of cold to the head and spine, the most indispensable agent in the treatment of meningitis.

Dr. Klebs, of Berlin, records twenty-six post mortem examinations of acute primary meningitis, seven of the patients having been military, and nineteen civilians. In the first two cases of the latter the brain alone was examined, in all the rest the spinal cord was also examined. The pia mater is the exclusive seat, according to Klebs, of purulent meningitis, he corroborates Mayne's researches in Ireland in 1847 as to the general exemption of the arachnoid from pus; he has constantly found the greatest accumulations of pus in the cerebral membranes at the base of the brain and in the sub-arachnoïd spaces situated between the infundibulum and the pons, along each side of the latter to the under surface of the cerebrum, and with very variable intensity into the fossæ Sylvii.

The vertex, over the fissures, around the larger venous trunks, and thence more or less on the lateral surface of the cerebrum, are frequently the seats of purulent collections. The microscope, however, frequently reveals more or less extensive cell formation, where the eye can only detect opacity or dulness of the pia mater; so that in all cases the affection is much more widely spread than one would be inclined to assume from the distribution of the purulent collections.

The anterior surface of the spinal cord often exhibits only microscopically perceptible changes; while even considerable deposits of pus may occupy the dorsal surface of the pia mater. The greatest deposits of pus are found in the lower cervical portion and in the lumbar part of the cord, being so placed as Klebs considers by gravitation, so far as obstacles in the structure of the parts permit. To the same influences is due, as it appears to us, the position of the purulent deposits so often found at the base of the brain and at the occiput along the sinuses, &c.; other agents affecting the site, in which pus is found, are the different mobility of the several parts of the column, and the so-called spontaneous motion of the corpuscles, demonstrated by Von Recklinghausen. In the spinal cord the dura mater is rather more often affected, being also found adherent to the pia mater on the dorsal surface in the lower cervical, and upper dorsal portions of the canal.

Dr. Fischer has lately striven to show that the composition of arachnoïd pus approaches, that of the secretion in catarrhal processes, in the great amount of mucus present.

Purulent substances are rare in the ventricles of the brain. In the great majority of cases there is a slight increase of the fluid of the ventricles, which in that case is usually slightly turbid, but without containing pus corpuscles. Sometimes we meet also with fine fibrinous coagula, deposited especially upon the lateral plexuses, but likewise without admixture of pus corpuscles.[1]

Changes of a double nature occur in the cerebral and spinal medullary substance in meningitis, namely, extensive softenings and purulent encephalitis.

Œdema and suppuration co-existing, especially if the fluid contents of the medullary sheath are increased, loosen the connection of the nervous elements by the consequent distention, especially in those directions in which the most numerous blood-vessels run. Thus, the extensive motor disturbances, which are observed with slight amount of pus in the arachnoïdeal spaces are explained; while on the other hand cases occur, in which immense purulent accumulations are met with, without very considerable motor or sensory lesions having existed. Œdema is, however, regularly present in a high degree in those cases where the greatest purulent masses lie in the arachnoideal spaces.

"But that the contact of the pus does not, as might be supposed, produce a maceration of the adjoining parts, is evident from the fact that at the base of the brain, where precisely the greatest purulent masses usually lie, such softenings do not occur.

"Moreover, at least in recent cases, the pus mixed with a large quantity of mucus is not a substance specially adapted to produce such maceration.

"I have left it for many days in contact with cerebral substance, without the latter being more softened or broken up, than in the

[1] Dr. Klebs "reserves the expression pia mater for the entire membrane investing the whole cerebral and spinal medulla, within which the meshy arachnoïd is very sharply distinguished from the sheath (tunica propria), composed of denser and finer fibres, which, as well as on the testicles and kidneys, presents a superficial expansion of the interstitial connective tissue."

other parts which were not in contact with pus. The case is of course different with the ventricular surfaces, which have long been exposed to the action of the fluid dropsical contents of the ventricles. These are reduced by softening to the well-known white pappy substance."

A second kind of softening occurs not unfrequently, around small extravasations of not movable blood-points, in the white substance of the brain; this softening usually forms only a slender softened zone around the blood-point which was not, in the cases examined by Dr. Klebs, of embolic origin.

If diffuse encephalitis be looked upon as embolic, this change must be reckoned rather among the complications of the process giving rise to the epidemic meningitis.

Forget, of Strasbourg, alludes to implication of the heart; articular affections are more common. Dr. Klebs, however, only found moderate increase of thickness in the synovia in a case where the knee-joint was said to have been extremely painful.

On the other hand, Corbin gives four cases of purulent affection, von Jacquenin gives two, and Ziemssen and Zenker another similar case.

Klebs regards Boudin's views as to the likeness between epidemic meningitis and puerperal fever in the tendency to form pus, as drawn by Boudin, as exact only in reference to the septicæmic precursors in the puerperal process.

Pathological changes of the other organs.—External Skin.—Petechiæ or roscola-like exanthem. In many cases which ran a rapidly fatal course I found small bluish spots, scarcely as large as the head of a pin, appearing as extravasations of blood in the upper layer of the corium, and mostly in the vicinity of the duct of sudoriparous gland.

As to the blood, the French have found it rich in fibrin, and have consequently referred the disease to the phlegmasiæ.

Forget has found a thick crust on blood taken from the arm, and from his experience advocates bleeding. Maillot[1] likewise found increase of the fibrin, especially in the further course of the disease, but has also perseveringly bled, though he makes the remark that the first bleedings often did not improve the pulse, and that the patients who recovered had lost less blood than those who died.

The author remarks that when we consider how rapidly, even during venesection, the quantity of fibrin increases, all the statements, such as the above, that have been made possess but little value.

In the dead body we find the blood in very different states;

[1] 'Gaz. de Paris,' 1848. 'Hôp. Milit. d'Instruction de Lille.'

but in the great majority of cases that proved rapidly fatal, it has been found fluid, or containing only a few soft fibrinous coagula. The colour of the blood in the vessels was very dark; both conditions met with, also, in other so-called infectious diseases, especially in typhus and spotted fever.

The large abdominal glands and muscular system Dr. Klebs states show highly important pathological changes. The *spleen* was generally rather small, very flabby, the pulp dark-greyish red, the follicles in one case small and few, in another numerous and enlarged; only in some very recent cases was there a considerable recent splenic tumour.

The changes of the kidneys and liver are much more uniform. These organs are seldom enlarged; and when they are so, the enlargement appears to be of older date. The kidneys present a very characteristic condition: more flaccid than usual, capsule easily separable, surface smooth, of a grey or greyish-red colour. On section, the medullary portion is usually found very full of blood, and the cortical portion presents an alternating sanguineous and turbid greyish-red condition.

In many, even very recent cases, the cortex is pale, greyish, yellow and opaque. The convoluted tubes contain often a very large quantity of fine fat granules, rendering them dark to transmitted, and white and shining to reflected, light. The same change exists in the thicker variety of loops, while the straight canals and finer loops remain free or often contain fibrinous cylinders and fibrinous scales.

In cases of longer standing there is, also, usually well-marked papillary catarrh. The more minute changes in the liver are analogous; here, also, affecting the secretory elements, which consist in granular, albuminous, or fatty turbidity.

The macroscopical changes in the liver are like those seen in abdominal typhus:—The liver is scarcely, if at all enlarged, very flaccid, its section being always of a dirty greyish-yellow, or greyish-brown colour, dry as if "boiled." These changes, from which that of the kidneys is distinguished by an alternating, usually moderate amount of albumen in the urine, is attended with an extremely extensive and early affection of the voluntary muscles, an affection the results of which are seen most plainly in convalescence.

In recent cases we find granular depositions in the muscular fibres distinguished from those in other toxæmic diseases—such as typhus and phosphorus poisoning—by the greater fineness of the fatty molecules, the fibres appearing to be sprinkled or filled with an extremely fine dust. The change of colour is, in this case, also less characteristic. Thus, while in the greater degrees of phosphorus poisoning the muscles acquire a dirty greyish-red pale appearance, in CO poisoning a cherry red, in abdominal typhus a dry dark violet-red aspect, in meningitis epidemica they are mostly rather dry, flaccid, brownish-red. With this change is combined enormous emaciation of the muscular tissue.

Similar changes are found in the heart, but on the whole slight fatty infiltration; the heart is usually flabby, its substance is of a rather greyish-red colour.

The lungs are generally not much altered; in more recent cases they are usually highly hyperæmic. The lymphatic system is not much affected, its glands are usually somewhat reddened, but are otherwise normal; the swelling of the solitary glands of the intestine is too slight and too inconstant to possess a special importance.

The lesions of the organs of sight and vision stated to occur in this disease appear to be rather sequelæ developed only in protracted cases without fatal result, and they therefore do not often come under the cognizance of the pathological anatomist.

The Position of the Epidemic Meningitis in the Pathological System.—The earlier writers, who had an opportunity of observing the disease in the commencement of the present century, brought it into close relationship with typhus ("tifo tetanico-apoplettico" of the Italians, Hildebrand's cerebral typhus). This is owing to the undue extension of the terms "typhus fever," "status typhosus," &c. Griesinger speaks most decidedly against the identification of the cerebral typhus of earlier writers with epidemic meningitis. The author looks upon the phenomena of the latter disease as "passive lesions of nutrition." In conclusion he gives a brief résumé of the cases of meningitis noted in the records of the Pathological Institute since the beginning of 1861. This exhibits a great increase of the disease in the years 1864 and 1865. The following table presents a clear view of the course of the two epidemics in these years. The cases are those of acute primary meningitis, proved by dissection to have been such. In the month of—

	1864.							1865.						
	Feb.	March.	April.	May.	June.	Aug.	Dec.	Jan.	March.	April.	May.	June.	July.	*Total.*
Military .	1	2	2	1	1	...	...	...	...	...	...	...	...	7
Civil . .	(1)	(1)	1	2	...	1	1	3	3	3	1	(1)	1	19

It will be seen that the epidemic began in the coldest season of the year, and extended with diminishing intensity into the summer.

"The first case in the year 1864 from the civil population died on the same day, as the first from the military did, on 13th

February; from that time the disease lasted in both divisions of the population until May and June respectively. With the year 1865 an epidemic begins affecting, as it seems, exclusively the civil population. It would be very desirable if the results thus obtained were verified by the publication of as many cases as possible observed in this place. Nevertheless, this result is probably essentially reliable, that the disease has confined itself to the first five or six months of the year. This agrees with Boudin's statements, who in a report including 172 cases, reckons 109 in the first half of the year and sixty-three in the second. The maximum fall in the months of January (26), February (29), and December (20), and the minimum in August (3). It cannot be denied, therefore, that the coldest seasons of the year are specially disposed to produce the disease. Two elements may come under consideration with respect to this point: the local effect of cold and the condensation of a portion of the population in confined spaces. Comparison with the years 1861 to 1863 shows that in those years, too, isolated cases of quite analogous nature occur; thus, in 1861, cases 1 and 3; in 1862, case 3; in 1863, cases 1 and 2 (?) may belong to this disease. It is very remarkable that in these sporadic cases also distinct indications are met with, in the reports of dissections, of the presence of parenchymatous organic diseases, and this circumstance may still further confirm the identity of the cases in question with the epidemic affections of the last two years."

(*To be continued.*)

Review VII.

1. *Diseases of Children: a Clinical Treatise based on Lectures delivered at the Hospital for Sick Children, London.* By Thomas Hillier, M.D., &c. London, 1868. Pp. 402.
2. *A Practical Treatise on the Diseases of Children.* By D. F. Condie, M.D. Philadelphia, 1868. Pp. 783.
3. *The Surgical Treatment of the Diseases of Infancy and Childhood.* By T. Holmes, M.A., Surgeon to the Hospital for Sick Children, &c. London, 1868. Pp. 648.
4. *Leçons Cliniques sur les Maladies Chirurgicales des Enfans.* Professées par M. J. Giraldès, Chirurgien de l'Hôpital des Enfans malades, &c. 1er—3ème fascicules. Paris, 1868.

Clinical Lectures on the Surgical Diseases of Children. By Dr. J. Giraldès, Surgeon to the Hospital for Sick Children. 1st—3rd parts. Paris, 1868.

Of the four works whose titles we have just enumerated two deal with the medicine, and two with the surgery, of early life. They represent almost entirely the state of science with regard to the most important of all our specialities, and we should be wanting, therefore, in our duty to our readers if we did not give some account of them.

Dr. Hillier's book is so good in one way as to be disappointing in another. By modifying lectures delivered at the Hospital for Sick Children, he has produced what he rightly describes as "a series of short monographs," which testify to so much careful observation and study as to give us serious grounds for complaining that he has not published the results of his experience with regard to many other infantile diseases of importance. Thus, gastric, intestinal, hepatic, and renal affections are not noticed, scrofula is only incidentally referred to, and syphilis dismissed in a dozen lines. We trust that Dr. Hillier has only postponed these subjects for a second edition of this valuable work, in which case room might be, perhaps, made for them by curtailing the articles on pleurisy, pneumonia, and scarlatina, which, although not too long for the importance of those diseases, are out of proportion to the size of the work. Where all is instruction, it is difficult to select anything for special praise; but, to our minds, the articles on tuberculosis, pyæmia and otorrhœa, and acute and chronic hydrocephalus, are the most interesting. In his account of diphtheria he argues strongly—against those who (as Sir Wm. Jenner and Mr. Squire) would distinguish between croup and diphtheria. One very commendable feature of the volume is the formulary for prescriptions; another is the large number of illustrative cases, of which many are very valuable.

Dr. Condie's book is, in many respects, the most striking contrast to Dr. Hillier's. The English author avoids, even more than most clinical lecturers, quotations from other authors, and seems to rely almost exclusively on his own experience, while Dr. Condie is profuse in his references to others, and keeps his own judgment and experience too much in the background. Were his subject philosophy, and not medicine, we should say that he is a syncretist, and not an eclectic—that is to say, he collects and registers various, and often conflicting, opinions, without any effort to conciliate or systematise them. This tendency is most notable in those points where the progress of science has led to the abandoning of opinions which were formerly held. Thus, we believe that the accounts of lung-collapse and gastro-malacia would be unintelligible to any one who has not studied those questions, simply because contradictory statements, introduced into successive editions of the work, have been allowed to remain in juxtaposition without sufficient explanation. The articles on diphtheria and croup, again, are very confused, from the same defect. The description of "remittent fever" as a disease of the digestive organs, and the omission of all reference to the true nature of the epiphytic skin diseases, are also, no doubt, due to the incomplete preparation of the later editions of this work, which is the more unfortunate because it contains a great amount of useful matter. The whole of the first part (containing the general pathology, semeiology, &c., of children's diseases, is remarkably superior to the rest of the work; the description of infantile cholera, which is much more frequent in the United States than in Europe,[1] is interesting, both as to pathology and treatment. The disease is attributed to the action of hot, moist, and impure air, primarily upon the skin, and secondarily upon the mucous surface of the alimentary canal, which is already predisposed to disease by dentition; the chief means of treatment is, therefore, to supply abundance of pure and cool air.

Epidemic cerebro-spinal meningitis has, in some American epidemics, been observed almost exclusively in children under fifteen years of age. Many American physicians still seem to rely upon the action of mercury in this disease, while others have found decided benefit from the administration of large doses of quinine.

The reader will meet with some expressions here and there in this book which will remind him that the author is not an Englishman, of which, perhaps, "illy," for the adverbial use of "ill," is the most curious; but the style is, on the whole, more flowing and easy than that of most American books we have read.

It is a pleasure to us to turn to a work of which we can speak in terms of unqualified praise. Mr. Holmes's 'Surgical Treatment of the Diseases of Infancy and Childhood' is a book quite *hors ligne*, and one which the student and the experienced practitioner may alike study with pleasure and profit. It combines all the good qualities which should be found in such a work, being the fruit of considerable book-study, as well as of an extensive practice, clearly and elegantly stated; and (what is no little matter) with everything done, in the way of excellent plates and engravings, and judicious typographical arrangements, to facilitate its perusal.

Our best course will be to give an analysis of the more important parts of the work, as it may be taken to represent the present state of infantile surgery.

It is divided into two parts, the first treating of malforma-

[1] Dr. Condie is wrong in supposing it to be unknown here.

tions, the second of injuries and surgical diseases. Some of the malformations (hermaphroditism, joined twins, tracheal fistula, &c.) are too rare to have any practical interest, although even the description of these contains cases of importance which have occurred in the author's own experience; but others are among the most important surgical affections of childhood.

We are glad to see that Mr. Holmes gives prominence to the fact that all dangerous operative procedures for nævus should be avoided, since the disease is, when left unchecked, seldom or never fatal. He condemns excision by the knife in all dangerous positions, and thinks it should always be confined to hospital practice. He recommends the use of setons (which he generally dips in solution of perchloride of iron) for large nævi, subcutaneous ligature for smaller ones, and repeated applications of strong nitric acid to those which are merely superficial. Ligature of the carotid, or even of the vessels at the base of the tumour, is condemned as dangerous, and uncertain in its results; vaccination is only available in such cases as may be much better treated by nitric acid; and injection of the perchloride of iron, though it has the advantages of being very effectual, and leaving hardly any visible trace of its action, has in several instances proved fatal, by coagulating the blood in a large vein. If employed at all, only two or three drops should be injected into different parts of the tumour at each sitting. The purely palliative treatment, by pressure or cold, is tedious and troublesome, and offers no security against a return of the disease.

In harelip Mr. Holmes always uses pins, and not the suture. He leaves the pins in for only forty-eight hours, so that no mark of them remains, and he has never yet had a case in which the wound gave way after their removal.

The operations of staphyloraphy and uranoplasty are recommended to be performed, in otherwise healthy children, before they have acquired the habit of speaking in the peculiar way characteristic of cleft palate, say at about three years of age. It would formerly have been very difficult to operate on such young patients, but the use of the gag invented by Mr. T. Smith, which holds the mouth open to the widest extent, has made it much easier. The operation will, however, always be a troublesome and tedious one (Billroth estimates its duration at about three quarters of an hour), and union is, perhaps on the whole, less likely to occur in infancy than later in life.

In case of imperforate rectum we are advised to wait for a day, if the symptoms are not urgent, so as to allow the lower part of the bowels to become distended. If there be then no positive evidence of the presence of the rectum, it is probably better only to make a puncture with a grooved needle or exploring trocar, and, if no meconium is found, to proceed at once to colotomy. If any further search in the perinæum is to be made it should be a careful dissection, aided by the presence of a staff, kept strictly in the middle line, in either the bladder or vagina, according to the sex. If the colon has to be opened, Littre's operation should be preferred to Amussat's; and this because in infants the colon usually has a long mesocolon (so that it could not be opened from behind without wounding the peritoneum), and presents at once in the wound in the groin. M. Rochard is quoted to prove that colotomy has been successful in ten cases, and in three of these at least the artificial anus did not prevent the patient from leading an active and easy life. In those cases where an imperforate rectum opens into the vagina there is little danger to life; but when the natural passage is restored the vaginal fistula does not, as a matter of course, close. Recto-vesical fistula, in the male infant, is more serious, as the urethra is choked by the accumulation of solid fæces; such cases require, therefore, earlier operation than the last variety; generally, if the intestine opens into the urethra, it will be accessible from the perinæum, but if into the bladder, colotomy will be necessary.

The general remarks on operations in childhood, which are prefixed to the account of the surgical injuries and diseases of infancy, will be read with great interest. Mr. Holmes is, on the whole, of opinion that operations are less dangerous in children than in adults, inferring this, partly from some statistics (which he published in vol. i of the 'St. George's Hospital Reports'), partly from the *à priori* considerations that the healthy state of the viscera, and the ease with which long confinement or pain can be endured, more than counterbalance the more transitory effects of violent pain or hæmorrhage, which are more serious in the infant than in the adult.

A very minute account is given of the operations for opening the windpipe, which are so frequently necessary in children, and several points of detail brought into notice, which, although we have not space to mention them here, should be read by those who are likely to have to perform them. Mr. Holmes has established, we think, that Mr. Marsh's objections to the performance of laryngotomy (in vol. iii of the 'St. Bartholomew's Hospital Reports') are not valid, and that in very early life it would be extremely unwise ever to operate below the thyroïd isthmus; even in older children tracheotomy should only be performed when a larger opening (for the extraction of foreign bodies) is required than can be made above. In case there is reason to suppose the existence of some polypus or other tumour in the larynx, he recommends that, after laryngotomy, the thyroid cartilage should be divided through its whole length, so as to expose the interior of the organ, care being taken to avoid the vocal cords. Although this does not appear to be so dangerous an operation as one might have supposed, we cannot say that the results, as he gives them, of his cases are sufficiently encouraging to lead to its general adoption.

We hope that Mr. Holmes is mistaken in fearing that, because he dissents from the ordinarily accepted views of the nature of "struma," he will be regarded as ill-informed on the subject. We quite agree with him that the way in which low inflammations and diseases of very different kinds are "lumped together" betokens an unscientific habit of mind, and leads to errors in prognosis and treatment. He follows Sir William Jenner in discarding altogether the word "struma," and in distinguishing sharply between tuberculosis and scrofula, two diathetic states which, so far from requiring the common term struma for connecting them, need to be kept carefully apart. But he also brings into prominence a point which has been too much neglected, viz. that many of the cases known as "scrofulous" are only examples of local disease which, by long continuance, have produced general disorder. These are constantly confused with the totally opposite state in which local disease is produced by a pre-existing diathesis, and a careless diagnosis (for which the words "scrofulous" and "strumous" are an excuse) leads to serious mistakes in treatment.

We cannot do more than refer briefly to the excellent article on rickets. Mr. Holmes tells us that the experience of the Children's Hospital is that phosphate or superphosphate of lime is of no real use in this disease; with regard to its surgical treatment, he is decidedly in favour of applying splints to prevent deformity. With all deference to his authority, we should be inclined to add Dr. Hillier's qualification—that the splints will only be of use when the general disease has been cured, and merely the local softness of bone remains.

The account of periostitis will be read with particular interest, as the disease has only been accurately known within the last ten years, and this is probably the best description of it in the English language. In cases of this kind, where the disease has gone so far as to separate the bone from its surroundings, and periosteal abscess has resulted, the author strongly advocates subperiosteal resection of the diseased bone, even before new bone has formed. He claims two advantages for the operation over the expectant plan of treatment—the one, that it removes a constant cause of dangerous irritation; the other, that the operation is more easily performed, and is more rapidly recovered from, before than after the formation of a large sequestrum, and it is fair to admit that his detailed cases bear out his statement. The principal objection is the great probability of shortening of the limb.

On the other hand, Mr. Holmes is decidedly in favour of not removing bone which caries has destroyed, but of trying perfect local rest, with or without the application of a strong liniment of mineral acid, as recommended by Mr. Pollock.

The chapter on joint diseases is remarkably practical; the author rightly attaches little importance to the distinctions usually established by systematic writers between affections of the bones, cartilages, tendons, ligaments, and synovial membranes, but confines himself particularly to questions of prognosis and treatment. Abscess, the almost invariable result of joint disease in children, should generally be freely opened, and, supposing any further operative measures to be required, the smaller joints may be excised; but if the knee, hip, or shoulder be affected, amputation will generally be required.

Mr. Holmes applies to the consideration of chronic joint diseases the general principles which he lays down under the head of struma. He is of opinion that the majority of what are called "strumous joints" are merely instances in which long-continued local disease has produced constitutional mischief, basing his opinion on the ordinary course of such cases, which, in their origin, progress, tendency to spontaneous cure, and usually complete recovery after excision or amputation, resemble local rather than diathetic diseases. The pathological appearances, too, are rather those of simple chronic inflammation than of any peculiar morbid action.

The whole question is analogous to that raised by some modern German and French physicians—whether pulmonary phthisis is, in many instances, not a simple chronic pneumonia; but it is more easily soluble, and much more immediately practical, for, if chronic joint disease is essentially constitutional, operations ought only to be performed when urgently called for; while if it is local, it may generally be advisable to perform them, if only for the sake of preventing the local disease affecting the general health.

Of all the operations for diseased joints, Mr. Holmes is best entitled to speak with authority about excision of the hip, which he has performed more frequently than any other surgeon. He has preserved notes of nineteen cases, in all of which he followed the rule of not operating as long as there was any reasonable chance of natural recovery. To abridge his statistics, we may say that three of the nineteen made complete recoveries, three have useful limbs, but with sinuses, one case was doubtful, and two were very little benefited. Seven died from the direct results of the operation, one of whom was in a dying state when it was performed, and five of pyæmia, which seems then to have been prevalent at the Children's Hospital. The remaining three cases died some time after the observation; one from independent disease, the other two from long-continued suppuration.

These, as Mr. Holmes very fairly allows, are not satisfactory results; but, considering the ordinarily long duration of the disease, and the hopelessness of a natural cure in poor children once the bones have become carious, they imply that the operation will often be advisable merely to save life, while the limb is far more mobile after a successful excision than after spontaneous cure.

With regard to the vexed question of excision of the knee, our author is of opinion that it is a more severe operation than amputation, being both more immediately dangerous to life and requiring a longer time for convalescence. It is, therefore, only to be recommended in the most favorable cases, in which it has over amputation the great advantages of leaving a useful limb instead of a stump.

We have dwelt so long on this subject that we have no space to give an account of the important suggestions made by our author for the various operations in disease of the tarsal joints, but we regret this the less that we hope what we have already said will induce every practical surgeon to study the book for himself. We would especially call attention to the chapters on infantile prolapsus, hernia, and stone. The highest praise we can give them is that they are fully equal in merit to the rest of the volume.

Mr. Holmes is unable to find room in his volume for diseases of the eye; if any special account of these is desired, probably none can be found better than that given by M. Giraldès, in the lectures which stand last upon our list. As these are still unfinished, it would be unfair to say more in the way of criticism than that they are, perhaps, *too* clinical. Being the exact reproduction of the lecturer's words, the continuity necessary in a book is constantly broken by digressions to cases of a totally different kind, under treatment at the same time. Besides diseases of the eye, hydrocephalus, harelip, hydatid cysts of the liver, ovarian and myeloïd tumours, are the principal subjects treated of, and all of them in a way that is thoroughly practical and interesting. When finished, these lectures will probably form a complete course of infantile surgery, worthy of the successor of Guersant, and of the unrivalled means of observation at M. Giraldès' disposal.

Review VIII.

1. *A Manual of the Principles of Surgery, based on Pathology, for Students.* By William Canniff, Licentiate of the Medical Board of Upper Canada; M.D. of the University of New York; M.R.C.S. England, &c. Philadelphia, 1866. Pp. 402.

2. *Conservative Surgery, as exhibited in remedying some of the Mechanical Causes that operate injuriously both in Health and Disease.* By Henry G. Davis, M.D., Member of the American Medical Association, &c. New York, 1867. Pp. 315.

3. *A Practical Treatise on Surgical Apparatus, Appliances, and Elementary Operations.* By Philip S. Wales, M.D., Surgeon U.S.N. With 642 Illustrations. Philadelphia, 1867. Pp. 685.

4. *Plastics: a new Classification and a Brief Exposition of Plastic Surgery.* By David Prince, M.D. Philadelphia, 1868. Pp. 93.

The books which we have grouped together in this article are all of American origin, and they well illustrate the eminently practical character of the American mind. They deal with a variety of subjects in surgery, as it is at present practised, and supply us with some valuable suggestions; but they hardly touch upon any questions of theory. Our American brethren have singular advantages for the pursuit of medical and surgical inquiries. They have a country so vast that it presents the most different climatic and hygienic conditions; they are brought in contact with a variety of races; and the recent war has supplied them with an amount of experience and with a mass of statistics which can hardly fail to prove of service to science. And it must be admitted that the American surgeons have made a good use of their opportunities, for they have given us some valuable additions to the practice of surgery, and have helped to advance this department of knowledge in a notable degree.

In reading these books one can hardly fail to be struck—and not a little flattered—by the frequent references which are made to our professional literature, and the weight and authority which are allowed to the opinions of English surgeons.

Thus, if we turn to the first, 'The Principles of Surgery, based on Pathology,' we must admit that the author is certainly very candid, for in his preface he gives a list of German and English works which he has "freely consulted;" and as we read through his pages we see that he has not over stated the use that he has made of them. We find scattered throughout his whole work long quotations from Paget's 'Lectures on Surgical Pathology,' Virchow's 'Cellular Pathology,' and Holmes's 'System of Surgery,' besides many shorter passages extracted from other authors. Moreover, the whole of his illustrations are taken from Mr. Paget's Lectures; so that we are thus furnished with a very good skeleton for a work upon 'Surgery, based on Pathology.' It needs only that the extracts from standard authors should be carefully selected, and that the woodcuts should be used with judgment, in order to make a trustworthy 'Manual of the Principles of Surgery.' And this much praise we may certainly accord to our author. If there is nothing original in his work, and if his quotations appear to us to be taken with rather too free a hand from the writings of others, at any rate he deserves the credit of having pieced them together so as to form a useful and readable book.

The writer is a Canadian, and, as he tells us in his preface, his grandfathers and his father were pioneers in the wilderness of Canada, so that he himself did not enjoy the advantages of early literary training. It is only fair that we should bear this in mind, for the volume before us certainly reflects credit upon the energy and industry of the author. He appears to have received his medical education partly in this country and partly in the United States; and he has evidently made a good use of the extensive opportunities for observation which he has had both in Europe and America. He has served as a military surgeon with British troops and with the United States army; and perhaps the most interesting part of his book—the only part indeed which has any novelty about it—consists of his experiences during the recent civil war in America. The chapters upon gunshot wounds are those that we have read with the greatest pleasure, because they are almost the only ones which have an air of freshness. In most of the others we are sensible that we are perusing what we have read elsewhere, and the occurrence of the well-known illustrations from Mr. Paget's Lectures helps to confirm the fact.

On the question, How far an alien race can flourish in a country to which they do not originally belong, our author makes the following remarks. They are intended to supply an answer to the theory that colonising races will inevitably die out, unless they are constantly recruited by fresh emigrants from the mother country. Upon this subject Mr. Canniff's opinion may be allowed to have weight, for he has himself been brought up among the descendants of English and French emigrants in Canada, and he has subsequently had opportunities of comparing the colonists with the European races from which they have sprung.

"The French of Lower Canada, even under many adverse circumstances, have fully retained their ancient bodily vigour, and can compare favorably with the present inhabitants of old France, while their number has increased; yet their ancestors, many of them, emigrated to New France 200 years ago; and, since the colony became a part of Britain, no 'replenishment' has been received from the old stock. Turning to Upper Canada, we find a fact no less important, and quite as antagonistic to the theory that native Americans are doomed to die out. In consequence of the American revolutionary war, some 25,000 or 30,000 united empire loyalists were forced or induced to seek a home in the Canadian wilderness. Many of these were descendants of those who had first peopled New Holland along the banks of the Hudson. A large number of these limited empire loyalists settled along the St. Lawrence and the Bay of Quinte. In the main, indeed, almost altogether, until very recently, these old settlers have intermarried. Now and then an emigrant might settle among them, but it was an exception. The great grandchildren of those American pioneers now live on the old homestead, and are found scattered over the whole province; and although I have no positive data upon which to base my assertion; yet, from careful observation, I have no hesitation in declaring that in physical development, in slight mortality among the children, in length of life, in powers of endurance, not to say in bravery and patriotism, they cannot be excelled by any class of emigrants."—P. 141.

The next extract that we shall offer to our readers is intended to show the value of *veratrum viride* as a vascular sedative and antiphlogistic. The American evidence in favour of this drug seems to indicate that it deserves a more extensive trial than it has yet had in this country. The tendency of medical practice of late years has been so much opposed to anything like active antiphlogistic treatment, that we have perhaps failed to give the *veratrum viride* a fair trial. However this may be, here is our author's testimony in its favour, and very strong testimony it is.

"*Veratrum viride* is a medicine which does not enjoy the long established reputation which belongs to antimony; but it successfully rivals that drug as a powerful antiphlogistic. . . . . . I have found it particularly useful in inflammation of the lungs. Two cases of such, which were also most interesting in a surgical point of view, I will particularly refer to. These were cases under my immediate care in Lincoln Hospital, at Washington. Both came in at the same time, direct from the battle-field. The first one which engaged my attention was unable to lie down, but had to sit leaning forward considerably. His efforts to breathe were most urgent. He could barely tell me that he was wounded in the chest—that the ball had passed through his body. I hastily looked at the wound in front, which was the place of entrance; it was between the fifth and sixth ribs, on right side of and very near to the sternum. I not only considered his case hopeless, but thought he could not live an hour. I ordered him a draught of compound spirits of ether, which was shortly repeated. In half an hour he went to sleep, his head resting forward on a chair, made somewhat comfortable for the head. He slept for two hours or more. During the sleep and on waking he could breathe a little more easily. I examined the wound posteriorly, and found that the ball had passed almost directly backwards, and had made its escape. It was now some thirty hours since the reception of the wound. The action of the heart was laboured, and there was some fever; the skin was hot and dry. I put him on veratrum viride, to be given often, in small drop doses. I had every reason to believe, from the symptoms, which I cannot here fully describe, that the ball had passed through the lung. It was several days before the patient could lie down. Symptoms of pneumonia presented themselves, but were kept under control, and finally the patient became convalescent.

"The other patient was also wounded with a ball, which had struck the right clavicle at about its middle, and, shattering the bone, had buried itself in the part. In this case, also, there were well-marked signs of the lung being wounded. At first the distress was not great, the dyspnœa but little; in two days, however, it was very great. He was ordered the veratrum viride every three hours. I found it unnecessary to give anything else. In a few days the symptoms of inflammation subsided. Because of the important structures immediately beneath the clavicle, the ball was but slightly searched for. When the patient passed from under my care, his condition was most promising. The wound remained open, from which was a little purulent discharge, which might have been due to the necrosed state of a part of the clavicle. Now, both of these men were young and full of healthy blood; but neither general nor local bleeding was deemed necessary. The inflammation was successfully treated solely by the veratrum viride."—P. 87.

We have already said that some of the best chapters in the volume before us are those which relate to gunshot wounds, and that some of the most interesting passages are Mr. Canniff's experiences during the late conflict in the United States. In speaking of the strange course which a bullet will sometimes take, he says—

"One case which I saw after the battle of Chancellorsville will serve to illustrate the extraordinary direction the ball may take. A private of one of the New Jersey regiments had been wounded in the right arm, while in the act of putting a cartridge into his musket.

The ball had completely cut off his forefinger, then passed directly through the body of the hand, and again entering the back of the arm, about two inches above the wrist, had ploughed a furrow for a few inches, and then entering into the deeper part of the arm, had finally made its exit and escape a little above the external condyle of the humerus."—P. 229.

Here is another case of a somewhat similar kind, which illustrates not only the curious course that a ball will sometimes take, but also the necessity of looking for it in the most unlikely places.

"Sergeant C— had been wounded in the leg, I think, while advancing at the double quick. The ball had entered the front of and a little below the knee. It was a fortnight afterwards when he came under my care. In the mean time the wound had been repeatedly probed in the efforts to find the ball, but without success. Believing from the character of the wound that the ball had taken a downward course, I proceeded to carefully examine the leg on every side. Coming to the ankle, and while pressing the finger inward beneath the tendo-Achillis, near its attachment, he complained of soreness, and at the same time I could distinctly feel a hard substance. The patient, assured that this must be the ball, submitted at once to the use of the knife, and a large Minié ball was extracted through the wound. It had not caused, so far, any inflammation. The ball was slightly battered from striking the bone. Of course the patient was delighted as well as surprised. Now, here is a case illustrative of the usefulness of trying, by external examination, to find a ball, when the probe fails to reach it through the wound. The ball was not only more than a foot from the place of entrance, but it was also at the opposite side of the limb."—P. 234.

In the chapter upon the diseases of the joints, our author relates a very interesting case in which he excised the ankle. This is an operation which is so seldom attended with a really satisfactory result, that one is glad to hear of an example in which, at the end of the year, the patient could run upstairs three steps at a stride. This certainly sounds a very successful case, and we congratulate Mr. Canniff upon it. The subject was well chosen to begin with, the experiment was cautiously and carefully made, and the result appears to have been most encouraging. It may be that an increased experience will show that the operation of excision of the ankle, like excision of the wrist, has been somewhat hastily condemned; and that in well selected and suitable cases the surgeon should consider whether he could not perform it with advantage. Many operations have suffered more from their friends than from their foes; and this is particularly true of the excisions. It is because the advocates of excision of the knee have pushed their practice to an extreme length, and have operated on suitable and unsuitable cases alike, that so many doubts have been entertained regarding the value of this mode of practice. But if the cases had been more carefully selected, we believe the verdict of the profession would long ago have been given in favour of the operation. And the same may be said of excision of the wrist. Within the last few years cases have been brought forward showing the value of the operation, and it now bids fair to take its place among the established proceedings of surgery. Perhaps the same thing may be found to be the case with the operation here discussed. Excision of the ankle, after having been hastily tried and hastily laid aside, may at length assert its proper value and come to be recognised as an important means of dealing with those diseases of the ankle which are so common in this country.

There are some idioms and expressions in this volume which fall rather harshly upon an English ear; but we are not disposed to criticise them severely, because there can be no doubt that our language has received useful additions from the other side of the Atlantic, and it may be that words, which displease us when they are first heard, may hereafter be found well suited to the requirements of this busy age in which we live.

The book appears to have been passed through the press without the revision of the author, and in several places it bears evident marks of a want of finish. It seems to have been hastily written and hastily printed, to meet a supposed want, which might have been better supplied by one of the existing text-books, either American or English. Mr. Canniff would have done well to have waited until he had a larger experience of his own to draw upon, and until he had leisure to give his work the careful revision which it ought to have had.

The book which stands second upon our list is of a different kind from that which we have just noticed. It is called 'Conservative Surgery,' but this title is somewhat misleading—at any rate to a person who is imbued with English ideas. In this country the term "conservative surgery" is generally applied to those partial operations, such as resections and excisions, whereby we are enabled to avoid the necessity of amputations. But the volume before us does not deal with "conservative surgery" in this sense. Indeed, it does not treat of any operations whatever. Truly conservative it undoubtedly is, and that in the best sense of the word; but it aims at preserving parts, not by limited operations, but by mechanical appliances which shall make operations unnecessary. If it were called "The Application of Mechanism to the Treatment of various forms of Disease," it would give the reader a clearer idea of its contents than the present title does.

It would appear that Dr. Davis was conversant with surgical mechanism before he began the study of the medical profession; and it is evident that he has approached the subject from a mechanician's point of view. It seems to us that he is inclined to lay too much stress upon mere mechanism, and to think that it is capable of accomplishing more than experience warrants us in believing.

The mechanical principle which he has applied to the treatment of various injuries and diseases, is that of *continued elastic extension.* This is his *hobby*—if we may be allowed the expression—and he claims to have been the first to introduce this method to the notice of the profession. It is a principle which is now very generally used in dealing with contracted joints and other cases, and if we owe it to Dr. Davis we have reason to be grateful to him for his suggestion. But we doubt very much whether it is capable of effecting all that he supposes. Thus, he applies the principle very ingeniously to transverse fractures of the patella, and then he says—

"This plan so effectually overcomes the contraction of the muscles, and leaves the patella so free to be kept in apposition, that we can but think the vast majority of fractures of this bone will unite by bone. It is certain they will not fail to do so from want of coaptation of parts."

We are afraid this opinion—which, by the way, is almost entirely unsupported by cases—is rather too sanguine, and that Dr. Davis's plan of treating these fractures, like many others that have been proposed for the same purpose, will fail in securing bony union.

In speaking of dislocations our author is still more confident of the good results which may be produced by the method he advocates.

"At the present time [he says] by means of the principle we have promulgated, that a ligament or other soft tissue can be elongated to any desirable extent by the use of *continued elastic extension*, we have it within our power to reduce almost any dislocation, without reference to the length of time it has been luxated; and it can be done, not only without pain or suffering, but without the slightest danger to life, and with almost a certainty of a perfect restoration of the joint to all its functions."

This may be so. It is not wholly impossible; but such a statement ought to be supported by well selected cases before the profession is asked to adopt it and act upon it.

In cases of congenital dislocation Dr. Davis has used the same method, and, as he assures us, with the most satisfactory results. He relates two cases, one of a girl aged six years, the other of a girl aged fourteen, in both of which he effected what he considered a perfect restoration of the parts to their natural situation and function; but we trust he will pardon us if we say that it requires more evidence than he adduces to convince us that it is desirable to attempt to reduce a congenital dislocation of the hip in a girl fourteen years of age, or that lost parts can be restored in the way that he supposes. Still we shall be glad if the publication of the volume before us should induce surgeons to give *continued elastic extension* a fuller trial, and to apply it to some cases for which it has hitherto been thought hardly suitable.

In dealing with cases of club-foot Dr. Davis applies his method of treatment in a manner similar to that which has been employed by Mr. Barwell in this country—that is to say, he supplies the place of the paralysed and atrophied muscles by Indian-rubber cords, which draw the foot into its proper position. Dr. Davis claims to have been the first to make use of this method, and he complains that in this point, as well as in others, his "discoveries" have not had justice done to them. It may be so; but he should have taken care to make his principles of treatment known when he first began to use them. There was no need to wait until he had tried them for thirty years. The gist of the present volume might have been given in a pamphlet. The accounts of the pathology and symptoms of various diseases, which he has introduced, and which he has drawn out to wearisome length, might have been omitted with advantage. This much might have been taken for granted, or very briefly recapitulated; and then the principles of *continued elastic extension* might have been explained and enforced by cases. But, instead of this, Dr. Davis has adopted a very different course. He has evidently been desirous of writing a *book;* and as his methods of treatment might have been explained in twenty pages, and as he appears to have but few cases to support them with, he has filled up the space with a tedious account of the symptoms and pathology of some of the diseases that he has treated—a subject with which it might be supposed that his readers were already sufficiently acquainted. Indeed, all through the book, as we read we are constantly desiring to have cases quoted in support of the opinions which are expressed. Strong statements are made—statements which it appears to us must be very much exaggerated—and yet they are left to stand alone. Thus in a long chapter in which the author discusses phthisis and its treatment by regulated movements calculated to expand the chest, not a single case is related. Dr. Davis is very sanguine of the benefit which will arise from his method. He says, "When the treatment of phthisis we here propose commences with the disease, and is pursued uninterruptedly, we have every reason to expect that every case will recover." And, again, "The views advanced in this chapter have been accumulating for thirty years, and the application of the principles has *never failed of producing results more or less favorable.*" These are strong statements; and yet Dr. Davis expects us to receive them without the evidence of a single detailed case.

This may serve as an example—and a notable example it is—of the faults that we have to find with the volume before us. That *continued elastic extension* and regulated muscular movements are excellent modes of treatment in some cases we do not doubt for a moment, and we are glad to give Dr. Davis the credit of having applied them to various parts of the body with considerable ingenuity. But he appears to us to have ridden his *hobby* to death; for his statements are so strong that he can hardly expect his professional brethren to receive them, except upon much better evidence than any he brings forward.

The third book on our list is a treatise on 'Minor Surgery and Bandaging,' wherein the chapters upon bandaging and surgical apparatus are drawn out to great length. Indeed it appears to us that the whole work might have been abbreviated with the utmost advantage. What is wanted in such a book is not a catalogue of all the bandages and splints that have ever been devised, but a judicious account of those appliances which are in most frequent use at the present day, and whose value has been proved by long experience. If Mr. Wales had kept this object steadily in view he would have produced a smaller, but a much more useful book. As it is, the volume before us contains a great deal that is obsolete, and would have been far better omitted. Moreover, our author would have done a greater service to the profession, and particularly to medical students, if he had adopted fewer old-fashioned terms and old-fashioned illustrations, and given an account of the bandages and splints that he is in the habit of using in his own practice. The tendency of the present day in medical and surgical practice is towards simplicity. Hence it happens that many appliances which suited the views of our forefathers have been allowed by us to fall into disuse. We are inclined to think them unnecessarily heating and irksome to the patient, and so we have laid them aside. Sometimes we find that a single turn of a roller, or a strip of adhesive plaster will take the place of a complicated bandage. Sometimes a simple light wooden splint may be made to serve the purpose of a heavy and cumbrous one. Our object in each case is to carry out the indications for treatment as simply as possible, and with the least amount of restraint or inconvenience to the patient. But this is not what Mr. Wales aims at teaching. He has collected together a number of bandages and splints, and these he has explained at length, notwithstanding that some of them are never used at the present day. And not only has our author drawn out his book to great length by introducing obsolete appliances, but he has adopted the obsolete names for them as well. Whoever speaks now of a "Bis-oculo-occipital triangle," or a "Compound dorso-bis-axillary cravat"? When Mr. Wales addresses himself to his patients or his assistants does he use such terms as these? Surely not; and yet his book is full of them, as any one may see by a glance at the table of contents. In works upon bandaging which were published thirty years ago—such, for example, as "Cutler's Practical Guide,"—this pedantic phraseology is employed. It may have been in common use among surgeons at that time, but it is a dead language now. No one uses it. No one understands it. Moreover, it is a barbarous and obscure language, which has no merit of its own to make one regret the loss of it. What, then, is the advantage of employing it? What good purpose can be served by perpetuating it? We can see none. It only makes a simple subject complicated, and repels men from the study of what is a very necessary branch of surgical practice. Some few old-fashioned terms are still current among us. Thus, some still speak of a *capeline* bandage and a *spica;* but even these terms are rapidly falling into disuse. We have observed with pleasure that the books which have recently been published upon this subject in this country are almost entirely free from such pedantry.

Not only has Mr. Wales adopted an antiquated phraseology, but he has also introduced a number of old-fashioned illustrations. These he appears to have culled from a variety of sources, and almost all of them we recognise as old friends. Very few appear to be new; and some he has adopted so carelessly that he has not even taken the trouble to explain the letters of reference upon them! Indeed the whole work has a strong air of *book-making* about it. It seems as if our author has been seized with the *cacoethes scribendi*, and had determined to write upon minor surgery and bandaging. With this view he has brought together, from different sources, as much material and as many woodcuts as possible, and these he has digested into the present volume. But we are afraid he has overshot the mark, and frustrated his own purpose. A book upon minor surgery ought to be concise and cheap, suited to the wants of students and junior practitioners; and the best advice we can give Mr. Wales is to simplify and shorten his work if he wishes to adapt it to the wants of the present day.

Mr. Prince's modest volume is a reprint of a "Report made to the Illinois State Medical Society, in 1867," and the author has done good service by publishing it in a separate form. Plastic surgery is a subject which has attracted a great deal of attention of late years, especially since the introduction of anæsthetics; and the variety of reparative operations is very numerous—indeed, one is hardly aware how numerous such proceedings are until one sees them all collected and brought together as they are in the volume before us. There is scarcely any part of the body which may not be the seat of a plastic operation—for example, to release the cicatrix following a burn—and the nature of the operation which has to be practised must vary of course with the position and extent of the injury. But there are some situations in which operations of this kind are much more important and much more interesting than others. Thus, plastic operations about the face for the restoration of the nose, the eyelids or the lips have occupied the attention of some of the greatest surgeons. Again, those painful and distressing cases of extroversion of the bladder, which are occasionally met with, have taxed the ingenuity of surgeons to the utmost: but now, if we may judge by reported cases, it seems as if we had it in our power to relieve them materially, and to make the condition of the unhappy sufferers much more tolerable. A full account of the operations which have been undertaken for the relief of these different malformations and injuries will be found in Mr. Prince's work, and any surgeon who has cases under his care which may require plastic operations, and who is debating what course he ought to pursue, and what method he ought to adopt, cannot do better than refer to this monograph. He will find under each head a variety of operations described and figured, and he will be able to make his choice among them.

Mr. Prince begins by giving a brief account of the various modes of arresting hæmorrhage and dressing wounds, and the influence of each upon cicatrization. This is a matter which has lately occupied a great deal of attention among surgeons, and has a direct bearing upon plastic operations, in which immediate union is of the utmost importance. This forms a very appropriate introduction to the subject of the report, and some of the most interesting passages it contains are those in which the author speaks of the experiments he has made with antiseptic lotions and the success that has attended the use of them.

Mr. Prince then takes the different plastic operations in detail and discusses them at some length, giving us the results of his own experience as well as the opinions of others. His special object has been to reduce the various kinds of plastic operations to a system, and to classify them, so that henceforth they may be described with greater accuracy than has hitherto been possible; and we will only add that if his classification should be found to meet the wants of surgeons, so as to come into general use, he will have done good service by giving precision and clearness to an important department of our art.

PART SECOND.

Bibliographical Record.

Art. I.—*A Manual of the Pathology and Treatment of Ulcers and Cutaneous Diseases of the Lower Limbs.* By John Kent Spender, Surgeon to the Mineral Water Hospital, and to the Eastern Dispensary, Bath. London, 1868. Pp. 89.

This short and simple treatise on a subject eminently suited to the position and opportunities of its author will well repay the time necessary to peruse it, and to master its details. The doctrines which Mr. Spender desires to teach are not very recondite, perhaps it may be said not very novel; but every day's experience will teach any one who chooses to open his eyes that they are very free from being generally acted upon. They are shortly summed up in the following extract from Mr. Spender's preface, which we give with the more pleasure as illustrating the literary honesty and the engaging candour and modesty with which the work is written.

"Filial piety and literary justice dictate that my earliest acknowledgments should be made to a work written and published by my father in 1835, entitled 'The Pathology and Treatment of Ulcers of the Leg.' This work is, in part, the basis of the book which I now offer to the profession; and the portions which I have specially made use of and endorsed are two in number.

"Firstly, the argument that ulceration of the leg does not come from a natural imperfection in the limb, but from a fault or weakness to which its vascular structures are liable. This argument was originally developed with singular logical force and perspicuity. I have not attempted to add to the completeness of the exposition, but I have rewritten the language in which it is conveyed. Secondly, the proposal to treat the ulceration by imitating the natural process of healing by scabbing, which can be done by using a particular form of ointment; and I have attempted to explain and illustrate this proposal by reference to the surgical pathology of our own day."

The connection of the great majority of ulcers with a varicose condition of the veins, the means of best relieving this condition by judicious support given by bandages applied *secundum artem,* the imitation of the natural process of union under a scab by a peculiar form of ointment, in which Mr. Spender has found reason to place peculiar reliance—such are the main topics which the reader will find dwelt on in this volume. The pathology of ulcers is plainly and well stated, not so fully as in some formal treatises, but perhaps sufficiently for ordinary practice, special prominence being given to the great prevalence of the varicose variety. In treating of this condition, Mr. Spender calls attention to a circumstance which has often attracted our attention, and which has been noticed by Mr. Hilton, but to which less weight is usually given than it deserves—we mean the pigmentary degeneration which so often occurs in the skin of elderly persons, a little above the inner ankle. On this subject the following extract may be worth our readers' notice (p. 15).

"But there is a nerve-element in the case which must not be disparaged or ignored. The pigmentary discoloration spoken of by Mr. Hilton represents a neurose derangement, leading, not to an exaltation of nutrition but to a degradation of it. It is a local index of diminished physiological force. It is a sign of loss and waste in a circumscribed area of tissue, denoting early embarrassment and distress in the blood-changes; the cuticular epithelium is fed with a lower quality of hæmatine, and textural metamorphosis is less free. And this deviation from a normal state arises very much from a want of power in the vaso-motor nerves of the part affected; more or less they cease to preside over its nutrition, and they are engaged in the same troubles which cause the stasis in the blood-vessels.

"The form and extent of this patch of tegumentary pigment are worth attention. Most often it is, I think, seen as an ovoid mark extending from the inner malleolus at its lower edge, and having its long axis in a perpendicular direction three or four inches up the inside of the leg. Not unfrequently the pigmentary stain extends on the back of the leg in the line of the 'tendo Achillis,' and to a higher extent than on the inside. Now and then it surrounds the whole of the leg below the calf as a broad band of pale tint, and only very little darker than the neighbouring skin. As a rule, the smaller the patch the darker it is. It is seldom seen before middle life; and though usually a sign and note of varicose veins, it is sometimes present without any superficial varix being visible.

"Moreover, this cuticular pigment has its practical interest. If at all intense, it leads to the suspicion of varicose veins, and ought to induce us to search for them. At the very least, it betrays a low organisation of the skin-structure; very often there is a shedding of white, dry, thick scales, which may even conceal a partial ulceration of the cutis, further masked, perhaps, by overhanging borders of hardened connective tissue. We are obliged to predict a tardy cure of any varicose ulcer which is complicated with this condition."

The plan which Mr. Spender advises for ordinary ulcers is very careful bandaging from the toes upwards, with a "domette" flannel bandage, and forming an artificial crust over the ulcer, with the following dressing:

"An ointment containing a very large quantity of prepared chalk forms the best artificial crust. The earthy matter must be in a much greater proportion than enters into any ointment in the 'Pharmacopœia,' consisting of about three pounds of chalk to two pounds of lard. The best way of preparing this application is not by rubbing the chalk down with the lard; but, having previously reduced the chalk to a very fine powder, melt the lard in any convenient vessel over a slow fire, and then add gradually the chalk to the liquefied lard. This should be stirred and thoroughly mixed until nearly cold, and it is then ready for use. A much more homogeneous compound is thus obtained than could possibly be procured by simple admixture or trituration; the materials are more intimately blended together."

Rest in bed Mr. Spender is inclined to regard as generally superfluous, and therefore deleterious, and as far as ordinary cases go, we are prepared to agree with him, and greatly deprecate that absurd addition to the specialistic hospitals which was started a few years ago in the form of a hospital for ulcers. Still there can be no doubt of the rapidity with which the healing process proceeds in some cases on confinement to bed; but we cannot here spare the space to point out the appropriate cases.

If such details appear to our readers unnecessary, or too universally known to be worth writing, we beg to assure them that it is not so. A very moderate experience of hospital or dispensary practice is sufficient to teach any one that an unknown amount of suffering, loss of activity, and consequent loss of the means of livelihood is caused to our poorer patients, in large towns especially (but in the country, perhaps, even more in proportion to the smaller number effected), by the fact that almost all such cases are either shovelled off on the pupil or assistant, or are treated in the most cursory manner, and usually by telling the patient to apply water-dressing. The trifling expenditure of time and money required for the application of a proper bandage, and proper permanent dressings to the ulcer is grudged, and the consequence is, that an easily curable disease is allowed to persist for an indefinite length of time. To tell a poor ignorant man to apply water-dressing is a mere absurdity. The least attention to the case would show the surgeon that the ulcer is for about five sixths of the twenty-four hours covered and irritated by a dry, dirty piece of rag. To allow a man to go about without a bandage is equally negligent, since, if the surgeon would look at the leg he would see that after an hour or two all the parts around the ulcer are thickened and turgid with congestion. To remedy this state of gross neglect it is not necessary to found more private hospitals, but merely to treat these somewhat troublesome cases with a little more care and more humanity—to give, in fact, to the ailments of the poor a similar attention as to those of the rich. Mr. Spender's method seems to us an excellent one; but the great essentials—cleanliness, equability of temperature and moisture, and constant equable support—may, no doubt, be attained by means of different methods. The great merit of his book is that it recalls the attention of the profession to a subject, humble, perhaps, but very important, and one which is too much neglected in favour of more brilliant and alluring themes. We wish the volume every success.

Art. II.—*Lectures on Inflammation: being the first Course delivered before the College of Physicians of Philadelphia under the bequest of Dr. Mütter.* By John H. Packard, M.D., Secretary to the College of Physicians, &c. Philadelphia. Pp. 276.

We are happy to learn from the author's preface, that our transatlantic brethren are following the example which has long been given in this country in the founding of lectureships, designed by the individuals to whom we are indebted for them, to elicit information, and more especially to incite inquiry on special subjects,—lectureships, which amongst us have been found so eminently useful; for instance, those of the Royal Society, of the Royal College of Physicians and the Royal College of Surgeons,—and that they will prove equally prolific of good in the United States who can have any doubt, seeing the wonderful progress its people have made in all things requiring the exercise of mind?

The College of Physicians of Philadelphia has, we think, been peculiarly happy in the choice it has made of the gentleman to inaugurate the undertaking, and Dr. Packard not less so in the subject which he has selected to begin with. These lectures, comprising a history of inflammation, are, in our opinion, highly creditable to their author, and to the American medical school, which has already done so much for medical science: we can strongly recommend them, as affording a good example of inquiry in accordance with the best modes of scientific research. One of their peculiarities—we should say, excellences—is, that their author never confounds facts with conjectures, and is always ready to acknowledge what is obscure as obscure, and what is inexplicable as inexplicable.

The doctrines advocated by him are those of the advanced English and German schools; and no authors are more frequently quoted by him with approval than Mr. Paget and Professor Virchow. We need hardly observe that they are founded on the cellular theory.

These lectures are sixteen in number; that they should be so many will surprise no one who considers the importance of the subject, and how much inflammation is connected with the majority of the diseases to which the organism is liable. Following an orderly method, Dr. Packard has succeeded in giving a comprehensive account of all that relates to his great subject. He holds inflammation to be a disease (never a healthy process) and "always and everywhere the same thing," a morbid change in the nutrition of the affected part, taking its origin from irritation, beginning with excess, and ending with diminution of supply, and that all its phenomena are either those which go to make up the process of nutrition, or are the results of such modifications. Further, he offers the conjecture —offering it for consideration—that its seat is in every instance the connective tissue, which, is distributed everywhere through the body, forming as it were the seat of the soft skeleton, supplementary to the bony one. This conjecture is supported by him by many facts, and some ingenious reasoning.

We had some intention to give an analysis of the work, but reflecting how often the subject matter has been noticed in our periodical, especially in those numbers in which the lectures of Professors Paget and Virchow were reviewed, it has seemed to us rather unnecessary. We shall limit ourselves to a few particular passages; and these, not so much for the information they afford, as for the logical and strict method which they display.

True to the cell-theory, the author holds that the cell is equally the agent of healthy life and function, and "the true theatre of whatever morbid change may take place."

In considering the influences to which the physical organization of man is subject, mechanical, chemical, vital, he observes that, so far as mind affects the body, it is through the incomprehensible connection between it and the nervous system; adding, as to expression somewhat obscurely: "Our observations begin outside of this, which we cannot attempt to explain:" and that the brain "the physical exponent of the mind," as regards the agencies affecting it, is analogous to other organs, all that we know about it being, that if unduly stimulated, if it have an undue amount of work, inflammation of it may ensue, just as it might ensue in the liver or kidney, and as in skin the result will be a certain morbid change in nutrition.

Adverting to the unmistakable evidence of inflammation, such as is afforded by the effusion of lymph, ulceration, suppuration, he remarks that, singularly enough, the phenomena by which it is first made known, viz., redness, heat, swelling, pain, alteration in function, are only non-essentials by themselves, pain being often met with where no structural lesion is discoverable, whilst any of the other phenomena may be due to causes purely physiological. When discussing the question of the heat of an inflamed part and its cause, he maintains, and we think with good reason, that it never exceeds in degree that of the central organs, heat being communicated from the blood; in other words, that no heat is generated in the process of inflammation, or if any, of inappreciable amount, the great source of animal heat being oxidation, and that chiefly displayed by the production of carbonic acid. In confirmation, he states that in sloughing there is an apparent exceptional rise of temperature, insisting, however, that it is merely apparent, inasmuch as sloughing is essentially an oxidating process, adding the practical truth, that if a wound be cleaned and a dressing used not yielding oxygen and excluding air, the gangrene will be arrested. This, we may mention, well accords with the fact of the strong attraction of lymph for oxygen, and of the rise of temperature out of the body resulting from their union amounting, as we have witnessed, to many degrees, when the mass of fibrin exposed was considerable.

He offers some just remarks on the utter passiveness of the tissue elements, these being subject to as inevitable laws as those governing chemical changes, or the phenomena of gravitation; and he takes occasion to point out how delusively expressions are applied to their passive changes, denoting, as it were, feeling and intelligence.

Whilst commending highly the excellent work of Mr. Hilton on the influence of rest in the treatment of surgical diseases, he introduces a passage from it for comment, in which the term "assisting nature" is freely used, and as he thinks misused. We quote the passage with the comments, inasmuch as the higher the standing of the individual, the greater is the danger in the way of force of example. Mr. Hilton's words are—"In fact, nearly all our best-considered operations are done for the purpose of making it possible to keep the structure at rest, or freeing nature from the disturbing cause which was exhausting her power, or making her repeated attempts at repair unavailing. The operation does not cure; it only makes recovery possible, where, without the aid of the hand or head of the surgeon, nature would have ceased her competition with the results of the injury, or succumbed to the exhausting influence of unmitigated disease. In aneurysm, I think I am not in error when I say, that aneurysm is cured by rest, and not by the surgeon—the surgeon takes care to stop the blood, or to moderate its flow; nature herself actually cures the disease by rest."

Dr. Packard says—

"I feel constrained, correct as I believe the *bearing* of these statements to be, to enter a protest against this idea of a gigantic female doctor, to facilitate whose treatment is the sole function of human skill and experience. Nature has no power, and does not even exist in the sense implied in such expressions. Their incorrectness would be of less consequence, if it were not that they are apt to lose the figurative meaning to which they are alone entitled, and being literally interpreted, to colour professional thoughts." . . . . "The true function of the physician or surgeon is then to assist in carrying out, in the body, of the laws governing matter in general, and organised matter in particular."

This comment is followed by another in the same strain on the *vis medicatrix naturæ.* It shall be our last quotation:

"Much has been said in medical writings about the *vis medicatrix naturæ*, and about the natural tendency of parts to return to health. This idea of *nature* has been very much misapprehended and misused. It really means no more than the system of laws under which the atoms of which all material things are composed act and react upon one another. And as I have before urged, these laws are the same, or at least in perfect harmony for organised and inorganic matter. Only, in the organised beings, we have a new condition superadded, and if I may so speak, a new code also. It is much the same as when a body of men organise themselves for any purpose. They adopt certain rules and regulations, differing perhaps from the laws of the community in which they live, because their aim is a special one, but not clashing with these laws."

After what we have already said of these lectures, we will merely add, that throughout they bear marks of careful inquiry and original thought. Works thus written, in so philosophical a spirit, are rare: when they do appear, they cannot, we think, be too highly prized, as tending to form a scientific taste and check the tendency to a loose, illogical, and popular style of thinking and writing, of which we have too many instances in our own medical literature.

ART. III.—*Praktiske Bemærkninger om Behandlingen af Klumpfod.* Meddelte i Philiatrien af Prof. A. G. DRACHMANN. (Særskilt Aftryk af Ugeskrift for Læger, 3 R., v, Nr. 28 og 29, 1868). Kjöbenhavn. 1868.

Practical Remarks on the Treatment of Club Foot. Communicated in the Philiatria, by Prof. A. G. DRACHMANN (Reprinted from the Ugeskrift for Læger, 3rd series, vol. v, Nos. 28 and 29, 1868). Copenhagen, 1868, 8vo, pp. 27. Illustrated.

IN the above pamphlet the author gives a concise and clear description of the deformity of which he treats, laying especial stress upon the points of distinction between the congenital affection and that which arises at a later period; a distinction of considerable importance "as the congenital variety is scarcely ever accompanied with paralysis or atrophy of the muscles concerned from birth, while that which has arisen at a later period is almost invariably referable to the results of the so-called infantile paralyses, which attack the child in the first years of life."

The properly active mechanical forces which in this deformity are in play, and produce the increased extension, adduction, and rotation of the foot, are the muscles of the calf, and the tibiales posticus et anticus. The more passive forces are the different ligaments connecting the several bones of the foot, and in some cases, especially those of long standing, the aponeurosis plantaris. The author's

"Experience led him gradually to deviate from the English mode of operation, and to confine himself to division of the tendo Achillis alone, except in cases where the other tendons mentioned, after tenotomy of the tendo Achillis and in the course of the mechanical treatment, evidently prevented the reduction of the deformity, under which circumstance it became necessary to divide them at a later period. This is the method which I have adopted during the last decennium, and which has afforded me as good results as that I formerly employed."—P. 11.

The author remarks upon the frequency of the occurrence of the deformity in England, 1780 cases having been under treatment in the course of ten years in the Orthopædic Hospital in London. He enters into the details of the operative and mechanical treatment adopted by himself, to which he attributes the following, among other special advantages:

"1. That it is the simplest, and requires only a very plain and easily procurable apparatus.

"2. That tenotomy is confined to the tendons which are evidently in fault, and do not yield to the mechanical treatment; and

"3. That it is completely painless."

ART. IV.—*Nouveau Traitement de l'Angine Couenneuse du Croup et des autres localisations de la Diphtherie par la Baume de Cophapu et le poivre Cubebe.* Par M. H. TRIDEAU. D'Andonille (Mayenne).

A new Mode of Treatment of Plastic Croupy Angina and of other Modifications of Diphtheria by Balsam of Copaiba and Cubebs. By M. H. TRIDEAU. Paris and London. Pp. 32.

THE new method described in this pamphlet of treating diphtheria, a disease of late years so formidable and fatal, appears to be of a very promising kind; should its success be confirmed by extended and varied experience, great will be the boon conferred, and great will be our indebtedness to the author who has introduced it.

We learn from M. Trideau that he was first led to give attention to the disease in question at a time that it raged with unusual severity, indeed to such an alarming degree as to incite the inhabitants of the districts, with the hope of arresting it, to undertake pilgrimages to a particular chapel, an event never before witnessed in the memory of man.

M. Trideau reflecting on the nature of the malady, as affecting the constitution generally, and keeping in mind the inefficacy and bad effects of local treatment, was induced, reasoning from analogy—that which he traced between diphtheria and general catarrhal affections, especially of the adynamic and gangrenous varieties—to make trial of balsams, medicines of acknowledged efficacy in catarrhal ailments, they alone, as he holds, having the property of drying up the sources of mucous secretions.

He first made trial of styrax with copaiba, but finding the former so often adulterated, he substituted for it cubebs—a medicine more readily taken, rapidly absorbed and diffused and followed by no bad effects, but, on the contrary, by an improved appetite and digestion.

Of the results obtained, he speaks with the most perfect confidence, stating that after the experience of many years, and the treatment of more than 300 cases, he has been almost invariably successful; at least, when he had to contend with the malady in its first or second stage; and further, that the convalescence has been of short duration. He qualifies, however, his statements, by insisting on the necessity of distinguishing between simple croup (le croup d'emblée), and croup the consequence of pseudo-membranous angina,—the latter so often baffling any remedial means.

His special mode of treatment, as described by him (we almost literally translate it) is the following:

For adults half a table-spoonful of the syrup of copaiba every second hour, followed by a gramme (about fifteen grains) of freshly powdered cubebs in a table-spoonful of syrup, also every two hours, but in the intervals of the administration of the copaiba.

For infants the dose should be one half of the preceding; or six grammes of cubebs in the twenty-four hours in a tea-spoonful of copaiba every two hours.

In some cases the cubebs may be given to the amount of twenty-four grammes a day to adults and of twelve to infants.

After twenty-four hours, it usually happens that the copaiba is no longer tolerated. Then it should be suspended. It should be discontinued also if the strength of the patients be much reduced, or should there arise a repugnance towards it. One, two, or three drops of laudanum are recommended to be added to the syrup, as likely to make it tolerated.

Commonly the malady yields to the treatment in three or four days. Nevertheless, it is sometimes prolonged to the seventh. Then, under the continued use of the balsam, the following symptoms not unfrequently occur:—a sensation of diffused itching, an increase of the angina, and of febrile excitements, with an eruption simulating that of scarlatina, sometimes discrete, sometimes confluent and resembling urticaria. This eruption never co-exists with false membranes. These infallibly cease when it shows itself, if they have not disappeared before the treatment has been so much prolonged. The ecthyma is most frequent when the cubebs and copaiba are administered together.

As parts of the general treatment alimentation should never be neglected, and the use of coffee is recommended as favouring the recovery of strength. Moderate exercise should be taken; indeed, it is considered essential by the author, and that the patients should not be confined to bed except when the eruption has appeared, or there has been an entire prostration of strength. M. Trideau forewarns the not unfrequent occurrence of profound and prolonged sleep as the effect of the medication, it having been often observed by him in instances in which no laudanum has been administered; it should, he says, create no alarm.

Relative to the *rationale* of the treatment, he is judiciously brief, not forgetting that in therapeutics our best guide, the only reliable one, is careful experience. The hypothesis which he advocates in accounting for the effects of the balsams is that of substitution. He thus reasons: The scarlatiniform eruption described which often occurs on the seventh or eighth day of the treatment is the infallible correlation of the disappearance of the false membranes. This phenomena, so remarkable, readily intelligible by those who comprehend the physiological and pathological affinities which unite the mucous and cutaneous tissue, demonstrates the *modus operandi* of the balsamic treatment. In brief, it is evident that the whole of the recovery is under the influence of the medicines, as it was before under that of the malady: on the other hand, that the cure is accomplished by the way of a general substitution; adding, emphatically: Now this substitution can result only from the antagonism between a morbid exanthema and the medicinal exanthema. Consequently, it is specially requisite to produce the latter. The treatment is illustrated by twenty-six cases. In conclusion, the author refers to Dr. Garreau, Chief Surgeon of the Hospital of Lowal, in confirmation of the efficacy of the treatment in question.

In a subjoined note he calls attention to a remarkable peculiarity, that though the exanthema ordinarily only occur after a prolonged treatment, yet it occasionally appears sooner, after three or four days; nevertheless, whether the treatment be restricted to the shorter period, or the doses be equally prolonged, *it is on the seventh or eighth day that the eruption is always produced.*

ART. V.—*Sound: A Course of Eight Lectures delivered at the Royal Institution of Great Britain.* By JOHN TYNDALL, LL.D., F.R.S.

THESE eight lectures on Sound, intended to interest not only those who possess special scientific culture, but all intelligent persons, have already attained popularity, and are likely to be widely read and respected by medical men of a physiological turn. The author informs us that he has made much use of Helmholtz's work, 'Die Lehre von den Tonempfindungen,' that he has been assisted, in going through the press and otherwise, by English and German friends, and that "one celebrated German friend" had read through all the proof sheets. Thus when it is also borne in mind that the subject is a pet one of so able a man as Dr. Tyndall, it will be perceived that this book must carry with it an unusual weight of authority. Besides, it abounds with excellent woodcuts, and its style is charming. It will, none doubt less than ourselves, be justly treasured not only as a pleasant, but a safe guide in acoustics.

On the other hand, we regard any statement made in a book so diligently supervised and polished, should it be inaccurate, as particularly detrimental to the spread of correct knowledge, and we therefore think it imperative on us as physiologists to venture to demur to a few statements that fairly fall within our province. At page 75 we read :—

"Dr. Wollaston was expert in closing the Eustachian tube, and leaving the space behind the tympanic membrane occupied by either compressed or rarefied air. He was thus able to cause his deafness to continue for any required time without any effort on his part, always, however, abolishing it by the act of swallowing. A sudden concussion may produce deafness by forcing air either into or out of the drum of the ear. In the summer of 1858 I was on the Fee Alp, in Switzerland, where, jumping from a cliff on to what I supposed to be a deep snowdrift, I came into rude collision with a rock, which the snow barely covered. The sound of the wind, the rush of the glacier torrents, and all the other noises which a sunny day awakes upon the mountains, instantly ceased. I could hardly hear the sound of my guide's voice. This deafness continued for half-an-hour, at the end of which time a suitable act opened the Eustachian tube, and restored, with the quickness of magic, the innumerable murmurs which filled the air around me."

We would ask for a reconsideration of this account. The author has just described the Eustachian tube as keeping itself closed, and only opened by means of muscles in the act of swallowing; but Wollaston never suspected but that it is naturally patent, and that it can only be closed artificially by the swelling of its wall on the diminution of the air-pressure usually there present; to him, unless Dr. Tyndall, which we cannot think, had some other source of information than Wollaston's paper in the 'Philosophical Transactions,' the experiment of retaining compressed air in the drum was unknown. It was only this ignorance of the fact that the tube, when passive, is shut, that led Wollaston to imagine that his powers of exhausting the air in the drums (causing the tubes to more completely collapse) by a forced inspiration with shut nose and mouth excelled those of other people. There is no reason to suppose that he could perform any particular feat in this way. Indeed, Dr. Tyndall's own tubes seem to be at least as well able to keep closed with unequal aerial pressure in the fauces and tympana, for they are related to have so remained for half an hour. But we own to be at a loss to understand his adventure. He expresses no opinion whether the concussion had increased or diminished the quantity of air in his drums. It must have been easy to have observed which event had happened, and yet it seems that it was only inferred from the effects that one or other had occurred. As the concussion operated perpendicularly to the paths of the two tubes, it would not *directly* tend to knock air either into or out of the drums; and certainly we cannot suppose that one drum was partially emptied and the other over-filled,—what happened to one happened to both. There would be, perhaps, on the sudden arrest of motion through the feet a tendency to send the air in the lungs towards the bottom, and withdraw some from the throat, and thus from the drums. But then the nostrils, if not the mouth, would supply a current of air, and swallowing would not take place at the instant of collision to open the Eustachian tubes—tubes too minute, when fully open, to permit an *instantaneous* escape of much air. In short, it is inconceivable that the quantity of air in the drums could be affected by an accident having far less tendency to lessen the air in the fauces than an ordinary act of inspiration; and an act of swallowing would have been sure to have quickly restored the aerial equilibrium, could such a thing have been. There is a mistake, we are convinced, somewhere, a lapse of memory, the shock on the nerves in scenes too exciting for careful observation, or transient confusion in the sensorium. The remarks are given by Dr. Tyndall in support of Wollaston's view, that a tense membrana tympani is unfitted for hearing low sounds, though it is fitted for hearing high ones as well or better than a slack membrane. However, our author seems to have been deaf to all the mountain sounds, various as they were, some of which may be presumed to be of high tone, and thus gives only a dubious support to Wollaston. We cavil at these statements because the correctness of Wollaston's observations as to the effect of a tense membrane upon the hearing of acute sounds has been disputed, and with sufficient force to render it desirable to have his experiments repeated by dispassionate observers.

Besides, Wollaston's observation of the different ranges of tones that are audible to different individuals, and which, he thought, indicated the different degrees of tension of their tympanic membranes, is now being used to support a theory that (p. 324) there is a lute of 3000 strings in the human ear that is adapted to the hearing of at least so many tones. In individuals the scope of this organ is supposed to differ, and to more properly account for Wollaston's observation. There is no impossibility, or even improbability, in this idea; but we always advise caution in deciding by the aid of such comparisons. The ear is a complex organ, and many of its parts may, if altered, produce similar results as to audition, and those who make such comparisons as to individual endowments ought to be very watchful against sources of error. We remember well the sensational announcement, made, some years ago, by a then Scotch professor, of the great proportion of mankind who were hopelessly affected with colour-blindness, and of the urgent necessity there was for ceasing to use coloured lights for railway signals; insomuch that the public was terrified: and yet we have known other persons deeply interested in eye-disease through long years of inquiry unable to meet with a single case of marked colour-blindness.

Art. VI.—*Observations on the Nature and Treatment of Polypus of the Ear.* By Edward H. Clarke, M.D., &c.

This pamphlet of Dr. Clarke's is a study of aural polypi chiefly as they presented themselves in a few cases treated by the author. The cases are fully related with an eye to pathology and treatment, and the morbid structures are illustrated by microscopic drawings which are well engraved. It is deduced that polypi may be divided into two varieties, the fibro-plastic and epithelial. The former rarely spring from the tympanum, and rather from the outer than inner half of the meatus, but may be attached to the membrana tympani itself. They grow from the fibrous tissues of the ear, are globular or pyriform, and covered by a kind of pavement epithelium. The latter group grow from the epithelial tissues of the ear, consist of epithelial elements, and are highly vascular. These may be found in any part of the meatus, or may spring from the mucous membrane of the drum. In all of both groups of cases, he infers that otorrhœa preceded the polypus; and he thinks the growths are due to something of the nature of inflammation. In the majority the membrana tympani was perforated to a greater or less degree, or at all events diseased. Both kinds may be successfully extracted, and by persevering after-treatment with caustics and astringents, their return may be obviated. In one instance an epithelial polypus which filled the meatus, obstinately reappeared even after a second removal. At length a swelling showed itself in the mastoid process, on which being opened, a copious discharge of pus resulted, and water injected into the meatus flowed out through the mastoid process. A polypus was seen in the cavity of the drum, which was injected with perchloride of iron, when the growth shrivelled up, and dropped out of the ear two days after, never to reappear.—A very instructive case. The essay is practical and philosophical.

Art. VII.—*Three cases of Spontaneous Fracture of Urinary Calculi in the Bladder.* By George Southam, Surgeon to the Manchester Royal Infirmary. Pp. 4.

In this pamphlet, Mr. Southam calls attention to a singular circumstance, which is sometimes, though rarely, noticed in vesical calculus, viz., that the stone will spontaneously separate into fragments. After relating three cases of this occurrence, two under his own care and one in Mr. Luke's practice, he contrasts with them the history of a case in which there were numerous calculi, which seemed to have been ground against each other during violent exertion, and so to have become partially disintegrated. He then goes on to say:

"In each of the cases I have brought forward, there being clear evidence that there was but one calculus, we must look for other causes of the fractures. Not only were they single, but, at the time of the fracture, two of them were composed almost entirely of lithic acid and oxalate of lime—materials not likely to be affected by any degree of force which might be applied to them in the movements of the body or from the action of the muscles of the bladder. As the calculus in Mr. Luke's case was composed chiefly of triple phosphate, the lithotrite may possibly have contributed to its fracture, although there is no evidence to that effect; but in the other two, though each patient had been sounded before he came under my care, the stone had not been detected. Nor could any injury have led to their fracture from my examination, for their structure was too compact to be affected by the sound; and, moreover, their surfaces plainly show they must have been broken some weeks at least before I discovered them. The cause of their fracture must, therefore, be sought in the calculi themselves. It may probably have taken place through the generation of some gaseous agent, from chemical changes in their earthy constituents; or through the decomposition of the animal mucus of which their cementing material is formed, and which exists in various quantities in different calculi. This view seems to be confirmed by the appearance sometimes, though rarely observed in calculi where the fracture is limited to the internal layers, of which there are two excellent specimens in the Dupuytren Museum at Paris, the separated portion of one appearing to have become again cemented together, and encrusted with a subsequent deposit."

The circumstance is curious and interesting, though somewhat remote from ordinary practice, and Mr. Southam's short paper is therefore well worth perusal.

ART. VIII.—*Nederlandsch Archief voor Genees- en Natuurkunde, onder Medewerking van* P. Q. BRONDGEEST, M. IMANS, A. P. van MANSVELT en H. SNELLEN, uitgegeven door F. C. DONDERS en W. KOSTER. Deel III. 3e Aflevering. Utrecht. W. F. Dannenfelser, 1868.

Dutch Archives of Medical and Natural Science. Edited by F. C. DONDERS and W. KOSTER, with the assistance of P. Q. BRONDGEEST, M. IMANS, A. P. van MANSVELT and H. SNELLEN. Vol. III. Part 3.

THE present number of the above excellent journal, from the former issues of which we have often drawn largely in the pages of this 'Review,' contains many important articles, the first of which, on the terminations of the gustatory nerves in the tongue of the frog, by Dr. Engelmann, has been noticed in our July number.

W. Koster contributes a paper on the 'Exudation of the Colourless Blood-cells through the Walls of Vessels, and the Morbid Processes resulting therefrom.'

The discovery of this phenomenon, so important in its relation to the study of morbid changes of nutrition, is due to Cohnheim of Berlin,[1] who has already pointed out its significance with reference to suppuration in general. Hr. Koster adds some observations and investigations, illustrating its influence on the estimation of some other morbid processes, and of suppuration in the liver.

The fact observed by Cohnheim is briefly this:

"By a simple and easily repeated experiment with the mesentery of a frog, we can satisfy ourselves that in the commencement of an inflammatory process, while the red blood-cells are still carried along with great rapidity through the axis of the vessel, the colourless blood-cells remain firmly adherent to the inner surface of the smallest veins and capillary vessels. We speedily see, particularly in the minute veins, the colourless blood-cells penetrate *into* and soon *through* the wall, and gradually pass further into the intervening tissue. At the same time they now and then, like amœbæ, alter their form, acquire one or more pointed outrunners, in a word distinctly manifest their contractility."

The fact then stands thus: that without rupture of the vascular walls, on irritation of a part of the body, the colourless blood-cells push their way outwards in great quantity through

[1] See his essay "On Inflammation and Suppuration," (Ueber Entzündung und Eiterung) in 'Virchow's Archiv,' Bd. xl, p. 1.

the vessels. The important conclusion naturally thence directly deduced by Cohnheim was:

"The pus occurring after inflammation is, so far as relates to the cells contained in it, if not entirely, certainly for the greater part, the product of the colourless blood-cells.

"This proposition at once throws an unexpected light upon some pathological facts: the analogy between pus-cells and colourless blood-cells, the impossibility of distinguishing the two in the blood, respecting which so much has been written, the morphological agreement between a recent exudation (in pneumonia or pleuritis, for example) and the product of purulent softening of the same, on microscopical examination, &c.

"Cohnheim's discovery, moreover, gives an unexpected blow to the generally received theory of inflammation, according to which the newly-formed cells in inflammation, and subsequently the pus-cells, are the product of a proliferation of the connective-tissue corpuscles, or in general, of the constituents of tissue."

The author illustrates the above condition in a case of highly developed leukhæmia occurring in a man aged twenty-five. He shows that when the blood is overloaded with colourless cells, formed in the spleen and lymphatic glands, these cells pass outwards in great number through the vascular walls. He also adduces an instance of the "emigration" of colourless cells in a case of abdominal typhus in a girl of sixteen, in which he considers "that the so-called 'leukhæmic new formations' must be regarded as products of the exudation of the colourless blood-cells." In the third section of his paper he adduces as an illustration a short description of the so-called "hydrops lymphaticus" of Virchow. His statements respecting the formation of pus in the liver of the rabbit he briefly sums up as follows:

"1. In the first two days there is swelling of the morphal constituents produced by fluid, coagulated or otherwise, with considerable modification of the circulation and nutrition, but without the occurrence as yet of fresh morphal constituents.

"2. On the third and fourth days, in the interlobular connective tissue around the divided vessels, we see densely compressed cells, sometimes lying on one another like an epithelium, resembling colourless blood-corpuscles; there is also distension of the intercellular passages and diffusion of the recent small cells therein.

"3. Through further development of this process and retrogressive change of the hepatic cells, softening and formation of matter arise. Around such "purulent foci" we then find especially a large quantity of connective tissue, in which again are many small round (pus) cells. Through the further progress of this process, and increase and condensation of the surrounding connective tissue, arises evidently the peculiar, sometimes tolerably thick membrane, 'the membrana pyogenetica of early writers,' often found around hepatic abscesses and other purulent cavities in man.

"The course of things in this increase of connective tissue, which around suppurating spots, or in a chronically inflamed part, even without suppuration, is so well known, lies still as a difficult problem before us, for the solution of which our knowledge of the normal formation of connective tissue likewise must be more accurate. At the same time the question directly arises of the signification of the colourless blood-cells, and the part played by the already existing connective tissue, whose protoplasmatic constituents undoubtedly increase in extent. The increase of connective tissue, although in a stricter sense deprived of its hypothetical importance with respect to inflammation and suppuration, continues certainly of great importance for the 'new formation' to which the inflammation may give rise. Thus, through Cohnheim's theory of pyogenesis, a much more defined boundary would exist between what Virchow called nutritive and formative morbid processes than can as yet be demonstrated.

"If we should soon succeed in obtaining a more positive and distinct explanation of the origin of the change of the circulation of the blood, as a starting point for inflammation, a great light will undoubtedly be shed by the facts discovered in late years upon a process of which we may in general assert what Cruveilhier said of phlebitis in particular—'qu'elle domine toute la pathologie.'"

Professor Donders contributes a long and very elaborate paper, based upon numerous experiments, upon the "Innervation of the Heart, in connexion with that of the Respiratory Movements." It would be impossible to attempt, in the present article, even an abstract of this essay: we must content ourselves with quoting the author's own summary of his results, which is as follows:

"The conclusion is this, that the object with which the above investigation was originally undertaken—the explanation of the connexion between the respiratory movements and the duration of the cardiac periods—has been in no part obtained; but that, however, among others, two unexpected facts have been discovered, which are not without importance for the nerve-mechanism of respiration and of the circulation of the blood:

"1. In dyspnœa a strongly increasing stimulation of the retarding nerves of the heart is associated with each inspiration.

"2. In the course of the nervus vagus run centripetally acting nerve-fibres, which depress the activity of the central organ of the retarding nerves of the heart."

Of the next paper, 'On the Seat of Irritation in the Muscular Fibre on the Closing and Opening of a constant Galvanic Current,' by Th. W. Engelmann, assistant in the Physiological Laboratory at Utrecht, a translation, *in extensô*, appeared in the

'Journal of Anatomy and Physiology for May, 1868, p. 435.' The question was, whether in the muscular fibre the irritation takes place on closing of the current only at the negative pole, on opening only at the positive. On this point von Bezold and Aeby came to different conclusions. The author communicates an experiment "which in a simple manner solves the question."

"The sartorius muscle of a frog was cleanly prepared, cut off, and suspended by means of a clamp at the upper end. If we now place a few millimètres beneath the clamp, one electrode on the right, and the second on the left sharp edge of the muscle, the free end of the muscle is drawn to the side of the cathode on closing the current, and to the side of the anode on opening it, because in the former case only those fibres of the muscle on the side in contact with the cathode contract, while in the latter case only those on the side in contact with the anode are thrown into action."

A paper by N. J. A. C. Stemberg, Med. Cand., is based upon the observation lately made, independently of each other, by Cohnheim and Recklinghausen with Hoffmann, that cells from the lymphatic vessels find their way into the irritated cornea. Professor Donders thought it important to ascertain whether the pus-cells in syndesmitis mucipara are likewise derived directly from the vessels, which, *à priori*, seemed to him not improbable. He also wished to investigate the remarkable influence of nitrate of silver, which usually rapidly produces an ordinary syndesmitis mucipara with increase of mucus; and, when applied to healthy connective tissue, is followed by temporary production of muco-purulent matter.

The action of the nitrate of silver was investigated first on the vessels of the mesentery of the frog, in solutions of various strengths. All produced dilatation of the vessels, lasting only some moments, and followed by strong contraction. After some hours, the latter again gave place to dilatation. The changes affected chiefly the arteries. As an immediate result of the contraction, the exudation of the blood-corpuscles was diminished, if not entirely prevented.

A second point investigated was the origin of the mucus-globules in inflammation of the conjunctiva. On touching the membrana nictitans of the frog with nitrate of silver in substance, numerous pus-cells were found collected, a couple of hours later, between the cornea and the membrana nictitans. The latter, cut off and brought under the microscope, exhibited a mass of colourless blood-corpuscles with some red ones, scattered in the tissue, but especially along the vessels. Here and there the blood-corpuscles were seen also situated between the epithelial cells.

On dropping into the eye of a rabbit one part of nitrate of silver in 480 of water, many colourless corpuscles were usually found, after the lapse of half an hour, in the plica conjunctivæ. On trying whether, without previous irritation, such corpuscles occur on the conjunctiva, a positive result was obtained: in the rabbit, and particularly in man, they are not entirely absent, and it is therefore not improbable, that in the normal condition also colourless blood-corpuscles penetrate to this mucous membrane.

The journal contains two or three other important communications, which our limited space will not allow us to notice at present.

ART. IX.—*Statistique Médicale des Hôpitaux de Paris.* Tome I. Pp. 196. Tome II. Pp. 408.

Medical Statistics of the Paris Hospitals. Volumes I and II. Paris, 1867.

THESE two large handsome volumes, in imperial quarto, recently issued from the 'Administration Générale de l'Assistance Publique,' are to be the first of a series of annual returns of the combined tabulated statistics of all the hospitals in the French metropolis. The idea of such a work is a happy one, and reflects the highest credit on the enlightenment, as well as on the energetic zeal of M. Husson, the director-general of this important department of the public service. As early as the end of last century, the then Prefect of Paris, Count Frochot, pointed out in a remarkable report the valuable results which might be expected from a yearly record of the administration of the nineteen hospitals of the city, and clearly indicated how such a scheme would in course of time conduce to important hygienic discoveries directly affecting the welfare of the sick, and leading to the prevention of much disease and death. But neither medical men nor the public at that time appreciated the significance and value of statistical researches; and although, under the direction of the "Conseil Général des Hôpitaux," much was done during the following sixty years to improve the condition and organisation of the Parisian hospitals, and to introduce a better registration and more accurate records of the cases admitted into them for the purposes of medical instruction, no real attempt was made to bring together the experience of the different hospitals until the appointment, in 1860, of M. Husson to the post which he so ably fills. When he found that nearly 100,000 sick, of both sexes and of all ages, pass every year through the various hospitals and hospices of Paris, he felt convinced that the registers, if kept on one uniform plan, of such a vast experience of disease, could not but afford most important data for the general elucidation of curative medicine and surgery, as well as for the special comparison of different hospitals, one with each other, with the view of determining various sanitary problems relating to hospitals much mooted in the present day.[1] He thereupon sought the co-operation and advice of the medical staff of these institutions, for the purpose of organising a general system of registration and classification of all the cases received into them, so as to give unity to the work he had in view. In 1862, he published his 'Etude sur les Hôpitaux,' wherein, after reviewing their past history, he pointed out the improvements required in their management and arrangements, and suggested the great advantages that might be derived from an annual record of the associated results of their united experience. In the same year, a report was issued of the examination which Dr. Blondel and M. Ser had made of the leading hospitals in London, with the view of comparing their condition and economy with those of the French hospitals, and of obtaining hints for carrying out the statistical records of the sick admitted into them. The two volumes now published are for the years 1861 and 1862. It is of course impossible to give the reader an adequate idea of the mass of tabulated statistics here presented to his notice; he must examine the work for himself; and this we strongly recommend all hospital physicians and surgeons to do, as it must be obvious, that if the scheme initiated by M. Husson in respect of the hospitals of Paris be a good one, it ought unquestionably, as he anticipates, to be extended to those of London and other European capital cities.

As a specimen of the information to be derived from these volumes, we have selected the data given respecting the number and results of amputations of the thigh and leg in the two years recorded, and also of the accouchements during the same period.

In 1861, the number of amputations of the thigh in the Paris hospitals were 42, of which 20 were for injuries, and 22 for disease of the limb. The total deaths amounted to 35, or in the proportion of 83·3 per cent. Six of the pathological cases recovered, but one only of the traumatic cases was saved. The fatal

[1] It would seem from the following observation of M. Husson, that unfavorable rumours have been current respecting the far-famed Lariboisiere hospital. "C'est pour avoir meconnu l'influence qu'exercent sur la mortalité d'un hôpital la composition de son personnel des malades et les habitudes de la population qui l'alimente, qu'on a osé dire, avec une legereté qui n'a pas été assez blamée, que l'hôpital Lariboisiere, placé dans les conditions notoires de salubrité, aussi bien au point de vue de sa position topographique que de son installation, était un hôpital insalubre." Future returns will show how necessary it is, in estimating the death-rates in different hospitals, to have due regard to the gravity of the cases treated therein.

issue is ascribed to purulent infection in 6 cases, to phlebitis in 2, and to secondary hæmorrhage in 1.

Of 14 amputations of the leg, 10 of which were for injuries, all were fatal with the exception of one of the pathological cases. Purulent infection and gangrene are assigned as the most frequent cause of the fatal termination.

In 1862, there were 40 amputations of the thigh, 11 for injury and 29 for diseases. Of the former 7 were fatal, and 14 of the latter. The death-rate for the whole number was nearly 55 per cent.; a marked improvement upon the former year.

Out of 15 amputations of the leg, 8 of which were for injuries, there were only 5 recoveries, viz. 2 among the traumatic, and 3 among the pathological cases.

With such disastrous results of operative surgery in Paris before us, it is certainly high time that some comparison be made with the results in other large cities.

The total number of "deliveries" in hospitals, in 1861, was 7309. Of these 5796 occurred in single, and 1513 in married, women.

The deaths amounted to 695, or at the rate of between 9 and 10 per cent. Of the total deaths, 550 were due to what are designated "puerperal affections," and 145 to "diseases which have preceded or followed delivery, and were foreign to the puerperal state." In the latter category are enumerated 9 from typhoïd fever, 12 from purulent infection, 16 from gangrenous erysipelas, 16 from smallpox and scarlatina, 20 from pulmonic affections, 17 from phthisis, 6 from metrorrhagia, and 5 from phlebitis.

In 1862, the number of deliveries was 7027, of which 5683 were in single, and 1344 were in married women. The deaths amounted to 490, or in the proportion of very nearly 7 in the 100. Of these, 399 were caused by "puerperal affections," and 91 by non-puerperal. The deaths from typhoïd fever were 4, from purulent infection 2, from arthritis and abscess 4, from erysipelas 7, from eclampsia 12, from metrorrhagia 12, and 1 from each of the following causes, viz. "suites de couches," rupture of the uterus, and application of the forceps, which surely belong to the category of "puerperal affections."

These few data suffice to suggest anything but a favorable opinion of the existing condition and arrangements of the lying-in wards in Parisian hospitals.

Art. X.—*The First Step in Chemistry.* By R. Galloway. Fourth edition. London. 1868. Pp. xxiv, 477.

This new edition of an old and favorite introduction to the theory and practice of chemistry scarcely requires any fresh commendation of its merits. The experiments described are interesting as well as instructive; they are selected with judgment and explained with clearness, while the ways in which they are to be performed are given with adequate detail.

We are not quite sure as to the propriety of the alteration made in the fourth edition. One fourth of the whole book is now occupied by a long chapter on a non-chemical subject—electricity; and in this, so far as we see, the most important new discoveries and new fundamental laws of the science, such as the correlation of forces, electrical and others, are unnoticed. Part II is, however, a perfectly new addition to the book. It contains a brief account, in sixty pages, of a new notation and nomenclature. But either the whole book should have been constructed in harmony with the new notation, or the old and new views should have been contrasted and compared throughout. Mr. Galloway "recommends those who really wish to study the science to learn the old notation and nomenclature first, because they must at one time or another make themselves acquainted with it, as all the past literature of the science is written on the old system, and for the beginner it has some advantages over the new." But really have we any right to teach the old errors because they are supposed to be more easily learned than the new truths? We demur to Mr. Galloway's reasoning and his practice on several grounds. The new notation is more valuable as an educational instrument, because it is, without doubt, more logical, more consistent in its parts, explains phenomena better, sticks closer to facts, and explains them more systematically than the old. We maintain, too, that it is wrong to say that the atomic weight of oxygen is eight when we are sure that it is sixteen, and when we should not dream of committing the exactly parallel error of assigning, instead of 14, the number 4·66 to nitrogen. While, therefore, acknowledging the not inconsiderable merit of Mr. Galloway's digest of the new views we think they ought to have taken a less subordinate place in his treatise, and have been credited with a larger measure of usefulness and authority.

Our author, indeed, is not always exact enough in carrying out the principles of the new notation. He refers to, but does not use, molecular formulæ, though we possess almost convincing proofs that the atoms of most elements cannot exist alone. Our author also employs the word atom or its derivatives in two perfectly distinct senses. For in order to describe the value of an element as compared with hydrogen—that is to say, the number of chlorine atoms with which it can combine, or of hydrogen atoms which it can replace—the word atomicity, already otherwise used, is inappropriate, and the older word equivalent confusing. A word such as *vinculance*, expressing with the addition of various prefixes, the number of bonds in an atom, would prove very convenient, and be at the same time precise in meaning.

Here we take leave of this new edition of a popular and useful book, commending our suggestions to its author's notice.

Art. XI.—*A Monograph on the Structure and Development of the Shoulder-Girdle and Sternum in the Vertebrata.* By W. Kitchen Parker, F.R.S., F.Z.S. 1868. Issued by the Ray Society to the Subscribers for the year 1867.

We have much pleasure in calling the attention of our readers to the exceedingly valuable work on the shoulder-girdle which has been presented by the Ray Society to their subscribers for the year 1867. It is one that will constitute an era in the progress of philosophical anatomy. It has required for its production infinite pains and consummate knowledge. It is a questioning of nature of the closest kind; and it will, we are sure, gain for its author, whose time is still occupied in the harassing cares of a general practice in the metropolis, a European reputation. It is, indeed, a matter of deep regret, and a reproach to our time and nation, that one of our finest and purest intellects should be compelled to descend to duties capable of being performed by any ordinary mortal, and to waste, for very maintenance, his energies, time, and thought, on subjects that are of necessity uncongenial to him. A mind so choicely gifted—and we are using, as those who know him know, no terms of hyperbole—should be treasured with the utmost care, should be encouraged in his progress, assisted in his difficulties, and have his labour lightened in every possible way. Such men appear but rarely.

That Mr. Parker should have produced the works he has done, the numerous admirable essays on the Foraminifera, the philosophical treatises on the Tinamous, on the Ostriches, and on the Boatbill, is only an evidence of his unconquerable energy, and of the way in which an earnest mind can strive against and overcome difficulties.

The nature of the treatise before us is such that it is scarcely possible to analyse or compress it; and we can only recommend all who take interest in such subjects to study the work for themselves. We may remark, however, in regard to its scope, that, beginning with the simplest examples of the vertebrate type, he fully describes the shoulder-girdle from its first appearance in the rays and sturgeon, the lepidosiren, the pipe and globe fishes, and the sticklebacks, to the mullet, pike, cod, salmon, and herring. To these succeed the Amphibia, including most of the known forms from the urodelous proteus and menobranchus, to the anourous frogs and toads. Then follow the reptiles, birds, and mammals. Under the head of reptiles, nineteen species are described at length, and allusions made to many others. The section devoted to birds and mammals contains full accounts of almost all the typical varieties. The many difficulties that surround this investigation are, perhaps, best shown by the frequency with which Mr. Parker finds himself compelled to dissent from views previously held, and the numerous corrections he has been able to make in the names applied to various bones by preceding anatomists, not even excepting such observers as Rathke, Owen, and Huxley. His own determinations have been made from frequent dissections of the various species in the earliest stages of their development; and it is curious to notice the freaks that nature plays in dividing and multiplying what was at first single, or in uniting and combining what was formerly manifold. The reason, indeed, is not always obvious; and Mr. Parker wisely refrains from any reference to the teleological meaning of the parts, partly because, as he says himself in his concluding remarks, structural fitness is self-evident in most cases; partly because teleological explanation is a mere impertinence in a morphological work—a biassing hindrance—"a pretty golden ball that diverts the racer from his course; and partly also because morphological science is more perfectible than teleological," the latter being often of very difficult, and even in some cases of impossible attainment; whilst morphology only requires materials and patience to enable us to acquire a very clear conception of the step-by-step stages of anatomical structure."

The variations in the structure of the shoulder-girdle found amongst the higher vertebrate birds and mammals are shown by Mr. Parker not to result from the adoption of new structures, nor by leaving out the old, but by *segmentation, arrest, and metamorphosis.* The raw material is taken up again from those larval and pupal types—the fishes, amphibians, and reptiles; but the primordial masses are cloven, selected, and brought into new and closer relation, so that their original reptilian and icthyic conditions have to be sought for in their earlier and rapidly changing states.

Here, as elsewhere, we are astonished at the singular fixity of type that exists in the general features of the organisation, whilst the individual parts appear to undergo endless modifications in detail. Mr. Parker has observed that the ostrich and monotremes present, in their arrested simple condition, numerous reptilian characters; but not the less may most unmistakeable Lacertian characters be noticed also in the noblest aërial types.

Mr. Parker makes some interesting observations on the various modes in which ossification takes place in the animal series, and the names he has suggested and employed are so simple and intelligible that we do not doubt they will come into general use, and supersede those at present employed. Thus the ossification which occurs in the intercellular substance of hyaline cartilage, and of which examples may be seen in the epiphyses of the long bones of mammalia, and in the sternum of lizards and birds he terms "endostosis;" and of this there are three varieties—central, superficial, and sub-central. The epiphyses of mammalia just mentioned are good examples of central endostosis, in which the ossification commences at the centre of the hyaline mass, and radiates outwards. The second form, superficial endostosis, occurs constantly in the rays and skates. The third, or subcentral, where the ossifying centre encloses and is itself enclosed by cartilage, occurs in many of the feebly ossified parts of the lizards, and in the sternum of most birds.

A second mode of ossification is that in which bony matter is deposited in the almost structureless inner layer of the periosteum, and in immediate contact with the outermost cartilage cells, as in the shafts of long bones generally. This mode, formerly called intra-membranous ossification, Mr. Parker terms "ectostosis."

Lastly, to such bony formations as appear primarily in the skin, in the subcutaneous fibrous mesh, or in the aponeurotic tracts, he applies the term "parostosis." Such parosteal tracts in the higher classes are apt to graft themselves upon primary cartilage, and thus to become practically the ectosteal plate of such cartilage, as in the clavicle of man and the furculum and basi-cranial splints of birds.

We may here remark in passing, that the furculum of birds is no longer to be regarded as merely composed of the coalesced clavicles, but consists first of a central angular portion formed by the so-called episternals (Mr. Parker's interclavicular bone), and of the elements of the *compound* clavicle of the mammal; that is, of segmented fragments of the shoulder-girdle, which combine at a very early period with the true (reptilian) clavicle.

For the benefit of our readers, who are probably for the most part more interested in human than in comparative anatomy, we will just add the morphological relations of the several parts of the shoulder-girdle in man. Beginning with the scapula, the thin epiphysial ossification running along the vertebral border is the suprascapular bone well shown as segmented from the scapula in the ray and sturgeon, and large, but not quite segmented off from the scapula proper in the frogs.

The whole of the infraspinous fossa represents the scapula proper, seen forming the greater portion of the scapula of the bird, but best differentiated as a distinct bone in the iguana and turtle.

The acromion process, with the whole of the spinous process, constitutes the meso-scapula, which is seen partly segmented from the body of the scapula in the pangolins, but is well shown also in the iguana.

The neck of the bone and the articular facet forming the glenoïd region has a separate internal ossification in the frog and toad.

The coracoïd process forms a large separate bone in the ovipara and in monotremes, of which only the part corresponding to the head is developed in the human subject, which speedily coalesces with the other elements forming the scapula.

The portion of bone around the coracoid notch, with the fore part of the supra-spinous fossa, is the præ-scapula. It is well seen as a distinct bar, above the glenoïd cavity in certain lizards, and exists as a separate cartilage even in the cat.

The clavicle, in a human fœtus, 2 in. 4 lines long, with its two fibro-cartilages, is thus described by Mr. Parker:

"The clavicle was stout and sigmoïd; it had drumstick-shaped ends formed of hyaline cartilage. These are the remnants of a rod primarily developed, quite independently of the clavicles (and as is best seen in the frogs and toads behind and within them). The meso-scapular segment (or outer extremity of the clavicle) is attached by fibrous tissue to the acromion, and a synovial cavity has commenced at this part. At the other end (inner extremity of the clavicle), the cartilage has been cleft into two segments, the outer one, the præ-coracoïd, has become hyaline cartilage, and is in close contact with the bony clavicle, whilst the inner or distal piece has become fibro-cartilage."

This inner portion corresponds to the well-developed omosternum of frogs, often erroneously called episternum, as though it were equivalent to the episternum of lizards.

The præcoracoïd is also well developed in frogs. The clavicle of man, regarded from a purely osteological point of view, may be stated to be a parosteal bar grafting itself on a delicate rod of cartilage.

The sternum of man is primarily double, with right and left portions, each moiety being segmented from the primarily undivided sternal extremities of the ribs, which with the sternum form at first a common cartilaginous mass or rib plate.

The monograph is enriched with thirty plates on stone, containing no less than 508 separate drawings, all made from Mr. Parker's own original dissections and drawings. They are most clearly represented, the nature of the tissue being easily distinguished by the colouring and the markings in the lithograph, dots, striæ, &c. All credit is due to the Council of the Ray Society for publishing so valuable a work at so cheap a rate; but it ought to be stated that the Royal Society granted a hundred pounds towards the expenses of the plates.

ART. XII.—*On the Immediate Treatment of Stricture of the Urethra by the Employment of the "Stricture Dilator."* By BARNARD HOLT, &c. Third Edition. London, 1868. Pp. 136.

THE method of treating obstinate stricture by forcible dilatation is one which has long engaged the attention of surgeons, and various apparatus have been devised for carrying it out, of which those which go by the names of Wakley, Maisonneuve, and Holt, are most widely known. Mr. Holt introduced his plan to the profession in London in the year 1861. It must have been well received, since he has now to publish a third edition of his treatise, and Mr. Holt's own experience of it must be allowed to be sufficient, since he tells us he has operated in 670 cases! There is one rather remarkable circumstance connected with this work of Mr. Holt, which, though it is not perhaps essential to the formation of a surgical judgment on the plan of treatment, yet surely deserves more distinct mention than Mr. Holt has thought proper, as far as we can see, to give it—we mean the originality of the proposal. Mr. Holt certainly in his first edition, denominated his instrument "A new stricture-dilator;" and he applies the same term to it still, on p. 4 of the present edition. But it was soon stated by Sir H. Thompson[1] and others, that this instrument bore a great resemblance to one introduced into practice by Perrève in the year 1847; and Mr. B. Wills Richardson, of Dublin, has reproduced Perrève's figures side by side with Mr. Holt's, in order to show that the "new stricture dilator" could really be made from Perrève's drawings; that, in fact, Mr. Holt's proposal is an entire plagiarism, down to its minutest details. Mr. Holt attempted to show that there was some essential difference

[1] Holmes's 'System of Surgery,' iv, 398.

between his dilator and Perrève's; but the only effect of this was to provoke his assailant to prove, beyond all possibility of doubt, that this was not so, and that any instrument maker could have made the one from the drawings of the other. Still Mr. Holt continues to consider himself as the inventor of the method, and all that we can find on the subject of his own originality in this edition is the following rather curious paragraph (p. 127).

"In conclusion, I may add, that in advocating the treatment of stricture by rupture, I claim simply that credit which attaches to the publication of a series of interesting cases (examples of many others) which have been subjected to this novel treatment. That the principle upon which the instrument is constructed is as old as the hills, and that the power of the wedge has been known as long as the simplest rules of mechanics have been taught, I freely admit, but I have yet to learn that that principle has been heretofore applied to the treatment of stricture of the urethra in the manner detailed above, and with such highly satisfactory results."

This is hardly an answer to the charge that Mr. Holt knowingly adopted all the details of Perrève's invention and passed them off on the public as his own, without mentioning Perrève's name in any of his three editions. The plain fact is, that if Mr. Holt was acquainted with Perrève's book before writing his own, he ought to have acknowledged his obligation to the French surgeon; if, on the contrary, so remarkable a coincidence of invention took place without any previous knowledge on his part of his predecessor's labours, Mr. Holt ought in justice to his own character to have distinctly asserted it.

We gladly leave this personal question, which however touches too nearly the character of English surgery to be altogether passed over. Apart from the originality of the proposal what is its merit? Mr. Holt has contributed an abundance of successful cases in detail in this volume. He has operated on the enormous number of 670 cases, and the operations have all been successful except two, the details of which are given. The experience also of other surgeons has abundantly shown that the operation is very often most successful, and affords an amount of relief in a period of time, and with an immunity from risk, which no other operation can promise. Besides, further experience has shown, that the stricture is not (in many cases at any rate) really ruptured, but only dilated, and therefore there is no risk of extravasation of urine, or of septic poisoning from the pangs of inflamed urine over a raw surface. The following observations (pp. 96—98) are of much interest:

"Since the above was written, a very interesting essay, for the

Fellowship of the Royal College of Surgeons by examination, 'On Organic Stricture of the Urethra, and its Treatment by Holt's Method,' has been published by Dr. Millar, of Edinburgh, in which he records the post-mortem appearances of the urethra of a patient upon whom he operated nineteen days prior to his death, occasioned by obstruction of the bowels; and after detailing the appearance of the intestine, he says: 'The bladder and urethra were removed. In so doing, an abscess, probably connected with Cowper's glands, and lying close upon the membranous portion of the urethra, was opened into. The bladder was hypertrophied, and the mucous membrane thickened, as is usual in cases of long-standing stricture. *The urethra, on being cut open, was without a trace of rupture or cicatrix* (*vide* plate). The membranous portion was attenuated, owing to the abscess formerly mentioned. A preparation was made of the bladder and urethra, which I had the honour of showing before the Medical and Chirurgical Society in December last.' And another very remarkable case was about the same time recorded by Dr. M'Donnell, of Dublin, in his paper 'On the Treatment of Stricture by the Stricture Dilator,' of a patient who died from cholera, and in whom the immediate operation had been performed fifteen days prior to his death. Dr. M'Donnell removed the bladder and urethra, and they were examined carefully by Dr. Cruise, Mr. William Stokes, and himself, soon after removal. The appearance, however, at that time was not materially different from what the members had now an opportunity of seeing. A No. 9 catheter could readily be passed along the urethra. Except for the hypertrophied condition of the muscular coat of the bladder, and the dilated state of the portion of the urethra behind where the stricture *had been, there was no other sign of the disease having existed.*

"Dr. Millar has also referred to three cases, published in my 'Opinions and Statistics on the Immediate Treatment,' where the parts were examined shortly after death, and where the mucous membrane was found to be entire, and arguing upon these facts, and the examination of numerous preparations, he infers that in most cases the mucous membrane is not *torn at all*, but that the deposit of lymph in the submucous tissue around the canal *is alone ruptured.*"

Thus it appears that the lesion is much less and the operation is much safer than was at first supposed. The question remains, is the treatment desirable? We have Mr. Holt's assurance that he has operated on 670 cases—420 since the publication of his second edition. This would seem to show that he has operated on every case of stricture which has come into his hands. If so, can this be good practice? Our own experience of Mr. Holt's operation is very restricted, since we have always confined operative treatment to cases presenting some peculiarity; but we have observed occasionally very severe and apparently cicatricial recontraction when the stricture has been neglected (as patients will do in spite of every warning), after forcible dilatation; and we confess to considerable incredulity as to the applicability of Dr. M'Donnell's and Dr. Millar's explanation to the severer forms of stricture and to traumatic strictures above all others. In these tougher cases we have little doubt that rupture is actually produced. In all cases there is certainly some danger of it, if there is any real obstruction present; and experience has shown that death or alarming symptoms sometimes, though rarely, follow the operation. We have no hesitation, therefore, in expressing a strong opinion that the operation performed in this indiscriminate fashion is a mistake. Used with judgment, and on properly selected cases, we believe it to be one of very great value; and we hold that Mr. Holt, by introducing it to the notice of the profession in England, has done us a real service, and is deserving of the highest credit if he is really original in the matter.

ART. XIII.—1. *Berättelse afgifven till Kgl. Sundhets-Kollegium om den Medikopneumatiska Anstaltens verksamhet i Stockholm under åren* 1863 *och* 1864. Af Dr. OSCAR THEODOR SANDAHL. Stockholm, P. A. Norstedt och Söner. 1865. 8vo, pp. 56.

Report presented to the Royal College of Health on the Operation of the Medico-pneumatic Institution in Stockholm during the Years 1863 *and* 1864. By Dr. OSCAR TH. SANDAHL. Stockholm, P. A. Norstedt and Sons.

2. *Nyare undersökningar och iakttagelser rörande de fysiologiska och terapeutiska verkningarne af bad i förtätad luft. Meddelade af* Dr. OSKAR TH. SANDAHL (Aftryck ur 'Hygiea'). Stockholm. 1865. 8vo, pp. 35.

Recent Investigations and Observations respecting the Physiological and Therapeutical effects of Bathing in Condensed Air. Communicated by Dr. OSCAR TH. SANDAHL (reprinted from the 'Hygiea'). Stockholm. 1865.

3. *Des Bains d'Air Comprimé: court Aperçu de leurs Effêts Physiologiques et Thérapeutiques; précédé d'une description de l'établissement médico-pneumatique de Stockholm.* Par OSKAR TH. SANDAHL, Docteur en Médecine et en Chirurgie, Professor Agrégé à l'École de Médecine de Stockholm. Stockholm. 1867. Pp. 60, with two plates.

Baths of Compressed Air: a short Sketch of their Physiological and Therapeutical Effects; preceded by a Description of the Medico-pneumatic Establishment at Stockholm. By Oscar Th. Sandahl, Doctor in Medicine and Surgery, Joint Professor in the School of Medicine, Stockholm.

In the above list we have arranged the three works of Dr. Sandahl, now brought before us, in chronological order; but as the third is a translation of the second, with the advantage of revision by the author, after two years' additional experience, we shall, in the present notice, in the first instance, place before our readers a summary of its contents, concluding with such remarks as may be suggested to us by the facts recorded in the report of the Medico-Pneumatic Institution in Stockholm, which stands at the head of our list.

According to the author, it is about thirty years since baths of compressed air were first employed in a rational manner. It is to M. Emile Tabarié, of Montpellier, that we are indebted for the first trial of this new remedy. Since that time their employment has spread widely. Medico-pneumatic institutions have been established at Montpellier, Lyons, and Nice; Stockholm, Gothenburg, and Upsala; Helsingfors; Copenhagen; Berlin, Doboeran, Dresden, Leipsic, Hanover, Nassau, Ems, Johannisberg, Wiesbaden, Frankfort-on-the-Maine, Stutgardt, Reichenhall, Vienna; in London; and at Petersburgh. The bibliography of the subject, of which a full catalogue is given by Dr. Sandahl, is already tolerably extensive.

The establishment in Stockholm was opened on the 10th October, 1860, and on the 1st April, 1866, it was found necessary, in consequence of the increase in the number of patients, to transfer it to a more roomy locality. It now consists of a large hall for the receivers, and of five chambers (two for the inhalation of pulverised medicaments, two little waiting rooms for patients, and one in which the mechanist of the establishment lives), with antechamber, all situated on the ground floor, and communicating with the underground chamber, in which the steam-engine and the pump for compressing the atmospheric air work.

The duration of the sittings is from one to two hours. The increase of pressure is usually from one quarter to one half atmosphere. A library is provided for those who wish to read during the sittings.

The *physiological* effects of compressed air are said to be as follow:

I. *Effects on respiration.*—1. The force of the respiratory muscles is increased. 2. The vital capacity of the lungs is augmented. 3. The respiration becomes slower in compressed than in ordinary air. 4. There is a change in the rhythm of the respiratory act, expiration becoming relatively longer. The experiments of Vivenot on himself and two other persons, with those of Dr. G. Lange, exhibit an average increase in the amount of carbonic acid exhaled in compressed over that exhaled in ordinary air of 22·26 q. p. per cent. Professor Panum, of Copenhagen, in his 'Physiological Investigations' ('Fysiolog. Undersögelser'), concludes that in compressed air, "during tranquil and natural respiration, in a certain lapse of time, a greater quantity of carbonic acid is expelled, and that doubtless a greater quantity of oxygen is absorbed, than during tranquil and natural respiration in the same space of time and under the ordinary pressure."

Panum, however, considers these changes in the chemistry of respiration to be only transitory, and lasting no longer than the bath of compressed air. Our author, on the contrary, believes that "the improvement produced is an effect of the oxidation of the blood, dependent on a quantitative change in the chemistry of respiration—a change which is maintained by the increase of the pulmonary capacity induced by the baths of compressed air, and prolonged after the cessation of the treatment."

II. *Action of compressed air on the circulation of the blood.*—The usual effect is a retardation of the pulsations of the heart and arteries. The mean of this diminution, in a large number of observations, was 9·24 beats per minute; the maximum was 26.

III. *Action on animal heat.*—Under the influence of the diminished evaporation and increased oxidation produced by the compression of the air the heat of the body is naturally increased during the bath, though at the end it becomes lower than before the commencement of the bath.

IV. *Action of compressed air on the muscular strength.*—The latter, as has already been experienced subjectively by workers in diving-bells, is increased in baths of compressed air. This increase is attributed to the augmented absorption, both by the lungs and skin, of oxygen during the bath. Is it not possible that there may be a source of fallacy in some of the experiments referred to, and that when it is stated that J. Lange "found that a weight which, under the ordinary pressure of the air, could be raised with the extended arm only for some seconds and with the greatest effort, was, under an augmented pressure of the air, raised more easily, and could be increased in proportion as the degree of pressure of the air rose," something may be due to the increased support given to the weight by an additional "quarter or half atmosphere"?

Under the head of "Action of compressed air on the transformation of substances in the organism," it is stated that—1st, the activity of the digestive organs is increased; 2nd, that compressed air renders resorption more active; 3rd, that under the influence of compressed air a change, both quantitative and qualitative, takes place in the urine, the total amount excreted being augmented, with an increase in the urea, accompanied either with an augmented excretion of sulphates or with a diminution of phosphates.

The list of diseases, deduced in part from the report above referred to, in which the therapeutical influence of compressed air is said to have been experienced, is sufficiently extensive; it includes general lesions of nutrition—anæmia, chlorosis, and scrofula; acute catarrhs of the mucous membrane of the nose, pharynx, larynx, and lungs; chronic catarrhs of the same; pulmonary emphysema and asthma; pulmonary tuberculosis; hooping-cough; certain organic affections of the heart; affections of the auditory apparatus. Under the eighth and last head we are informed that "compressed air exercises, in certain cases, a salutary action on the genital organs of the female." Our space does not permit us to enter into a critical examination of the statements made in reference to so long a catalogue. We would point out only the apparent contradiction between the idea of treating pulmonary consumption by the inhalation of compressed air and the views of Dr. Mühry, as set forth in our review of his work on climate, to be found in our twenty-third volume, page 62, where he is quoted as stating that "phthisis diminishes decidedly in elevated situations, *in consequence of the great rarefaction of the air.*" It is there argued that this exemption is due to increased expansion of the lung, caused by such rarefaction. Perhaps the same effect may be produced by the opposite condition of increased atmospheric pressure. We have endeavoured, as fully as the brief space at our disposal enabled us to do, to put our readers in possession of the physiological and therapeutical effects attributed to the mode of treatment we have been considering; we must, however, in conclusion, confess our scepticism as to the efficacy of a system from which its advocates promise so much.

Art. XIV.—*The Causes and Treatment of Lateral Curvature of the Spine.* By Richard Barwell, F.R.C.S., Surgeon to, and Lecturer on Anatomy at, the Charing Cross Hospital. London. 1868. Pp. 179.

We have read Mr. Barwell's book with much interest. His views are original and independent, and he has worked them out carefully and conscientiously. We have therefore the gratification which arises from hearing an ingenious theory well supported by arguments and facts. And when we reflect that this theory goes to explain a very frequent and distressing malady, our interest is heightened; for we can hardly help hoping that, when the causes of lateral curvature are better understood, a simpler and more efficient treatment will speedily be devised. That the present method of treating curvature by means of steel stays and supports is far from satisfactory is a matter which we take for granted. Such appliances are complicated and expensive; they are irksome if not actually painful to wear; and the benefit which results from their use is often very doubtful. Such being the case, it will be a substantial advance in surgery if a simpler, cheaper, and more efficient mode of treatment can be suggested. And if Mr. Barwell's estimate of his own method is not over-sanguine; if it is found to bear the test of time and trial, then we may reasonably hope that the treatment of lateral curvatures will henceforth be more satisfactory to all parties concerned than it has hitherto been.

Mr. Barwell begins by discussing the causation of lateral curvature, and he points out that any satisfactory theory must account for these three peculiarities of the disease, viz., that it affects almost exclusively the female sex; that the curvature almost always occurs on the right side; and that the vertebræ are twisted upon their own axis. He then proceeds to set forth his own views. We might do him an injustice if we attempted to epitomise them in a brief notice such as this. We must therefore refer our readers to the book itself. Suffice it to say here that our author considers that the serratus magnus muscle is the chief agent in producing dorsal curvature, and that the muscle of the right side is excited to excessive action sometimes by the greater weight which is thrown upon the right shoulder, sometimes by the greater capacity of the right lung. If this is so, we have a reasonable explanation of the three conditions with which we set out. The serratus magnus, using the ribs as levers, rotates the bodies of the vertebræ. The right arm is naturally heavier than the left and is frequently called upon to do more work, so that the serratus of the right side is more used and more developed than its fellow. Hence the curvature is generally towards the right. But the serratus is also a very important muscle of respiration and wherever the breathing is chiefly thoracic it is called into active operation. Now, in women the respiration is notably much more thoracic than it is in men; and hence arises the greater frequency of lateral curvature in females than in males. This is a very brief and imperfect outline of Mr. Barwell's theory. Those who are interested in the subject, and who wish to see how well it is worked out, must refer to the volume before us.

One cannot help hoping that Mr. Barwell's views may be found correct and that they may be confirmed by the experience of the profession at large, for the treatment which he founds upon them is so extremely simple. If simplicity is the test of perfection, then the treatment of lateral curvature has made a considerable advance under our author's guidance. But it remains to be seen whether the bandages of twilled cloth with elastic tension, the sloping seats, the postures, and the calisthenic exercises which he recommends are really so efficacious as he supposes. If it is so, the days of the costly and complicated steel supports are numbered, and the change will add not a little to the comfort of the patient, and to the credit of surgery.

We must not conclude without bestowing our tribute of praise upon the way in which this book is illustrated. In a former work by the same author which it was our duty to notice, we ventured to say that a few good woodcuts would have been better than a number of indifferent photographs, and we are glad to see that Mr. Barwell has acted upon our suggestion. Nothing can be better in their way than the illustrations in the present volume.

Art. XV.—*Hoarseness, Loss of Voice, and Stridulous Breathing in relation to Nervo-muscular Affections of the Larynx.* By Morell Mackenzie, M.D., &c. Second Edition, enlarged and revised. London, 1868. Pp. 74.

Dr. M. Mackenzie is a well-known assiduous worker in the investigation of the nature and treatment of throat-diseases, and the author of an excellent treatise on the laryngoscope, favorably noticed in this 'Review' about a year ago. The use of galvanism, and a convenient instrument for applying it, were described very briefly in that work, the reader being referred to a special pamphlet, previously published, on hoarseness and loss of voice. The small book now before us is a new edition of that pamphlet, conveying the results of five subsequent years' experience. It is also a reprint, with emendations, of an article published by the author in the third volume of the 'London Hospital Reports.' We welcome its appearance in a separate form as a valuable contribution to our knowledge of laryngeal disorders.

The practice to which Dr. Mackenzie has especially devoted himself has been productive, like most special branches of practice, each in its own department, in the recognition of several additional morbid conditions of the larynx of which we have hitherto lived in happy ignorance. And truly a certain measure of bliss attaches to ignorance of many refinements of modern pathology; for the self-satisfaction and complacency of the uninformed practitioner are thereby not marred or minished, and the even tenor of his way can be pursued unruffled by cares about the minutiæ and hair-splitting which special pathologists are so greatly concerned with. To those whose pride it is to style themselves practical men, we fear Dr. Mackenzie's subdivisions of laryngeal disorders will not prove acceptable additions to knowledge. Leaving out of the question all structural changes in the larynx, the author distinguishes nervous affections as connected with the motor or with the sensory system. The former group are again placed under two heads, according as there is paralysis or spasm of the vocal cords, and the following varieties of paralysis are enumerated:—Bilateral and unilateral paralysis of the adductors; bilateral and unilateral paralysis of the abductors; paralysis of the tensors and paralysis of the laxors;—the two last forms being also either unilateral or bilateral. Moreover, "some of these paralyses may co-exist together, and indeed are often found associated," and therefore an ingenious terminologist might manufacture as many different further kinds of laryngeal paralysis as there happen to be combinations, with corresponding additions to our nosological list.

Dr. Mackenzie has, however, compassionately stopped short of this complete terminology, and contented himself with treating in detail the varieties of paralysis above enumerated. Though we doubt whether the busy physician will stay in each case of aphonia or hoarseness to ascertain with certainty whether it is the adductors or the abductors, or the tensors, or, otherwise, the laxors of this or of that side, or of both sides, that may be deficient in power, it must be allowed that a certain advantage accrues in the more distinct conception of disease, from such minute pathological discrimination as is here attempted.

Spasm of the laryngeal apparatus occupies a much less space with its consideration than paralysis. Only spasm of the adductors and of the tensors is alluded to. And, lastly, the diseases of the sensory system—hyperæsthesia and anæsthesia, are discussed in a single page. An appendix on atrophy of the vocal cords concludes the treatise.

A good collection of cases,—some few of which are illustrated by wood engravings,—adds to the practical value of the book, and no medical man will fail to cull useful information respecting treatment from its pages. The direct application of electricity to the vocal cords is a remedy the author declares almost always successful in paralysis of the adductors,—the most common nervous disorder of the larynx, and also in lost power of the tensors. The instrument for applying electricity was an invention of Dr. Mackenzie's. It was formerly called a "laryngeal galvanizer," but the inventor now prefers to term it a "laryngeal electrode." It is an ingenious instrument; but it needs a practised or well-skilled hand to introduce one of its poles within the glottis so as to come into contact with the vocal cords.

ART. XVI.—*Rodent Cancer, with Photographic and other Illustrations of its Nature and Treatment.* By CHARLES H. MOORE, F.R.C.S., &c. London, 1867. Fcp. 8vo, pp. 128.

Clinical Illustrations of various Forms of Cancer and of other Diseases likely to be mistaken for them, with especial reference to the Surgical Treatment. By OLIVER PEMBERTON. London, 1867. 4to, pp. 128.

THE author of the first-named treatise enjoys particular opportunities, as Surgeon to the Middlesex Hospital, for the observation of the nature and treatment of cancerous diseases, and has already given proof of his good use of those opportunities by his previously published book on 'The Antecedents of Cancer.' In the present work he has undertaken to set forth the grounds for considering rodent facial ulcerations to have a close alliance with cancer, and to illustrate their fitness for adequate surgical operations and the large success that may be looked for from such operations. The definition, pathology, diagnosis, course, and general principles of treatment, are well conveyed in the first fifty-eight pages of the volume, the remainder of the book being occupied with the detailed history of fourteen cases of rodent cancer which justify the hopeful views of treatment expressed by the author. The first two cases are illustrated by five excellent photographs from life, and Cases III, V, and VI, by engravings.

Mr. Moore regards rodent cancer as a local textural ailment, which progressively advances upon and involves the adjoining healthy structures, producing induration; ulceration following thereon from the centre of the diseased mass. "Did the disease only spread in the skin by growth, it would form a broad tough plate, resembling keloïd; but it is a keloïd with all the central part of the flat plat ulcerated out. If it spread without a preceding solid growth, it would be rightly called an ulcer." The disease penetrates from the integument and involves all the subjacent structures, and after piercing the cranial bones may grow into the very substance of the brain, but is, at the same time, almost invariably concentrated into one mass by its continuity of growth. Unlike the solid substance of scirrhus, that of rodent cancer has no contractility, hence the absence of pitting and cupping, and the preservation of the contour and position of the as yet undestroyed integument. In this absence of contractility epithelial cancer agrees with it, and a similarity prevails between the microscopical constituents of the two cancerous lesions.

Rodent cancer is a disease of the decline of life, and as a rule makes its appearance in previously healthy persons. "It is not usual to find any disease of the subordinate glands in connexion" with it. The diseases from which it requires to be distinguished are lupus, syphilis, and epithelial cancer. To the last named it bears the closest resemblance, particularly in the early stage of epithelial cancer of the face. The particulars of diagnosis are well stated in a few pages, and deserve careful study, as does also the discussion concerning the nature of rodent cancer and the characteristics of cancerous disease. The conclusion drawn is that rodent cancer is one of the lowest forms of cancerous disease, possessing a lower vital energy than others. It possesses every local quality of cancer, being uninterruptedly continuous in its growth, but is, at the same time, so meagre a growth, "that it has no superfluous material for circulation in the blood to distant parts, and very little for the lymphatics and textures nearest to it."

In the matter of treatment, Mr. Moore's opinion is that "constitutional alteratives" are of little or no value. The essential part of the disease is not the ulcer, but the solid subjacent tissue, and it is this which must be destroyed. This end may be obtained by caustics, and the form preferred by the writer is the chloride of zinc. Ordinary mild superficial applications are of no use. The treatment by caustics is applicable only where the disease is of small extent; when extended to the size of a half-crown or crown-piece, excision is required; and both this proceeding and also caustics may be needed again and again, owing to the tendency of the malady to recur.

The foregoing remarks embrace many of the leading conclusions arrived at by Mr. Moore; but every practitioner called upon to deal with rodent cancer should not fail to make himself fully acquainted with the author's teaching, and to study also the cases placed upon record.

Mr. Pemberton's work is of a much more extended character.

It undertakes to illustrate the several forms of cancer and of diseases apt to be mistaken for it. This it does by pathological description, by recorded examples of disease, and by pictorial illustrations; the last named constitute a leading and valuable feature of the volume, for, besides many woodcuts intercalated with the text, there are twelve large lithographed plates more especially illustrative of cancer in bone. As the author puts it himself, the work "is simply a record of a very considerable number of the cases of malignant disease that have fallen under my own observation during the labours of many years, preceded by a brief account of the symptoms" presented, "and accompanied by such clinical comments as my experience has suggested. I have, consequently, omitted all reference to the writings of others and all controversial matters."

Mr. Oliver Pemberton has long been a well-known provincial surgeon attached to the large general hospital of Birmingham; but his reputation will for the future be greatly raised by this monument of his industry, and of his careful observation and excellent pathological knowledge. The work essentially addresses itself to practical surgeons, and constitutes for them, by its numerous cases and ample and truthful illustrations, a most valuable book of reference and a guide in forming an estimate of the nature, progress, and prognosis of cancer under all its best-known aspects.

The species of cancerous disease recognised are scirrhous and an acute variety of this hard cancer; medullary or encephaloïd, having three varieties—the firm, the melanotic, and the cystic; osteoïd, fibrous, colloïd, and epithelial, the last named presenting a melanotic variety. Rodent ulcer is introduced at the end of the treatise, but is not regarded as a true cancerous affection.

The species of cancer actually illustrated are scirrhous, encephaloïd, melanotic, and epithelial, each species being dealt with according to the region attacked—as, for instance, encephaloïd of the cranium, of the nose, of the jaw, breast, &c. The opening chapter is dedicated to the diagnosis of malignant from benign growths, and this object is well attained by a series of contrasts drawn between the two. This disquisition is followed, in the second chapter, by a descriptive account of the physical and microscopical characters of the several recognised species of cancer. The author thereupon proceeds to clinically illustrate "the symptoms, progress, diagnosis and treatment of the chief scirrhous growths, as they commonly fall under the notice of the surgeon," by a reference to cases that have occurred under his own observation. The same course is pursued with the other species of cancer above enumerated.

The preceding observations will suffice to convey a notion of the plan of the work; of the value of the matter it contains the highest opinion may justly be pronounced, but a further commendation is incumbent upon us for the manner in which the book is got up. The volume is certainly an *édition de luxe*, produced in quarto, with unusually good paper, wide margin, and very clear, large type, as though the author desired it to fall into the hands of those who would wish to preserve it in their libraries in company with the productions of our great surgeons of by-gone days, who wrote for posterity, and not for practice, and whose ambition it was to diligently observe and to record the teachings of experience, and not, as nowadays, merely to produce ephemeral compendiums of the observations and doctrines of the time.

Art. XVII.—*Hints to Certifying Surgeons under the Factory Acts.* By George Greaves, Consulting Surgeon, Charlton Union Hospital; formerly Lecturer on Medical Jurisprudence, Manchester Royal School of Medicine; Certifying Surgeon, &c. London, Knight and Co. Pp. 23.

This pamphlet is intended for the guidance of medical men who have to examine children and young persons according to the requirements of the Factory Acts. As these acts have lately been extended to a variety of occupations besides those for which they were originally framed, it is probable that many surgeons throughout the country will be called upon to certify who have hitherto had no special experience in determining the age of children from their physical development, or in testing their fitness for certain kinds of work. It is to supply these men with a few hints and rules for their direction that Mr. Greaves has written the pamphlet before us. His own experience seems to have been large, and to have extended over a number of years, and accordingly he is entitled to speak with weight and authority. The *brochure* that he has produced is excellent as far as it goes; but we could have wished that it had gone somewhat further. The information it contains is thoroughly practical, and it is conveyed in clear and precise terms; but the subject is one which might well have been extended beyond the limits of a pamphlet. If the author had entered more fully into it—if he had discussed in detail some of the difficulties which most frequently present themselves in examining children—and if he had given us the benefit of his experience upon such cases he would have conferred a still greater boon upon certifying surgeons.

PART THIRD.

Original Communications.

Art. I.

On the Range of Temperature in Typhus and Enteric Fevers. By J. W. Miller, M.D. Edin., Physician to the Dundee Royal Infirmary.

In the following remarks are given the results of thermometric observations of the range of temperature in some cases of typhus and enteric fever. A few of the cases occurred in private practice, the others in hospital.

In order to render more manifest the variations of temperature and pulse, I have had recourse to diagrams. In these the thick line indicates the temperature, and the faint line the pulse. The degree of temperature and rate of pulse indicated by the horizontal lines are so set down as to bring as nearly as possible within the same space the highest and lowest temperature and pulse likely to be observed. A pulse of 72 and a temperature of 98° Fahr. are taken as conveniently and very nearly representing the standard of health; this being indicated more prominently by the thicker horizontal line.[1] The spaces between the vertical lines represent the days of the illness in all those cases whose date of commencement could be ascertained with any approach to correctness; in those cases in which this was not known, they represent merely the current date. By a glance at such a diagram, a very good general idea of the case represented is at once obtained, much more readily than by a perusal of columns of figures and dates; and not unfrequently the case may be so diagnosed without any further knowledge of its symptoms. In a few instances a third line was added, to show the rate of respiration co-existing with the rate of pulse and degree of temperature, but this created an appearance of confusion, and it is preferable when the noting of the rate of respiration is thought desirable to do so by figures.

[1] In children this should be a little higher, especially for the pulse.

These observations have been made by placing the thermometer in the axilla for four or five minutes or longer. There are two observations daily, one about noon, and the other about eight in the evening.[1] Of course, this can, after all, only give an approximation to the actual range of temperature, for it seems evident that the temperature is almost constantly varying, and it can only be by an occasional chance that the observation happens to be made while this is at its maximum or minimum point. To obtain the real range of temperature would require observations so frequent as to be equally out of the question both for patient and observer.

With regard to the application of the thermometer to the investigation of the disease, although it has become more general of late, it may be still considered by some as an unnecessary refinement, and it may be thought that the temperature of the skin can be estimated with quite sufficient accuracy for practical purposes by the mere application of the hand of the observer. That this idea is erroneous may easily be proved by any one who will take the trouble to test the point by first applying the hand and forming his opinion as to the heat of the skin, and then observing the result as given by the thermometer. Not unfrequently the heat of the skin, as estimated by the hand, may seem to be not above what is healthy, while the temperature is in reality, as the thermometer in the axilla will show, several degrees above the normal standard.

It will be seen by a very cursory examination of these diagrams, that although on taking a general view of the temperature and of the rate of pulse over a series of days, they rise and fall very much together, yet this correlation is by no means uniform; a high temperature being very frequently found along with a slow pulse, and a low temperature with a quick pulse, the pulse also frequently rising in rapidity while the temperature falls, and falling while the temperature rises.

In Professor Aitken's work on the 'Science and Practice of Medicine,' he says that as a general rule the correlation of temperature and pulse may be given as follows:

Temperature	98°	corresponds	with a	pulse of	60
„	99°	„	„	„	70
„	100°	„	„	„	80
„	101°	„	„	„	90
„	102°	„	„	„	100
„	103°	„	„	„	110
„	104°	„	„	„	120
„	105°	„	„	„	130
„	106°	„	„	„	140

[1] In some of the cases which occurred in private practice, the morning observation was at an earlier hour, and to the diagrams of these cases a note to that effect is appended.

That is, that for every rise of temperature by one degree, there is an increased frequency of pulse by ten beats per minute. He states, however, that this correlation is not constant, and that it is far from being so is shown by the following table, in which is noted the number of occasions on which each degree of temperature was observed in thirty cases of typhus of eighteen years of age and upwards, with the average pulse, and also the maximum and minimum pulse, which on different occasions were found with each degree of temperature:

Table showing correlation of Temperature and Pulse in thirty cases of Typhus, of eighteen years of age and upwards.

Temperature.	Number of observations.	Average pulse.	Highest and lowest pulse.
— 96°	5	82	64— 96
96·1°— 97°	39	78	60—112
97·1°— 98°	77	77	50—120
98·1°— 99°	70	94	60—150
99·1°—100°	60	99	72—164
100·1°—101°	61	104	72—144
101·1°—102°	86	106	84—144
102·1°—103°	140	114	84—156
103·1°—104°	173	115	84—144
104·1°—105°	55	120	96—158
105·1°—106°	1	96	

The average pulse here rises, though very unequally, with each degree of temperature from 77 with 98°, to 120 with 105°. The range of pulse, however, corresponding with any one temperature is very wide, from 50 to 120, for example, with 98°, and from 84 to 156 with 103°. In some cases the disparity between the height of the temperature and the frequency of the pulse is very remarkable, and continues for several days, sometimes throughout the whole case. More particularly does this occur in enteric fever, in some cases of which disease the pulse may be throughout very slightly, if at all, above the natural frequency, while the temperature remains at a high standard. The occurrence of a rapid pulse with a low temperature need not cause surprise, the former being so easily accelerated in weak or nervous patients, even by the slight excitement consequent on the visit of the medical attendant; but the absence of excitement of the circulatory system while the high temperature shows the existence of much pyrexia is more difficult of explanation.

This table also brings out what was the most common temperature in these thirty cases of typhus. Putting aside the observations below 98°, and forty-six of those between 98° and

99°, as mostly occurring after the commencement of convalescence, we have 600 observations, of which 369 were above 102°; considerably the most frequent temperature was from 103·1° to 104°, this occurring 173 times. A higher temperature than 105° seems to be rare; it was only once observed among these thirty cases, and among seventeen other cases of typhus of ages below eighteen, it was observed only five times, twice in the case of a girl aged thirteen, twice in that of a male aged seventeen, and once in that of a girl aged fifteen; on these five occasions the pulse was twice 108, twice 120, and once 132.

In studying the range of temperature in typhus fever (and in other diseases likewise) one of the first points to arrest the attention is that neither in its rise towards the acme of the fever nor in its subsequent fall is it regularly progressive. It rises one day, falls a little the next, rises again the next to a point higher than it had previously attained, and so on, until the defervescence has commenced, when it descends in a similarly interrupted manner until it has reached or fallen below the normal standard. The same description applies to the acceleration and diminution of the frequency of the pulse before and after the turn of the fever. The evening temperature is most commonly higher than that of the morning, but to this there are very numerous exceptions. The difference between the morning and evening observations is sometimes very considerable; for example, in Diag. III, on the 8th day, it is 2°; in Diag. VI, on the 7th day, it is 2·7°, and on the 11th day, 2·6°; in Diag. IX, on the 7th day 2°, and on the 9th day 3·8°, besides other instances. It is, however, the exception in typhus for the difference to be much above one degree during the period between the 3rd or 4th day and the 10th or 11th; and in this relation of the morning temperature to that of the evening is to be found one point of distinction between the range of temperature of typhus fever and that of enteric. In those cases in which, during the height of the fever, the evening observation was below that of the morning of the same day, the difference was sometimes as much as from 1·7° to 2·4° (see Diag. III, 6th and 8th days; Diag. VII, 7th day; Diag. X, 9th day; and Diag. XVI, 8th day). About the beginning of the defervescence and during its progress, the difference between the morning and evening temperature is most likely to be considerable; and in some cases there may be one or two great fluctuations, but there is not in typhus the prolonged period of oscillating temperature which occurs towards the close of a case of enteric fever.

In typhus fever the day of illness on which the highest temperature occurs is much more uncertain than might have been expected. Twenty-seven cases were regularly observed morning and evening from not later than the fourth day, and in these, the day on which the highest temperature occurred was as follows:

In	4 cases,	maximum temperature occurred on	3rd	day.
,,	8 ,,	,, ,,	4th	,,
,,	3 ,,	,, ,,	5th	,,
,,	3 ,,	,, ,,	6th	,,
,,	2 ,,	,, ,,	7th	,,
,,	1 case	,, ,,	8th	,,
,,	1 ,,	,, ,,	9th	,,
,,	2 cases	,, ,,	10th	,,
,,	3 ,,	,, ,,	11th	,,
	27			

The early days of the fever would, therefore, seem to be commonly the period of most intense pyrexia, for we see that in twenty out of these twenty-seven cases the highest temperature occurred during the first week, and in twelve of these twenty on the third or fourth day. The difference, however, between the maximum temperature and what occurs up to the commencement of decided defervescence is in some of these cases but trifling in amount. As a general rule, it may be considered that the temperature rises very rapidly during the first two or three days of the case to a point which it rarely much exceeds. In the majority of cases, also, the rise seems to be much more sudden than the defervescence generally is, although it is not so easy to demonstrate this, the patients very rarely coming under observation until the third or fourth day, and seldom even so early as this. Three of these cases, however (Diags. I, XVII, and XX), came under observation, the last on the first day of illness, the other two on the second, and they are in accordance with this opinion.

Very commonly, though not constantly, there is a remission of the temperature during the second half of the first or first half of the second week, this abatement continuing sometimes one day, sometimes several days, after which the temperature again rises before the defervescence begins (see Diags. II, V, VI, VII, IX, XI, XII, &c.). In only four of the twenty-seven cases referred to above, it will be observed, did the highest temperature occur on seventh, eighth, or ninth day.

The day of maximum temperature does not seem at all to correspond with the day of most frequent pulse, being sometimes earlier in the case, sometimes later, by far most frequently the former. This is what might be expected, the mere rapidity of pulse being not so much dependent on the intensity of the febrile condition shown by the elevation of temperature as on the increasing effects of the poison on the whole economy, effects which in the worst cases rapidly bring down the temperature, while they accelerate the pulse.

The highest temperature observed in each of these twenty-seven cases respectively was as follows:

In 1	case the maximum	temperature	was	102·°
,, 1	,,	,,	,,	102·5°
,, 1	,,	,,	,,	102·8°
,, 1	,,	,,	,,	103·8°
,, 4	cases	,,	,,	104·°
,, 3	,,	,,	,,	104·2°
,, 1	case	,,	,,	104·4°
,, 2	cases	,,	,,	104·5°
,, 2	,,	,,	,,	104·6°
,, 3	,,	,,	,,	104·7°
,, 5	,,	,,	,,	104·8°
,, 1	case	,,	,,	105·1°
,, 2	cases	,,	,,	105·2°
27				

The highest temperature shown in these diagrams occurs in the case of a lad aged seventeen (Diag. X). The case did not come under observation till the fifth day, so that it is not included in the two preceding tables. On the evening of the seventh day, the temperature was 105·8°, the pulse being at the same time of so moderate a frequency as 108. The case was a pretty severe one, but notwithstanding this very high temperature, there was nothing specially alarming in the symptoms. In only four of the twenty-seven cases did the temperature fail to attain an elevation of 104°.[1]

With regard to the defervescence, the rule among the cases which I have observed has been that it is gradual. A sudden defervescence, occupying from twenty-four to thirty-six hours, and fully deserving to be denominated a crisis, has been quite an exception. The defervescence in Diags. III and XII approach most nearly to this character, and that in Diag. I would have been a fair example of crisis, but for the elevation of temperature on the 17th day; so also would have been that in Diag. XI, but for a similar elevation of temperature on the 13th day. In the great majority of the cases the defervescence has occupied several

[1] The results of these observations are in some respects at variance with what is stated in Prof. Aitken's work 'On the Science and Practice of Medicine.' For example, it is said that "both in mild and in severe cases the temperature always rises above 104·7°, and it frequently reaches 106° Fahr. or more." It is also said (page 44) that a temperature below 103·3° Fahr., without any external cause, between the middle of the first and the middle of the second week, in a person under eighteen years of age, is conclusive against the case being one of typhus. In the cases shown in diagrams I, III, IX, and X, and more markedly in XII and XIII, cases in which there could be no doubt about the diagnosis, the temperature was found below this point between the fourth and tenth day.

days, and, as I have already stated, it is not regularly progressive. In one case (Diag. VII) it is very gradual and steady however, beginning on the 9th day, and terminating on the 13th, thus occupying four or five days. By reference to the diagrams, the character of the defervescence in each case will be readily seen; and any detailed description is unnecessary. In most of the cases the pulse during the defervescence pretty closely corresponds with the temperature in its fall towards the natural standard, particularly so in Diags. I, II, IV, VI, and XII. Occasionally there occurs what might be described as an unsuccessful attempt at a crisis, some days before the defervescence really commences, the temperature falling as much as three degrees or more, but rising again the same evening or next day to its former elevation. There is an example of this on the ninth day of the case shown in Diag. IX.

A most important point as a distinctive characteristic of the range of temperature in typhus, particularly as an element of diagnosis between that fever and enteric, is the period of the case at which the normal temperature is permanently regained. In some of the earlier cases observed, the notes of the temperature were discontinued too soon after it had become normal; for not very unfrequently after having been normal it again rises even several degrees, and does not again fall to the healthy standard till some days later; and this may occur independently of any complication. In the following statement, therefore, with reference to the date of termination of the fever, only those cases are included in which the temperature continued to be noted for at least two days after it had become normal, with the result of showing that it continued at or below this standard. Cases are also excluded in which there occurred any complication so serious that it might be expected materially to affect the range of temperature. The suitable cases then are fifty-three in number. Their respective ages are as follows:

1 case	5	years of age.
27 cases	10 to 19	„
18 „	20 „ 29	„
4 „	30 „ 39	„
3 „	40, 46 and 53	„

Of these fifty-three cases, the fever ended—

In 1 case	on the	9th day.		In 9 cases	on the	15th day.
„ 1 „	„	10th „		„ 5 „	„	16th „
„ 5 cases	„	11th „		„ 5 „	„	17th „
„ 2 „	„	12th „		„ 6 „	„	18th „
„ 10 „	„	13th „		„ 2 „	„	20th „
„ 6 „	„	14th „		„ 1 case	„	21st „

The diversity shown here as to the period of restoration of normal temperature is very great, from the ninth to the twenty-first day. Notwithstanding this diversity, however, the period at which the abnormal temperature ends will go far in the great generality of cases to establish the diagnosis between typhus and enteric fever. We find only three of these fifty-three cases protracted beyond the eighteenth day. The day on which the largest number of cases terminated was the thirteenth, but no particular day possessed any marked pre-eminence in this respect.

After the commencement of convalescence, the temperature very generally falls for a few days or longer below normal, being frequently 97°, not unfrequently 96°, and sometimes even lower. In the case of a man aged fifty-seven, it fell as low as 94·8°. A temperature so low will probably only be found to accompany, as it did in this case, a state of great danger to life; the grave nature of the case being manifest, however, without the aid of the thermometer. Under a liberal administration of whisky this patient gradually improved and finally recovered. The temperature rose very gradually, and five days after being at the low point mentioned was 97·8°.

An elevation of temperature about or after the commencement of convalescence is said to be frequently the first indication of the occurrence of some internal local inflammation. In one such case an elevation of temperature during and after the fourth week was found to accompany a pleurisy with effusion, which ultimately ended in death. The daily use of the thermometer may occasionally be useful in thus drawing attention to a complication which might otherwise have escaped notice; and any considerable elevation of temperature, when not to be expected in the ordinary course of the case, or still more when the temperature has already begun the descent towards the normal standard, should at once call for a careful examination of the patient, in search of some cause for the occurrence. The existence of some complication, either engrafted on the fever, as a pneumonia, or of old standing, as phthisis, will occasionally very much alter the range of temperature, and this must be borne in mind in cases where a doubt as to the diagnosis might so arise. Pleurisy may occur, however, without affecting the temperature; it supervened in one case of typhus after the convalescence had begun, the temperature nevertheless remaining natural; and I may mention in passing two other cases of pleurisy unconnected with typhus, in which the temperature was noted and found normal. One was a case of phthisis; there was severe pain in the chest catching the breath, loud friction murmur, and the pulse was 120; the other was a case of simple pleurisy in a man about seventy years of age, in whose case the pulse, like the temperature, was normal.

Seven of the cases shown in these diagrams terminated in death.

In the case of A. M. (Diag. XV), the highest temperature was 104·5°, on the fifth day. There was nothing in the range of temperature to create any alarm, except perhaps that while it was falling, the pulse was rising. There was much delirium, floccitatio, muscular tremors, feeble dicrotic pulse, and albuminous urine with tube-casts. Death occurred on the ninth day.

In the case of P. D— (Diag. XVI), which was fatal on the twelfth day, the highest temperature was 103·6° on the eighth day. In this case, also, there occurred a falling temperature with a rising pulse. The cardiac sounds and the pulse were very weak, and there was coldness of the extremities and hiccup.

The case of W. D— (Diag. XVII) proved fatal on the twelfth day. So early as the sixth day the urine was found to be decidedly albuminous, contained abundant tube-casts, and was scanty in amount; and next day it was still more deficient. Notwithstanding these very unfavorable circumstances, the patient's aspect and other symptoms continued good till the tenth day. The highest temperature noted was 104·3° on the third day. There was nothing in the range of temperature to cause anxiety, except perhaps the fall on the sixth day, while the pulse rose, and this fall could scarcely be looked upon with suspicion, it being the period of the case when an abatement in the temperature was to be expected, and the pulse was by no means very rapid, being only 118.

In the case of M. L— (Diag. XVIII) the highest temperature was 102° on the third day. The temperature range was certainly very abnormally low, but the dangerous nature of the case was obvious without the aid of the thermometer.

The case of J. G— (Diag. XX) terminated in death on the sixteenth day. The highest temperature was 104·5° on the eleventh day. The only unfavorable symptom in the range of temperature was the fall on the fourteenth day while the pulse continued to vary between 132 and 144.

The case of J. A— (Diag. XXI) proved fatal on the eighteenth day. The highest temperature was 104·1° on the thirteenth day. While there was nothing abnormal in the range of temperature, the increasing frequency of the pulse up to the extreme rapidity of 166, and the severity of the other symptoms sufficiently declared the gravity of the case.

In the case of R. L— (Diag. XXII), there occurred a very abnormally low temperature with an extreme rapidity of pulse, but here too the danger was obvious without the use of the thermometer.

The results of these observations do not lead me to lay much importance on the use of the thermometer as an aid to the prognosis of a case of typhus. Bad cases may present nothing extraordinary in the temperature range from beginning to end. On the other hand, in several of these cases there occurred a very high temperature early in the fever, notwithstanding which the cases did not prove in any way serious. For example, in Diag. I, the temperature was 104·9° on the 3rd day; in Diag. II, 104·8° on the 6th day, and 105° on the 9th; in Diag. III, 105° on the 5th and 6th days, and 104·8° on the 8th; in Diag. IV, 104·9° on the 5th day; in Diag. VI, 105° on the 7th day; in Diag. IX, 105·2° on the 3rd day; and in diag. X, 105·8° on the 7th day.

There is one circumstance which when it occurs to a considerable extent must be looked on suspiciously, that is a falling temperature with a rising pulse. An exceedingly high temperature, again, 106° or upwards, indicates a dangerous height of pyrexia, and an exceedingly low temperature at any period of the fever may be considered a symptom of failing power, but in either case, in order to form a correct judgment, the collateral symptoms must be considered, and particularly the pulse and cardiac sounds. Indeed, if any one symptom is more than another capable, when considered by itself, of conveying an idea of the amount of danger present, it is the state of the pulse, not merely as to its frequency, but also its strength, volume, and rhythm. The intensity of the febrile condition will be of course much most certainly estimated by means of the thermometer, but the danger to the patient is by no means constantly in proportion to the severity of the pyrexia. In a patient of excitable temperament, the typhus poison having been received into the system may light up a violent febrile state, but the various emunctory organs being in a healthy condition, and rapidly carrying off the morbid matter, there may be no danger to life during the whole course of the malady. In another patient, on the other hand, the feebleness of his constitution or the malignancy of the poison may be such that he is prostrated from the first onset of the disease, and his condition may be one of absence of much febrile reaction throughout. The temperature in such a case may present nothing out of the ordinary course, but a bad prognosis will in all probability be correctly formed from the state of the pulse and the general symptoms.

While holding this opinion as to the value of thermometric observation for purposes of prognosis, and therefore that the use of this instrument in the great majority of cases of typhus is not of much practical service, I believe that every now and again a case will occur in which from absence of the characteristic eruption, for example, the diagnosis will be obscure, and that in such a case, with a rare exception, the employment of the thermometer will be of the greatest importance as an aid towards the formation of a correct judgment. Such an exception however may occur. For instance, had the specific rash been wanting in the cases of M. H— (Diag. XIII) and H. C— (Diag. XIV), in which cases the temperature was never observed higher than 102·8°, and in which it became normal so early as the ninth day in the one, and the tenth or eleventh in the other, the diagnosis of typhus could scarcely have been ventured upon. The rash was quite unmistakeable however, and could leave no doubt as to the real nature of the fever; and notwithstanding such cases must be very rare, their occurrence should render us very cautious how we lay down absolute laws founded upon observations however numerous.

Having referred so frequently to the cases shown in the diagrams, I will not allude to them further than to direct attention to Diag. VIII, as an instance of the occurrence of typhus almost immediately on recovery from enteric fever.

The range of temperature and its relation to the pulse in enteric fever, present several characteristics which differ very decidedly from what is observed in typhus; and these points of difference are such as to be frequently of the greatest service in contributing towards a correct diagnosis. The point in which they differ most conspicuously, perhaps, is the duration of the abnormal range of temperature; for while in the great majority of cases of typhus this has terminated by the middle of the third week, and in many cases considerably sooner; it is rare that it terminates in enteric fever before the fourth week, and it is not unfrequently protracted into the fifth, or even the sixth week. While referring to the duration of the fever, I would remark that it is frequently very difficult in enteric fever to fix the precise date of its commencement, the patient in many instances having felt his symptoms come on so gradually that he finds it impossible to say on what day they began. The onset of the fever seems to be more gradual than that of typhus, and the temperature during the earlier days is said to be less elevated than in that fever. This observation, however, would be of little value for diagnosis, for we have seen that in some cases of the latter fever the temperature is exceedingly moderate.

The evening temperature in enteric fever is almost constantly higher than that of the morning of the same day; there are occasional exceptions to this rule, but they occur much more seldom than in typhus. The difference also between the morning and evening observations is greater. This holds good during the whole course of the fever, but it becomes very remarkable in the great majority of cases during a period which immediately precedes the settling down of the temperature to the standard of health. At this stage of the case, generally about the end of the third week, but varying of course according to the total duration, there occurs a series of oscillations between low temperatures in the morning and high temperatures in the evening, in which the difference may amount to five, six, or even seven degrees. This alternation may continue from a few days to a week or more, and when it is well marked may be considered as conclusively diagnostic of enteric fever.

Another peculiarity not unfrequently met with in enteric fever is the co-existence throughout the case of a slow pulse, occasionally very slightly if at all above its normal frequency, with the high fever temperature; this being sometimes 103° or higher, while the pulse is only 72 or even less.

The following Table shows the correlation of Temperature and Pulse in twelve cases of Enteric Fever, their respective ages being 14, 17, 17, 18, 21, 22, 22, 22, 25, 36, 37, *and* 44.

Temperature.	Number of observations.	Average pulse.	Highest and lowest pulse.
96·1°— 97°	18	78	48—120
97·1°— 98°	68	86	48—144
98·1°— 99°	83	91	48—132
99·1°—100°	60	94	60—132
100·1°—101°	58	97	54—132
101·1°—102°	94	102	60—144
102·1°—103°	137	105	72—132
103·1°—104°	95	110	72—136
104·1°—105°	23	112	84—144
105·1°—106°	2		90—120

Though the number of observations is rather small, the table is interesting so far as it goes. By comparing it with that referring to typhus (p. 452) it will be observed that the average pulse with each degree of temperature, at least in its higher range, is somewhat lower, and the minimum pulse considerably so. The degree of temperature most frequently observed is also lower.

Having premised these few remarks as to the chief points of difference between the ranges of temperature in typhus and enteric fever, I will now direct attention, without going into the details of the cases, to the principal features which they present.

The case of G. B—, male, æt. 17 (Diag. XXIII), though somewhat short, was a very characteristic one of the enteric range of temperature. With two exceptions (tenth and eleventh days) the evening temperature was higher than the morning, and the difference between the two was considerable. The highest temperature observed was 103·8° (eighth and ninth day). The oscillation of temperature at the beginning of the third week was well marked. The case was a mild one, and terminated on the nineteenth day. The subsidence of the temperature proved that the rapid pulse which continued was not due to prolongation of the fever.

In the case of A. S—, male, æt. 22 (Diag. XXIV), the highest temperature observed was 103·7°, on the tenth day. Throughout there was no exception to the evening temperature being higher than that of the morning. The normal range of temperature was regained on the twenty-third day. The pulse was very slightly accelerated, being mostly about 84, and only once as high as 96. The notes cease on Nov. 1st, and the case is especially interesting from the patient taking ill of typhus on Nov. 12th, a few days after having left the hospital. This attack of typhus was also mild (see Diag. VIII), and taken along with this, furnishes a very good contrast of the ranges of temperature peculiar to typhus and enteric fever.

The case of M. P—, female, æt. 25 (Diag. XXV) was one of considerable severity. Notwithstanding this, it is an instance of the almost entire absence of one of the most prominent symptoms of enteric fever, the diarrhœa, which occurred on only one day, the fifteenth, the patient requiring several times castor oil and enemata to procure an evacuation. The evening temperature was, with a very few exceptions, higher than that of the morning. The oscillation of temperature towards the end of the case was not very well marked, continuing only over two days, but it occurred to a greater extent during the third week. At this period the case seemed to be tending towards convalescence, but on the eighteenth day a severe exacerbation occurred, and convalescence did not commence till the thirty-fourth day, making the total duration of the case five weeks. The highest temperature observed was 105°, and occurred on the twenty-second day.

The case of J. C—, female, æt. 18 (Diag. XXVI), was chiefly remarkable for the great oscillations of temperature throughout, at least from the period of its coming under observation. The highest temperature was 104·2° on the evenings of the tenth and fifteenth days. Convalescence commenced on the thirty-first day. With one exception the evening temperature was above that of the morning.

The next case, J. L—, male, æt. 36 (Diag. XXVII), was another of those characterised by a peculiarly low pulse. On Sept. 20th (the day of illness on admission could not be ascertained) the temperature was 103·6°, and with this exalted temperature the pulse was only 72. The highest temperature, 104°, occurred on the morning of the day of admission. The alternation of high and low temperatures was particularly well marked, and without exception the temperature was higher in the evening than the morning. Although the precise date of commencement was doubtful, yet from the specific eruption being present on admission, we cannot be far wrong in assuming at least eight days as the previous duration of the illness, which would put the beginning of steady convalescence (Oct. 4th) at the twenty-eighth day.

In the case of M. R—, female, æt. 18 (Diag. XXVIII), the date of commencement was also doubtful; but the day of admission was probably at least the eighth. The highest temperature, 105·7°, occurred on the evening of the ninth (?) day. Up till April 26th (twenty-fourth (?) day), which was probably about the termination of the fever, the evening temperature was invariably above that of the morning. When the notes ceased the temperature continued above that of health, its elevation being probably due to tuberculosis.

In the case of J. B—, male, æt. 21 (Diag. XXIX), there was less variation between the morning and evening temperatures than in most of the others, and the evening temperature was not so constantly above that of the morning. The highest temperature noted was 104·5° on the evening of the fourteenth day. The last stage of the fever was somewhat protracted, and convalescence was not steady until the thirtieth day.

The following case, that of C. D—, male, æt. 23 (Diag. XXX), was also a long one. With three exceptions, the evening temperature was higher than that of the morning. The highest temperature observed was 103·6°, on the evenings of the twentieth and the thirty-second day. Steady convalescence did not commence till the thirty-ninth day.

The case of T. S—, female, æt. 22 (Diag. XXXI), was a somewhat peculiar one. Considering its duration, it more resembled typhus than enteric fever. The oscillation of temperature occurred most unusually early, between the tenth and fifteenth days, and the normal range of temperature was regained so soon as the seventeenth day. The general symptoms however, namely, the diarrhœa, the characteristic eruption, the ilio-cæcal tenderness, and gurgling on pressure, were such as to make the diagnosis quite clear. Notwithstanding the short duration, the case was by no means a mild one, and the general symptoms as well as the temperature in its earlier stages were such as would have led to the expectation of a protracted illness. The highest temperature noted, 104·7°, occurred on the evenings of the sixth, tenth, and fourteenth days; and the evening temperatures are with a few exceptions higher than the morning.

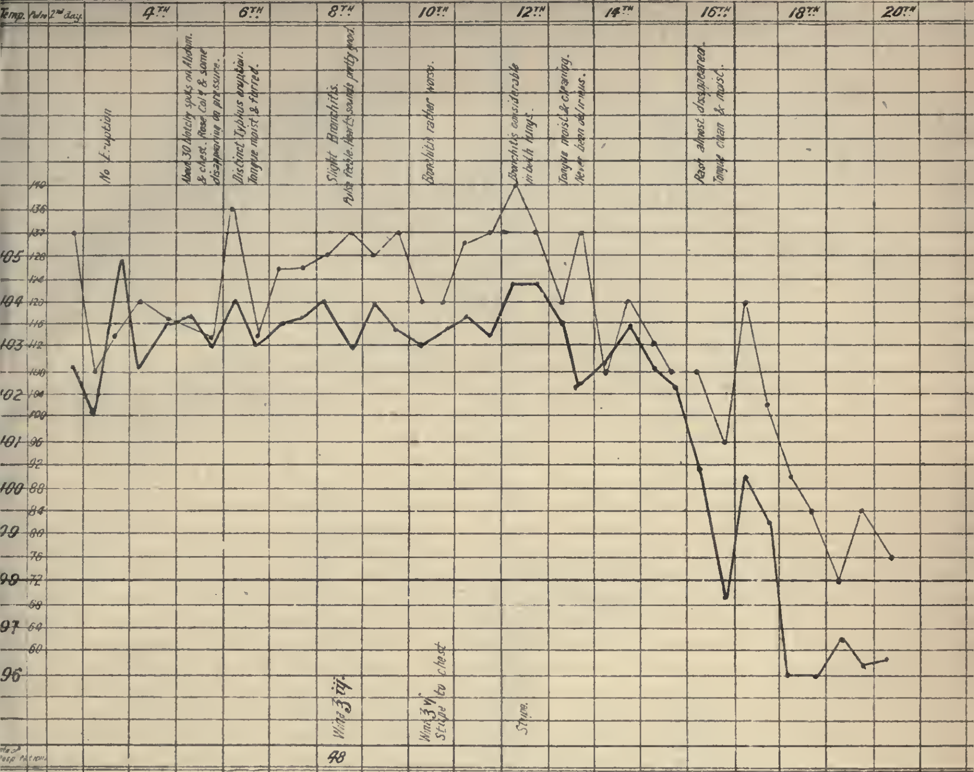
The last case to which I will direct attention (Diag. XXXII) was an example of the co-existence of typhus and enteric fever. The character of what little appearance there was on the skin when the case came under observation, the diarrhœa during the latter half of the second week, the ilio-cæcal tenderness, the late appearance of the typhus rash, the unmistakeable appearance of that rash, and, lastly, the advanced period of the case before the temperature had regained its normal standard, all in my opinion go to establish this diagnosis. It is confirmed by the range of temperature, which is rather irregular; it was frequently lower in the evening than in the morning, and had not become steadily normal by the twenty-fifth day.

My impression is that the principal value of the thermometer is, in enteric fever as in typhus, diagnostic, and that for prognosis our reliance must be in a careful consideration of all the symptoms presented by the case; among these of course the temperature will have a place, but not, I believe, the preeminence which is by some observers ascribed to it.

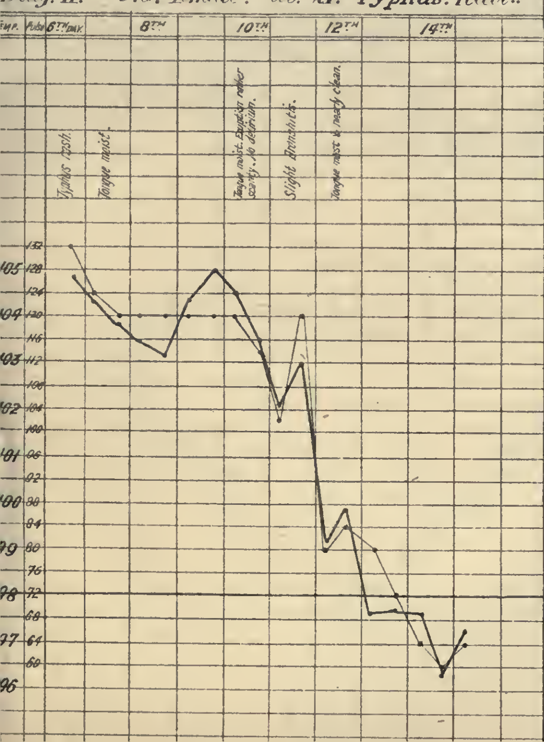
In conclusion, the leading diagnostic points between the range of temperature of typhus and that of enteric fever, may be briefly stated as follows:

Typhus Fever.	*Enteric Fever.*
The duration of elevated temperature is very rarely beyond eighteen days; it is generally shorter by several days, and may be even so short as nine days.	The duration of elevated temperature is very rarely less than twenty-one days; it is generally longer, and may be protracted to thirty-five days or even more.
The evening temperature is frequently lower than that of the morning.	The evening temperature is almost constantly higher than that of the morning.
The difference between the morning and evening temperature, during the height of the fever, or from about the third to the tenth or eleventh day, is comparatively seldom above one degree, and although about the period of defervescence the difference is sometimes much greater, the oscillation is not continued over more than one or two days.	The difference between the morning and evening temperature is generally, throughout the case, greater than in typhus, and towards the end of the fever there occurs the very characteristic oscillation of temperature, during which the difference is frequently five, six, or even seven degrees, and which may continue from a few days to a week or more.
A high temperature is, as a rule, accompanied by a high pulse.	A high temperature is frequently accompanied by a pulse but slightly accelerated, and occasionally by a pulse slower than normal.

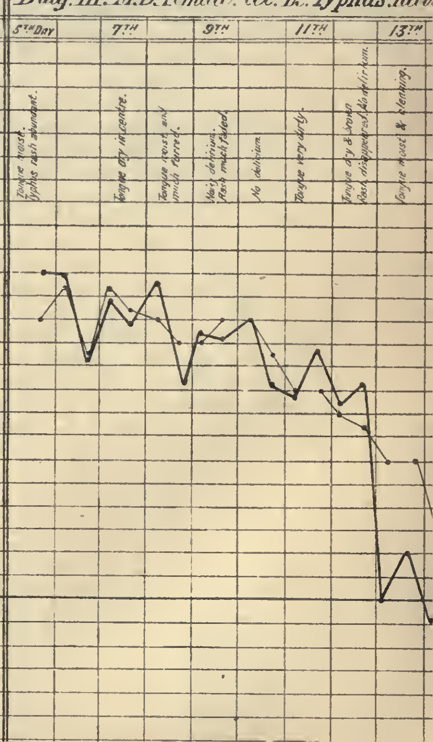
Diag. I. — H. M'H. — Female. Æt. 14. — Typhus. Recov.



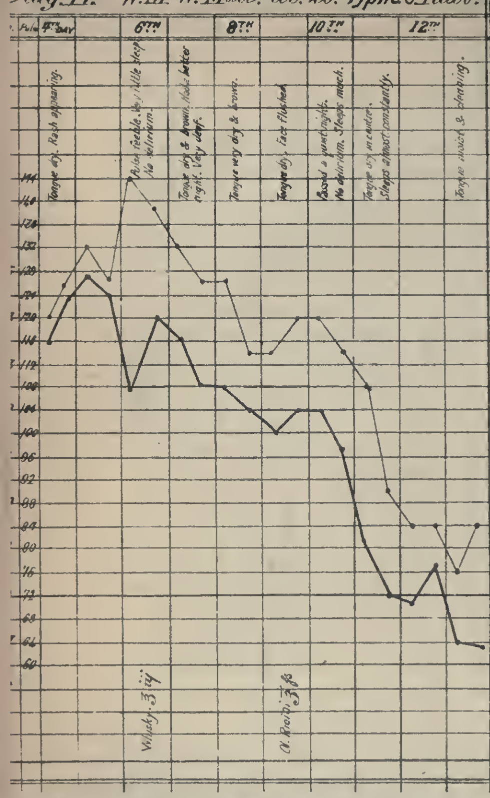
Diag. II.— J.G. Female. Æt. 21. Typhus. Recov.



Diag. III. M.B. Female. Æt. 12. Typhus. Recov.

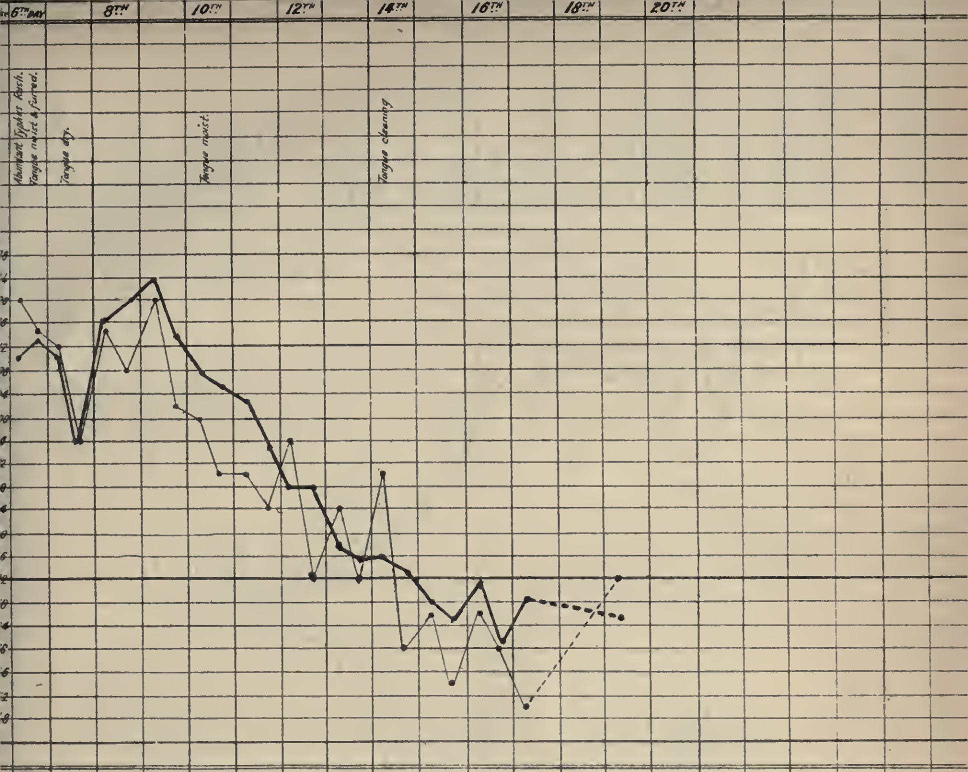


Diag. IV. W. McW. Male. Æt. 26. Typhus Recovd.

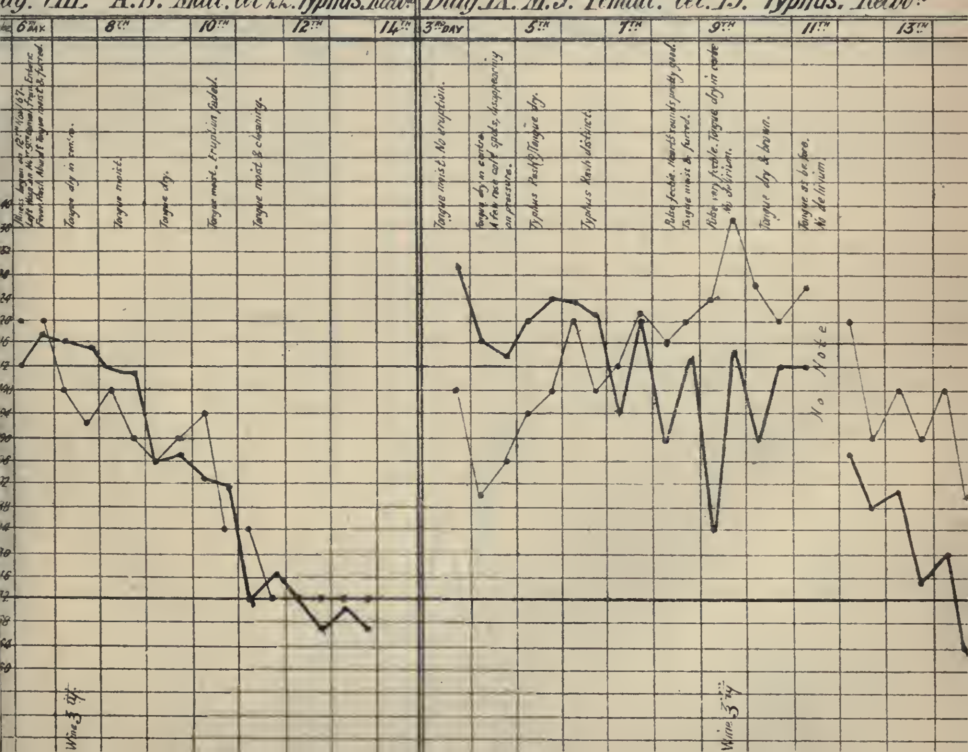


Diag. V. M. D. Female. Æt. 13. Typhus. Recovd.

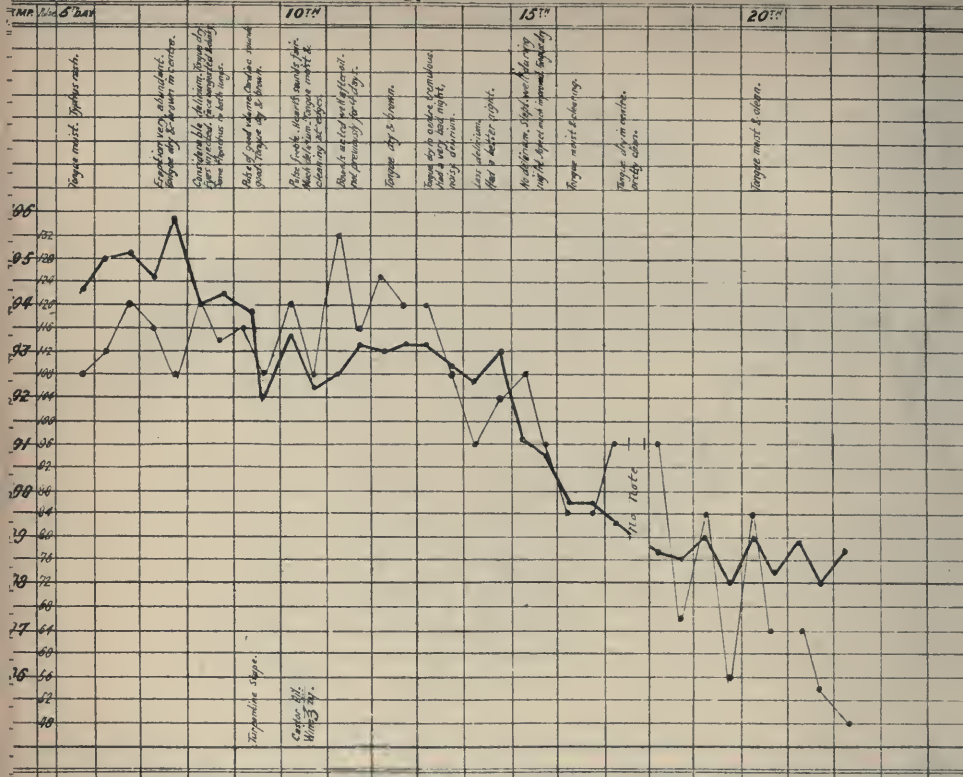
Diag. VI. J. M. Male. Æt 23. Typhus. Recovd. (Observations between 10 & 11 A.M. & about 8 P.M.)



Diag. VIII. A.S. Male. Oct 22. Typhus. Recovd Diag IX. M.J. Female. Oct. 15. Typhus. Recovd

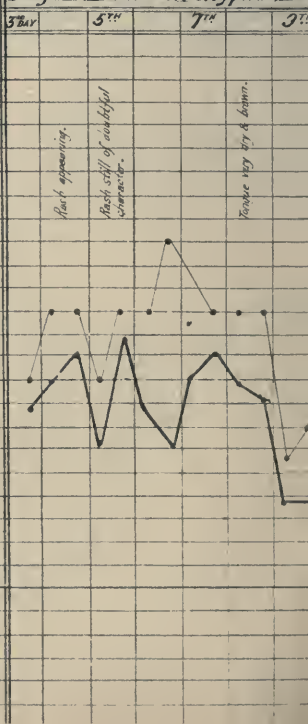


Diag X. R. McK. Male. Æt 17. Typhus. Recovd.

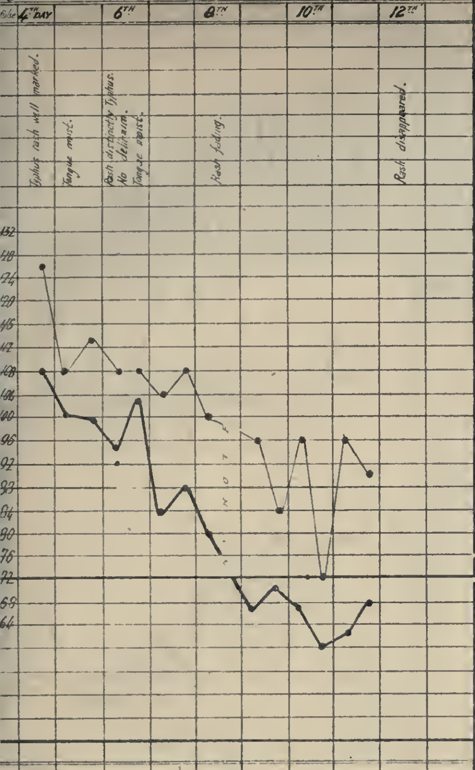


Diag. XI. B. L. Female. Æt. 14. Typhus. Recovd.

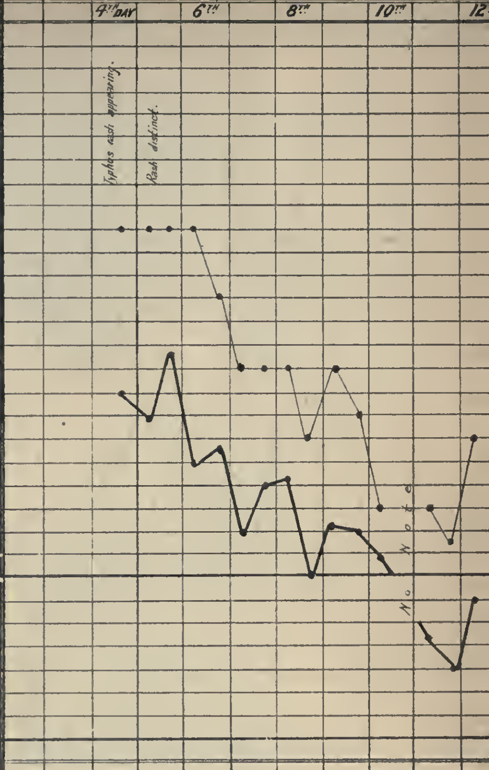
Diag. XII. Male. Æt. 12. Typhus. Re



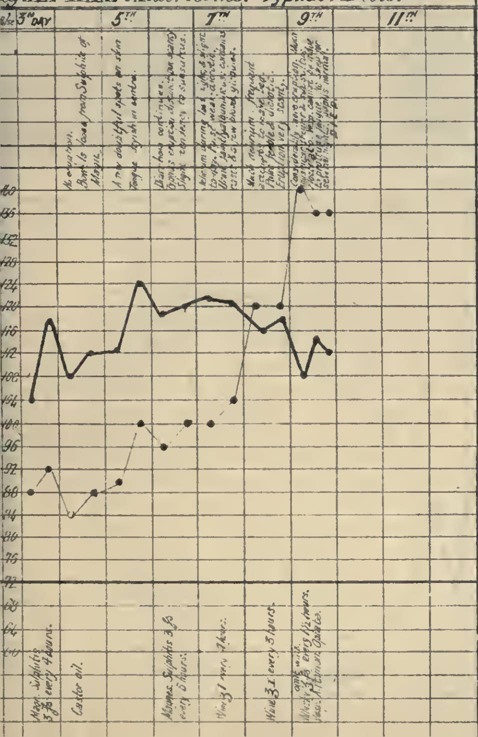
Diag. XIII. M.H. Female. Æt 16. Typhus. Recovd.



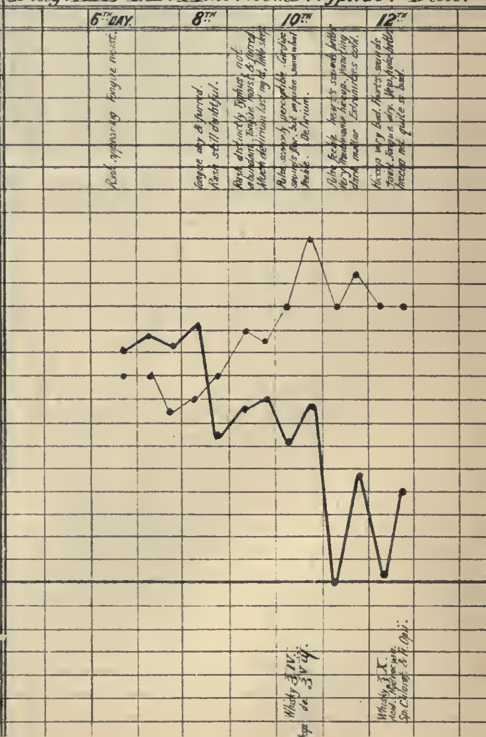
Diag. XIV. H.C. Female. Æt 18. Typhus. Recovd.



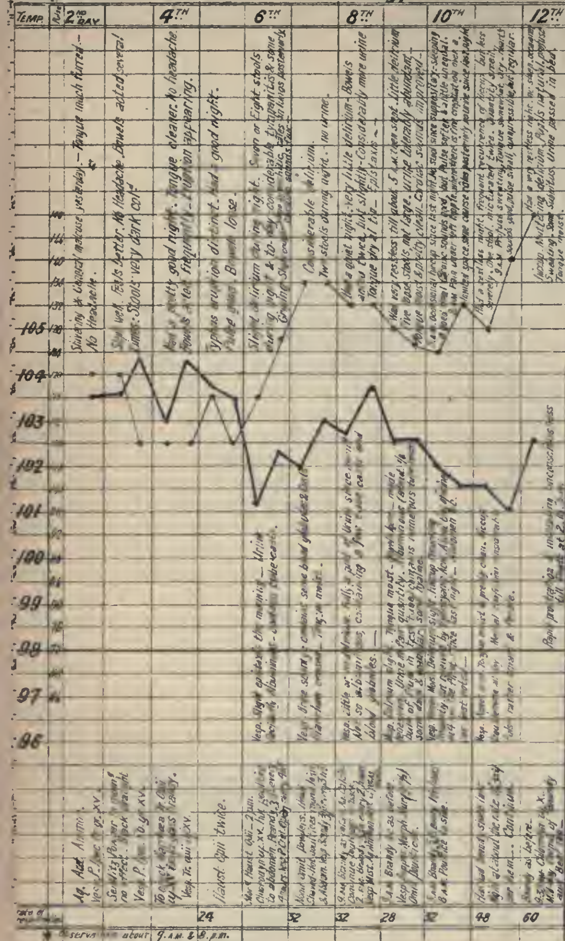
Diag. XV. A.M. Male. Æt. 28. Typhus. Died.



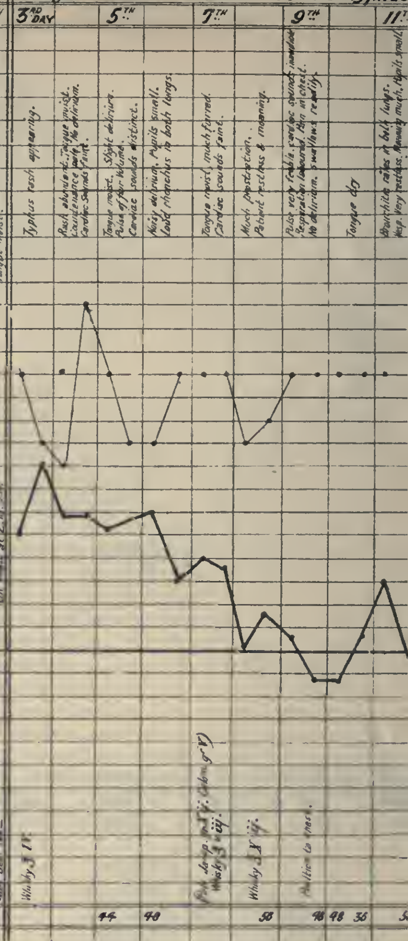
Diag. XVI. P.D. Male. Æt. 29. Typhus. Died.



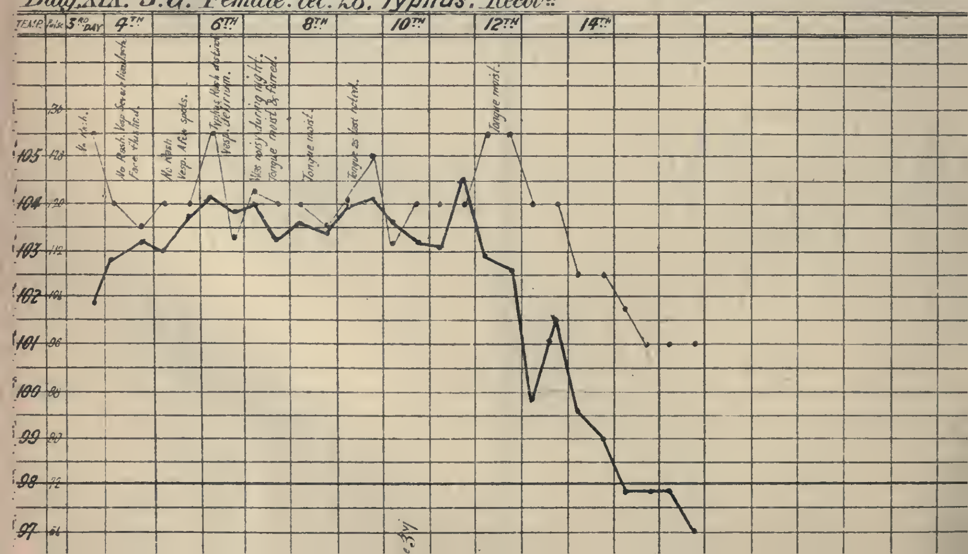
Diag. XVII.* W.D. Male. Æt. 32.—Typhus. Died.



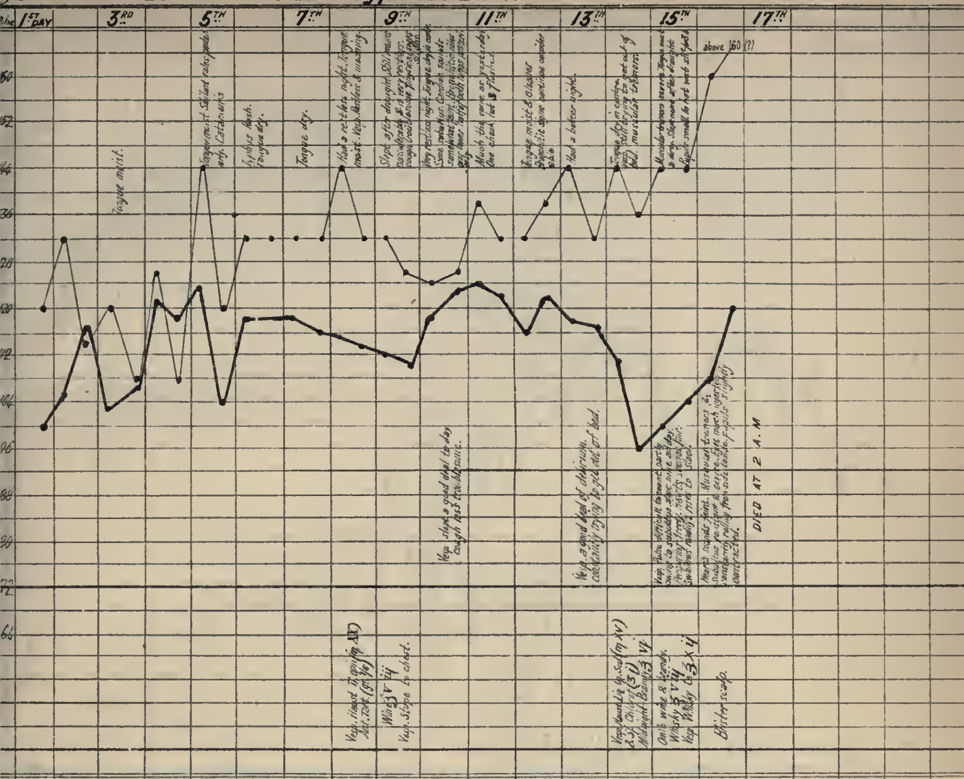
Diag. XVIII. M.I. Male. Æt. 60. Typhus



Diag. XIX. J.G. Female. Æt. 28. Typhus. Recovd.

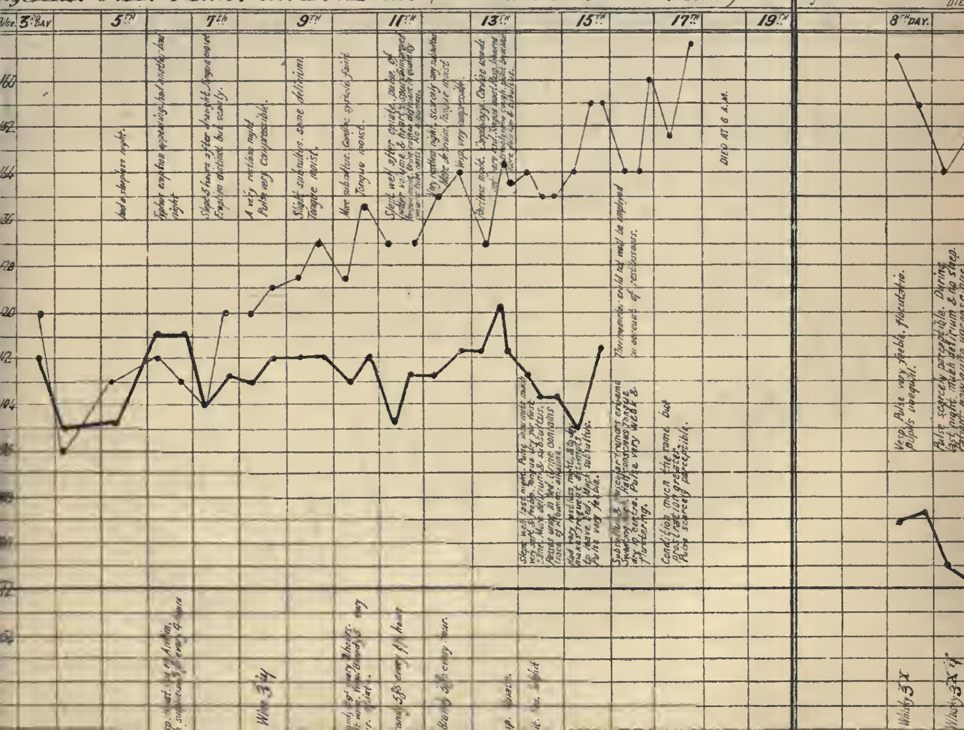


...g. XX. J.G. Female. Æt. 19. Typhus. Died.

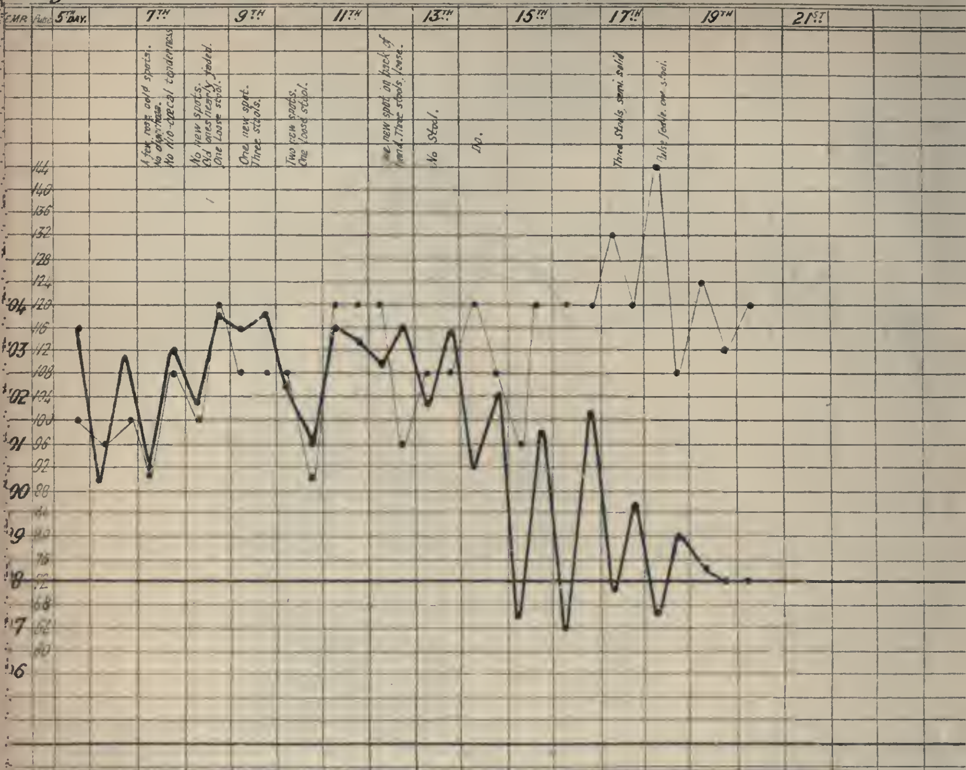


...ag. XXI. J.A. Male. Æt. 40. Died. (observations about 10. A.M. & 8. P.M.)

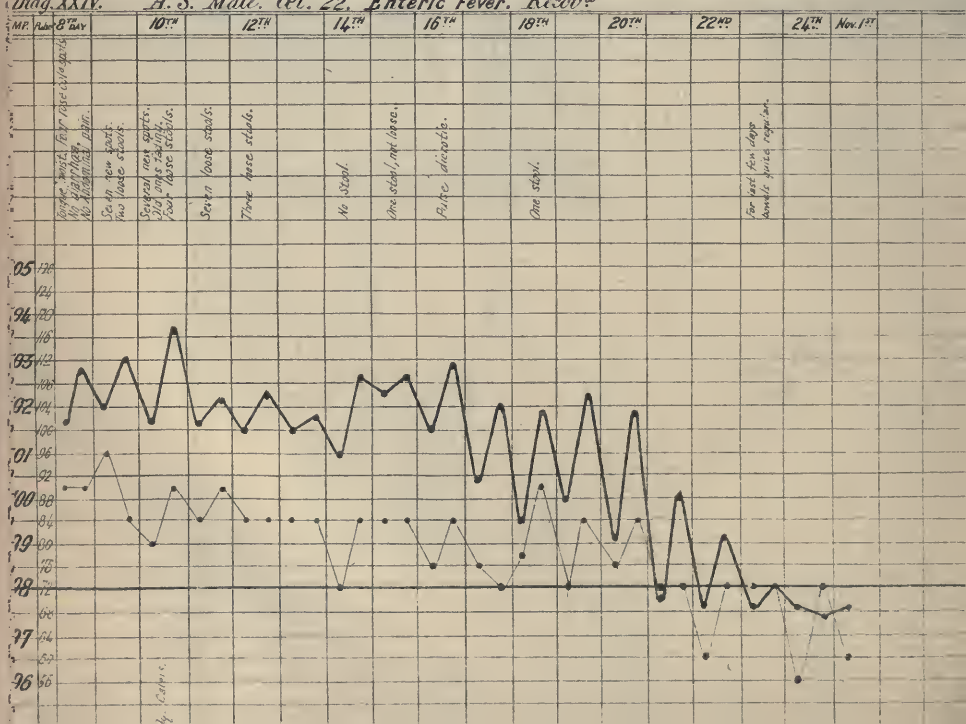
Diag XXII. R.L. male. æt. 53 Typh... Died



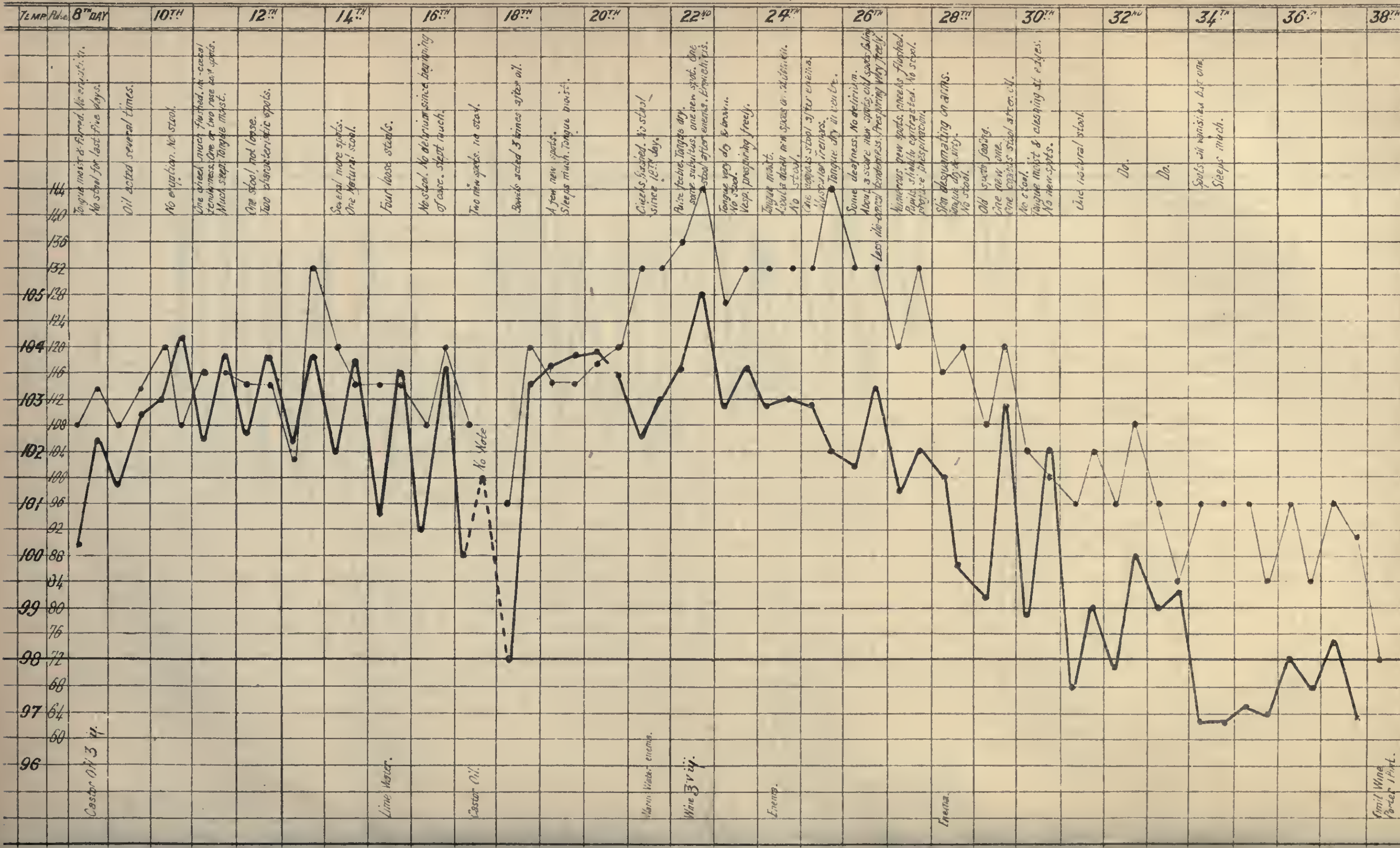
Diag. XXIII. G.B. Male. Æt. 17. Enteric Fever. Recov^d.

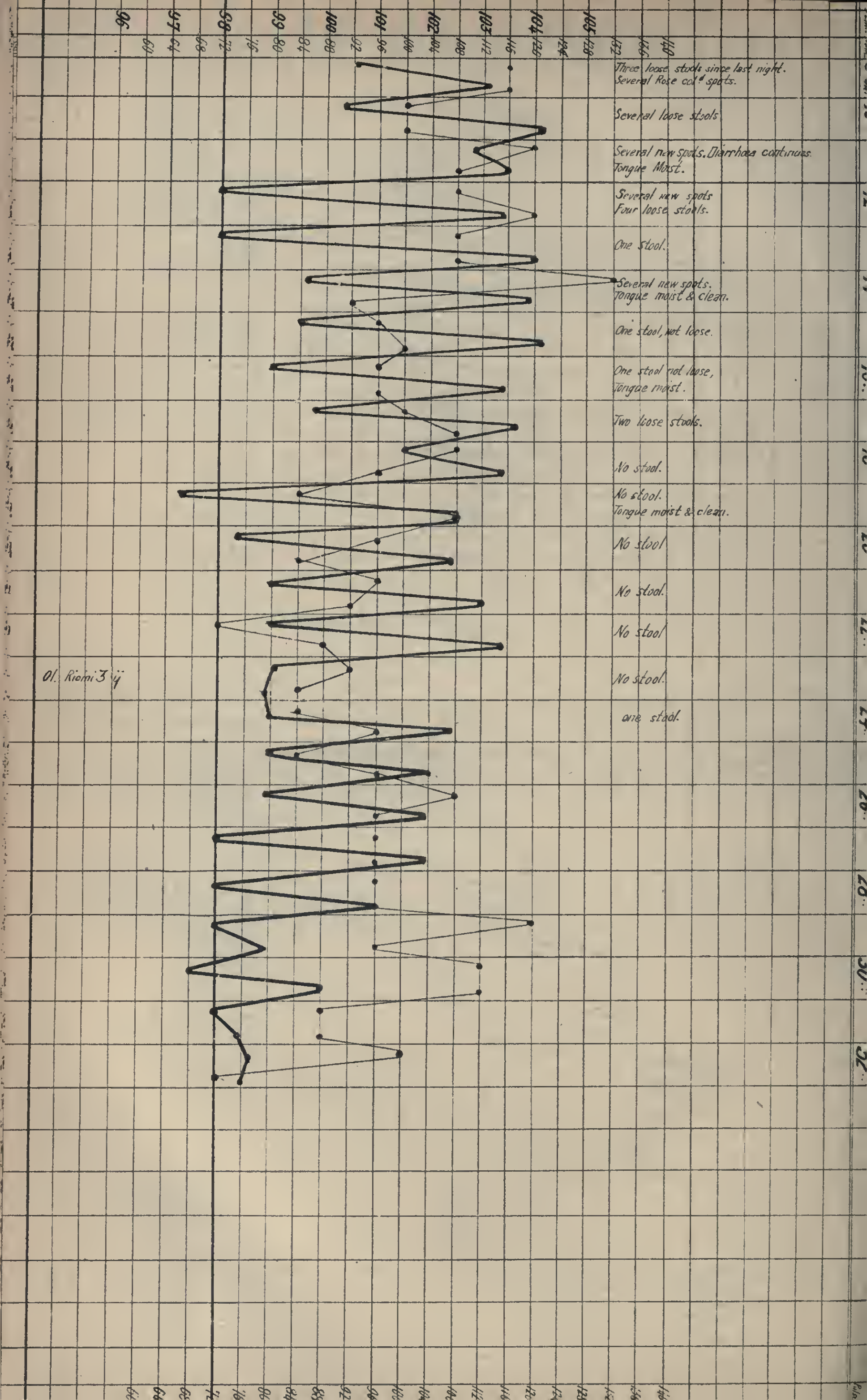


Diag. XXIV. A. S. Male. Æt. 22. Enteric Fever. Recov^d.



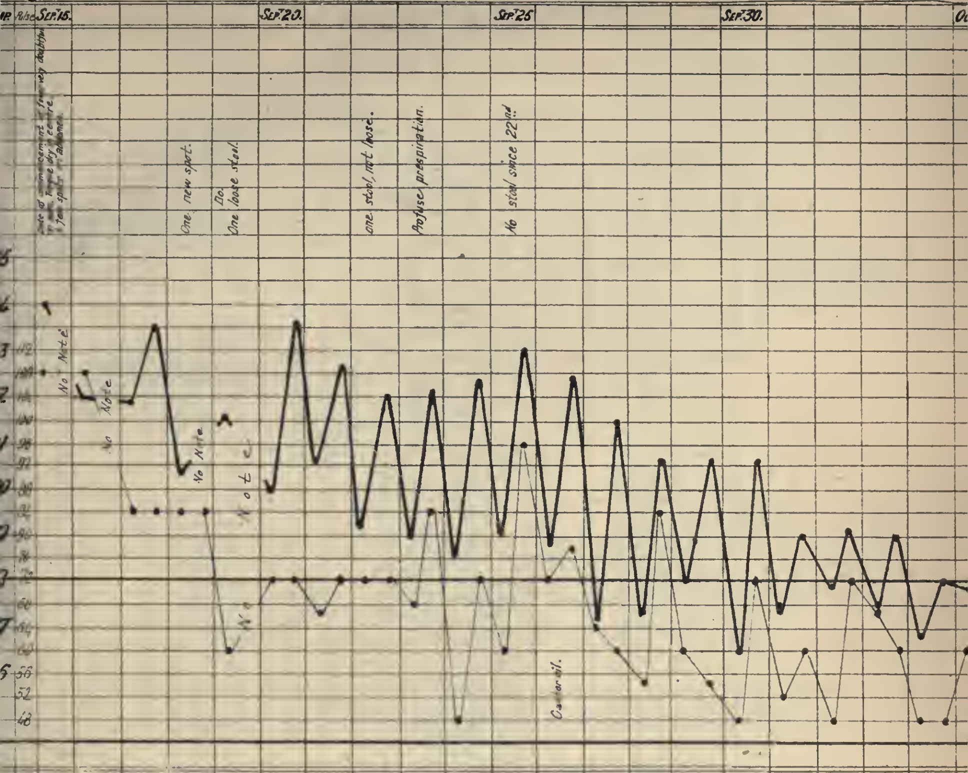
Diag. XXV. M.P. Female. Æt. 25. Enteric Fever. Recovd.



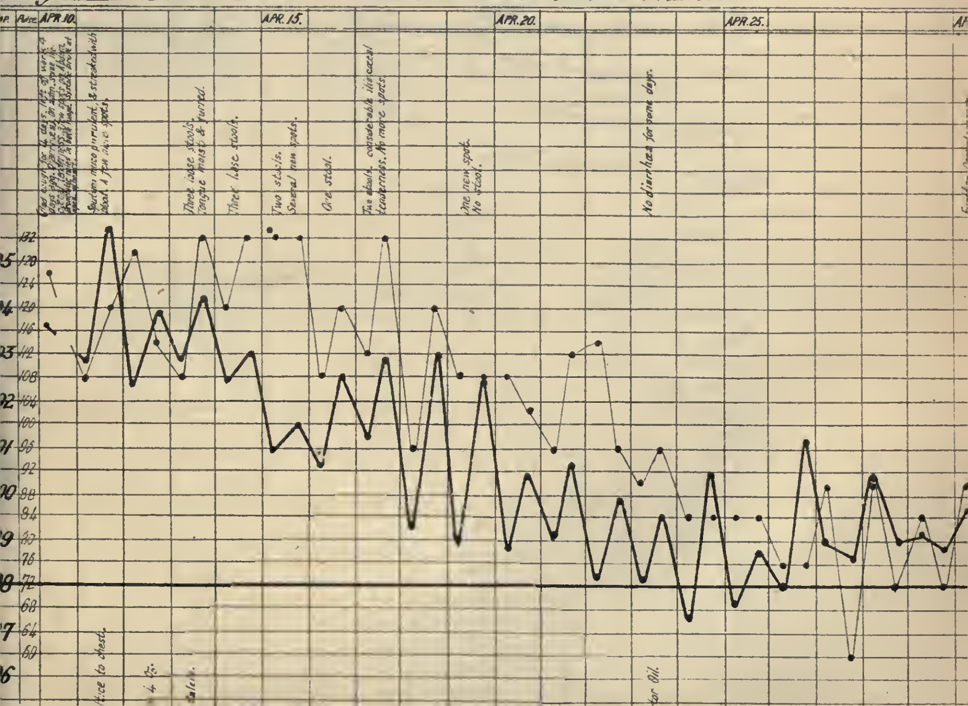


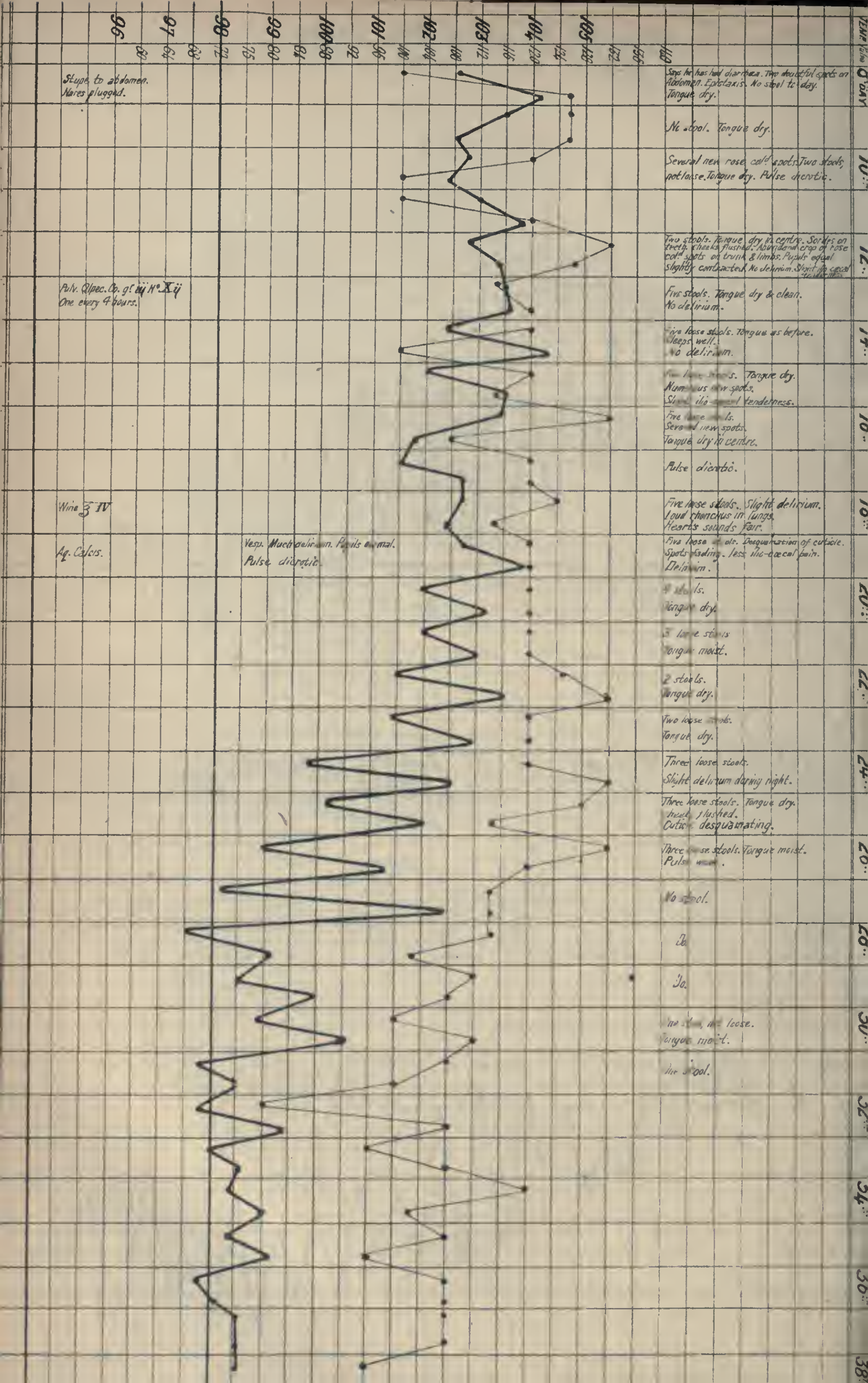
Three loose stools since last night.
Several Rose col'd spots.
Several loose stools
Several new spots. Diarrhoea continues.
Tongue Moist.
Several new spots
Four loose stools.
One stool.
Several new spots.
Tongue moist & clean.
One stool, not loose.
One stool not loose,
Tongue moist.
Two loose stools.
No stool.
No stool.
Tongue moist & clean.
No stool
No stool.
No stool
No stool.
one stool.
Ol. Ricini ℨ ij

Diag. XXVII. J.T. Male. Æt. 36. Enteric Fever. Recov.d



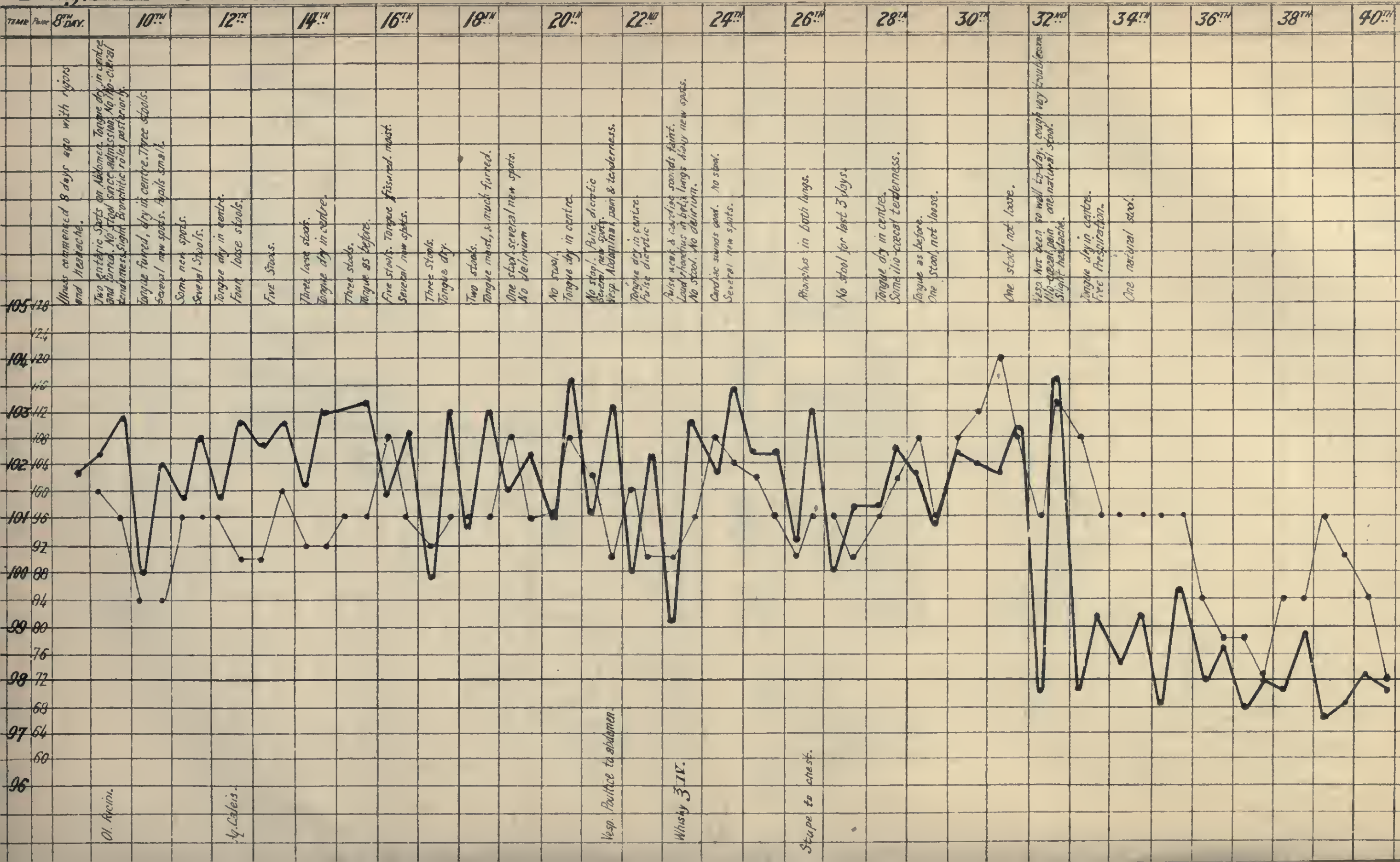
Diag. XXVIII. M.R. Female. Æt. 18. Enteric Fever. Recov.d



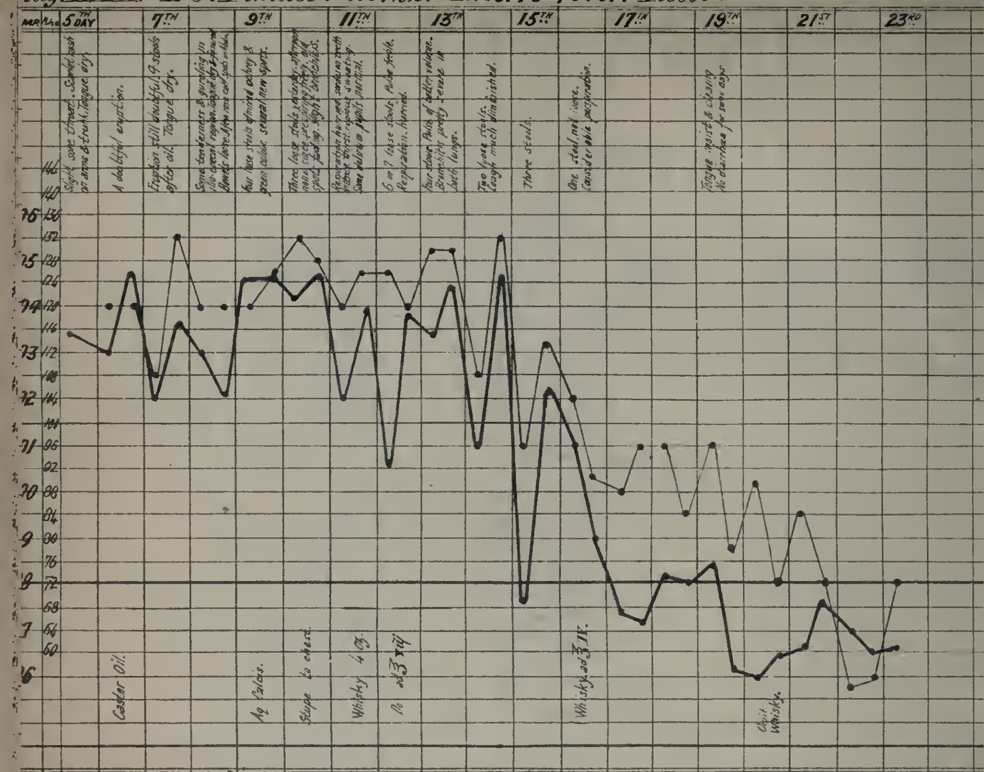


96 97 98 99 100 101 102 103 104 105
60 64 68 72 76 80 84 88 92 96 100 104 108 112 116 120 124 128 132 136 140
8 DAY 10 12 14 16 18 20 22 24 26 28 30 32 34 36 38
Stupe to abdomen. Nares plugged.
Pulv. Ipec. Co. gr. iij No. XII One every 4 hours.
Wine ℥ IV
Aq. Calcis.
Vesp. Much delirium. Pupils normal. Pulse dicrotic.
Says he has had diarrhœa. Two doubtful spots on Abdomen. Epistaxis. No stool to-day. Tongue dry.
No stool. Tongue dry.
Several new rose cold spots. Two stools, not loose. Tongue dry. Pulse dicrotic.
Two stools. Tongue dry in centre. Sordes on teeth. Cheeks flushed. Abundant crop of rose cold spots on trunk & limbs. Pupils equal slightly contracted. No delirium. Slight ilio-cæcal tenderness.
Five stools. Tongue dry & clean. No delirium.
Five loose stools. Tongue as before. Sleeps well. No delirium.
Five loose stools. Tongue dry. Numerous new spots. Slight ilio-cæcal tenderness.
Five loose stools. Several new spots. Tongue dry in centre.
Pulse dicrotic.
Five loose stools. Slight delirium. Loud rhonchus in lungs. Hearts sounds fair.
Five loose stools. Desquamation of cuticle. Spots fading. less ilio-cæcal pain. Delirium.
4 stools. Tongue dry.
3 loose stools Tongue moist.
2 stools. Tongue dry.
Two loose stools. Tongue dry.
Three loose stools. Slight delirium during night.
Three loose stools. Tongue dry. Cheeks flushed. Cuticle desquamating.
Three loose stools. Tongue moist. Pulse weak.
No stool.
Do.
Do.
One stool, not loose. Tongue moist.
One stool.

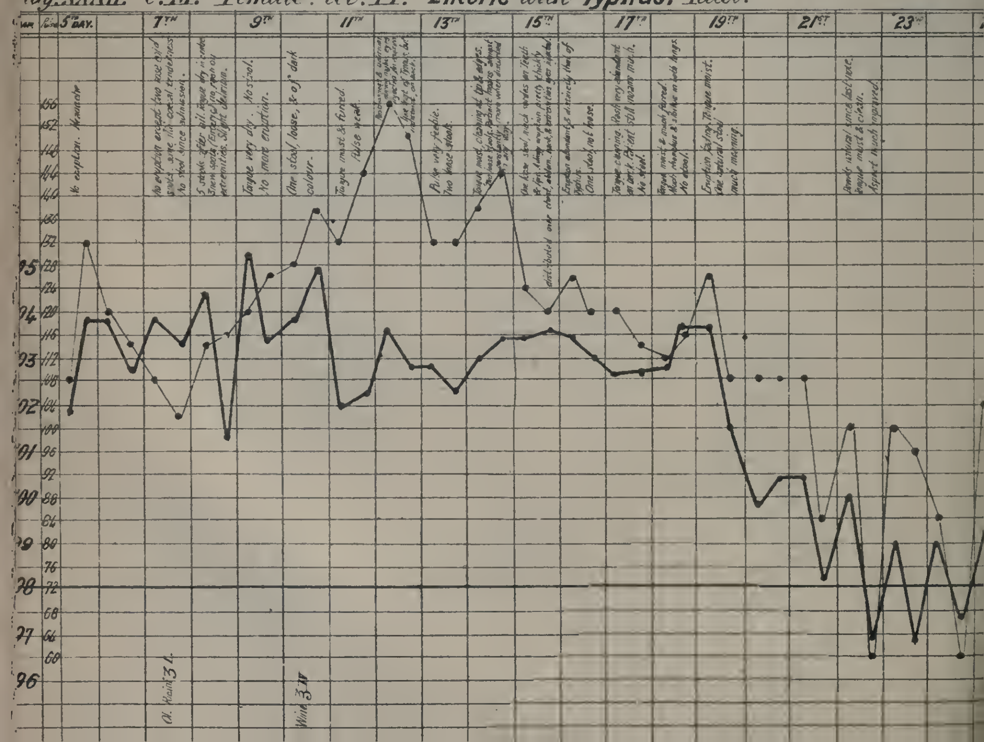
Diag. XXX. C.D. Male. Æt. 23. Enteric Fever. Recovd.



Diag. XXXI. T. S. Female. Æt. 22. Enteric fever. Recov.



Diag. XXXII. C. M.—Female. Æt. 14. Enteric with Typhus. Recovd.



I take this opportunity of expressing my thanks to Drs. Anderson and White for the trouble they have taken in observing and noting the cases.[1]

ART. II.

On the Properties and Products of the Toot Plant of New Zealand.[2] By W. LAUDER LINDSAY, M.D., F.R.S. Edin., Honorary Fellow of the Philosophical Institute of Canterbury, New Zealand, &c.

ALL the New Zealand species (4) of *Coriaria* appear to be more or less *poisonous,* the seeds and leaves especially being apparently the chief seats of the poisonous principle. The most usual poisonous form of the *Toot* plant—at all events in the eastern districts of Otago, which I visited in 1861—is the species that I have elsewhere described as *C. Tutu,*[3] the *C. ruscifolia* of authors. Dr. Hector, however, informs me[4] that in Otago *C. thymifolia* is considered the most poisonous of all the New Zealand *Coriariæ*. *C. arborea*[5] appears also to possess in abundance the poisonous alkaloid of the genus, as it occurs in New Zealand.

From the descriptions given me in 1861-2 of the action of the poison on man and animals [illustrations of which action I have supplied in my former Memoir in this 'Review'], I came to the conclusion that the said poison is of the nature of an *alkaloid,*[6] allied to, if not identical with, the active poisonous principle of the European *C. myrtifolia,* viz., *Coriamyrtine.*[7] If the poison of the New Zealand *Coriariæ* should yet prove so different from *Coriamyrtine* as to deserve separate nomenclature, I venture to propose for it the name *Tutuine* as appropriate, and under this name I will subsequently speak of it *provisionally* in this paper.

Desirous of extracting or isolating this poisonous alkaloid, in

[1] This paper was read before the Dundee Medical Society, 8th January, 1868.

[2] Supplementary to a memoir "On the Toot Plant and Poison of New Zealand," in this 'Review' for July, 1865, p. 153. Additional details relating to the *botanical characters* of the "Toot Plant" and its allies may be found in the author's 'Contributions to New Zealand Botany,' London and Edinburgh, 1868, genus *Coriaria*, p. 83; and in his memoir "On the Toot Poison of New Zealand," 'Proceedings of Sec. D, British Association,' 1862.

[3] 'Contributions to New Zealand Botany,' p. 84.

[4] Letter of October, 1865.

[5] 'Contributions to New Zealand Botany,' p. 84.

[6] Referring to this opinion, Dr. Hector wrote me (October, 1865) as to the "supposed *alkaloïd,* of the existence of which I have no doubt."

[7] *Vide* former paper in this 'Review,' pp. 162, 175.

order to render it the subject of physiologico-pathological experiment on the lower animals, and so to determine the character of its toxicological, and perhaps also therapeutic, action, I not only brought home considerable quantities of the dried plant, which were placed in the hands of a competent analytical chemist,[1] but I sowed under glass in the spring of 1863 *Toot* seed collected in Otago of the seasons 1861 and 1862. None of these seeds, however, came up; whether because of their age, or because they were not previously steeped in warm water, as was successfully done subsequently in the case of "Goai seeds (*Sophora tetraptera* Ait.),"[2] I have insufficient data for determining. Unfortunately all my efforts, and those of Professor Murray Thomson, failed in their main object—the elimination in a separate form of the *Tutuine.*

The government analyst of Otago, however, Mr. Skey, appears to have been more successful, with the greatly superior *local* advantages at his command.[3] He professes to have succeeded in extracting the "true alkaloid," which is supposed to be the source of the poisonous action of *Toot.* He failed to separate it by any of the recognised "standard processes" for the extraction of poisonous organic alkaloids; and succeeded only by means of a new process, wherein dry sulphate of soda was the prime agent of elimination. He obtained the *Tutuine,* however, in too small quantity to be available for experiment on its chemical composition or its physiological or toxic action. He found it in the *leaf* in as large quantity as in the ripe *fruit.* It is described as a very *bitter* principle, having "a pure bitter taste of great persistency" when dissolved in water. These experiments of Skey's are, however, far from being complete or satisfactory, though they will prove serviceable in paving the way for others of a more exhaustive kind.

In Otago, *Tutuine* is *supposed* to be analogous in its action to strychnine: "it has been used in epilepsy with supposed success" (Buchanan). In truth, however, nothing can yet be said to have been *determined* regarding its physiological or therapeutic action. It has yet to be proved whether it will be serviceable in medicine at all. My friend Dr. Irvine, of Nelson, says that Belladonna is the *antidote* of the *Toot* poison, but there is no sufficient evidence that this has been established as a *fact.* There are *anomalies* in the toxic or pathological action of *Toot,* which, however, are not singular, but find parallels in the action of *Amanita muscaria L.,* and certain other neurotic poisons; anomalies that have not yet been made the subject of proper

[1] *Vide* former paper in this 'Review,' pp. 175–6.
[2] *Vide* author's 'Contributions to New Zealand Botany,' p. 73.
[3] 'Jurors' Reports of the New Zealand Exhibition of 1865,' p. 428.

study by the lights of modern chemistry, physiology, and pathology.

The Toot plant continues to be, in all parts of New Zealand, the fertile source of accidents, fatal to *man*, as well as, on an enormous scale, to *cattle*. Fortunately, in the neighbourhood of the larger towns, which are surrounded by cultivation, *Toot* has now been more or less cleared away; and the accidents referred to are, therefore, becoming in and around these centres of cultivation more and more rare. This is especially true of the older towns, such as Nelson. But in and around the so-called "mushroom" towns, or "canvas" towns, that are constantly springing up in new and wild districts, following the successive gold-finds and gold-fields, the case is very different. Every now and then I still find recorded in the public prints the details of some coroner's inquest—generally relating to the death of a *child*, arising from eating some part of the plant. The jury generally requests the coroner "to bring under the notice of the proper authorities the very dangerous properties of the plant, with the hope that steps would be taken for its *total eradication*," which is indeed the *only* complete or certain means of preventing such accidents. The following newspaper account of one of such poisonings illustrates, sufficiently for present purposes, the general circumstances of such accidents. "An inquest was held at Cambridge [Waikato district, Auckland province] . . . . . on the body of . . . . . a little girl about four years and a half old. . . . . It appears that the child, while playing with some other children, picked up a piece of "*Tutu*" plant, which she ate of and offered some to her sister, who was a year older. The latter providentially did not partake of it, but her younger sister soon became ill, and was seized with violent convulsions; and, after lingering for about ten hours, died after much suffering. Dr. Sam presided at the inquest; and one of the witnesses who gave evidence was a *Maori* woman, who, having seen the symptoms displayed by the child, said she had eaten *Tutu*, and administered *salt*, which is invariably used by the *natives*, and with good effect if given in time."[1] The death of an *adult*, a gold digger at Hokitika, in two hours, from eating *Tutu berries*, is also recorded in the 'Lyttelton Times' [of Dec. 14th, 1865].

In the earlier days of settlement the so-called *berries* were made to yield a juice and a wine to the natives and colonists;[2] but the use of both seems to have been nearly entirely given up in consequence of the unenviable notoriety the *Toot* plant has acquired as a fatal poison. Dr. Geo. Bennett, of Sydney,

[1] 'Southern Cross,' Auckland, New Zealand, October 28, 1865.

[2] *Vide* also former paper in this 'Review,' pp. 161–2.

relates that the Auckland natives were careful to *strain* the juice of the berries, so as to separate the poisonous *seeds*. They soaked their baked fern-root[1] in this juice, or they drank the latter by itself.[2] The Bay of Islands Missionaries also "make an agreeable *wine* . . . . . which tastes like that made from elderberries."[3]

Simmonds says,[4] "The New Zealanders *used* to prepare a *Seaweed jelly called 'Tutu;'* but, like many other articles formerly employed by the natives, either as food or delicacies, it is now seldom or never seen. The preparation was, I am informed by Mr. Chas. Hursthouse, chiefly confined to the natives along the northern shores of Cook's Straits." My friend, Mr. Simmonds, here evidently confounds two quite distinct substances, and he is not the first compiler that has done so, viz., the *jelly of a Seaweed,* of the nature of "Carrageen," and the *juice of the berries of the "Tutu"* [species of *Coriaria*]. I have elsewhere shown that New Zealand contains at least one marine alga capable of yielding a jelly of the "Carrageen" class;[5] and in my former paper in this 'Review' I referred to the association of the *juice* of the *Toot* berry with seaweed *jelly* as a delicacy of the blancmange order.[6] Dr. Thomson says,[7] "*Almost all* the seaweeds *were* occasionally eaten" by the natives; but this must have been after the manner of our "Dulse." In another part of his work on New Zealand, he remarks, "*Several* . . . . seaweeds *are* edible; *one* of the latter, *with the juice of Tutu berries,* is converted into a *jelly*."[8] I am somewhat at a loss to determine what amount of credence to give to some of Dr. Thomson's assertions as to the *food* of the natives; for in the same place[9] he mentions *lichens* as edible, a fact whereof I have elsewhere found no record.[10]

All the New Zealand *Coriariæ* appear to abound more or less, and in every part of the plant, in *Tannic acid,* or in some of the allies of this acid—some, perhaps new, astringent acid, belong-

[1] *Vide* paper by author on "Otago Ferns," 'Trans. Botanical Society of Edinburgh,' vol. ix, p. 40.

[2] Dieffenbach ('Travels in New Zealand,' vol. ii, p. 50) says that the Maoris drink the juice *un*fermented; a statement which contradicts that made in my former paper in this 'Review,' p. 161.

[3] 'Wanderings in Australia and New Zealand.'

[4] 'Waste Products and Undeveloped Substances,' p. 216.

[5] *Vide* paper by author on "New Zealand Carrageen," 'Trans. Botanical Society of Edinburgh,' vol. ix, p. 137.

[6] Pp. 161–2.

[7] 'Story of New Zealand,' vol. i, p. 19.

[8] Ibid., p. 155. The juice obviously, however, only imparts flavour, taste, and colour; it does not assist in the formation or "setting" of the jelly.

[9] Ibid., p. 155. "Several *Mosses*, Fungi, *Lichens*, and Seaweeds are edible."

[10] I wrote to Dr. Thomson on this subject in 1862, but never received a reply. I have failed also in other quarters in eliciting information on this point.

ing to the *tannic* series. Astringent principles abound equally in *C. arborea*, the "*Tree Toot*," and in the more familiar herbaceous *Toot* (*C. tutu*). Hence the economic properties of the former promise to be the same virtually as those of the latter. In the New Zealand Exhibition of 1865 (Nos. 724-5 of 'Catalogue') specimens of Tannate of Quinine were shown, the tannic acid whereof was prepared from the indigenous "Toot." Astringent principles seem to pervade the genus *Coriaria* wherever distributed. The *root* of the European form *C. myrtifolia* is largely used for *tanning* purposes in Russia; and there is every reason for supposing that some at least of the New Zealand species may be applied to similar purposes.

Skey has made the *Tannin* of the *Toot* plant of Otago the subject of careful experiment. Among his results are the following:[1]—

Tannin abounds in the following parts of the plant:—Leaf, root, calyx, seed, flower-stalk, old wood and its pith. The parts of the plant richest in tannin are, in the order of their richness,—

1. Young leaves = maximum, from 0·30 to 2·08 per cent.; average, 1·35 per cent.
2. Old shoots.
3. Root.
4. Old wood.

The per centage is much higher in the *dried* than in the fresh state, varying from 2·14 to 8·32 (average 4·45) in the former. There is no perceptible loss of tannin by drying the plant at 130° to 150°, whence it is inferred that the small branches could be dried and stacked like oak-bark without losing any of their tanning properties. So abundantly does *Toot* contain material available for tanning, and so common is the plant in many, if not most, parts of the colony, that it is strongly recommended to the notice of the local tanner as a competitor to, or substitute for, the materials currently in use. In reference, however, to any future applications of Toot-products in the arts or in medicine, it must be borne in mind what I have had occasion to point out in reference also to the indigenous *Flax* (species of *Phormium*),[2] that future permanent market supplies must depend on the *cultivation* of the plant, inasmuch as the wild plant is rapidly hastening to its inevitable doom—scarcity or extinction —in that "struggle for existence" that long ago began with the hardier immigrant weeds of Europe.

Skey's experiments leave many points yet undetermined in the natural history both of the poisonous and astringent principles of the New Zealand *Toot* plant, and *inter alia*:

[1] 'Jurors' Reports of New Zealand Exhibition of 1865,' p. 427.

[2] "On the Obstacles to the Utilisation of the New Zealand Flax" (*Phormium tenax*). 'Seemann's Journal of Botany,' vol. v, 1867, p. 342.

I. *Tutuine.*

A. What are its precise nature, its chemical composition, and chemical or physical properties?

B. What is its mode of action on the human and animal system—physiologically, pathologically, or therapeutically?

C. Can it, and under what circumstances, be applied in medicine?

D. What is its appropriate antidote in man and animals?

E. What treatment is indicated as likely to be most serviceable under different classes of circumstances [man or animals, genus or species, age and sex, &c.], if no specific antidote exists?

II. *Tannin.*

F. On what precise modification of tannic acid, or on what ultimate principle, does the astringency of *Toot* depend?

G. To what extent can such principle or principles be rendered available in the arts or in medicine?

III. *Tutuine and Tannin.*

H. In what species or varieties—in what parts of the plant, and at what period of growth—are they in greatest quantity and of finest quality?

I. To what extent are they modified in quantity or quality by the *cultivation* of the plant?

J. What is the simplest and easiest mode of extraction—having regard to the quantity and quality of the product?

In my former paper[1] in this Review, I stated that *Toot,* while poisonous to certain animals, is *innocuous* to certain others; and I showed that the same may be said of several other of the most deadly vegetable poisons with which man is acquainted. In works of travel I not unfrequently meet with instances similar to those I have already narrated; and I believe, indeed, they might be largely multiplied. The subject is one of such importance in connection with physiologico-pathological experiment on the lower animals[2]—a department of the modern

[1] Pp. 161, 169, 170, 171, 175, 176, 177.

[2] Many years ago, while experimenting on the antidotal inter-relations of certain vegetable poisons, I pointed out the fallacy of supposing that substances poisonous to *man* were equally, or necessarily at all, so to the lower animals; or of drawing any inferences from the action of a given poison on a certain animal-genus or species regarding its probable action on any other genus or species—especially belonging to a different group ('Association Medical Journal,' June 9, 1854).

science of "comparative pathology,"[1] which is at length beginning to attract attention from the younger and more intelligent aspirants for fame in our profession—that I offer no apology for here transcribing one or two additional instances lately encountered in my miscellaneous reading.

Speaking of Abyssinian plants, Sir Samuel Baker remarks as follows:—Of *Asclepias gigantea,* whose stem and leaves yield a highly poisonous milk, "Although the poisonous qualities of the plant cause it to be shunned by *all other* animals, it is nevertheless *greedily devoured by goats, which eat it unharmed.*"[2] Of the fruit of the "Hegleek" tree (*Balanites Egyptiaca*), which produces diarrhœa in man [if eaten raw and in quantity], while boiled with honey it forms a harmless and pleasant preserve: "Elephants are particularly fond of the fruit [*i. e.,* berries] of the 'Hegleek;' they enjoy them beyond any other food, and they industriously gather them one by one. At the season when the fruit is ripe, the 'Hegleek' tree is a certain attraction to elephants, who shake the branches, and pick up the fallen berries with their trunks; frequently they overturn the tree itself as a more direct manner of feeding."[3] And of the familiar medicinal "*Senna*" plant:—it is "rich in a pale green foliage, which is a strong temptation to the hungry camel. Curiously, this purgative plant is the animal's *bonne bouche,* and is considered most nourishing as fodder."[4]

Dr. Hector writes me,[5] in reference to a statement in my former paper[6] regarding the food of the rare bird, the "Kakapo," "On what authority do you state that the 'Kakapo' eats the *Tutu* berries? Not that I doubt they would eat *them* or anything else almost: but the 'Kakapo' does not abound along with *Tutu* as a rule, but in pine and birch forests." *My* authority is Dr. Haast, whose experience as a New Zealand explorer is unrivalled, and whose knowledge of the habits of its native birds can be inferior to that of no other naturalist. *He,* however, again in this instance derives *his* authority or information from the *Maoris,* whose testimony on such a subject I regard as

[1] *Vide* paper by the author on the "Transmission of Diseases between Man and the Lower Animals." 'Edinburgh Veterinary Review,' July, 1858.

[2] 'Nile Tributaries of Abyssinia,' 1867, p. 31.

[3] Ibid., p. 369.

[4] Ibid., p. 73. Son e Notanda on the varying effects of certain poisonous plants on the quality of the flesh of *fish* used as human food will be found in a memoir by M. Auguste Duméril, on "Venomous Fishes," in the 'Annales de la Soc. Linnéenne du Département de Maine-et-Loire' for 1866; or, translated, in the 'Annals of Natural History,' vol. xx (1867), p. 153. The reader may also consult with advantage the following two papers:—"On the Innocuousness of *Belladonna, Datura,* and *Hyoscyamus* to Rabbits," Runge, 'Journal de Pharmacie,' vol. x, p. 85; and "On the Comparative Immunity of Rabbits to the Poisonous Action of Atropine," by Dr. Wm. Ogle, 'Medical Times,' May, 1867, p. 466.

[5] October, 1865. [6] P. 170.

unimpeachable. Speaking more especially of the western and wild districts of the province of Nelson, he writes in 1860, "The 'Kakapo' lives in holes burrowed in the ground, where it remains during the day, coming out in the night. It feeds on *berries* and roots. Although able to fly, it rarely or never takes to the wing, as the *natives* assured me, who in former years *often hunted* it. For this purpose they generally went to the *plains*, when the berries of the *Tutu* (*Coriaria sarmentosa*) were ripe, which *are a favorite food of that bird*, selecting fine moonlight nights. They ran them down partly with dogs, or even killed them with long sticks *upon the Tutu bushes*."[1]

Art. III.

Table of Cases, with Weights of the Bodies and Lungs of Live and Stillborn Children. By Alexander Ogston, M.D. Aberdeen.

No.	Sex.	Live or still born.	Condition when examined.	Weight of body in grains.	Weight of lungs in grains.
1	Male.	Live birth.	Fresh.	58,625	940
2	,,	,,	,,	53,812·5	1421
3	Female.	,,	Decomposed.	31,500	420
4	Male.	Still birth.	Fresh.	33,410	494
5	Female.	,,	,,	15,968	488
6	,,	Live birth.	,,	44,515·6	568
7	Male.	,,	Decomposed.	54,031·2	1200
8	,,	Still birth.	Fresh.	14,054·7	300
9	Female.	,,	Decomposed.	47,468	410
10	Male.	,,	Fresh.	19,250	690
11	,,	,,	Decomposed.	45,937·5	640
12	,,	Live birth.	Fresh.	54,250	1205
13	Female.	Still birth.	Decomposed.	8,750	200
14	,,	Live birth.	Fresh.	45,718·7	730
15	,,	,,	Decomposed.	35,000	706
16	Male.	,,	Fresh.	56,437·5	736
17	Female.	Still birth.	Decomposed.	45,062·5	640
18	Male.	,,	Fresh.	20,125	540
19	Female.	Live birth.	,,	56,617·5	859
20	,,	Still birth.	,,	31,937·5	589
21	Male.	,,	,,	13,125	120
22	,,	,,	,,	57,750	1103
23	Female.	Live birth.	,,	37,625	745
24	,,	Still birth.	,,	51,843·7	1315
25	,,	,,	,,	15,968·7	507
26	,,	Live birth.	,,	37,625	726
27	,,	,,	Decomposed.	24,937·5	655
28	,,	,,	Fresh.	44,187·5	746

[1] 'Report of a Topographical and Geological Exploration of the Western Districts of the Nelson Province, New Zealand, undertaken for the Provincial Government.' Nelson, 1861, p. 138.

No.	Sex.	Live or still born.	Condition when examined.	Weight of body in grains.	Weight of lungs in grains.
29	Male.	Live birth.	Fresh.	46,932·5	816
30	"	"	"	32,998·5	640
31	Female.	"	"	37,625	661·5
32	"	"	"	42,875	600
33	"	"	"	44,625	1008
34	"	Still birth.	Decomposed.	16,734·3	470
35	Male.	Live birth.	Fresh.	51,625	723
36	"	"	"	39,648·5	1220
37	Female.	"	Decomposed.	41,545	890
38	"	"	Fresh.	47,468·8	761
39	"	"	"	38,500	709
40	Male.	"	"	35,000	464
41	Female.	"	"	49,000	858
42	Male.	"	Decomposed.	49,546·7	660
43	Female.	"	Fresh.	48,125	856
44	"	"	"	47,507·5	774
45	"	"	"	43,750	643
46	"	"	"	40,796·7	960
47	Male.	"	"	45,500	781
48	Female.	"	"	47,468·7	1057
49	Male.	"	Decomposed.	49,000	680
50	"	"	Fresh.	42,000	1127
51	Female.	"	"	31,500	990
52	"	"	"	32,812·5	810
53	"	"	"	57,750	1035
54	Male.	"	"	53,375	1046
55	"	Still birth.	"	15,750	488
56	Female.	Live birth.	"	42,000	692
57	"	"	"	46,593·7	865
58	"	"	"	43,750	690
59	Male.	"	"	56,000	920
60	Female.	"	"	56,000	1029
61	Male.	"	"	55,562·5	875

Out of a total of eighty-one cases, in which the question as to the live or still birth of a child had to be answered by the medical jurist to the law authorities from a post-mortem inspection of the bodies of the children, and which are recorded in the medico-legal reports of Professor Ogston, University of Aberdeen, there are sixty-one in which the particulars as to *the weight of the body and lungs* are given, as well as every other information necessary to be provided with in judging of the *value* of these weights as corroborative evidence in the question of live or still birth. More than corroborative they certainly cannot be, the condition of the lungs is in competent hands an unfailing means of answering this important question, and the variation in the weights of body and lungs, as well as of their relations to each other, are too great and too general to admit of such a claim.

The above sixty-one cases, drawn up in the form of a table, give us, however, some results which it may be well to remember in judging of such cases. They are given in the order in which they occurred in practice, with the exception of the first, which is drawn from the practice of a physician in a neighbouring county. In deciding as to live or still birth, the condition of the lungs was the evidence relied on. In the column relating to freshness or putridity, the condition of the body was not so much taken into account as the state of the lungs themselves, for it is well known that the lungs are not the organs of the body soonest affected by decomposition, and a commencing decay of the body does not at first affect the lungs at all. The weights of the bodies were taken in imperial pounds, ounces, and parts of ounces, which explains the decimals of grains so often occurring in this column. The lungs were weighed by apothecaries' weight, excepting Case 31, where imperial weight was used. In the above table the weights have been reduced to grains for the sake of greater ease in comparison.

Out of the sixteen cases of still birth, eight are males and eight females. The aggregate weight of the lungs in the males is 4375 grains, of the bodies 219,402·2 grains, or a proportion of 1 : 50·103. In the females, lungs= 4628, and bodies 233,723·7 grains, or 1 : 50·502.

In the live births, where the lungs are fresh, a total of thirty-eight cases shows fourteen males, with the total weight of the lungs 12,914, and of the bodies 681,767 grains, or 1 : 52·792, and twenty-four females with the total weight of the lungs 19,390·5, and of the bodies 1,064,437·2 grains, or 1 : 54·847.

In live births, where the lungs were decomposed, out of seven cases three were males and four were females. The males have the total weight of the lungs 2540 grains, and of the bodies 143,577·9 grains, or 1 : 56·526; the females having the total weight of the lungs 2671, and of the bodies 132,982·5 grains, or 1 : 49·787.

Now, comparing these figures, we find the proportion of the weight of the lungs to that of the body to be, disregarding sex—

In still births.	In decomposed live births.	In fresh live births.
1 : 50·302	1 : 53·156	1 : 53·819

And in all live births, disregarding the state of freshness or decomposition, 1 : 53·487.

This leads us to the result that, *although common sense indicates an increase of absolute weight in the lungs after inspiration, and consequently an increased ratio of weight to that of the body*, in the above table of cases the weight of the lungs to that of the body is greater in *still* than in *live* births—just the reverse of what might have been expected. Why this should be so is not to be explained, but the conclusion to which we are driven is, that in deciding between live and still birth the ratio of the weight of the lungs to that of the body is quite worthless.

Looking next at the *absolute* weight of the lungs, great differences present themselves. In one case of still birth the lungs weighed 1315 grains, and in one case of live birth they weighed only 420 grains. But in the general run of live births the weights of the lungs are greater than in the generality of still births; and the averages bring this out in a pretty marked degree, for we have—

Average weight of	still-born lungs		= 562·6
„ „	live-born decomposed lungs		= 744·4
„ „	„ fresh	„	= 850·1
„ „	all live-born lungs		= 833·6

The average absolute weight of still-born lungs being thus about five eighths of that of live-born lungs; whence we conclude that, although in any one given case the absolute weight of the lungs is inadequate to decide the question of live or still birth, still, as corroborative evidence, it is not without value, and may be allowed a certain amount of weight along with the other uncertain signs of live birth in deciding this often very important question.

Art. IV.

On Hæmodynamics. By W. Handsel Griffiths, Ph.D.

In the latter part of the year 1865 I had the honour of reading before the Royal Irish Academy a paper descriptive of an instrument which I proposed as a substitute for, and an improvement on, the hæmodromometer of the celebrated Volkmann. From that date up to the present time I have devoted myself almost exclusively to the perfection of my instrument, and to making experiments with it, and I trust that the results, which I now publish for the first time, will prove an acceptable contribution to physiological literature.

Before proceeding to give an outline of my researches, I think it advisable to state some modifications in the design of my instrument, which I have lately adopted.

Dr. Robert Macdonnel, of Dublin, the well-known physiologist, kindly suggested to me that the method of heating the support of my apparatus could be more easily and better accomplished by means of hot water than by the use of the metallic heaters. The suggestion was very valuable, for the cost of the instrument would thus be lessened, and a more equable temperature would be gained than by my method.

I am not less grateful to Dr. Macdonnel for another suggestion, which however I have not adopted. He recommended that a *circular* index tube should be employed instead of the oblong one which I had designed. Anxious to test the value of this suggestion, I made repeated experiments with the circular tube, but was invariably dissatisfied with the result; the course of the index-ball was not so free as in the original tube, and the circulation was in some measure retarded.

In the course of a lengthened correspondence with many eminent physiologists concerning my instrument, a source of failure was frequently anticipated in the event of the index-ball sticking, and Dr. Sharpey particularly drew my attention to this point. After a series of some 300 experiments, I am happy to say that I have never had to suspend an operation from this cause, nor have I reason to think that one experiment was thus invalidated. My objects in describing my instrument previous to detailing experiments made with it were—first, in the hope that my brother physiologists would have instruments made on my design, so that when my own experiments were published, they might be compared with those of other observers; and, secondly, I hoped to draw the attention of physicists to the mechanism of the instrument, so that I might profit by their criticism, and eventually produce an accurate and reliable hæmodromometer. Whether my first object has been fulfilled I am not in a position confidently to state, but I have reason to believe that several continental physiologists have studied the subject, and that the results I have herein mentioned will meet with corroboration at their hands.

I have forwarded copies of my paper on hæmodromometers to most of the eminent physicists, and it is a matter of no little pride to me, that no further strictures have been made on my instrument other than those I have just alluded to. It has indeed been objected, that my instrument is somewhat complicated, and that its use requires delicate manipulation. On seeing the instrument in work, however, no such objections could be urged, and it is but right to state that they have been only advanced theoretically.

Before proceeding further, I should remind my readers that I merely purpose in this paper to give an outline of the general results I have experimentally obtained, and to describe in general a few of the more important of my experiments. Hereafter I hope to publish a complete register of my researches, so that future experimentalists may have a standard of comparison.

The first animal subjected to experiment was a healthy dog about one year old. The animal was securely bound, so that by its exertions it could not interrupt the course of observation. The right carotid artery was exposed for about an inch and a half, and the instrument adapted in the manner I have detailed in my former paper. The operation was perfectly successful, no exertions were made by the animal, which had been fed plentifully about half an hour previous to the experiment. The velocity with which the index-ball was propelled was at the rate of 425 millimeters per second!

With reference to this experiment I wish it to be particularly noticed, that the dog was twelve months old, in good health, and about sixteen inches high; that the time of experiment was noon, and that the animal had shortly before partaken of a full meal.

The subject of my second experiment was a bitch which had been born of the same parents, and at the same time as the dog previously operated on. Its size was exactly the same, the time of experiment was noon, and the animal had been fed previously to operation. The velocity of the circulation in the right carotid in this instance was at the rate of 448 millimeters per second.

The same animals were again submitted to experiment two weeks after the above operations, but the conditions under which they were now operated on were different. The dog was on this occasion placed on the operating table in a state of faintness from hunger; his left carotid was exposed, and the instrument adapted. The rate of velocity I estimated at 340 millimeters. The bitch was on the same day operated upon in the same manner and under the same conditions, and the index-ball showed a velocity of about 380 millimeters. I should mention here, that in all these experiments the animals were confined to the horizontal posture. The results of these inquiries would seem to point to the conclusions, that the velocity in the carotid of a dog averages about 400 millimeters per second, that the rate of velocity is greater in the female than in the male, that the velocity is decreased by hunger, and that the effect of hunger on velocity is more apparent in the female than in the male.

I can only say that, after an experience of over 100 experiments similarly performed, I am convinced that these conclusions are correct. Another dog, of same size, weight, and age, was submitted to experiment about an hour after a full meal. Instead of placing him in the horizontal posture, he was operated on while bound to a perpendicular support. On testing the velocity of the circulation in the right carotid, I found it to be about 380 millimeters per second.

The conclusion to be drawn from this experiment, and which is warranted by the results of seven other trials, is that posture affects velocity, and that it is greater in the carotid while the animal is in the horizontal position.

The same dog was similarly operated on in about a week afterwards, the only variation in the experiment being that the animal was placed head downwards. The left carotid was experimented on, and showed a velocity of about 470 millimeters.

This experiment was several times repeated, and the conclusion to be inferred is that velocity in the carotids was greater while the animal was in this posture than while in the horizontal or erect position.

My next inquiry was directed to the velocity of circulation as affected by age. A pup, one month old, was operated on, the right carotid opened, and the velocity taken; it proved to be about 460 millimeters. An animal was next submitted to experiment whose years numbered twelve. The velocity of the circulation in the right carotid, in this instance, was measured at about 300 millimeters per second.

This proves indubitably that age affects velocity to a remarkable degree. I should mention here that age in the female does not influence the velocity to the same extent as in the male. This fact I have gathered from about twelve experiments.

The question next suggested itself, does size affect velocity? The following are two of the experiments undertaken to solve the query.

The right carotid of a dog nearly three feet in height and two years of age was opened, and showed a velocity of 480 millimeters. A dog of same age, but only eighteen inches high, was next operated on under similar conditions, and the velocity was ascertained to be about 420. This result was contrary to my expectation, for I expected to find a greater variation in velocity according to size. I am, however, now convinced, from the result of six experiments, that size does not affect velocity to any very great extent. It now remained for me to ascertain whether velocity varied according to the time of day. A dog, about eighteen months old, was operated on at 6 a.m., and the velocity of the current in the right carotid found to be at the rate of 460 millimeters per second. The left carotid of the same dog was opened at 10 p.m. on the same day, and the velocity ascertained to be only 380 millimeters. I may mention that no blood was lost by the animal during the first operation, and that the dog had been moderately fed about an hour previous to experiment. Seventeen experiments of this nature were performed by me, and I invariably found that velocity was at its maximum about 6 a.m. or 7 a.m., and at its minimum between 7 p.m. and 10 p.m. I now performed a series of experiments to ascertain the effect of various medicinal agents on the velocity of the blood's current, a few of the more remarkable of which I may here detail.

A healthy dog was anæsthetised by inhalation of chloroform after the velocity of the circulation in the right carotid had been ascertained to be 420 millimeters per second. While under the influence of chloroform the left carotid was opened, and I was not surprised to find that the velocity suffered a considerable decrease, showing only a rate of 300 millimeters.

Another dog, somewhat similar to the above, with a natural velocity in the right carotid of 400 millimeters per second, was found to have that velocity reduced to 330 millimeters, while the animal was under the influence of narcotism by opium.

A dog of rather large size, with a velocity in the right carotid of 430 millimeters, was subjected to alcoholic stimulation. The velocity in the left carotid was taken while in that state, and the result showed a rate of 550 millimeters nearly. On the stimulation being carried further, the rate was over 600 millimeters; and, lastly, when the stage of stupefaction had been induced, the rate decreased rather quickly to 430 millimeters per second.

Hitherto I have spoken only of the rate of circulation in the carotids of dogs, inasmuch as most of my experiments were performed on these animals. I have, however, subjected rabbits and cats to experiment, in the same manner and under similar circumstances. As a general result, I may state that the average velocity in carotids of rabbits of one year old I found to be 350 millimeters in the second, and that in the carotids of cats of the same age the rate was about 400. From this the question would naturally arise as to whether velocity varies according to the nature of animals. I regret to state that I have not worked out that question sufficiently to warrant my giving an opinion on it.

I had one and only one opportunity of examining the rate of velocity in the arteries of a horse. It is a matter of much regret to me that I have had no other opportunity of experimenting on horses, for the one to which I now allude was in such an unhealthy and unnatural state from hard work, bad feeding, and old age, that I am reluctant to record the results I obtained, as I believe that they cannot be accepted as affording a good example of average rate. The velocity of the current in the right carotid measured only 370 millimeters, that in the metatarsal artery showed a velocity of 78 millimeters, and in the maxillary artery 126 millimeters per second.

Of course my experiments were not confined to the carotid arteries, and I now proceed to state briefly a few of the general results of experiments on the principal vessels. I should mention that dogs were the subjects in all the following cases.

Axillary Artery.—As the result of twelve experiments on the axillary arteries of dogs, I would state the average velocity in this artery to be about 430 to 440 millimeters per second.

Brachial Artery.—The brachial artery of seventeen dogs was opened, and the average velocity found to range between 400 and 430 millimeters.

Radial and Ulnar Arteries.—These arteries were experimented on in eight cases, and the velocity in each was generally from 360 to 400 millimeters in the second.

Femoral Artery.—The femoral was opened twelve times, and the velocity found to be very nearly equal to that of the axillary.

Popliteal Artery.—The popliteal was operated on in only four cases, and the velocity in each case was 410, 415, 425, and 400 respectively.

Tibial Artery.—This artery, in seven cases, showed an average velocity of 330 millimeters.

Metatarsal.—Five arteries were opened, and showed a velocity of between 210 and 250 millimeters.

On one occasion I attempted to take the velocity of the abdominal aorta, but the experiment was unsuccessful.

From my observations I am enabled to corroborate the statement of Volkmann respecting the jerking character of the arterial currents. I was also led on several occasions to put to the test of experiment Volkmann's assertion that the velocity is lessened by loss of blood. On three occasions dogs were submitted to experiment about an hour after severe depletion. In one case the velocity of the current in the right carotid showed a rate of only about 270 millimeters. In the other case the velocity was about 320, and in the last instance the rate per second was 345 millimeters.

I have instituted several other original observations, with a view to determine the effect of the heart's action on the velocity. I, however, decline to publish the results until I have studied the subject more extensively.

It will be seen, on a reference being made to the writings of Volkmann, that the results obtained with his instrument differ considerably from those afforded by mine. I am indebted to Dr. H. Bence Jones for a very valuable suggestion, whereby I was enabled to test the exact differences afforded by experiments with the two instruments. He proposed that I should insert my hæmodromometer into the artery of one limb, and a Volkmann's instrument into the corresponding artery of the other limb.

Last year I followed this method of experiment:—A dog about two years and a half old was bound to the operating table. The left carotid was first opened, and circulation established through my instrument. The right carotid was then exposed, Volkmann's instrument adapted, and the results noted. Volkmann's instrument showed a velocity of 320 millimeters per second, whereas the velocity according to my instrument reached the rate of 430 millimeters! The instruments were now reversed—Volkmann's instrument being inserted into the left carotid and mine into the right. The velocity, according to Volkmann's apparatus, was 325 millimeters; and, according to mine, it was very nearly 440 in the second. This experiment was quite sufficient to demonstrate the fact, that a much greater velocity would be registered by my instrument than by Volkmann's, and the result of many subsequent trials on the same plan were abundantly confirmatory.

The experiments which I have made on the circulation in the veins have not been nearly so numerous as those performed on the arteries, inasmuch as I was anxious to study the velocity of arterial circulation thoroughly, before turning my serious attention to that of the veins.

The only veins I operated on were the external jugular, the basilic, and the femoral.

In the external jugular on two occasions I found the velocity to average 400 millimeters per second. The femoral was five times operated on, but showed some variation in the velocity; in one case it was 435, in another 420; in two it reached 440, and in another case it only reached in a second 400 millimeters. The basilic showed a velocity in three cases averaging 350 millimeters. I hope to prosecute my researches on the velocity of venous circulation when I have completed my study of the velocity of the current in the arterial vessels.

I shall now briefly recapitulate the general conclusions which I have arrived at from a consideration of the results of my experiments.

I. The average velocity in the carotids of dogs is at the rate of 400 millimeters, or sixteen English inches, per second.

II. The rate of velocity is always greater in the female than in the male.

III. Velocity suffers a decrease from hunger.

IV. Velocity is greater after a full meal.

V. The effect of hunger on velocity is more apparent in the female than in the male.

VI. Velocity in particular arteries is influenced by posture.

VII. Velocity varies with age, but the variation is more marked in the male than in the female.

VIII. Velocity varies with the time of day.

IX. Velocity varies slightly according to size.

X. Velocity is affected by medicinal agents.

XI. Velocity is greater in the larger arteries, and in those nearest the heart.

XII. Velocity is decreased by loss of blood.

In concluding this paper I must again remind my readers, that the experiments herein recorded have merely been selected from a voluminous series of notes, and that the conclusions at which I have arrived have not been based on the few leading facts herein mentioned, but have been justified by the results of some 300 experiments.

When I shall have prosecuted my researches still further under different conditions, on other animals, and with other objects in view, I trust that physiologists will then be qualified, from actual experience of, and experiments with, my instrument, to correct and assign to my researches whatever value may attach to them.

I cannot conclude without expressing my thanks to the many eminent physiologists who have so kindly favoured me with their opinions of my instrument, and with suggestions for its improvement. More than all, my thanks are due to Dr. Macdonnel, of Dublin, for his valuable advice, for his great kindness in offering to place his library at my service, and for many other acts of kindness.

I need not say that it is scarcely likely that a hæmodromometer will ever be used on the human subject; and the question would now arise—how can we ascertain the average velocity of the blood-currents in man? I cannot answer the question, but I do not despair of being able hereafter to establish some law, whereby we may be able to calculate with tolerable certainty the velocity with which the blood circulates in the human subject.

ART. V.

Miscellaneous Contributions to the Theory of Pathology. By JOHN W. OGLE, M.D., F.R.C.P., Physician and Lecturer on Pathology at St. George's Hospital.

(*Continued from* vol. xli, p. 234.)

CHAPTER II.

Containing the history of a series of Fatal cases of Idiopathic and Traumatic Tetanus, and of cases which may be called Tetanoïd.

The following cases are intended to illustrate the phenomena of tetanus, and the appearances presented on *post-mortem* dissection.

Almost all occurred at St. George's Hospital, but I shall subjoin additional cases which have, from other sources, come under my notice.

I have purposely, with a few exceptions, only quoted those cases of tetanus which proved fatal, and were examined after death.[1] One or two of the cases have been cited already in the periodicals, and to the citation of these cases I shall refer.

As I have done in respect of the cases of chorea included in Chapter I., at the conclusion of each case in the present series, I shall notice any particulars which strike me in its history, and at the close sum up the general inferences and observations which the cases collectively suggest.

CASE 1.—*Tetanus apparently following cold and exposure.*

Richard H—, æt. 37, was admitted into St. George's Hospital May 28, 1841, with symptoms of tetanus, but the course of the disease has not been recorded. It is stated that he had suffered no accident, but that he had led a very irregular life, and of late had been exposed to damp and wet, sleeping in the fields, &c. He had been ill for three days before admission, and died April 5th, about the twelfth day after the accession of the attacks.

On *post-mortem* examination, excepting rather more than usual vascularity of the cranial membranes, all the various parts of the body appeared to be healthy.

Remarks.—The only points of interest in this case are—1st, the absence of any traumatic cause of the disease, according to the

[1] Among those which proved fatal and were not examined was an interesting case of traumatic tetanus, under the care of my colleague Mr. Holmes and myself, treated by nicotine, and recorded at length in the 'Medical Times and Gazette,' 1865, March 12, 1864, p. 277.

history given; 2nd, the time of death being about *twelve* days after the commencement of the symptoms. [106.]

Case 2.—*Tetanus following sloughing of a wound.*

Frederick B— was brought into St. George's Hospital September 19th, 1841,[1] with a sloughing wound at the inner and middle parts of the foot, and affected by tetanus; but no history of the attacks or of their cause has been given. He died on the 22nd of the same month.

On *post-mortem* examination, a portion of hard leather was found embedded in the wound of the foot, the parts around being inflamed and sloughy; the inner division of the inner plantar nerve was found to lie directly below the wound of the foot, and to be surrounded by a thin screen of a yellow colour; but the nerve was itself healthy, and so were the nerves of the limb generally. The arachnoïd cavity in the spinal column contained rather more fluid than usual, and the bloodvessels at the posterior part of the cord were gorged, but in other respects, excepting some congestion of the lungs, all the parts of the body appeared healthy.

Remarks.—Though details of the history are wanting, yet it is worthy of note that the nerve implicated appeared to be healthy after death, notwithstanding that the wound was in a sloughy state. [156.]

Case 3.—*Tetanus of traumatic origin, Softening of the spinal cord, &c.*

Thomas C— was admitted into St. George's Hospital April 8th, 1843. No history was recorded excepting that he had symptoms which were very like to those of tetanus. He died on the 18th.

Post-mortem examination.—Bruises of the body and an extensive sloughy wound exposing the os calcis existed. The peroneal nerve was exposed, and seen to be of a canary colour and very firm in the slough. Above and below the injury the nerve was softened and white.

[1] During this year, 1841, the following case of convulsive fits of pain, attended by *opisthotonos*, occurred in our hospital:

Wm. S—, æt. 22, was admitted Dec. 1st, 1841. The tongue was clean, the pulse not full or strong, skin cool, bowels regular, urine natural, appetite good. He complained of most acute pain in the right temple and cheek-bone—not in the jaw-bone—in the right ear and round to the back of his head, down the neck and across the loins, not at all in the hands or legs. The pain was not constant, but came on in fits, during which he was obliged to fall and lie with the *head and back curved backwards.* The pain sometimes lasted an hour, more or less, and recurred once in two or three days severely, but he felt a little of it continually. Had been subject to it about a year and a half. His friends attributed it to a fall on the back of his neck on some stones, but he did not himself do so. He died very suddenly, but was not examined after death.

Spinal cord.—This structure was healthy, excepting the dorsal portion, which was softened throughout its substance.

Abdomen.—Blood was found extravasated *behind* the peritoneum at the posterior part of this cavity. The various organs appeared natural.

Remarks.—Notice the affection of the peroneal nerve; its softened state, &c.; also the softening of the spinal cord. [270.]

CASE 4.—*Tetanus following a wound and fracture of the foot and gangrene.*

William L—, æt. 22, was admitted into St. George's Hospital, January 20th, 1844, with a contused lacerated wound of the foot and fractured bones. There was much tension and inflammation requiring incisions. After separation of the toes by gangrene and the knife on the 28th day after injury, there was stiffness of the muscles of the neck and shoulders, and on the day following the jaw was "locked." Some dyspnæa and dysphagia and general spasms, with depression and anxiety came on. The spasm became more frequent and violent, and the urine and motions were passed involuntarily. On the 29th day after injury (January 31st) he died suddenly in a violent convulsion fifty-eight hours after the commencement of the tetanic symptoms.

Post-mortem examination thirty-four hours after death.—The nerves of the foot were examined, but presented nothing unnatural. *Spine.*—Congestion of the spinal veins and of the veins (posterior) of the spinal marrow was great. Clear fluid existed in the theca vertebralis. The medulla spinalis was natural.

Cranium.—The brain was natural, but its veins were congested.

The medulla oblongata and pons Varolii showed much vascularity.

Remarks.—Notice the coming on of symptoms on the *twenty-eighth* day after injury. Sudden death in a convulsive attack. [26.]

CASE 5.—*Hydrophobia and tetanus following the bite of a dog. Mania.*

Charles E—, æt. 13, was brought into St. George's Hospital June 11th, 1844. He had been bitten by a spaniel in the hand *fifty* days previously. The dog had been ill for three or four days, and became worse, being affected by frequent catching and snapping of the jaws, and died very shortly after inflicting the injury on the patient. The dog had also bitten a cat, which, after constantly running up and down the stairs, refusing food, &c., had died after several convulsive attacks. The wound on the patient's hand healed in three days, and he remained well until June 11th, when, as he was being washed by his mother, he refused to let her wash his face, putting his hands to his face in great fear and saying that he felt as if he was about to be choked. In the course of the day he was brought to the hospital. The pulse was frequent, the skin hot and dry, the tongue furred, the fauces injected, and there was slight difficulty in swallowing. During the night of the 11th the patient obtained no sleep, and on the morning of the 12th he was greatly distressed, and the countenance became very anxious on the slightest disturbance. The least breath of air, the noise made in pouring out water, and the sight even of water, or the idea of swallowing it, brought on violent spasm of the muscles of the throat, principally those connected with deglutition. The patient said he had pain in the throat on any attempt to swallow, which was attended with much convulsive effort. The spasms became more frequent, and his mental faculties, which had been unimpaired, became affected. Thus confusion of thought and visions of unreal things and persons around him came on, and in the evening he became quite maniacal, talking incessantly and foaming at the mouth.

On the 13th, between one and two o'clock a.m., he had several attacks of general spasms, of which two were very severe and attended by *emprosthotonos.* After this he became collected and freer from spasm, and took some beef tea without much difficulty. He was, however, greatly exhausted, and his expression indicative of great anxiety. At 10 a.m. he again became violent, talking incessantly, and refusing to take anything, shouting and singing, and vomiting a black matter, which he brought up with hiccough; the respiration was greatly hurried but not difficult. At one p.m. the delirium continued, but the patient was evidently very much weaker. The pupils were dilated, the eye wild and prominent, and the mouth constantly filling with dark-coloured foam, which he snatched with his fingers and threw at the bystanders. The extremities were cold and the hands blue. The pulse and action of the heart became more and more enfeebled, and the patient quietly and gradually sank, and died at two p.m.

Post-mortem examination 24 *hours after death, and the weather being extremely hot.* The brain was found to be very dark and congested generally, the grey matter being very pink. It was also softer than natural throughout. The cerebral membranes and ventricles presented nothing unusual, and no unusual sub-arachnoïd or ventricular fluid existed. The cerebellum, pons Varolii and medulla oblongata congested and pink, like the other parts of the brain. The substance of the medulla oblongata was carefully examined, by the microscope, by Mr. Toynbee and Dr. B. Jones, and, as I learn from the latter gentleman, showed nothing unnatural. The spinal cord and membranes were natural. The pharynx and fauces were very vascular as well as the upper part of the larynx. The lower part of the larynx and the bronchi were somewhat vascular;

and the lungs, which contained some small deposits of scrofulous matter, were much congested posteriorly. The pneumogastric nerves on both sides were examined, and found to be healthy. The heart was natural, the contained blood being dark and thick, and mixed with a small amount of dark coagulum. The mucous membrane of the œsophageal end of the stomach was rather congested; also the spleen and kidneys. The other parts of the body were in a natural condition.

Remarks.—This case is one of a most interesting nature, as showing the complication of hydrophobia and tetanus.[1] Thus we have the intense spasm, the snapping, the emprosthotonos and the dysphagia of tetanus, accompanied by the choking feeling and distress consequent on the stimulus of the cold water to the face or even the very thought of water (emotional), the peculiar form of spasm observed in hydrophobia. The foaming also is remarkable, being such as occurs commonly in epilepsy, though by no means distinctive of that affection. The case is one which seems to a certain degree to link together all the three forms of disease—hydrophobia, tetanus, and epilepsy—under one common pathological classification. Given, excessive irritability of the common spinal centre arising from agents acting either centrally or peripherally through the centripetal nerves, we have resulting from the application of any suitable exciting stimulus or irritation, motor action or spasm, which may either be of the general muscular system as in epilepsy; or of particular regions as those of the jaw, neck, throat, or back, &c., as in tetanus; or more especially of the throat and pharynx, as in the present case of hydrophobia, the spinal derangement being attended

[1] This is the only case of hydrophobia, I believe, that we have ever had in our hospital. For the following case of chorea, in which symptoms were presented simulating hydrophobia, I have to thank Dr. Bence Jones:

Sarah Williams, æt. 17, was admitted into St. George's Hospital, May 22nd, 1839, with chorea of a fortnight's duration, and with a general cachectic appearance, the twitching being confined to the right arm and leg. Pulse very weak and quick. Bowels open. Tr. Ferri Amm., ʒij, Ammoniæ Sesquicarb., gr. vj, Mist. Camph., ʒx, Syrupi, ʒj; bis die. Pil. Aloës c. Myrrh., gr. vj omnî noct. Bal. Imbrif. omnî mane. Ordinary diet, without vegetables.

27. Bandage to the ankle.

29. Omitr. Bal. Imbrif. Want of power over the right arm. Irregular action of the muscles much diminished. Emp. Canth. nuchæ. rep. alia.

31. Some increased power over the right arm since the blister.

June 3. Tr. Assafœt., ʒj; Liq. Cal., ℥j; Aquæ Cinnam., ℥ss; Syrupi, ʒj; ter die.

5. More movement of the leg and arm on the right side. Bal. Imbrif. omnî mane. 76.

10. Perstat. Great improvement in power over the lower limbs.

22. Mist. Ætheris Co. ℥iss, statim; Enema aper. hac vesp.

23. Calomel. gr. v, statim.; Ht. Sennæ, post horas iv; Cuc. ad ℥xij nuchæ; Ht. Salin. 4tis horis; Bal. tepidum horâ somni.

24. Was attacked the night before last with violent spasms, principally of the

by cerebral disturbance, as in the present instance, with mental delusions, and even mania. The detailed points worthy of remark in the case are—1st, the length of time which elapsed between the time of the seizure and the commencement of the symptoms, viz., *fifty* days, during which period no uneasiness either about the injured part or in the body generally was complained of; 2nd, the existence of emprosthotonos, a much rarer form of spasm than its counterpart, opisthotonos; 3rd, death by exhaustion; 4th, the presence after death merely of a congested state of the brain; the spinal cord and the cerebral and spinal membranes being healthy, and no increased serous effusion existing either within the cranium or spinal column. The generally softened state of the brain may be fairly attributed to approaching decomposition, owing to the great heat of the weather and the long time which had elapsed between death and the post-mortem examination. (136).

CASE 6.—*A second attack of tetanus, apparently not traumatic.*

James G—, æt. 26, was admitted into St. George's Hospital December 31st, 1845, quite unable to open his mouth and with some dysphagia. There was also stiffness of the muscles at the back parts of the neck, and pain at the sides of the face in the temporal and masseteric regions. The attack had begun the day before, and very much in the same manner as did *a previous and similar one* which he suffered from about *a year* previously. At that time he was attacked with stiffness about the jaws and pain, which first began in the right and afterwards in the left side of the face, and was attended by pain in the eyes and ears. All his symptoms were at that time worse during the night. He was brought into the hospital, and went out quite well in a week's time.

On this, his second admission, he was freely cupped and blistered at the back of the neck.

On the 1st of January the pain was so bad at the side of the

muscles of deglutition simulating hydrophobia. She used the warm bath last night, and was cupped with great advantage. At present the involuntary motions of the muscles of the arm are greater than they have been during the last few weeks. Is at present working herself into an hysterical fit. Ht. Ætheris Co. statim; Bal. tepidum vespere; postea emp. Canth. spinæ.

27. The blister-sore well. The involuntary action of the muscles continues very violent. Deglutition is again difficult. Her senses remain entire. Enema Terebinth. Bal. tepid. vespere; Acetatis Morphiæ, gr. ½; Aceti, gr. vj; Aquæ fort., ℥x; Syrupi, ʒj; hora somni.

28. Rep. enema Terebinth. cal., gr. v, in pulv. hac noct.; Ht. Sennæ mane.

29. Quieter night; involuntary motion much less to-day. She appears to have been relieved by the shower-bath. Bowels open freely. Swallows with difficulty. Rep. Ht. Morphiæ hora somni; Bal. Imbrif. cras. mane.

July 1. Passed a restless night. Has been relieved by the shower-bath. Bowels not open. Ht. Sennæ.

Went out at her own request.

face that leeches were applied, and with relief. The bowels were open. The pulse small and weak, but regular. Abdomen tense and hard. Turpentine injections were given.

On the 2nd no relief had resulted from the injections, but the face was better owing to fomentations. Poultices to the sides of the face were ordered, also beef tea was freely given, and calomel and opium every four hours.

On the 5th the pills were omitted as ptyalism had come on. The patient was much the same.

On the 6th there was less pain in the jaw, and much less tightness about the throat. The abdomen was less tense and hard, and the patient said he was much better, but the mouth was still quite closed. Alum gargle ordered to be freely used.

7th.—The pain and stiffness had more abated, but a pain was complained of under both axillæ. The abdomen was natural, and the bowels open.

8th.—The abdomen was very painful, and the muscles in a state of tonic tension. There was no tympanitis. There was dyspnœa and slight dysphagia and pain with a diffused redness of the integuments at the upper parts of the back. The muscles of the neck or back were, however, not contracted. All pain in the sides of the face had gone, but the countenance was haggard. The bowels were confined, and the pulse quick. No sleep could be obtained. Morphia was given and a blister applied to the chest. As the pain proved excessive, tincture of opium (20 drops) and again 40 drops were given before the next morning.

On the 9th the jaw could be slightly opened, and the pain in the abdomen and side was less. There was, however, pain on respiration, and the muscles of respiration seemed rather fixed. Some dysuria was complained of. Cough with expectoration existed, and some dullness on percussion at the lower parts of the left side of the chest, both in front and behind. The tongue was white; the skin cool; pulse weak. A third of a grain of tartar emetic was given every six hours and a blister applied to the chest. In the evening, as more pain existed in the chest, tincture of opium was given.

On the 10th he was very restless, and the pain at the side was increased, but that in the abdomen had left him. Pulse 95.

On the 11th great pain in the abdomen was complained of, whilst that in the chest was less. No pain existed in the face. Great prostration of strength existed. The pulse was weak, and the skin hot. Leeches were applied to the abdomen, and brandy given.

On the 12th, early in the morning, as the pain was intense at the abdomen, tincture of opium was given. During the day collapse and dyspnœa came on, and the patient sank and died.

Post-mortem examination, 19 hours after death. The masseter muscles were found to be very contracted. The brain was itself natural, excepting the existence of many and large bloody puncta or section of it. Much sub-arachnoïd fluid and adhesion of the dura mater to the cranium existed. The spinal cord was natural. Both pleural cavities contained pus and recently-exuded fibrine, and the left lung was highly hepatized. Pus and recent fibrine also existed in the pericardial sac and in the peritoneal cavity.

Remarks.—The points of interest worthy of notice in the above case are—1st, the absence of any known traumatic cause of the attack; 2nd, the repetition of the attack, a similar one having previously existed, and being recovered from quickly; 3rd, the affection beginning by rigidity of the jaws; 4th, partial remission of the symptoms; 5th, the supervention of extensive inflammatory disease in the thoracic and abdominal cavities, the symptoms of which it became necessary to distinguish from those strictly tetanic in character; 6th, death apparently from exhaustion. [15.]

CASE 7.—*Tetanus following fracture of the bones of the hand.*

George M—, æt. 42, was admitted into St. George's Hospital January 31st, 1845, with a compound comminuted fracture of some of the matacarpal bones of the hand. Portions of the fractured bones were removed. The wound suppurated foully, but improved under the green dressing (sulphate of copper).

February 8th.—Sickness was complained of, and uneasiness in one of the axillæ; and on the 9th febrile symptoms set in, and the absorbents of the arm became inflamed. Decoction of bark with Liq. Ammon. Acetatis was given.

13th.—The redness was less, but when the tongue was protruded rigidity of its muscles was complained of. Wine was ordered in addition to the medicine.

14th.—An erysipelatous state of the arm came on, and an abscess formed, which was opened on the 18th, when also, in addition to the rigidity of the tongue, rigidity of the jaw came on, so that he could only slightly open the mouth.

On the 19th there was a very anxious expression, and the muscles of the forehead were very rigid. *The muscles of the neck were* NOT *affected.*

On the 21st the muscles about the throat were rigid, and the mouth was quite closed. Morphia was given, and turpentine injections.

22nd.—Muscles of neck more rigid. The oil of turpentine in drachm doses given.

24th.—Muscles of neck less rigid, and powers of speech greater.

25th.—Some uneasiness about the epigastrium, and slight opisthotonos came on. Sanious purulent fluid was discharged from the abscess. The turpentine injections have been repeated.

26th.—Muscles of neck more rigid. Pulse 128.

27th.—Several tremors of the lower limbs took place. Dysphagia and fits of sneezing came on. Perspiration profuse.

28th.—Muscles rather more relaxed. Injection repeated.

March 1st.—Universal trembling of the limbs came on, and also pain at the epigastrium. Extreme depression supervened, and finally death.

On *post-mortem* examination, 35 hours after death, the muscles of the jaw were very rigid, but not so the other muscles of the body. The brain was very wet and congested, as was also the pons Varolii and medulla oblongata. The spinal cord was natural. Both lungs were hepatized in patches. The heart's cavities contained large fibrinous coagula, and its walls were softer than natural. The spleen was very soft and congested.

Remarks.—1st. The history of this case shows very clearly the course which the symptoms took; for instance, on the thirteenth day of the month we have noticed the stiffness of the muscles of the tongue; on the eighteenth, the affection of the muscles of the jaw; on the twenty-first, the affection of the muscles of the throat; on the twenty-fourth, slight remission in the rigidity of the muscles; on the twenty-fifth, opisthotonos and epigastric uneasiness; on the twenty-seventh, dysphagia and tremors of the lower limbs; March 1st, universal trembling. The early date of the affection of the lingual muscles, and the late period of the opisthotonos are specially interesting; as also the "partial *remission*" of the symptoms. 2nd. The presence of disease of the lungsis worthy of note. 3rd. The mode of death, by depression, must be observed; and also, 4th, the rigidity of muscles after death, confined to those of the jaw. [54.]

CASE 8.—*Tetanus following a burn.*

Sarah W—, æt. 19, was brought into St. George's Hospital December 28th, 1846, with an extensive burn of the upper extremities and back, chiefly superficial, owing to a hot cinder falling on her dress. She was warm, and her general condition was hopeful on her admission. She went on favourably, the sloughs separating well, until January 5th, when she became weaker daily, and the discharge from the burns became fœtid. Her respiration also became much hurried, and on the 6th and 7th of January was very laborious. The pulse also became weak and frequent, and the countenance was expressive of much suffering. The tongue was inclined to be dry, but was clean. On the 8th of January, slight stiffness in the jaws was complained of, accompanied by pain, and these symptoms increased until decided trismus was established.

On the morning of the 9th, the jaws were firmly fixed, and their muscles very rigid, and throughout the day occasional attacks of dyspnœa came on, but no general spasms. The perspiration was very profuse, and the patient became weaker, and died on the 10th.

Post-mortem examination.—All the viscera were found to be healthy.

Remarks.—The points of interest in this case are—1stly, the occurrence of untoward symptoms first on the *eighth* day after the injury, but it is difficult to say whether the dyspuæa which at that time came on should be attributed to a tetanic condition of certain muscles engaged in respiration, or to some congestion of the bronchial or pulmonary vessels such as is not uncommon in cases where destruction of any extensive part of the skin from any cause has taken place; the dyspnœa being paroxysmal, and the lungs and bronchi being found natural after death leaves one at liberty to suppose these symptoms to have been of a tetanic nature. 2ndly, the accession of stiffness about the jaws on the *eleventh* day after the burn. 3rdly, the absence of any more general spasm, emprosthotonos, &c. 4thly, the uniform persistence of the spasmodic state of the jaw-muscles, no remission being observed. And, 5thly, the method of death, being that by exhaustion, aided possibly to some extent, although not very materially, by that of apnœa, consequent on the rigid state of the respiratory muscles. [17.]

Case 9.—*Tetanus following a wound of the cornea of the left eye.*

Thomas S—, æt. 33, was admitted into St. George's Hospital January 10th, 1847, into the hospital, with a wound of the left cornea; an opening being made by a whip lash, through which the aqueous humour had escaped. On admission, much redness and tension of the eyelid existed, preventing free opening of the eye. On the 12th of January, inflammatory fever set in with pain in the globe of the left eye and in the forehead, and on the 16th suppuration of the eyeball, giving much relief to the pain. The mouth became drawn to the right side, and the whole countenance expressive of great distress and agitation. The patient also complained of slight pain, with stiffness about the jaws. On the 17th decided symptoms of tetanus had come on; there was great pain in the jaws and neck, and the jaws were firmly closed. On the 18th much purulent fluid escaped from the affected eyeball, an incision having been freely made into it; and throughout the day all the peculiar symptoms became aggravated, the patient sitting for hours holding the jaws in his hand, in great suffering. Besides rigidity and hardness of the masseter and buccinator muscles, there was rigidity of both sterno-cleido-mastoïd muscles, especially of the left one. The muscles at the back of the neck were not affected.

During the paroxysms of spasm of the affected muscles which occurred at longer or shorter intervals, the respiration was interfered with, becoming hurried and interrupted, and performed with convulsive energy for a few seconds at a time. These attacks became more and more frequent, and the patient gradually weaker. During the night of the 19th, violent spasm of the muscles in front of and at the back of the body came on, and during most of the night alternate attacks of emprosthotonos and opisthotonos took place, the respiration being greatly interrupted. The patient died early on the morning of the 20th.

Post-mortem examination. The muscles of the body generally were found to be very rigid. The left eyeball was somewhat shrunk, the lids being closed by purulent fluid. The brain presented, on section, numerous bloody puncta, and the vessels of the dura mater as well as the surface of the brain were greatly engorged, the cortical parts being very pink. The mucous membrane of the fauces, specially the upper part, was very congested, as was that of the trachæa. The lungs were congested, but otherwise natural. The heart was natural, with its cavities contracted, the blood contained being unusually fluid. The contents of the abdomen presented nothing unusual. On examining the orbit, the eyeball was found to be almost entirely occupied by purulent fluid, and recently effused fibrine existed amongst the muscles. The optic nerve presented nothing unnatural.

Remarks.—The case is interesting. 1stly, from the unusual form of injury giving rise to the symptoms, the cornea of the eye being the affected part. 2ndly, from the *sixth* day after injury being that on which the symptoms set in. 3rdly, from the occurrence, first of the rigidity of the jaw-muscles and of those at the anterior part of the neck, the muscles at the back of the neck being free. 4thly, from the accession of *alternating* emprosthotonos and opisthotonos, showing that the existence of those forms of spasm was not owing simply to over action of one set of muscles, the other and antagonistic ones being overcome by reason of their comparative weakness, but was due to the peculiar capricious character of the affection, different series of muscles being affected. And 5thly, it is interesting as much as there was no remission of the severe symptoms. [24.]

CASE 10.—*Tetanus following extensive laceration of the thigh.*

C. E. K—, æt. 10, was brought into St. George's Hospital May 15th, 1849, having an hour previously extensively lacerated the fore part of the thigh by falling on an iron spike. On the next day sphacelus of part of the integument had come on, and much febrile action speedily assuming a typhoïd character came on with delirium. The slough separated, and after a time the pulse gained in power, and general action was much less depressed, the wound going on well. On the *thirteenth* day after the accident, the jaws were noticed to be slightly stiff, and there was pain and distress at the epigastrium, and in a few hours decided trismus had set in. Turpentine ℥ xx. was given every four hours. On the following day, slight opisthotonos and great tenderness and tension of the abdomen came on. There was great præcordial distress, and the slightest pressure at the epigastrium brought on strong spasm. The jaws became quite closed, and the arms involved in the general tetanic action; deglutition became difficult, and at last almost impossible. The alvine evacuations were dark and offensive, the skin moist. Pulse from 120—140, and feeble. At first the spasms were slight, and occurred at intervals of about half an hour, but they daily increased in strength and frequency, and during night of June 1st he had two violent attacks, in the last of which he died.

Post-mortem examination.—Two sloughing wounds were found, one occupying the front and outer part of the left thigh and another of the size of half-crown on the outer side of the left hip.

The blood-vessels inside the spinal canal were very congested, and a small amount of extravasated blood was found on the posterior surface of the dura mater, corresponding to the lower part of the cervical and the upper of the dorsal part of the cord. The dura mater of the cord was slightly thickened and opaque. The other membranes were healthy, the spinal cord itself quite healthy, as also the brain and its membrane. Excepting slight congestion, the thoracic and abdominal organs were natural.

As regards the wound of the thigh, &c., some of the branches of the anterior crural nerve in the situation of the wound appeared more vascular than natural. Nothing more was found.

Remarks.—Notice 1, The traumatic origin of the disease; 2, The fact of the tetanic spasm commencing, and that in the jaw-muscles, on the *thirteenth* day after the injury; 3, The congestion, even to extravasation of blood within the spinal canal, and the vascularity of the nerve in the neighbourhood of the wound. [118.]

Case 11.—*Tetanus following a comminuted compound fracture of the leg.*

George P—, æt. 27, was admitted February 22nd, 1850, into St. George's Hospital, having an hour or two previously suffered a compound and comminuted fracture of both bones of one leg. He went on well until the 25th, when much swelling and tension of the limb requiring incisions came on. On the 27th there was much foul suppuration, the pulse, however, being quick and the tongue clean.

On the 28th the leg was very painful and more swelled, and he had passed a sleepless night. There was slight stiffness of the jaws, but there were no general convulsions and no dysphagia, etc. The skin was hot and dry, the tongue coated, and the pulse 120, and rather full. Incisions into the leg were made, letting out much pus. A calomel and scammony powder was given, and at night a morphia draught given. On the 1st of March very decidedly tetanic symptoms came on. The countenance was much altered, the mouth was firmly closed, and there was violent spasm of the muscles of the back, the chest, arms, and legs, slight opisthotonos existing. The injured leg was more swelled, and the wound gangrenous. The pulse was 120, but not weak. The skin was dry and the bowels confined. A turpentine injection was administered and half-drachm doses of turpentine given internally every six hours. The injection brought away no fæcal matter, and the patient became worse, and the spasms more frequent. The patient sank gradually, and died at six a.m. on the 2nd.

Post-mortem examination fifty-five hours after death.—It was found that the posterior tibial nerves passing through the sloughy mass were covered by a coating of recent fibrine, on the removal of which the neurilemma of the nerve was discovered to be in a highly vascular condition. A portion of the peroneal nerve included within the slough presented also a very vascular and almost a sloughy condition. The other parts of the body were examined, but nothing unnatural was found.

Remarks.—The points of interest about this case are—1stly, the accession of tetanic symptoms on the *sixth* day. 2ndly, the stiffness of the jaws, without any dysphagia, showing itself first of all the symptoms. And 3rdly, followed on the *seventh* day by general tetanic convulsions, the extremities being affected as well as the back and thorax. 4thly, the absence of any remission in the symptoms. 5thly, the mode of death being by exhaustion. 6thly, the extensive implication of large nerves in the sloughy wound. [41.]

CASE 12.—*Tetanus, apparently caused by an ulcer of the leg.*

Henry M—, æt. 63, was admitted into the hospital June 10th, 1851. It was said by his friends that he had been suffering for some days previously from slight fever, but no cause for the attack of tetanus was known. On the morning of the day of admission he had been suddenly seized with difficulty in moving the lower jaw, and very shortly afterwards became affected with general tetanus. When admitted the whole body was in a state of spasm, opisthotonos existing. Turpentine was given in half-drachm doses every four hours, and a turpentine injection administered, which produced a copious discharge of fæcal matter. During its administration the nurse detected a small ulcer under the right knee, which turned out to have been produced by the tying of a bandage round the knee very tightly for support to the part. It was found that the patient could only swallow a little of the turpentine, and the general spasm continued until coma set in, of which he died. A second turpentine injection was given; but the patient died shortly afterwards, on the day following his admission.

Post-mortem examination thirteen hours after death.—The body was found to be in good condition and well made, with a small ulcer, having livid margins and surrounded by redness of the skin, behind the right knee, below and to the outer side of the flexure of the joint.

The *spinal* cord, excepting being generally rather softened, was healthy.

The *brain* was natural, excepting slight general softening.

The *lungs* were healthy; the *heart* was enlarged, with thinning of the walls of the right ventricle. The abdominal viscera were natural.

On examining the ulcer the surrounding parts were found to be callous, having the peroneal nerve imbedded within, but not apparently much compressed by them, just as it turns round the fibula. There was no redness or injury of the nerve itself.

Remarks.—The points of interest in this case are—1st, its *supposed* idiopathic character until the ulcer was accidentally found; 2nd, the pre-existence of febrile symptoms before the spasm set in; 3rd, the muscles of the jaw being the first to be affected; 4th, the short duration of trismus before the general spasm set in; 5th, the mode of death being that by coma.

The general softening of both the brain and spinal cord was most probably dependent on the approach of decomposition. (3.)[1]

The two following cases may be termed tetanoïd, exhibiting spasms arising from affections of the nervous centres, and may be well compared with true tetanus.

Case 12 a.—*Tetanus with twitchings following a fall in a patient affected by scrofulous deposits within the brain.*

James S—, æt. 15, who was admitted into St. George's Hospital April 1st, 1851, having fallen from a horse and struck the back of the head seven days previously. For two days after the accident he

[1] In the same year (1851) the following case was brought into the hospital, illustrating a disposition to tetanic spasm, and it may here be timely quoted. It was that of James P—, æt. 40, who had fallen from a height and sustained fracture, with depression of the left parietal base and correspondent laceration of the dura mater, and also laceration of the surface of the brain. Trephining was resorted to, and the patient went on well for four days. After this *he lost all power of speech*, though he retained his mental powers. On the least exertion,

was insensible, but subsequently became partly conscious though drowsy. After this, inflammatory symptoms were set up, and great struggling, with twitchings of the arms and legs, which first began in the legs.

On the 5th of April, *strabismus* came on, and the jaws became firmly closed, and so remained for two or three days.

Post-mortem examination twenty-nine hours after death.—Extensive serous effusion in the cerebral ventricles and beneath the arachnoïd membrane were found; and scrofulous deposits in the substance of the brain. Excepting congestion of the blood-vessels nothing unusual was noticed about the membranes of the brain or spinal cord. The other organs of the body were natural. [76.]

Case 13.—*Opisthotonos connected with diffuse Inflammation of the Spinal Membranes following puncture of a Spina Bifida.*

Lewis M—, æt. 20, was admitted into St. George's Hospital June 25, 1852, with a large tumour, of the size of a man's head, attached to the lower part of the back, which fluctuated, and had one or two ulcerations on its surface. It appeared that the tumour, which had existed since birth, had burst once or twice previously, and had discharged clear fluid; and at these times some twitchings had been observed, and the bowels had acted involuntarily. A trocar was passed, and much albuminous liquid let out. This was followed by pain in the head and belly and retraction of the head, whilst, however, the superficial muscles of the neck were quite flaccid. The evacuations were passed unconsciously. The head gradually became drawn back, and fixed so as hardly to form a right angle with the spine; and pain at the sternum, with dysphagia, came on. Inflammation of the surface and bursting of the tumour took place, the pulse gradually failed, and the patient became unconscious and died, the deep muscles of the neck becoming more tense, and the head more dragged back until death.

Post-mortem examination.—The cyst which communicated with the spinal canal was found lined by recently formed fibrine, and the arachnoïd and pia mater of the lower part of the chord showed decided evidence of inflammation, being coated by recent fibrine. The chord itself was also *softened* at the lower part of the dorsal region.

The details of this case are not here given,[1] as it is only adduced

and when he was *touched* or *spoken to*, convulsive action came on, and the muscles of the lower jaw were especially affected, the face being drawn to the left side. The patient died comatose. ('P. M. Book,' 1851, fol. 175; also 'Hospital Catalogue,' series viii, No. 66.)

[1] The case has been related at length by my colleague Mr. Holmes, in the 'Transactions of the Pathological Society of London,' vol. iii, p. 10.

with reference to the tetanic symptoms which came on, and which were obviously connected with the inflammation of the spinal chord and its membranes, for which he was treated.

Remarks.—The points worthy of attention in the case are—1. The absence of any remission in the tetanic symptoms. 2. The spasm of the deep muscles of the neck, whilst the superficial ones remained relaxed. 3. The order of the symptoms, the pains in the belly and head following the spasm of the neck, and being followed in their turn by the dysphagia and pain at the lower parts of the sternum; and 4. The death apparently from exhaustion. [141.]

CASE 13A.—*Tetanus following ulceration of the integuments about the knee-joint.*

Martha K—, æt. 44, was admitted into St. George's Hospital April 11, 1852, having been thrown down by a bull, and thus incurred a laceration of the integuments of the right knee just above the patella. The accident occurred March 28th. The wound had gone on fairly well until April 9th, when the lower jaw became fixed, and so remained. On admission she could just move the jaw, so as to separate the teeth a very short distance, and the muscles closing the jaw were very rigid and contracted. She could speak plainly, and could drink small quantities provided she swallowed slowly; but if she tried to swallow quickly, choking sensations came on. The wound was very large, and healthy in appearance, having a free discharge, but it was very tender; the patella was not exposed. No pain was complained of, except when the wound was touched. After admission the bowels were freely opened with a calomel purge, and she was put on calomel and opium every four hours.

On the 12th she seemed comfortable. The expression of the face was natural. The pulse was 104, and rather weak. Porter was given to her, and the calomel and opium continued.

On the 13th the jaws were more firmly fixed, and a peculiar grinning expression of the face existed; moreover, the powers of speaking were interfered with. Much pain was complained of about the jaw and occasional attacks of spasmodic dyspnœa. The pulse was regular and 100. The bowels were not open. The gums were distinctly affected by the calomel. She was purged, the calomel and opium were discontinued, and the hydrocyanic acid given, six minims every three hours.

On the 14th the patient was very weak; the pulse was very feeble and quick. She could with difficulty swallow anything owing to choking sensations, and the muscles about the back of the neck were very rigid, the head being slightly drawn back. Occasional pain was felt near the lower end of the sternum. The bowels were not open. The wound of the leg was deeper, and rather sloughy.

On the 15th the rigidity of the muscles of the neck and jaw was diminished, but she had been unable to swallow since the noon of the previous day. The bowels being confined, two aperient enemata were given without any result.

On the 16th she had become greatly exhausted, and the pulse imperceptible. The pain at the end of the sternum had increased, and the skin was covered by cold perspiration. The patient was quite sensible; she gradually sank and died in the evening.

Post-mortem examination.—The body was emaciated and the patella was found deprived of its periosteum. The knee-joint itself was natural.

Within the *cranium* the subarachnoïd fluid was slightly increased, and the brain generally rather more vascular than natural.

Within the *spinal column* the veins at the posterior part were full, and the whole of the cord was slightly softened from decomposition.

The contents of the thoracic and abdominal cavities were natural.

Remarks.—The points of interest in this case are—1st, the order of succession of the symptoms, the lower jaw becoming fixed on the *twelfth* day after the injury, on the *sixteenth* day the spasmodic dyspnœa coming on, and on the *seventeenth* day the muscles of the neck becoming rigid and the head drawn back; 2nd, the occasional pain near the end of the sternum; 3rd, the sensibility and consciousness remaining until the last; 4th, the partial remission of the symptoms; 5th, the method of death being apparently that by exhaustion. (93.)

Case 14.—*Tetanus following a lacerated wound of a hand and fracture of the bones of a finger.*

Isaac B—, æt. 26, was brought into St. George's Hospital August 25th, 1852, having attended for about one week as an out-patient, owing to a lacerated wound of the right hand and fracture of the bones of a finger. One of the joints of the finger was also torn open, and much inflammation with sloughing of the wounds resulted. After admission he was treated by wine and good diet, and considerably recovered; but on the morning of the 29th the patient complained of "sore throat" and pain about the middle of the abdomen, and in the course of the day decided symptoms of tetanus set in, the jaws being fixed and the head being forcibly drawn back at times with much pain.

On the 30th the jaws were so fixed that they could not be separated beyond a quarter of an inch, and the face assumed a peculiar grinning expression, the masseter muscles being very rigid. Any attempts to swallow usually brought on attacks of opisthotonos, but he could swallow fluids. There was also pain in the back and abdomen, and some difficulty in moving all the limbs. No sleep had been obtained. The pulse was quiet and natural and the bowels open. A turpentine injection was administered and calomel and opium given every six hours. In the evening a moxa was applied to the back.

On the 31st the patient said he thought he was better. The muscles of the jaws were less rigid; the spasms, though frequent, were less severe; and the limbs could be moved with greater ease. No sleep could be obtained. The pulse was very weak, and about 134 in the minute; and perspiration was profuse. During the afternoon the spasms became more intense, and the patient was obviously sinking. The urine was noticed to be turbid and of a greenish hue, and was also found highly albuminous and containing blood-globules.

At about 5 a.m. on the 1st of September the patient died in an attack of spasm.

Post-mortem examination, thirty-three hours after death. The integuments generally were found to be much congested. Great rigidity of the muscles of the lower limbs existed, but only slight rigidity of the upper ones.

The spinal cord generally and the central white parts in the brain were much softened, and the veins within the cranium and spinal column were highly congested.

Nothing more of note was observable about the nervous centres. The kidneys were very fatty, and slight extravasation of blood was found beneath the mucous membrane of the pelvis of one of them. The other abdominal viscera presented nothing unnatural. The heart and lungs were healthy. Some old pleuritic adhesions existed within the chest.

Remarks.—The points of interest in the above case are—1st, the appearance of symptoms on the *tenth* day after the injury, the first symptoms being fixedness of the jaws and pain in the abdomen, being preceded for a few hours by a feeling about the throat described by the patient as a "sore throat;" 2nd, the production of opisthotonos on attempts to swallow; 3rd, difficulty in moving "all the limbs;" 4th, the partial remission of symptoms not long before death; 5th, the sloughy state of the wound and the co-existence of disease of the kidneys.

The pains continued in the back and abdomen may have been simply the result of the spasmodic action of the muscles of those parts. [174.]

Case 15.—*Tetanus following wounds of the scalp and thumb.*

Robert H—, æt. 43, of good general health and temperate, was admitted into St. George's Hospital June 20th, 1854, having been thrown from a restive horse on the same day. He had a scalp wound on the left side of the forehead, not exposing the bone, and also a superficial wound of the right thumb. The wound of the thumb was painful on the first day, the pulse was quiet, the tongue clean, and the bowels open.

On the fifth day after the injury the patient left his bed, as all had been going on well.

On the sixth day difficulty in opening the mouth came on, so that the patient could only just protrude the tongue, which was then greatly furred, and there was much difficulty in swallowing, but no pain was complained of. The wound of the thumb appeared to be going on favorably, but that of the scalp was in a sloughy state. The pulse was 72 and soft, the bowels confined. Calomel and opium, followed by a senna draught, were given. There was great difficulty in swallowing the draught; but no spasm was produced by the act of swallowing, and the patients had a good night.

On the morning of the 27th, at 6 o'clock, a distinct attack of opisthotonos came on, and the jaws were more firmly closed, so that the patient could not protrude the tongue at all. The masseters and abdominal muscles were observed to be rigidly contracted, but the muscles of the extremities were unaffected. The attack of opisthotonos passed off in a few minutes, but the contraction of the other muscles continued. Attempts to swallow produced spasm about the muscles of the throat. The patient was quite sensible, and complained of pain in the back and shoulders, but in no other part. A purgative enema brought away much dark fæcal matter. About noon an attempt to swallow induced another attack of spasm of the muscles of the neck and back, and the expression became rapidly more distressed, the features becoming pinched and "sardonic." The pulse was 96, and very weak. Large doses of calomel and opium were given, and turpentine injections at intervals. The spasms continuing, chloroform was inhaled at 7 p.m. After five or six inspirations of the chloroform, air being properly admitted along with it, spasmodic closure of the glottis came on, and for about one minute respiration was quite suspended. He was relieved by ammoniacal vapour applied to the nostrils, and cold water affusion to the face; but the spasms persisted at intervals during the evening. The pulse became quick but weak. An injection was given, bringing away copious dark stools, but a second injection was immediately followed by an attack of spasm. After another such attack, about 9 p.m., he died; and immediately after the cessation of the breathing the lower jaw dropped, and the muscles, which had been contracted, were observed to be perfectly relaxed and flaccid.

Post-mortem examination twenty-four hours after death.—The scalp wound was seen to be sloughy, but that on the thumb was granulating healthily. The contents of the cranial and spinal cavities were quite natural. The lungs were quite natural; the heart was also natural, the ventricles being firmly contracted. The liver and the kidneys were congested, the latter being large and coarse. The left frontal nerve in the neighbourhood of the scalp-wound was quite healthy, and not implicated; nor were the branches of the median nerve, which were found passing through the wound of the thumb, at all affected.

Remarks.—The points deserving attention in the case are—1st, the accession of symptoms on the *sixth* day; 2nd, the rigidity of the jaw-muscles and of those connected with the act of swallowing coming on first, at the same time no pain being complained of, and good sleep being obtained; 3rd, opisthotonos coming on upon the *seventh* day, the extremities being unaffected; 4th, spasm of the muscles of the back and shoulders being set up by attempts at swallowing, and of the muscles of the glottis during the inhalation of chloroform, as also the general spasm by the act of the administration of the enema; 5th, the absence of any remission.

It may be worthy of remark that one of the wounds was sloughy to the end of the patient's life. It is also of interest to notice the contraction of the heart's walls found after death, and the very rapid relaxation of the spasmodically affected muscles which took place. [164.]

CASE 16.—*Tetanus from injury to the surface of the fingers* (?).

Ebenezer D—, æt. 38, a watchmaker, was admitted into St. George's Hospital March 19th, 1855. He was pale and sickly, in look, but said that he had always enjoyed good health, except a slight cough and bronchitic symptoms of two months' duration. He acknowledged that he often ran small pieces of glass and wire into his fingers, an evil incident to his calling, but had not done so of late, and knew of nothing to cause his present attack. On the evening of the 15th he had been much exposed to wet and cold, and on the morning of the 16th, when at breakfast, he felt a "tightness" about his jaws, and had to cut his bread rather thinner than usual in order to get it into his mouth. This difficulty in moving the jaw increased until the evening of the 17th, when he could only open his mouth to half the natural extent, and was obliged to place a piece of gutta percha between the teeth; with this in his mouth he was admitted into the hospital. When seen his face was flushed, his skin warm, and his pulse 86 and soft. He complained of an aching pain between the shoulders, and he had a cough with frothy expectoration, which was only got rid of with difficulty. The jaws were so closed that the tongue could not be seen, and the muscles of the neck and abdomen were very rigid, but no opisthotonos existed. His mental powers were intact. He stated that he had lately slept quite comfortably as far as the cough would permit him, and that his bowels were freely open. A blister was applied to the nape of the neck, and he was galvanized.

On the morning of the 20th the mouth could be opened more freely, and the muscles of the neck were less rigid. He had had a good night's rest. The pulse was 72, and rather feeble. The expectoration was the same. Stimulants were given, and sinapisms applied to the chest.

March 21*st.*—During the night the cough was unusually troublesome, and this morning, although again galvanized, the jaws were more closely fixed. He complained of pain in the abdomen and rigidity of its walls, but there was neither rigidity nor pain about the extremities. The pulse was feeble, and the skin cold and damp. But little food could be taken. His mental functions were unaffected, and he was himself quite conscious of his extreme danger. The bowels were confined. Two purgative enemata were administered, and the bowels opened by them, and when I saw him at 10 p.m., the jaws had become almost quite closed. Opiates were given, and galvanism repeated.

March 22*nd.*—Cough was somewhat easier, and there was less distress owing to difficult respiration, but he had passed a very restless night. His aspect was dejected, and he complained much of pain above the body, specially at the abdomen, striking up thence to the chest. There were slight startings of the muscles of the leg. No dysphagia existed, and nourishing fluids were poured into his mouth, which he swallowed. He was quite sensible, but somewhat drowsy from the opiates, and said that he "wished to choke rather than be tortured any longer by the phlegm." The pupils were quite natural, and also the general sensibility of the skin, which was freely perspiring. Quinine was ordered to be given in small and frequent doses. I saw him again at 10 p.m., when he was suffering more from pain, specially at the abdomen, which was, as before, very rigid, and in the head. The abdominal pain was somewhat relieved by hot epithems applied. No twitchings of the muscles were perceived, and the cough was less, the urine was passed freely and naturally, but the bowels were confined. The pulse was weak and the skin moist.

23*rd.*—It was reported by the nurse that he had slept much in the night, that at 3 a.m. there had been considerable starting of the right arm and thigh, and that at 7 a.m. the patient had seemed brighter and more cheerful, and had passed urine naturally and plentifully. At 8.45 a.m. there was much starting in both arms and legs, but no spasm in the face, and no dysphagia and no dyspnœa. The vacuations were passed voluntarily and naturally. The pupils were natural, but the pulse much weaker. The patient gradually sank, the lips becoming dark purple, and he died at 9 p.m. without suffering any pain. The nurse spontaneously remarked that she "never witnessed a quieter death," and that there was "not even that long-drawn breath of death" which is so common. She stated that when he slept there was no relaxation of the muscles of the jaw, which remained quite fixed, but that directly after death the jaw fell, and had *twice* to be tied up.

Post-mortem examination made twenty-seven hours after death.—The body was in good condition and well made. There was rather a large amount of clear fluid beneath, both the cerebral and spinal arachnoïd membranes and the brain being watery, showing numbers of bloody puncta on section, and rather softened generally. The lungs were highly congested, and the lower part of the left one was partially hepatized. The heart was healthy, and the blood contained fluid. There was much blood under the fascia lining the posterior surface of the abdominal muscles as high as the umbilicus, and the right rectus abdominis muscle *was found to be ruptured almost entirely across at a point about two and a half inches above its origin.* The ruptured muscle was exhibited to the members of the Pathological Society by the late Mr. Gray, and is described at page 381 of the sixth volume of their 'Transactions,' but the history of the case is not there given *in extensó.*

Remarks.—The points of history connected with this case appear to be—1stly, the primary symptom being the "stiffness" of the jaw, and the pains about the shoulders and neck on the following day resembling rheumatic pains; 2ndly, the occurrence on the *fifth* day of pain and rigidity of the abdomen, whilst the extremities were free, and on the sixth day of headache and startings of the legs, and on the *seventh* day of startings in the right arm and thigh; 3rdly, the freedom from any extreme pain, the good sleep obtained, and the partial remission of symptoms; 4thly, the rupture of the muscle during life; 5thly, the complication of symptoms by previous lung disease; and, 6thly, the absence throughout of opisthotonos, or of dysphagia, or dyspnœa. [92.]

Case 17.—*Tetanus following a gun-shot wound of the thigh and hip-joint: softening of the spinal cord.*

Timothy C—, æt. 28, was admitted into St. George's Hospital January 8th, 1858, having sustained a gun-shot wound of the thigh a hand's breadth below the groin and into the hip-joint. On admission collapse existed, and the pulse was small, and 108 per minute. Diffusible stimulants were given, but he became worse, and much pain was experienced. The pain increased in paroxysms, and a sloughy state of the wound came on. Castor oil and an enema of turpentine were exhibited, and subsequently port wine and full diet were given. In the evening of the 13th, he complained of stiffness about the neck and jaws, and on the following morning had slight convulsive action of the muscles of the back, which somewhat subsided in the course of the day. In the night tetanic convulsion increased, and also strong opisthotonos had set in. He died at 1 o'clock p.m.

Post-mortem examination.—The limbs generally were very rigid.

The heart weighed fourteen ounces; the left ventricle was very contracted.

The bones and the cerebral membranes were natural. A slightly increased amount of fluid existed in the ventricles, and beneath the arachnoïd membrane, which was a little thickened. The brain was pale.

The bones of the vertebræ and spinal membranes were natural. The spinal cord was firm, excepting about the level of the eleventh dorsal vertebra, where it was so softened as to be diffluent.

Remarks.—Notice, 1st, the traumatic origin of the affection; 2nd, the early symptoms being rigidity of the muscles of the jaw and neck coming on at the 6th day. [14.]

CASE 18.—*Tetanus following compound fracture of a leg.*

E. W—, a quiet and dull child, æt. 8, was admitted into St. George's Hospital, January 27th, 1858, with compound fracture of the leg. On the day after admission he was restless and feverish, and a calomel purge was given. On the 30th the pulse was small and rapid, the tongue furred, and he had pain and rigidity about the face and neck. Wine and ammoniated salines were given. During the next few days the pain and tetanic symptoms of the face were more marked, and he soon lost power of mastication.

On the 6th of February he was attacked by paroxysmal tetanic stiffness of the jaw and muscles of the back, and five minims of tincture of opium were given every four hours, to which tincture of cinchona bark was subsequently added, and wine given *ad libitum.* Beef-tea enemata were administered. The tetanus increased, and belladonna was subsequently given every six hours, but in vain, and he died on the 18th.

Post-mortem examination.—A considerable sized nerve was traced into the large granulating surface of the leg, but it appeared to be quite healthy. The tibia was found fractured.

Brain and spinal column.—The contents appeared to be quite natural.

The other organs of the body were natural.

Remarks.—Notice the supervention of tetanic symptoms on the third day after injury, and the freedom of the nervous centres from congestion. [44.]

Case 19.—*Tetanus following injury of the thigh; a portion of iron being retained in the wound.*

William D—, æt. 61, was admitted into St. George's Hospital Nov. 5th, 1858, having received on the same day a gunshot wound of the thigh. Suppuration resulted, and the pus was evacuated. On the 13th he became feverish, and great pain in the thigh came on, and at night trismus supervened, followed by delirium. On the day afterward the tetanic spasms increased, attended by difficulty in articulation and dysphagia, and the muscles of the neck and abdomen became rigid and hard. Attacks of opisthotonos came on, and the spasmodic paroxysms continued constantly with intervals of only about five minutes. The wound sloughed, and a piece of iron (two inches by one) was found imbedded beneath the fascia lata. Stimulants were freely given, and turpentine injections every three hours. The tetanus continued until death, no pain except at the wound being experienced, and this was excessive just before the tetanic attacks came on. Three quarters of a grain of morphia were injected subcutaneously, but no good result followed. Death occurred November 16th,

Post-mortem examination.—The rigor mortis was very strong. No nerves were found involved in the wound. *Cranium* and *Spinal* column. Much subarachnoïd fluid in the cranium existed, and the dura mater was very adherent to the bone. Otherwise the brain was natural.

The spinal cord was natural.

Thorax.—Excepting pleural adhesions the thoracic organs were natural.

Remarks.—Notice the trismus as the earliest symptom, as occurring on the *eighth* day after the injury. [272.]

Case 20.—*Tetanus following the wound of an artery.*

Alfred B—, æt. 6 years, was admitted into St. George's Hospital June 8th, 1859, with a punctured wound of the ulnar artery, which sloughed. Much blood was lost. On the 11th he became very sick, and convulsions came on, followed by loss of consciousness and dilatation of the pupils. The entire body was thrown into a condition of tonic spasm, and in spite of treatment he died on that day.

Post-mortem examination.—Excepting that the brain was wet and rather pale, nothing observable was found which might account for the tetanic spasm.

Remarks.—Notice, 1st, the advent of the convulsion; 2nd, tonic spasm on the *third* day after the injury. [135.]

CASE 21.—*Tetanus following a burn, incurred by a fall during an epileptic attack.*

John D. R—, æt. 24, was admitted into St. George's Hospital October 19th, 1859. When aged 7 he had a blow on the left ear followed by a fit, and since then had had a fit every year. In one of them he fell into the fire a few days before admission, and burnt the left arm, side of chest, and hip. Opium was given to allay pain, and subsequently bark and wine.

On the 22nd he became slightly delirious, and passed his motions in bed. Four ounces of brandy were given in the twenty-four hours, and he improved until the 27th, when he became restless, and slept badly. After eating dinner on this day he observed some stiffness of his jaws, and on the next day could only open his mouth slightly, the pulse being 112 and weak. The soft parts about the shoulder sloughed much. Ammoniated salines with excess of ammonia and laudanum were given. He slept slightly during the night, but on the 29th had frequent pain in the affected arm, running up to the face. The jaws were quite rigid and firmly closed, the abdomen hard and tense, the pulse 126, and feeble, and the skin profusely sweating. The bowels were confined, and an enema of turpentine was administered. An increase of laudanum was ordered, but in the evening opisthotonos came on, and attacks of this continued during the night, and on the next morning (the 30th). During one of the attacks the jaw became relaxed, but in several he was almost suffocated owing to spasm of the glottis. In spite of stimulants, &c., he sank, and became quite unable to swallow, and died shortly after one of the paroxysms.

Post-mortem examination.—*Thorax.*—The heart was natural, and its cavities firmly contracted; the lungs were natural.

Abdomen.—One or two patches of congestion existed on the inner surface of the duodenum, and the kidneys also were congested; the other organs were natural.

Cranium and spinal column.—The brain and spinal cord and respective membranes were natural. The nerves of the brachial plexus were traced into the affected arm, and found to be natural, excepting the sheath of the median nerve, which was a little more vascular than usual; but this was doubtful.

Remarks.—The earliest symptom being rigidity of the jaw-muscles and the traumatic origin of the affection are to be noticed. It may be asked how far did the epileptic tendency render tetanus the more likely to occur? [233.]

Case 22.—*Tetanus following a wound of the hand.*

William K—, æt. 32, was admitted into St. George's Hospital Aug. 25, 1860, with a lacerated wound of the left hand. He was going on well, when on the 27th, he had some stiffness of the neck and pain in the back, and violent cramps in the legs. The bowels were confined, and he had calomel and senna given, which gave much relief to symptoms. On the 31st, the forefinger looked very dark, and a small gangrenous spot was apparent near its extremity. On the following day all the fingers became gangrenous and sloughy, and on the next day (September 2nd) he was found in a cold sweat, with spasm in the muscles of the neck and back, and with a coated and brown tongue. Late in the evening the muscles of the abdomen were hard, and the jaws fixed. Spasms came on every quarter of an hour. Turpentine enemata were given and brandy, but the spasms increased in severity and intensity, and during the night opisthotonos came on. On the 3rd he had no rest from continuous spasms which occurred every five minutes. He was unable to swallow without severe convulsion occurring, and often after an attempt to swallow, spasm of the glottis came on. Of this spasm of the glottis he eventually died.

Post-mortem examination.—The wound was found to have been granulating well. The radial nerve was traced into the hand, and its branches were found to be natural. The median nerve was healthy. The brain and spinal cord and all the organs of the body were found to be natural.

Remarks.—Notice the occurrence and subsidence of suspicious symptoms on the *second* day after the injury; their recurrence in an aggravated form on the *eighth* day. [240.]

Case 23. *Tetanus following lacerated wound of the hand.*

James P—, æt. 60, was admitted into St. George's Hospital Oct. 8th, 1860, with a lacerated wound of the hand, which he had received six days before, but stating that on the morning of admission he had been seized with pain extending along the back to the occiput, and in the jaws, which he had an inability to open. In the afternoon he was found lying on his side with his head and shoulders drawn forcibly back, and unable to open his mouth more than half an inch. The pulse was weak, and 84 per minute. Three grains of calomel were given every four hours. On the 9th it appeared he had had but little sleep. He was in constant spasm, and the opisthotonos was more marked, the jaws being nearly closed. The pulse was 104, and very feeble. Dysphagia existed, and each attempt to swallow produced spasm. Three-quarters of a grain of morphia were injected subcutaneously into the arm, and some hours afterwards repeated with some apparent relief. He gradually sank and died without any spasm at the last on the same day, the 9th.

Post-mortem examination.—The branches of the nerves leading to the wound were found healthy. The brain and spinal cord and all the organs were found in quite a natural state, excepting the kidneys, of which the capsules were adherent.

Remarks.—Notice, 1st, that the early symptoms were pain in the jaws and occiput, and rigidity of the jaw-muscles; and, 2nd, that the *sixth* day after the injury was the day on which they began. [277.]

Case 24—*Tetanus immediately following cold and (at some distance) a graze on the hand. Rupture of the psoas muscle.*

Richard N—, carman, æt. 43, was admitted into St. George's Hospital April 9th, 1862. He had two months previously grazed his right elbow, and the back of the right thumb, but the wound had healed in a week. One month after this he caught cold owing to exposure to weather, and on the 6th of April, shivering, with sore throat, and some dysphagia occurred, and increased until his admission, when he could not open his mouth more than a quarter of an inch. Rigidity of the limbs, with pain over the abdomen generally, came on, the tongue was white, perspiration excessive, the pulse weak, and 96 per minute. A rhubarb draught, and subsequently stimulants and bark and hyoscyamus were given. He had turpentine stupes applied to the abdomen. He became much relieved, but two days after admission an attack of general spasm set in, followed by a second one, which immediately followed the exhibition of a dose of medicine.

On the 12th the abdomen was very tense, a slight "sardonic grin" existed on the countenance, and there was much hardness of the temporal and masseter muscles, but he could open his mouth half an inch. Dysphagia was great, and when he swallowed great contortion of the face was produced.

Very decided opisthotonos existed on the 13th. Æther and laudanum were given, and subsequently ice in bladders applied from the nape of the neck to the sacrum, and kept carefully in contact with the spine for three hours without any good effect. Subsequently a blister dressed with morphia was applied, and after that it was dressed with six grains of quinine every four hours. Two days later a third of a grain of extract of belladonna was added to each draught. On the day afterwards (the 16th) he was easier, the belly was softer, and the belladonna was increased until he took a grain at a time.

On the 18th he became speechless, and in all ways worse, the opisthotonos being greater. Six drops of the tincture of aconite were given every three hours. Delirium and delusions supervened.

He drank largely of wine and porter notwithstanding the choking. At last he died rather suddenly on the 20th.

Post-mortem examination.—Thorax.—The lungs were congested posteriorly.

Abdomen.—Much blood existed in the peritoneum *owing to a rent in the psoas muscle* two and a half inches in length.

Cranium.—The brain was healthy.

Spinal column.—The bones were natural. The spinal cord was natural excepting slight softening at the lower part; but this was thought to be probably owing to violence in the removal.[1]

Remarks.—Notice the approach of the attack by rigidity of the muscles of and about the jaw. [93.]

Case 25. *Tetanus following swelling and inflammation of the cheek.*

George W—, æt. 6, was admitted into St. George's Hospital July 12th, 1862, with trismus, rigidity of the limbs and hurried, noisy, and forcible breathing, through the nearly-closed teeth. It appeared that a few days previously, the cheek had been red and swollen, but the swelling had gone down. At intervals of from one to twelve hours tetanic convulsions affected the whole body, and after each attack he drank water, and often fell asleep. Extract of Belladonna was given in doses of one sixteenth of a grain, and a purge of calomel and scammony. Two days later a blister was applied to the nape of the neck, and the purging was continued. On the 16th he died in a violent tetanic convulsion.

Post-mortem examination.—Thorax.—The lower lobes of both lungs were solidified.

Cranium and spinal column.—The bones were natural. The cerebral dura mater was found adherent to the skull, and some old arachnoïd adhesions existed between the hemispheres. The brain and spinal cord were much congested, and especially the surface of the spinal cord.

Remarks.—Notice the great congestion of the nervous centres. [193.]

Case 26.—*Tetanus following a fall and bruise.*

William B—, æt. 38, was admitted into St. George's Hospital May 12th, 1863, having April 30th fallen a long distance to the ground, bruising the skin of the right leg. On the 11th of May he complained of stiffness of the neck, and some dysphagia. In the night the mouth became quite closed, and he had several "fits" of severe dyspnœa, accompanied by contraction of the muscles of the back. When admitted on the 12th the surface of the body was cold and clammy, and the

[1] This case has been related in the 'Lancet,' for Sept. 6th, 1862. See p. 256.

back was so curved that the arm could be passed between it and the bed. The risus sardonicus was decided. Pulse 100, regular. Every ten or fifteen minutes a momentary contraction of the muscles of the face and spine took place, brought on by attempts to swallow, or take deep inspiration. The breathing was almost quite abdominal. There was a deep sloughing wound of the surface of the right leg. Under the influence of chloroform, the actual cautery was applied along the back from the occiput to the middle of the dorsal region. The spasmodic attacks increased, and he died in one of them on the morning of admission. During the last four hours he could move his jaw freely.

Post-mortem examination.—Only the spinal cord could be examined; and this was done by Dr. Dickinson according to Dr. Lockhart Clarke's method of examination. Congestion of the vessels of the dura mater and of the spinal cord itself existed but nothing more.

Remarks.—Notice the stiffness of the neck and the dysphagia as the earliest symptoms, and their occurrence on the eleventh day after the injury; also the congestion of the spinal cord. [123.]

Case 27.—*Tetanus following a fall.*

Thomas S—, æt. 40, having been exposed much to cold and draughts, was admitted into St. George's Hospital June 5, 1864, with trismus of three days' standing. He received some injury in the back from a fall, but no wound or discoloration existed. He had much pain in the neck and chest, and slight dysphagia. The skin and pulse were natural. He was treated by belladonna and chloroform inhalations, and subsequently by turpentine injection, and extract of belladonna, gr. $\frac{1}{4}$ every four hours. His sleep was disturbed by twitchings, and the next day, though he had much pain from the clavicles to the spine and numbness of throat, yet he felt himself better. Opisthotonos, however, came on, and inability to flex the knees. Tobacco injections were given, and 1 gr. of extract of belladonna every three hours, and brandy freely administered. The pupils were not dilated. The pulse and respiration became very quickened. Later on chloroform inhalations were resorted to, but alarming lividity of the face and neck were produced by it. He died after two more severe spasms than usual, June 10th.

Post-mortem examination.—*Cranium.*—The vessels of the brain and its membranes were very congested. The arachnoïd was rather opaque.

The *spinal cord* and its membranes were very unusually vascular.

Thorax.—Old pericardial adhesions existed.

Abdomen.—The kidneys were granular. The liver fatty.

Remarks.—The cause of the affection in this case is doubtful. Most probably it was unconnected with the fall which the patient sustained. [190.]

(*To be concluded.*)

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.,
Member of the Royal College of Physicians, Physician to the Eastern Dispensary, London.

On the Use of Iodide of Potassium in the Treatment of Cachexiæ and other Diseased Conditions. By Sir Henry Cooper, M.D., of Hull.—After some general remarks upon the effects of iodine on glandular tumours and thickened structural deposits, and upon the almost specific action exerted by the iodide of potassium over periostitis, Sir Henry Cooper refers more particularly to the efficacy of the iodide in syphilis and chronic rheumatism. In these last-named diseases the iodide, although often successfully employed, sometimes fails; and Sir H. Cooper attributes the failure to the fact, that the drug is given only in moderate doses. He, therefore, following the plan formerly recommended by Dr. Elliotson, gives very large doses of the iodide, namely, from ten to thirty grains three times a day. It is a remarkable fact that, although a drachm of iodide of potassium represents a poisonous dose of iodine, yet thirty-grain doses may undoubtedly be given three times a day with perfect impunity; and it is still more remarkable that the occurrence of physiological symptoms, such as coryza, is almost unknown where the large doses are given. Sir H. Cooper then relates nine cases of syphilitic cachexia, in which the iodide of potassium was prescribed in large doses with marked benefit. With respect to the rheumatic cachexia the results were not so decisive, but still great amendment of the general symptoms usually followed, and sometimes also much relief of the local symptoms, from the same kind of treatment. In cases also of inflammation of mucous membranes, as in bronchitis, Sir H. Cooper thinks that relief is afforded more rapidly by combining the use of iodide of potassium with that of tartarised antimony than when either remedy is used alone, and he adduces two cases in support of this view.—*British Medical Journal*, September 28, 1867.

On the different kinds of Cod-liver Oil, and on those which are best adapted for Medical Use. By Dr. Cazin, of Boulogne-sur-Mer.— Dr. Cazin describes three kinds of oil, namely, the white, the brown, and the black, and there are intermediate varieties; and the white oil presents very different qualities, as superior white, ordinary white, and steamed white, the last being obtained by the aid of steam. The *white oil* is slightly acid to litmus paper; the taste is mild, like that of the fresh fish, and the smell is similar, and has nothing disagreeable. The *brown oil* has a reddish-brown colour, like Malaga wine or old rum, the smell of salt herring, a strong fishy taste, and a decidedly rough after-taste, and is more acid than the white. The *black oil* is of a greenish black-brown colour, analogous to that of tar, of a thicker consistence than the other two oils, not transparent, and very acid; the taste is very acrid, and the smell is empyreumatic and nauseous. The white oil is proved by chemical analysis to be rich in inorganic principles, especially iodine, phosphorus, and phosphoric acid, but the biliary principles and volatile acids occur only in small proportion. The brown oil may be considered as intermediate between the white and the black, as to its richness in active principles. The black contains the least iodine and inorganic principles; but the volatile acids, and the biliary principles left by the disintegrated parenchyma of the liver, predominate in a marked degree. Dr. Cazin, after considering the different constituents of the three kinds of oil, arrives at the conclusion that the white is the best adapted for medical practice; for although its action is certainly more slow, yet it is better borne by the stomach, and is taken without difficulty. It contains more iodine than the black, but rather less than the brown. The disgusting smell and the disagreeable taste of the black, and even of the brown oil, prevent their being regularly administered.—*Bulletin Général de Thérapeutique*, November 30, 1867.

On the Action of Belladonna. By Dr. D. de Savignac.—Although belladonna is a stupefying agent when it acts fatally, it is also an excitant; and this latter property, according to Dr. de Savignac, constitutes the fundamental character of its mode of operation. Its excitant properties are directed especially to the great sympathetic nerve, as is proved by the therapeutical applications of belladonna. The drug excites the contractility of the muscular fibres of the intestine, and thus promotes the alvine discharges. It acts besides on the biliary tubes and on the common bile-duct, rendering the stools bilious, and hence its beneficial influence in cases of hepatic colic due to the presence of biliary calculi. Its action on the sphincters appears to be of a nature to place their powers in harmony and equilibrium, for as these structures both dilate and contract, it is necessary that neither function should preponderate over the other. Thus belladonna in small doses excites the motive power both of the hollow viscera and of their sphincters, acting both on the dilators and contractors, and giving to the viscera the power of moving onwards the excrementitious matters until they are out of the system. Thus retention of urine may be due to paralysis or to spasm of the neck of the bladder; in the first case, belladonna excites the expulsive contractions, and in the second it removes the spasm. Dr. de Savignac also alludes to the advantages obtained from the local use of blisters spread with belladonna ointment in rheumatic pericarditis and endocarditis, and in organic diseases of the heart; and he attributes this effect to the use of belladonna in checking the disorder, the irregularity, and the morbid energy of the beats of the heart, and in restoring their normal rhythm. This property, which is analogous to that possessed by digitalis, may probably be referred to an exciting action of the vagus nerve, after what has been taught by M. Germain Sée on the functions of this nerve as a regulator of the movements of the heart. In its action as an external application, belladonna is at once a sedative and resolvent; and thus, when applied in the form of ointment to joints affected with rheumatism, it relieves the pain and dissipates the swelling, and its resolvent powers are further proved by its efficacy in the treatment of glandular swellings; and oculists daily make use of it for the purpose of promoting, after the operation for cataract, the absorption of the fragments of the capsule of the crystalline lens, or of the products of consecutive iritis. Where there is resolution there is necessarily excitation, both nervous and vascular, of the parts affected. Dr. de Savignac then refers to the employment of belladonna in cases of asthma, which is caused by spasm or paralysis of the muscular fibres of the bronchial tubes, most probably the latter: but in whichever way the phenomena of asthma are explained, the action of belladonna is intelligible, for it will act as a sedative in case of spasm, and as an excitant in case of paralysis. But although Dr. de Savignac has proved that, in many cases, the therapeutical action of belladonna depends upon a common excitation of the dilating and constricting muscles, he does not wish to deny that its excito-motory properties have an elective tendency to act upon the dilators, as is well seen in the case of the iris, the dilators of which are excited by the smallest doses of belladonna or atropine. The anæsthetic and sedative powers of belladonna, although well known, are not easily explained upon physiological principles; and all that is certain is, that the drug, like opium, relieves pain, but the *modus operandi* is still to be sought for.—*Bulletin Général de Thérapeutique*, November 30, 1867.

On the Treatment of Malarial Fevers by the Subcutaneous Use of the Sulphate of Quinia. By Dr. E. C. Seguin, of New York.—After giving a brief sketch of the recent history of the subcutaneous injection of quinia in malarial fever, Dr. Seguin states that this method was first tried in the New York Hospital by Dr. G. M. Smith in 1866. Since then it has been applied to all cases of that fever, and is now a part of the regular practice of the institution. The solution used is composed of sixty grains of subsulphate of quinia, forty minims of dilute sulphuric acid, and a fluid ounce of water, and thirty-five minims of this solution are equal to four grains of quinia. The solution may be varied by the addition of four or six grains of sulphate of morphia, by which the injection is rendered less painful. It was found that the abscesses sometimes caused by the injection were due to the presence of undissolved crystals of quinia, or the occasional accidental introduction of particles of dust; and it was also found that an excess of acid generally removed the most fertile source of danger, while it only slightly increased the pain of administration. The cases of fever received were very severe, and it was therefore necessary to use much larger doses of anti-periodic medicines than are usual in northern practice. One grain of quinia under the skin was equivalent to five or six by the mouth, but in the treatment mouth-doses of quinia and iron were employed at the same time. In the intermittent cases the most satisfactory results were obtained, but in the remittent ones the experience was small and not satisfactory. By the contemporaneous use of the hypodermic method and giving doses by the mouth, an immense saving was effected in the use of the medicine, not more being used in the entire course of many cases than was formerly required to avert a single paroxysm.—*New York Medical Journal*, December, 1867.

On the Treatment of Cancer by Acetic Acid Injections. By Dr. F. D. Lente, of New York.—Dr. Lente has tried the acetic acid treatment in two cases of cancer; and as he does not intend, at least for the present, to repeat the trial, he gives the results in detail. One case was that of a scirrhous tumour of the left mammary gland in an unmarried lady about sixty years old. It did not involve the nipple, and the axillary glands were perfectly free from disease. Dr. Lente punctured the tumour; and having passed a needle into its substance, he injected upwards a quarter of a drachm of a solution of acetic acid in water, one part to three, and the same quantity downwards. The injection caused intense pain, which was relieved only by anæsthetics. A drachm more was subsequently injected while the patient was under ether, and afterwards half a drachm more. An abscess was formed, which however soon healed; but the patient was so worn out by the pain and the confinement, that an operation was at length resorted to, and the tumour having been removed the patient recovered. The other case was one of epithelioma of the prepuce and glans penis, and the disease had lasted about eight years. The diseased mass was nearly as large as a duck's egg; and when it was ulcerated, it had the appearance of a cauliflower excrescence. Bromine was employed at first, after the excrescences had been removed by a bistoury and the application of the actual cautery, the bromine being applied to the raw surfaces, but acetic acid was subsequently injected. The progress of the disease, however, was not arrested, and the glans was becoming still further involved, and amputation was therefore advised.—*New York Medical Journal*, December, 1867.

On the Use of Powdered Alum in a case of extensive Burns. By Dr. W. M. Turner, of Philadelphia.—The case mentioned was that of a man who, when in a state of intoxication, was scalded by a large quantity of boiling coffee; and when he was seen by Dr. Turner, a week after the accident, the exterior portion of the right thigh and hip was burned to the length of fifteen inches, with an average breadth of eight inches and a half. The left thigh was also scalded, the injury extending from the scrotum to the patella. There was an open wound on both legs, and there was profuse and offensive suppuration. The usual treatment was employed for some days, consisting of poultices, rags wrung out of carron oil, tonics, opium, and moderate stimulation, but without much benefit, and the application of nitrate of silver to the edges of the wounds caused such intense pain, that it was discontinued. To add to the gravity of the case, hæmorrhage supervened one day with such violence, that Dr. Turner was obliged to have recourse to the first styptic he could find; and as there was some burnt alum in the house, he applied it in powder to the bleeding surface. On examining the wounds next day, he was surprised to find that not only the hæmorrhage was arrested, but the surface of the wounds to which the alum had been applied presented a healthy appearance, and seemed inclined to heal. Dr. Turner therefore employed the alum regularly as a dressing; and from the time that he did so, each wound continued to improve till it entirely closed; and what is still more extraordinary, it left no drawing cicatrix. Dr. Turner ascribes the successful termination of the case entirely to this accidental employment of alum, and he thinks it worth a trial in other similar cases.—*New York Medical Journal*, January, 1868.

On the Therapeutical Action of Medicines in dilated conditions of the Blood-vessels. By Dr. A. Reith, of Aberdeen.—In this paper, which is altogether theoretical, Dr. Reith offers an explanation of the action of medicines, founded upon recent views connected with the pathology of the nervous system. The origin of almost all diseases is supposed to exist in the nervous centres, consisting of the joint cerebro-spinal and vaso-motor systems. Irritation of the vaso-motor nerves, as has been shown by Bernard and Brown-Séquard, causes contraction of the blood-vessels, while division of the same nerves causes dilatation, and in the latter case inflammation is set up. Now Dr. Reith assumes that the action of all medicines is primarily on the vaso-motor system, and he considers that this action is analogous to the production of inflammation. In other words, medicines, whatever may be their operation, possess the double property of stimulating and paralysing the sympathetic system, that is to say, of contracting and dilating the blood-vessels. As a practical application of this theory, it is stated that, if inflammation is a paralysis of the vaso-motor system, and if medicinal agents also paralyse the same system, inflammation ought to be aggravated by medicines in their usual doses, and experience confirms this supposition. But if, continues Dr. Reith, medicines be given in less than their usual doses, so as to produce only one of the two effects first mentioned, namely, so as to induce only contraction, then inflammation is antagonised. But the doses which ought to be used are not yet determined, and must be fixed by experience, which Dr. Reith is preparing to supply. He goes on to state that the theory of the action of medicines being analogous to the process of inflammation, and of medicines in small doses being antagonistic to disease, seems to lend some countenance to the doctrines of homœopathy; and he admits that it does so, but not in the sense in which the followers of Hahnemann carry out these doctrines, the extravagances of that sect being such as to obscure whatever is good in the theory.—*Edinburgh Medical Journal*, February, 1868.

On the Medicinal Properties of the Cherry Laurel. By J. Bröker.—Mr. Bröker, a Dutch pharmacologist, has instituted a series of investigations to show the influence exercised on the proportion of prussic acid in the leaves of the cherry laurel (*prunus lauro-cerasus*), by the mode of preparation, and the season of the year in which the leaves are gathered. After giving the details of the chemical processes by which the relative amount of hydrocyanic acid in the leaves is estimated, he states that the proportion of this acid varies very considerably; and he gives a table showing that leaves gathered in different months of the year contain very different amounts of the acid. Thus he finds that the leaves gathered in July contain the greatest proportion of prussic acid, and those gathered in February contain the least; and he states, besides, that the nature of the soil, the character of the weather, and the constitution of the leaves themselves, and their age, whether one or two years, influence the proportion of prussic acid which they contain. He is, therefore, inclined to think that the *cherry-laurel water* might be safely excluded from the pharmacopœias, inasmuch as the bitter almond contains a more definite and constant proportion of prussic acid, and is, moreover, more readily to be obtained than the cherry-laurel leaves. This view is confirmed by the results of the investigations made by two Dutch pharmaceutists in the laboratory of the University of Utrecht, who found that out of twenty-seven specimens of cherry-laurel water, obtained from different shops, only nine contained more or less exactly the amount of prussic acid prescribed in the codex, and that the highest proportion of prussic acid compared with the lowest was as 9 to 1.—*Nederl. Tijdschr., quoted in Schmidt's Jahrbücher der Gesammten Medicin.*, April 2nd, 1868.

On two novel Applications of the Iodide of Potassium. By Dr. A. De Beaufort.—The treatment of the chronic inflammations of the mucous membranes is most simple and efficacious whenever topical remedies are employed on the diseased structures; but it is often difficult, or even impossible, to apply these remedies in this direct manner. Among the mucous surfaces which it is difficult to reach directly are those of the lacrymal canals and the uterine canal; and practitioners are well aware of the unsatisfactory results of the treatment of the morbid conditions of those passages. Dr. Beaufort has, therefore, been induced to try the introduction, through the channel of the circulation, of a medicinal agent which might produce similar results to those obtained by balsamic and terebinthinate remedies in the vesical and urethral mucous membranes. He turned his attention to the iodide of potassium, which, when taken internally, is eliminated in such great abundance by the secretion of the tears, and which may also be detected, though in less quantity, in the mucous secretion of the uterus; and clinical observations confirmed his therapeutical hypothesis. His first attempt was in the case of a young woman of a scofulous temperament, affected for three months with an engorgement of the lacrymal sac, with incomplete obstruction of the nasal canal. He instituted an exclusive treatment by the iodide in the progressive dose of twenty-five centigrammes to a gramme (a gramme is about fifteen English grains). At the end of a fortnight the symptoms had ceased, and the tears had resumed their normal course. Dr. Beaufort thinks that the iodide could not have acted in so short a time upon the constitutional malady under which the patient suffered; and he, therefore, infers that the salt must have been carried by the circulation to the diseased membrane, and fresh observations confirmed him in this view, for he subsequently treated patients of a good constitution attacked with chronic inflammation of the lacrymal passages, and the results were equally favorable. He expresses his conviction that in many cases this treatment will supersede other painful and tedious methods of medication. In cases of chronic inflammation of the uterine mucous membrane the success of the treatment by the iodide was less rapid, and the instances were less numerous; but they seem to Dr. Beaufort to be sufficiently remarkable to deserve the attention of the profession. In several instances of internal metritis, with abundant leucorrhœa and the train of symptoms which torment so many females, he has seen remarkable benefit follow from the use of the iodide, after other treatment had been tried in vain. It is necessary te increase the dose to a greater extent than in the other cases mentioned, namely, from one to two grammes, and to insist on its employment for a longer period. The engorgement of the organ is relieved in the most striking manner, and when this has disappeared, the symptoms due to the affection of the mucous membrane cease more easily. Dr. Beaufort has also found the iodide serviceable in cases of granular and ulcerative affections of the neck of the womb. He conceives that the remedial action is owing to the alterative properties of the salt in *devascularizing* the inflamed mucous membrane, and in restoring its capillary texture to its normal type. Even if it is insufficient by itself to effect a perfect cure, it is a useful auxiliary to other methods of treatment.—*Bulletin Général de Thérapeutique*, January 30, 1868.

On the Treatment of Phagedenic Chancre by the Internal Employment of Calomel. By Dr. Belhomme.—Dr. Belhomme remarks that only two medicines employed internally have been considered efficacious in the treatment of phagedenic chancre, namely, opium and arsenious acid, and that although Ricord has sometimes succeeded by the mercurial treatment in curing the disease, yet the preparations of mercury have been regarded as injurious by most authors. The local applications recommended, however, are numerous, including the actual cautery, nitric, sulphuric, and hydrochloric acids, chloride of zinc, nitrate of mercury, &c. But although mercury has been supposed by most authors to promote the appearance and the spread of phagedena, and even, as it would appear, by Dr. Belhomme's admission, by himself in a former memoir, yet he now believes that calomel, employed internally, may be used with advantage in this affection. He was induced to try its effects by recollecting that one of his former instructors, Dr. Gibert, of the Hôpital St. Louis, occasionally administered calomel in non-syphilitic phagedena, and hence Dr. Belhomme used it also in syphilitic cases. But he has always given the mineral in small doses, repeated at regular intervals, the patient taking from five to seven centigrammes (a centigramme is the one hundredth of a gramme, equivalent to about fifteen grains) every day in packets of one centigramme each, two in the morning, two in the afternoon, and two or three in the evening. Only four cases are related, and in them the treatment appears to have been successful; the duration of the cure is said to be from three weeks to a month in the simple cases, but much longer in the severe cases of ulceration.—*Ibid.*, June 30, 1868.

On the Indications and Contra-indications of the Employment of Alcohol in the Treatment of Pneumonia. By Dr. Pécholier, of Montpellier.—The treatment of pneumonia by alcohol has lately found considerable favour among French physicians; but they by no means advocate the indiscriminate use of this agent in all cases of pneumonia. M. Pécholier has lately endeavoured to determine the question as to the indications and contra-indications of the alcoholic treatment, by a series of clinical observations made on a certain number of cases which seemed favorable to the attempt, and the summary of the results in five cases is recorded, a cure having been accomplished in each instance. From the facts observed, and the theories advanced as to the properties of alcohol, especially the property it possesses of awakening the dormant remedial powers of the constitution, and that of arresting the waste of the tissues, M. Pécholier concludes that alcohol is suitable for the treatment of cases of adynamic pneumonia, such as are comprised in the following categories:—1. In habitual drunkards, in whom the disease assumes an entirely special character, and in whom the beneficial effects of the alcoholic treatment have long been recognised. 2. In cases where the pneumonia presents itself under special characters, whether dependent on the *etiology*, as the adynamic medical constitution, the spring or autumn season, old age and weakness, enfeebled constitution, &c., or in relation to *local symptoms*, as in latent pneumonia, where the rational signs are wanting, or even the physical signs are slow in manifesting themselves, or in cases marked by *general symptoms*, as prostration, dorsal decubitus, coma, delirium, pale face, pulse rather slow or too frequent but very compressible, weak action of the heart, &c. 3. Where the disease, although sthenic at the beginning, has changed its character, and presents symptoms of adynamia, whatever may be the cause of the change, the peculiar nature of the affection, the abuse of bleeding, and other depleting measures, &c. 4. In many cases of double pneumonia, especially when the second lung is attacked a certain number of days after the first, and when the patient has been weakened by the treatment of the primitive lesion. 5. In the pneumonia of the aged. 6. In secondary cases of pneumonia, which supervene in the course of other febrile diseases. 7, and lastly. In all cases, whatever may be the age of the patient, or the season, or the climate, when an attentive analysis of the symptoms, the state of the pulse, or that of the functions of innervation, shows that the pneumonia is really asthenic. The contra-indications of alcoholic remedies in pneumonia are easily deduced from the above considerations. The principal of them are the winter season and dry cold, the youth and vigour of the patient, a full and hard pulse, a red and injected face, or, in a word, when the symptoms are characteristic of the condition known as *inflammatory fever*. A very red and dry tongue, and a painful condition of the epigastrium, may also be regarded as contra-indications, even in adynamic cases. The editors of the 'Bulletin,' in quoting M. Pécholier's views, agree with him that the doses of the alcoholic fluid should be proportionate to the severity of the case and the peculiarities of the constitution; and they further observe that the doses should be so divided as never to produce too great an excitement, which may be followed by depression; and that the remedy should never be too quickly stopped when it has produced its effects, but should be given for some days longer in decreasing doses.—*Montpellier Médical, reviewed in the Bulletin Général de Therapeutique*, May 15, 1868.

On the Employment of Phosphorus in Medicine, and especially in Progressive Locomotor Ataxy. By Dr. DUJARDIN-BEAUMETZ, of the Hôpital de la Pitié.—In the first part of the series of papers on the above subject, Dr. Dujardin-Beaumetz gives a sketch of the different preparations employed by the earlier physicians who prescribed phosphorus for the cure of disease, the medical applications of this substance having followed close upon its discovery in 1667 by Brandt, an alchemist of Hamburgh. The solution of phosphorus in oil is the preparation most usually employed, for the solution in sulphuret of carbon is dangerous, owing to the poisonous character of the latter menstruum. Dr. Dujardin-Beaumetz then proceeds to describe the most suitable doses of phosphorus, those employed in early times having been manifestly too large, and even dangerous. Thus a Jew was poisoned by three grains of phosphorus given as a medicine for an attack of apoplexy. Dr. Dujardin-Beaumetz recommends that the phosphorus should be given at first in doses of a milligramme (one thousandth of a gramme, which is equivalent to about fifteen grains), and to increase the dose gradually until some marked effect is produced, either on the digestive system, or on some other system or organ. It must be recollected that phosphorus is an accumulative medicine, and that great care must be taken in the progressive increase of the doses.

Dr. Dujardin-Beaumetz has been led to employ phosphorus in progressive locomotor ataxy for two reasons, first, because this disease has hitherto resisted all kinds of treatment; and, secondly, because in this disease two symptoms are found, in which the curative action of phosphorus has long been recognised, namely, amaurosis and impotence. He then gives the details of four cases of progressive locomotor ataxy, in which the phosphorus was administered with considerable benefit. Three were males, and one was a female. In two cases the phosphuretted oil was employed, and in the other two capsules of phosphuretted chloroform (ten decigrammes of chloroform to one milligramme of phosphorus). In all the cases there was decided improvement of the symptoms, the gait became less uncertain, and the incoordination less marked; there was more firmness in walking, and this was proved by the power of walking to considerable distances, and of going up and down stairs. The general sensibility was but little altered by the treatment except in one case. The eyes, which were more or less affected in all the cases, were not at all improved by the use of the phosphorus. The genital organs were strongly affected only in one case, in which the patient had numerous erections. A curious effect of phosphorus was observed, in addition to the other results, namely, a general feeling of satisfaction or contentment, which made the patients who were subjected to the treatment ask for its continuance. The patients all bore the phosphorus well, and their general health was never disturbed for a moment. Some symptoms, referable to the digestive organs, such as diarrhœa and sickness, were sometimes developed during the treatment; and they are of great importance in determining the graduation of the doses. As soon as these symptoms appear, the doses must be suspended for a time, and resumed after a day or two have elapsed. Dr. Dujardin-Beaumetz has reached from one milligramme of phosphorus to eight milligrammes without producing any disturbance of the digestive organs; but in general this effect is produced when five milligrammes have been reached. He admits that the cases recorded are too few to afford definite results at present; but he recommends a further trial of phosphorus in the affection described.—*Bulletin Général de Thérapeutique*, Jan. 15, Feb. 29, March 18, 1868.

On the Assimilation of Phosphate of Lime and its Therapeutical Employment. By Messrs. Dusart and Blache, of Paris.—The authors of this paper have endeavoured to determine the question whether the phosphate of lime enters into the system by the transformations it undergoes in the stomach, or whether it is necessary, for the purpose of assimilation, that it should undergo a previous elaboration in a living organism. The experiments they have instituted appear to show that the solution of the phosphate in the juices of the stomach is influenced by the form which the phosphate assumes, for while the hydrated phosphate is rapidly dissolved, calcined bones and hartshorn are not sensibly dissolved; and specimens containing carbonate of lime are dissolved only imperfectly. Messrs. Dusart and Blache, therefore, propose, as the best preparation for assimilation, the hydrated phosphate which has already been subjected to the action of the gastric acids, and which they call *lacto-phosphate of lime*. This substance has an agreeably acidulous taste, and is readily digested. Experiments were made upon some of the lower animals, with a view of determining whether the repair of fractured bones was accelerated by the internal use of the phosphate, and it was found that such was really the result. Under the use of the *lacto-phosphate of lime*, Messrs. Dusart and Blache found that the increase in weight of the bones of the animals exceeded by more than 33 per cent. the weight of the animals subjected to ordinary treatment. The animals chosen for the experiment were guinea-pigs.—*Ibid.*, July 30, 1868.

On the Treatment of Aneurism by Iodide of Potassium. By Dr. G. W. Balfour, of the Royal Infirmary, Edinburgh.—Dr. Balfour commences this very interesting paper by relating the particulars of three cases, treated under his own direction, in which the results were very satisfactory. In one of the cases there was apparently an inveterate aneurismal diathesis, as there were several aneurisms; but still, under the use of the iodide of potassium, given in thirty-grain doses twice a day for a considerable period, the improvement was well marked, and the symptoms of aneurism disappeared in certain arteries, although they persisted in others. Dr. Balfour, after remarking upon the serious character of the disease, and the hazardous remedies sometimes proposed for its relief, expresses his belief that the success achieved in his cases has been sufficiently well marked to justify him in recommending a further trial of the iodide in the treatment of aneurism. The use of the iodide for this purpose has been adopted, not from speculative ideas, but from empirical observations, which have been made almost accidentally, for the first case so treated seems to have been one under the care of M. Nélaton, who administered the iodide only because the patient stated that he had derived benefit from its use under other surgeons. To the surprise of M. Nélaton all the symptoms were improved, and the tumour almost entirely disappeared. M. Bouillaud next followed out this plan of treatment in four cases, in one of which the improvement was well marked; and in the East Indies Dr. Chuckerbutty, of Calcutta, found out, almost accidentally, that an aneurism of the innominata became gradually solidified under the use of the iodide given to relieve a bronchial complaint, from which a patient was at the same time suffering. Hence Dr. Chuckerbutty was induced to treat several other cases of aneurism in the same manner, and he has recorded the particulars of three. In two of them the result was fatal; but still, even in these, considerable relief was obtained, and in one the sac was found after death filled with dense, solid coagula. Dr. Chuckerbutty points out that the consolidation of the contents of the aneurismal sac is the important fact in the history of these cases, and he thinks that this result depends upon some property of the iodide in reference to the coagulation of the blood. Dr. Roberts, of Manchester, has also employed the iodide in the treatment of aneurism with very considerable success. Dr. Balfour remarks that out of fifteen cases of aneurism treated by the iodide of potassium, of which the particulars have hitherto been published, there has been relief in a marked degree to the sufferings of the patient in all but one, and in twelve there was undoubted diminution in the size of the sac; while in a few there has been apparently a perfect cure. In order to effect the desired end, several circumstances must be carefully attended to, and especially the dose, which, in the cases related, varied from five grains to thirty, three times a day. Coryza, salivation, and diarrhœa, occasionally follow the use of the iodide; but in such cases the inconveniences may be obviated by the temporary discontinuance of the drug. The iodide must also be continued for a very considerable time in most instances, some cases requiring at least twelve months. Rest in the recumbent posture is also a matter of paramount necessity, and the diet should be carefully regulated, avoiding the evils of starvation on the one hand, and of plethora on the other. Dr. Balfour does not agree with Dr. Chuckerbutty, that the efficacy of the iodide depends upon its power of increasing the coagulability of the blood; but he attributes to it a sedative action on the nervous system analogous to that exercised by the bromide of potassium.—*Edinburgh Medical Journal*, July, 1868.

On the Action of the Hyposulphite of Soda in Intermittents. By Dr. Chubb, of Cambridge, U.S.—Dr. Chubb made trial of the hyposulphite of soda in several cases of malarial disease, and the results were very satisfactory. Out of twenty-seven cases in which it was administered, the paroxysms were arrested in twenty-five, and in eleven of them the arrest was immediate. In five cases, however, relapses occurred; but in three of these the disease was again arrested by the use of the hyposulphite, and did not return; in the other two of the relapsing cases, sulphate of quinine was resorted to in order to complete the cure. In one case the patient, a female, had been the victim of ague for twelve months, during which she had been drugged to excess with quinia, iron, &c., but had never passed more than two weeks without a recurrence of the chills, and her general health was much impaired. She took the hyposulphite in doses of fifteen grains every two hours, and had but one paroxysm after the treatment was instituted. She had no relapse since her recovery. Out of the twenty-seven cases two are recorded as failures; but in one the failure was not complete, for the paroxysms were mitigated in severity. In the other case the failure was decided, for although the patient persevered in taking from fifteen to twenty grains of the hyposulphite every two hours for a week, no real improvement was obtained; but the disease was at once cut short by the adoption of ordinary treatment. Dr. Chubb considers that the hyposulphites constitute a valuable addition to the remedies used in the treatment of malarial disease, but that in the majority of cases they are less prompt in their action than the preparations of cinchona.—*American Journal of the Medical Sciences,* April, 1868.

REPORT ON MIDWIFERY

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I.—The Non-Pregnant State.

1. *Spontaneous Rupture of the Uterus with Intra-uterine Polypus.* By Dr. Larcher.
2. *On Sounding the Fallopian Tubes.* By Dr. Hildebrandt.

1. A woman was admitted into the Hotel-Dieu with pain in the abdomen. After four days profuse bleeding set in. She refused examination. Two days later meteorism and peritonitis appeared and she died. Section revealed diffuse peritonitis and adhesion of all the organs of the small pelvis. A polypus was found in the uterus seated in the anterior wall near the isthmus. The posterior surface of the cervix was ulcerated, and at one spot torn through, communicating with the cavity of the abdomen.—*Arch. Gén. de Méd.*, Nov., 1867.

2. Dr. Hildebrandt relates two cases in which he was satisfied that he passed the uterine sound several inches along the Fallopian tube. In one case, the patient had worn an intra-uterine pessary, and immediately after removing this, passing the sound he found its point went freely along the tube, and was felt through the abdominal walls. The end of the pessary he conjectured had distended the uterine orifice of the tube, and thus facilitated the entry of the sound, for after discontinuing the use of the pessary the entry of the sound was no longer easy. He refers to analogous cases by Veit ('Virchow's Handbuch der Speciellen Pathologie,' 1867), and by Matthews Duncan ('Edin. Med. Journ.,' 1856). He calls attention to this patency of the tubes as explanatory of those cases in which air or fluids penetrate from the uterus into the abdominal cavity.—*Monatsschr. f. Geburtsk.*, June, 1868.

II.—Gestation.

1. *A Case of Polypous Hyperplasia of the Decidua.* By Prof. Dohrn.
2. *Fibrous Myxoma of the Placenta: gestation in a two-horned uterus.* By Dr. Hildebrandt.
3. *A Case of Primary Abdominal gestation.* By Dr. Matecki of Posen.
4. *A Case of Tubo-ovarian Gestation.* By Dr. Baart de la Faille.

1. Professor Dohrn describes and figures with great care a case

of hyperplasia of the decidua. The condition has been described by Virchow, Strassmann, and Gusserow, as consisting in the knotty outgrowths on the free surface of the decidua. Dohrn's specimen came from a young woman who aborted in her first pregnancy at the end of two months. The decidua reflexa was covered on its free surface with small, long-stalked growths resembling polypi. No glandular openings, such as existed everywhere else on the decidua, were seen on these growths. · Microscopic examination showed distinctly the hyperplasia described by Virchow. The decidua contained many vessels and little fat, as is usual in aborted ova of this data. The polypus excrescences were especially vascular, and their tissues interspersed with small blood-extravasations. The decidual cells were larger than normal, larger than the cells taken from the parts of the decidua not affected by the polypus excrescences, and having much larger nuclei. The chorion was surrounded by villi, which were in many places undergoing cystic change. Dohrn considers the decidual disease to have been the cause of the abortion, by its disturbing the vascular development of the ovum.—*Monats. f. Geb.*, May, 1866.

2. Professor Hildebrandt relates the following case:—A woman pregnant for the third time. There was an excessive collection of liquor amnii. Labour set in spontaneously at about six months. The fœtus was living, but soon died. The placenta was very remarkable. It weighed one pound. The membranes were very thick, but normal. The rent was central. The cord was inserted in the margin; from this point there ran a venous branch diametrically across the placenta: in the same direction ran an arterial branch, but crossing the vein at an acute angle. Both vessels traversed the membranes, and formed a swelling the size of a fist supported on a stalk about an inch long. This tumour was embedded in the completely normal placental tissue. It had no connection with the placental tissues, except through the vessels described as its stalk. The mass of the tumour consisted of fibrous tissue, containing small round and oblong nuclei. In other places the tissue had the characters of mucous membrane. The cause of this tumour H. considers to be the abnormal condition of the vein and artery connected with it; the tumour itself being a degenerated cotyledon.

3. A woman about thirty years old, who had borne five living children, came under care in October with signs of marked hectic fever. She had menstruated in February. A quantity of foul-smelling purulent matter had been escaping from the vagina. Below the navel and on the left side the child's head was felt, and fœtal movements were also felt. She complained of extreme pain in the left side, which continued until she died next day. During life no os uteri could be felt. The omentum was closely adherent to the peritoneum everywhere. The uterus was about seven inches long and five wide, and directed to the right; the tubes were enormously hypertrophied, so that they formed with the uterus a kind of fork: the upper margin of the tubes was free, the others adhered to the neighbouring structures. On lifting up this fork, the embryo was seen in its membranes, lying transversely, not at all decomposed, and of full

growth. The placenta was very large, and was in no way attached either to the uterus or tubes. The uterus was removed: it showed no sign of scar; the cavity showed no trace of decidua. The transition of the cavity into the tube was free, and the canal so large that the little finger would pass some way into it. The condition of the fimbriæ could not be ascertained. The ovaries were quite normal. The author dilates upon the difficulty of diagnosis in these cases.—*Ibid.*, June, 1868.

4. B. B— had had three children normally. On the 5th June, 1866, she suffered acute abdominal pains. There was no fever. Two days before this she had sat out in the garden, and believed she had caught cold. The pain subsided, but on the 9th a sudden and severe pain set in: she shrieked and fainted, collapse followed. The appearances indicated internal hæmorrhage. She was estimated to be three months pregnant. Rupture of an extra-uterine fœtal sac was diagnosed. Dr. de la Faille's father believed the gestation was interstitial, basing his opinion upon the facts that he found the pain very acute in the uterus itself, when it was lightly touched, and that no circumscribed tumour could be felt in the flaccid abdomen. Much bloody mucus flowed from the vagina. The woman died in forty-two hours. On section a three-months' fœtus was found in the abdomen. On the right side of the uterus was a projecting place in the shape of a large tumour, and in it an opening with torn edges. The uterus was of the size found in the fourth month of pregnancy. The cervix was filled with a mucous plug; the cavity was lined with a very thick decidua. The placenta was seated in the forepart of the tumour, and behind it was the cavity which had contained the fœtus. There was a slight trace of a wall between the uterine cavity and the fœtal sac. Both tubes were closed.

Dr. de la Faille analyses briefly the other cases of interstitial gestation recorded by other authors. *Ibid.*, June, 1868.

III.—Labour.

1. *On Tardy Labour.* By Dr. Rigler.
2. *A red line round the Funis as a sign of Retarded Birth.* By Dr. J. B. Bond.
3. *On Turning in Narrow Pelvis.* By Dr. Strassman.
4. *On the Mechanism of Turning, and on Turning in Narrow Pelvis.* By Dr. Scharlau.

1. Dr. Rigler relates the following case:—A woman, æt. 28, menstruated on the 7th December, and expected labour in the middle of the following September. This, however, did not occur till the middle (14th) of October, that is, at the period of the eleventh menstruation. Menstruation had always recurred regularly every four weeks when not pregnant. She quickened in the middle of May. The labour was natural; the child very large; there was nearly complete absence of liquor amnii. The placenta weighing three pounds was expelled; it was covered with calcareous concretions. The child died during labour. Dr. Rigler, who seems to have

taken pains to eliminate all sources of fallacy, concludes that gestation lasted 308 days.—*Monats. f. Geb.*, May, 1868.

2. Dr. Bond gives cases to support the opinion that when labour has been retarded beyond the normal period, a bright red ring is found encircling the umbilicus just where the funis joins it. Dr. Farish contributes a case in confirmation.—*Med. Times and Gaz.*, August, 1868.

3. Dr. Strassmann shows by cases that turning in narrow pelvis may give successful results even when the greatest or bi-parietal diameter of the child's head is caught in the narrowed conjugate, and that it is therefore not essential in performing the operation to take care that the occiput shall fall into the wider half of the pelvis. He says it is difficult or impossible to secure that the occiput shall so fall. In one case he delivered with great difficulty a child which died soon after birth, by the forceps through a pelvis, the conjugate of which measured 2·75″, although the occiput came through the wider half of the pelvis; in the second labour he delivered the same woman of a live child which survived by turning, although the occiput came through the smaller half of the pelvis. Three other cases illustrate the same point. In all four cases the conjugate gave at the utmost 3″. Three children were born alive, one having died from prolapsus of the funis before turning. Strassmann insists much upon the importance of aiding the extraction of the head, by pressing upon the head through the abdomen externally.—*Monats. f. Geb.*, June, 1868.

4. Dr. Scharlau gives a summary of 64 cases in which he turned; 50 of the children were alive before the operation, of these 43 were delivered alive. Five mothers died; 2 of metro-phlebitis, 1 of peritonitis, 2 of ruptured uterus. Dr. Scharlau prefers the decubitus on the side, and seizing one foot rather than both, selecting that foot which belongs to the presenting side of the child. In placing the woman he lays her on that side to which, in transverse presentations, the breech of the child is directed; and in head presentations, on that side to which the child's abdomen is directed. In discussing which foot ought to be seized, he cites the dogma of Kristeller, who enjoins to seize the foot which corresponds to the presenting side when the child's back is directed forwards, and the opposite foot when the belly is directed forwards. In this Scharlau does not concur. He says that he turned in 22 cases in which the belly was directed forwards; in 20 of these he seized the lowermost foot, in 2 the upper. In the 20 cases turning took place easily; in the 2 it was difficult, in one of them impossible until he seized the lower foot. He further says that in other cases in which colleagues had seized the opposite foot and could not turn, he succeeded without difficulty by seizing the foot corresponding to the presenting side. Twelve of Scharlau's cases were complicated with narrowing of the pelvis. Referring to the statement of Martin that the depression in the fœtal skull caused by the projecting promontory is always considerably in front of the bi-parietal or larger transverse diameter, Scharlau exhibits four casts of heads in which the depres-

sion appeared near the bi-parietal diameter. He says two questions must be considered before deciding on the indications for turning in contracted pelvis:—1. In what way do children perish when the head presents in a narrow pelvis? 2. What advantage is offered by bringing the head last through a narrow pelvis? In answer to the first he says the child dies through a premature formation of a caput succedaneum, which in protracted labour becomes very large, and the child dies of brain-hyperæmia and œdema before the forceps can be applied. In other cases the forceps may just extract a child in a deeply soporous condition from which it cannot be restored. The advantage of bringing the head last consists in the turning to account the elasticity of the head-bones. He says it is not alone the smaller or bi-temporal diameter that can be reduced, but also the bi-parietal. In some cases the elasticity was so great that the bones yielded readily during extraction, and recovered their normal form very quickly afterwards. Of the 12 children, 11 were alive before turning; 1 died during the operation from laceration of the cervical vertebræ; 10 were born alive, but 2 of these died soon after birth. Dr. Scharlau concludes that the conjugate diameter may be narrowed to 7½ centim. (a little under 3 inches), and the operation give a good result, even with a full-grown child; that in a conjugate diameter of 7½ centim. not only the bi-temporal, but also the bi-parietal diameter of the head may be safely compressed to pass; that it is desirable but not essential for the transverse diameter of the pelvis to be wide enough to admit the occiput by the side of the promontory.—*Ibid.*, May, 1868.

IV.—Childbed.

1. *Researches on the Involution of the Uterus during the first eight days of Childbed.* By Dr. Schneider.
2. *A Case of Gonorrhœal Rheumatism occurring after Parturition.* By Mr. Harley.

1. Dr. Schneider, referring to the recorded observations upon the progressive diminution of the uterus after labour, instancing those of Hecker and Scanzoni, objects that the measurements were made on pathological specimens, that is, of uteri after death. He has repeated the inquiry by taking measurements during life. In 111 women during the first eight days following labour he measured twice daily the height of the fundus uteri above the symphysis, as well as the greatest breadth of the organ. To measure the height, he pressed the fingers perpendicularly to the abdomen over the fundus, so that the palmar surface of the hand marked the level. The distance of this level from the upper edge of the symphysis was then measured. The measurement so obtained was generally trustworthy for the first six or seven days; but after that there is generally some degree of anteflexion, and the os uteri is tilted backwards, so that the upper point of the line of measurement is taken from behind rather than from the fundus. The measurements of every case are given in detail. The height of the fundus diminishes con-

stantly, with the exception of a slight increase on the fourth day. The extent of diminution from the first to the fifth day amounts to 5·1 centim. It proceeds much more quickly during the first four than during the succeeding four days. The breadth of the uterus loses about 3 centim. in the eight days. Comparing thirty-seven primiparæ with seventy-four primiparæ, it was found that the involution of the uterus proceeded with much greater irregularity in the first—a result certainly not expected, the increase noticed on the fourth day being chiefly observed in the primiparæ. Schneider explains this more imperfect involution in primiparæ as being not probably the rule in normal puerpery, but as the result of causes acting more especially on primiparæ. Forceps deliveries are much more common. Severe labour more frequently called for chloroform. They more frequently did not suckle. They were in greater proportion subject to puerperal processes. All these conditions Schneider proved by his observations to have a marked influence, the last one especially, in retarding involution.—*Monats. f. Geb.*, May, 1868.

2. Mr. Harley's patient had in all probability contracted gonorrhœa at the end of gestation. The child had obstinate gonorrhœal ophthalmia. The mother had severe acute rheumatism two days after labour, attacking the left ankle, left wrist, hand, shoulder, and neck of the same side. She slowly recovered, some stiffness of the joints remaining.—*Dub. Quart. Journ. of Med. Science*, Aug., 1868.

V.—The New-born Infant.

1. *On Hot Baths as a cause of Trismus in New-born Infants.* By Dr. Keber.

1. It was observed that a large number of cases of tetanus had occurred in new-born infants in the town of Elbing. It was further found that almost all these cases had arisen in the practice of one midwife. Inquiry was instituted. It appeared that during 1864 and 1865 this midwife had attended 380 labours with a result of 99 cases of tetanus in the children! It further appeared that hardly any cases occurred in the practice of the eleven other midwives in the town, Dr. Busch, a physician in the town, who was called in to several of the cases, thus describes the symptoms:—The first was the child's refusing the breast. This never occurred before the third day from the birth, most frequently on the fifth or sixth day, and once on the tenth. If the finger is at this time inserted between the jaws, stiffness is felt. The child cries, and the features contract spasmodically. In twelve or fourteen hours these symptoms are more marked. After a warm camomile bath, the cramp subsides; the child sleeps quietly, but its breathing is scarcely visible. Presently the jaws cannot be separated. The slightest touch evokes spasms in the face and limbs. The fingers are held fast in the palm, the thighs are bent upon the abdomen, the muscles of the abdomen are hard. Warm baths bring no more relief. Later, it is not necessary to move the child to cause spasm. It is enough to touch the bed or to clap one's hands. The child becomes, in the

fit, blue in the face, cries plaintively, and foams at the mouth. Death usually ensued on the third or fourth day. Shortly before death the convulsions ceased, the respiration, which had always been regular, became shorter and at length ceased without signs of dyspnœa. The rigor mortis was very strong, but did not last long. Dr. Busch never observed inflammation of the navel or of its vessels.

The practice of the midwife in the conduct of the labour and the management of the child was minutely observed. The conclusion formed was that she washed the child in water that was unusually hot. She was cautioned to use a thermometer, and not to exceed a temperature of 28° C.

Whether this was followed by the disappearance of tetanus is not reported.—*Monats. f. Geb.*, June, 1868.

The following memoirs, for want of space, are referred to by title only, or very briefly:

On the Postural Treatment of Prolapse of the Funis. By Dr. C. H. Kidd. Dr. Kidd narrates a successful case in illustration.—*Dublin Quart. Journ.*, August, 1868.

Case of Enormous Fibro-cellular Tumour of the Vagina. By Dr. Beatty. The tumour was successfully removed by ligature.—*Dublin Quart. Journ.*, August, 1868.

On Recurrent Typhus in Pregnant Women. By Dr. Zuelzer. Dr. Zuelzer cites the observations of various authors upon the relations of fevers to pregnancy.—*Mon. f. Geburtsk*, June, 1868.

Two Cases of Anterior Encephalocele. By Drs. Hecker and Buhl. —*Mon. f. Geburtsk.*, June, 1868.

A Case of Quintuple Birth. By Dr. Galopin. (The children were all male, of about five and a half months' development. Five umbilical cords were inserted into two placentas.)—*Journ. de Bruxelles*, 1867.

Eighty-four Observations on the Bodies of New-born Infants in reference to Breslau's Respiration-test. By Dr. Liman.—*Vierteljahrschr. f. Gerichtl. Med.*, 1868.

Version in Contracted Pelvis. Dr. Ringland relates an interesting case illustrating the value of this operation.—*Dublin Quart. Journ.*, August, 1868.

A Pelvimeter for Internal and External Measurement and Simultaneous Estimation of the Inclination of the Pelvis. By Dr. Lazarewitsch.—*Monats. f. Geb.*, May, 1868.

A Fibrous Tumour of the Uterus eliminated by Softening in a Female who had Disease of the Heart. By F. Oppert, M.D.—*Med. Times and Gaz.*, August, 1868.

Labour Obstructed by Enlarged Kidneys. By Dr. Wolff. *Berlin Klin. Wchnschr.*, 1867.

A Case of Vesico-Vaginal Fistula following the Passage of a Vesical Calculus. By Dr. Mendel.—*Monats. f. Geb.*, June, 1868.

REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,
Member of the Royal College of Physicians, Physician to the Great Northern Hospital.

The Diagnosis of Diseases of the Nervous System by Means of the Ophthalmoscope.—In a memoir presented to the Academy of Sciences, Mr. E. Bouchut draws the following conclusions:—1. The ophthalmoscope enables us often to discover in the interior of the eye lesions of circulation, secretion, and nutrition which indicate organic disease of the cerebro-spinal system. 2. Optic neuritis, neuro-retinitis, choroïditis and papillary atrophy accompany the greater part of acute and chronic diseases of the brain and of the cord. 3. By the anatomical and physiological relations of the eye with the brain and cord we can explain the law of coincidence of optical neuritis with organic lesions of the nervous system. 4. When a chronic or acute inflammation has its seat in the brain, that inflammation, through the medium of the optic nerve, may be propagated to the eye. 5. Diseases of the anterior pillars of the cord may, through the medium of the sympathetic, produce in the eye the phenomena of papillary hyperæmia which will later give rise to atrophy of the optic nerve. 6. Optic neuritis and neuro-retinitis produced by acute or chronic diseases of the nervous system are generally observed in both eyes. 7. In lesions of the encephalon or its meninges optic neuritis is in general more marked in the eye corresponding to the hemisphere which is most gravely affected. 8. Alterations of the optic nerve and of the retina complicated by nervous troubles of sensation, intelligence or movement, always indicate an organic disease of the brain. 9. Alterations of the optic nerve and of the retina must not be isolated from other morbid symptoms, whilst their establishment adds to diagnosis an element of incontestable certitude. The diseases of the nervous system in which optic neuritis and neuro-retinitis may be observed are — Phlebitis of the sinuses, acute and chronic meningitis, chronic encephalitis, cerebral hæmorrhage, tumours of the brain, cerebral contusion and compression, chronic hydrocephalus, abscess of the brain, acute myelitis, locomotive ataxia, the contraction known as *essential*, and certain forms of epilepsy, paralysis or neurosis connected with organic lesion of the nervous substance.

Tumours of the Brain.—Dr. R. Bartholow relates three cases in which tumour of the brain was diagnosed. In one only, however, did circumstances permit the verification of the diagnosis after death. After a résumé of the observations of previous writers, he thus sums up the combinations of symptoms which, he thinks, may indicate the situation of the morbid growth. "In cases of tumour of the *cerebrum*, the following symptoms are observed:—Headache, not, however, confined to the seat of the morbid growth, and thus indicating its position, but limited to one side of the head, or deep-seated and diffused; epileptiform convulsions and mental derangement. Alterations of sensibility and of the special senses do not usually occur. Paralysis is not generally present in tumours of the posterior lobes, but is common in tumours of the middle and anterior lobes. Alterations in the special senses occur more frequently in tumour of the middle lobe, except the sense of smell, which is more usually affected by tumour in the anterior lobe. Tumours of the *corpus striatum* and *optic thalamus* are accompanied by the following symptoms:—Hemiplegia, partial or complete, on the side opposite the tumour, and convulsions; common sensation and the special senses are not frequently affected, and the mind is not often impaired. In tumour involving the *crura cerebri*, lesions of sensation and paralysis of the face and of the limbs on the opposite side, giddiness and paralysis of the motor oculi have been observed. In tumour of the *pituitary gland* the symptoms are frontal headache, amaurosis, first in one eye, then extending to the other. The mental powers are generally unimpaired, and there are no alterations of speech, sensation, or motion. In a case which has been brought to my notice, non-saccharine diuresis and epileptic convulsions were prominent symptoms. The symptoms are more complex in tumour of the *pons*. We find here crossed paralysis; face paralysed on the same and limbs on the opposite side; pain or anæsthesia in the paralysed parts, disorders of the special senses; dysphagia and mental derangement. Convulsions are so uncommon in tumours of the pons that Ladame lays down the following rule:—'If a tumour has attained sufficient size to allow of its presence being diagnosed, and if convulsions be present, the probability is that the seat of the tumour is not in the pons Varolii.' He also considers, and no doubt justly, that the simultaneous affection of several of the organs of sense is indicative of tumour of the pons. In tumours of the *medulla oblongata*, pains in the limbs, anæsthesia, convulsions, and sometimes partial or complete paraplegia, giddiness, vomiting, staggering gait, pains in lower extremities, amaurosis, dulness of intellect, hallucination, delirium, &c., have been observed. The following symptoms have been observed in tumour of the *cerebellum:* occipital headache, convulsive attacks, defect in the power of co-ordination, whence walking or standing are difficult or impossible, convergent strabismus, amaurosis; usually no disturbance of sensation except headache; no paralysis; no lesions of speech; no mental derangement. Mental derangement, however, does occur sometimes in cases of tumour of the cerebellum, as a result of the changes in the circulation of the brain produced by the new growth."—*Dr. R. Bartholow, American Journal Medical Sciences*, April, 1868.

Fatal general Emphysema supervening on Chronic Pleurisy and Hydrothorax.—Dr. J. R. Thomson relates the case of a bricklayer's labourer, æt. 34, who having apparently been in good health (except that he had complained of palpitation and dyspnœa on exertion), and engaged in his work, became suddenly sick, and, whilst vomiting felt something give way in his chest. Shortly afterwards he brought up a little blood; great difficulty of breathing followed. Emphysema showed itself in the cellular tissue of the neck, and rapidly spread, especially on the right side. On percussion there was hyper-resonance over the right apex, and as low as the level of the third rib. Below that there was relative dulness. On the left side percussion was natural. On auscultation over the right apex the respiratory murmur was harsh, but there were no moist sounds or friction murmur. On the left side the breathing was slightly harsh. A systolic bruit was audible at the ensiform cartilage. The emphysema rapidly increased, the respirations rose to sixty, the pulse could not be counted, and he sank rapidly.

Post mortem twenty-five hours after death.—One of the cusps of the mitral valve was converted into a small hard nodule. The right pleural cavity contained several quarts of a brown fluid. The lung was compressed against the vertebral column and bound down by adhesions which were most dense at the apex. The pleura over the lower part of the lung and diaphragm was covered with lymph. There were no traces of emphysema beneath the costal pleura. The lower and middle lobes of the right lung were solidified and friable. The apex contained cretaceous tubercle. The left lung and pleura were healthy except some interlobular emphysema over the anterior aspect of the former. The right bronchi contained a little blood. The posterior mediastrum contained no air, but air was present in large quantity in the anterior. The author supposes that rupture took place in the anterior part of the right lung.—*Dr. J. Roberts Thomson, Edin. Med. Journ.*, June, 1868.

Thoracic Aneurysm and Dementia.—Dr. W. Moore relates three cases of thoracic aneurism in which dementia was a prominent symptom. In the first there was aneurysm of the transverse portion of the aorta probably interfering with the carotid supply; in the second there was a large aneurysm springing from the upper part of the transverse portion of the aorta, with the left carotid impervious from its origin to its bifurcation and atrophy of the left hemisphere of the brain; in the third there was an enormous aneurysm springing from the front of the ascending portion of the arch, the carotids and large arteries of the brain were found after death well-nigh empty, and there was general atrophy of the brain. The author's object in adducing these cases is, he writes, to show that "mental diseases," so called, may be entailed by comparatively remote physical conditions, and hence arises the importance of taking the widest range in the consideration of these affections. If in a case of dementia we can detect a latent aneurysm or intra-thoracic tumour which from its situation, it is to be presumed, would cause obstruction of the cerebral supply and consequent atrophy of the brain, it is clear that the prognosis and treatment of the case would be materially affected.—*Dr. W. Moore, Dub. Quart. Journ.*, May, 1868.

Ascending and Descending Breathing: its Value as a Symptom and its Mechanism.—The peculiar irregularity of breathing referred to in this paper was first described by Dr. Cheyne. The early cases in which the symptom was observed were all cases of fatty degeneration of the heart, and it was supposed to be a symptom pathognomonic of that disease. It was thus described by Dr. Stokes. Among the indications of the malady he says there sometimes occurs "a form of respiratory distress peculiar to this affection, consisting of a period of apparently perfect apnœa, succeeded by feeble and short inspirations, which gradually increase in strength and depth until the respiratory act is carried to the highest pitch of which it seems capable, when the inspirations, pursuing a descending scale, regularly diminish until the commencement of another apnœal period." Other cases where this symptom was observed, however, occurred in which the heart was found to be free from fatty degeneration, but the left ventricle was found hypertrophied in consequence of valvular or arterial disease. This condition has hitherto generally been associated by writers with a weak state of the right ventricle, or attributed to some perverted action of the nervous centres. Dr. Little offers a new explanation :—"In health the right and left ventricles, though differing so much in the thickness of their walls, are equally competent for their duties ; the right ventricle is able to fill the pulmonary capillaries as thoroughly as the left one, with the aid of the other forces which contribute to the circulation, fills the systemic. But if an abnormal burden is imposed on the left, if rigid valves narrow its outlet, or permit the blood it discharges at each systole to fall back into its cavity, or if the arterial coats, their elasticity destroyed by disease, no longer help the heart ; if the aorta, instead of taking charge of each wave of blood as it leaves the ventricle, and propelling it onward by the steady recoil of its walls, is permanently dilated, and allows each portion of blood to remain in its ascending trunk, and so to impede the entrance of that which follows—under any of these conditions the left heart, however hypertrophied, may be quite unable to rid itself of the blood as rapidly as it is supplied to it by the right ventricle. Blood would, therefore, accumulate in the left auricle, in the pulmonary veins, and in the capillaries of the lungs. That blood having already absorbed as much oxygen as it required, would fail to produce that impression on the ultimate filaments of the pneumogastric which black blood does, and which impression is converted by the nervous centres into the motor impulse which produces breathing. Breathing would, therefore, cease ; and inasmuch as the respiratory act seems to assist in carrying the blood to the left side of the heart, it would no longer be so over-stimulated by fresh supplies, and its contractions would become less frequent and more regular. After a few systoles, however, it would succeed in discharging the red blood collected in its cavities to such an extent that they could receive some of that which lay in the pulmonary veins and lungs. Space being thus gained, the black blood which the pulmonary artery contained would reach the capillaries of the lung in amount proportionate to that of the arterial which had gone forward, and sufficient air would be drawn into the chest to aërate so much blood. That very act would carry forward a still larger charge of arterial blood to the left side, and make room for the reception, by the lungs, of a still further increase of venous blood, and, as a consequence, a still deeper inspiration would follow, and the deepest would occur when the largest quantity of venous and the smallest quantity of arterial lay in the lungs. The red blood, reaching the left heart, would excite it to those frequent and irregular contractions which accompany the respiratory distress, but, frequent and irregular, they would be also ineffectual, red blood would begin again to accumulate in the left heart, the pulmonary veins, and the lungs, till at last these capillaries would contain little else, and the exciting cause of inspiration, the venous blood, being no longer present, the act itself would again cease. Precisely similar conditions might, it seems to me, be supplied by fatty degeneration, for if it were—as I believe is often the case—somewhat more advanced in the left than in the right ventricle, or if, in addition to fatty degeneration, disease of the valves or atheromatous deposit in the aorta were present, the balance between the two sides of the heart would be destroyed. In Dr. Cheyne's original case, indeed, it is mentioned that the aorta was studded with steatomatous and earthy concretions."—*Dr. James Little, Dub. Quart. Journ. of Med. Science*, August, 1868.

Hypertrophy of the Lymphatic Glands (L'Adénie).—MM. August Ollivier and Louis Ranvier have published an interesting case of a woman, æt. 59, who died asphyxiated from pressure of the bronchial glands. In the commencement of her illness in October, 1866, she perceived two or three hypertrophied lymphatic glands on the sides of the neck. Her health, however, did not appear at first to suffer, but some months after she began to experience pain in the hypochondria and dyspnœa. The disease extended; the axillary, the inguinal and pelvic glands enlarged; afterwards debility and true cachexia set in, and she ultimately died asphyxiated after eight months' illness. The post-mortem revealed great enlargement of the thyroïd body, the glandular vesicles of which had undergone an hypertrophy allied to a colloïd transformation, large masses of glands in which the pneumogastric and recurrent nerves were imbedded, and by which these nerves must have been compressed; the bronchial glands were enormously enlarged; the heart was atrophied; all the vessels at the base of the heart were compressed and flattened, especially the brachio-cephalic trunk. In the abdomen was found an enormous tumour, weighing 640 grammes, and composed of a mass of lymphatic glands. The authors distinguish between the hypertrophy of the lymphatic glands (l'adénie) and the various forms of cancer, by the fact that in the former the tumours are composed entirely of lymphatic tissue—the adenoïd tissue of His. Clinically, they distinguish between adénie and adenitis by the absence of pain and all trace of inflammatory action; pathologically, by the fact that in adenitis the histological process tends to the formation of pus, cheesy matter, or fibrous tissue. In adénie the ganglia keep their essential structure; the follicles are enlarged, but they are always formed of characteristic tissue, lymphatic tissue, the adenoïd tissue of His. The reticulated stroma remains sound, or frequently the fibrillæ are better marked and more voluminous; at some of their points of junction collections of nuclei may be observed. This latter fact differentiates the condition from the normal, the presence of nuclei indicating a certain degree of irritation, and being common to adénie and inflammatory neoplasy.—*Drs. Auguste Ollivier* and *Louis Ranvier*, *Gaz. Méd. de Paris*, Juillet 4, 1848.

Hodgkin's Disease.—Dr. J. J. Black relates a case of progressive enlargement of the lymphatic glands, "Hodgkin's disease." J. S—, large, well-developed mulatto, entered the Philadelphia Hospital, Blockley, in the early part of August, 1865, suffering from enlargement of the lymphatic glands and dropsical symptoms. Eight weeks before had got wet, when the swellings began and increased rapidly. No syphilitic or hereditary taint to be discovered. There appeared to be an enlargement of every discernible lymphatic gland in the body—the chains along the lower jaw, sub-occipital, axillary, those in the groins, abdomen, and other parts. The glands on the left side were decidedly and correspondingly larger than those on the right. The left thigh and leg were very much enlarged and œdematous, the right limb slightly so. There was much fluid in the abdomen; left side of the chest was full of fluid, and a considerable quantity on the right side. Urine normal in quantity; contained no albumen; chlorides abundant; bowels regular; pulse 100; respirations 24, with all the evidence of effusion into the cavity of the chest. Under treatment by iodide of potassium and iron, cod-liver oil, &c., he improved, but afterwards relapsed and died eight weeks after admission. *Post-mortem*, twelve hours after death.—Lymphatics of left groin enormously enlarged above and below Poupart's ligament. Femoral vein obliterated by a semi-organised clot; right inguinal glands two thirds as large as the left. Chains of lymphatics on each side of lower jaw much enlarged; left internal jugular vein very much dilated; external jugular vein of left side very much dilated. The left side of thorax was full of a thin serous fluid; the lung compressed and carnified. Effusion also in right cavity to less amount; azygos and hemi-azygos veins nearly or quite obliterated by semi-organized clots. The lymphatics of the neck under the sternum running deep down, enormously enlarged, and pressing on all the neighbouring parts. Along the trachea was a mass of lymphatics enormously enlarged. Two of them contained tuberculous masses. Spleen enlarged, and full of waxy-looking bodies. A mass consisting of one half the mesenteric glands weighed two pounds five ounces. In the whole mass was only one very small and hard tuberculous spot. There was a large mass of glands pressing on the primitive internal and external iliac veins. In the primitive iliac veins were large pouch-like dilatations which would admit a hen's egg. The author calculates that all the lymphatic glands of the body together weighed thirteen pounds fourteen ounces. The blood appeared deficient in red corpuscles. The microscope showed the enlarged glands to consist of numerous nucleated cells and a large amount of fibrous matter, in fact differing little from the healthy gland. Sulphuric acid, and afterwards iodine, applied to the spleen and lymphatic glands, gave a characteristic blue. Owing to accident, the kidneys and liver were not carefully examined. The author states that he has met with two other cases of the disease, one that of a soldier, æt. 65, the other that of a mulatto boy, æt. 16. Both cases proved fatal. A somewhat similar case is recorded by Dr. W. Carson, 'Western Journal of Medicine,' Feb., 1860; but the patient was a woman, æt. 28. The duration of her illness was ten or twelve weeks.—*Dr. J. J. Black, American Journal of Medical Sciences, April,* 1868.

Acute Leucocythæmia in Diphtheritic Resorption.—M. E. Bouchut relates the case of a child, æt. 5, who was brought into the Hospital for Sick Children, suffering from croup. Tracheotomy was performed; albuminuria, with leucocythæmia, as proved by the large number of white globules seen in the blood by the microscope, was developed, and the child died. M. Bouchut believes that diphtheritic resorption took place, and that acute leucocythæmia accompanied it. He writes, "That which is most curious in this observation is the acute leucocythæmia accompanying diphtheritic resorption, a phenomenon of which *cliniciens* have hitherto made no mention. I have heretofore spoken of the acute leucocythæmia of puerperal fever as a pathological state, distinct from hepatic, splenic, or ganglionic leucocythæmia; and I have asserted that, without study of this variety of leukæmia, the history of this alteration of the blood must be incomplete. Without wishing to create a new kind, I have stated that in certain severe cases of puerperal metritis and in purulent resorption an acute leucocythæmia terminating rapidly in death is present. For some years I have shown in my clinique cases of grave diphtheritis equally accompanied by acute leucocythæmia, and I again call the attention of observers to this variety of alteration of the blood."—*M. E. Bouchut, Gaz. Méd. de Paris,* Juin 20, 1868.

Intermittent or Paroxysmal Hæmaturia.—Dr. E. Headlam Greenhow states that within the last ten years he has had under his care seven cases of this disease. In all the immediate exciting cause of the attack was some definite exposure to cold or wet. The paroxysms come on suddenly, almost immediately after the chill has been experienced, and pass off rapidly after the effects of the chill have been counteracted, and the patient has become warm. In every instance the paroxysms have begun with coldness of the extremities, followed by general chilliness, amounting in the severe attacks to rigors. In every case, also, the chilliness or shivering has been attended by a feeling of weight and pain in the loins, and by pain or a sense of weakness or stiffness in the lower limbs. The chilliness is usually followed by an imperfectly marked febrile hot stage. During the paroxysms urine, which looks as if it were mixed with blood, is passed, and in each case observed the paroxysms have run the same course. "From half an hour to two hours after the chilliness or rigors the patient has never failed to pass the first dark-coloured urine, which has always been highly albuminous, and has contained numerous crystals of oxalate of lime, with more or less of brownish- or yellowish-red amorphous granular matter, and a few hyaline casts, but only occasionally some stray blood-corpuscles. At each succeeding micturition after the chilliness the urine has invariably shown more or less diminution of colour, of albumen, of oxalate of lime, and of its other abnormal contents, resuming its natural character and appearance by the second or third micturition after slight attacks, and usually by the fourth or fifth after severer paroxysms." By the second day after an attack the patients have usually recovered, and continue well until some fresh exposure. The patients all had a pale, sallow, cachectic aspect. Two were jaundiced, and the others had at times an icteroid tint of skin. From the history of these cases Dr. Greenhow concludes that there is no cause of hæmorrhage in the kidneys themselves, but rather that the disintegrated blood transudes through the walls of the blood-vessels in the Malpighian bodies. The kidneys, he thinks, are rather the organs of elimination than the seat of the disease; and he suggests that possibly "the paroxysms may consist in the sudden disintegration of an unusually large quantity of blood-corpuscles, setting free so considerable an amount of hæmato-globulin that it cannot undergo the normal changes, but is eliminated through the kidneys in a comparatively unaltered state." In the disease as he has observed it there has been no malarious influence to produce it; and whilst it resembles ague in its paroxysmal form, it differs from it in not being periodical, and in requiring a fresh exposure to cold and damp to excite each paroxysm. He infers the existence of some form of dyscrasia upon which the external chill acts as the exciting cause of the paroxysm. He considers the presence of oxalate of lime crystals in the urine during the paroxysms significant. In four cases which he relates so-called rheumatic pains were also experienced during the attack.—*Dr. E. Headlam Greenhow, Edin. Med. Journ.*, May, 1868.

On Hippuric Acid, and on the relation existing between its Deposit and Vomiting in Organic Diseases of the Stomach.—Dr. J. J. da Silva Amado records the case of a man, æt. 37, who suffered from tumour in the epigastrium, and in whose urine there was a large proportion of hippuric acid, which deposited spontaneously. The details of the case seemed to support the belief that there was a correlation between the vomiting aud the abnormal urinary deposit. On those days on which there was no vomiting there was no deposit. When the vomiting was excessive the deposit of hippuric acid was abundant. The following are the conclusions the author appends:—1. In a normal state man excretes, by urine, about two grammes of hippuric acid in twenty-four hours. 2. This quantity augments (*a*) under a purely vegetable diet, (*b*) by the ingestion of benzoic acid or one of its derivatives, (*c*) by exaggerated exercise, (*d*) in pyrexias, (*e*) in diabetes. 3. The proportion of hippuric acid excreted diminishes or disappears more or less completely (*a*) under the influence of an exclusively animal diet, (*b*) by prolonged repose, (*c*) by abstinence, (*d*) by icterus. 4. Exaggerated production of hippuric acid following ingestion of benzoic acid, or one of its derivatives, seems to result from the reaction of these substances on the glucose of the liver. 5. When an exaggerated production of hippuric acid is not due to the ingestion of benzoic acid it seems to be due to the oxidation of tyrosine. 6. It is not proved that there is any malady characterised by an exaggerated and continued excretion of hippuric acid. 7. Hippuric acid may appear in the urine as a sediment. 8. In organic affections of the stomach a relation may exist between vomiting and an exaggerated excretion of hippuric acid.—*Dr. J. J. da Silva Amado, Gaz. Méd. de Paris,* Juillet 11 and 18.

Microscopic Examination of the Lymph of Variola Vaccina and Variola Ovina.—Professor Hallier and Docent A. Zuern have found in the lymph of sheep-pox very fine threads and small bodies, the latter moving actively. Examination of the pustules of sheep-pox in the process of development showed a large number of small nucleated cells. The elements of the epidermis contained nuclei and threads in large quantity. By cultivating with the lymph obtained from vaccine and ovine pocks, they have always been able to reproduce the same organisms. In the lymph of smallpox M. Hallier found numerous micrococcus cells in larger quantities than in vaccine or in ovine lymph. Magnified 800 times, these vegetable organisms appeared to be distinct rounded bodies, moving about, and here and there they were seen to be attached to lymph-corpuscles. Fine threads were also present in abundance. Dr. F. Keber, of Dantzic, states that both in vaccine and variolous lymph, and in the lymph of varicella, he has observed peculiar organic bodies. He has not satisfied himself of their occurrence in the blood of the affected, or in the air surrounding them. The bodies are granular cells $\frac{1}{150}$th to $\frac{1}{300}$th of a line in diameter, innumerable free nuclei from $\frac{1}{800}$th to $\frac{1}{3000}$th, and numerous minute molecules. The cells have a fine membrane, which swells with water, and they differ entirely from mucus- or pus-corpuscles.—*Virchow's Archiv,* November, 1867; February, 1868; and *Edin. Med. Journ.*, June, 1868.

Infusoria in the Air expired in Whooping-Cough.—M. Poulet has communicated to the French Academy of Sciences some observations made during an epidemic of whooping-cough. He collected the breath of the patients, and on examining the vapour microscopically he states that he found a large number of minute infusoria, which were in all the cases examined identical. The species of infusoria observed were the *Monas* or *Bacterium termo*, *Monas punctum*, and *Bacterium bacillus.*—*Gaz. Hebd. de Méd. et de Chir.*, August 16, 1867; *American Journal Med. Sci.*, April, 1868.

Parasitic Forms infecting the Epithelial Cells of the Urinary and Generative Organs.—Dr. J. H. Salisbury figures and describes a number of cryptogamic forms and also of animal organisms which he says he has found in the parent epithelial cells lining the genital and urinary organs. These parasites all produce more or less irritation, and as a consequence the physiological function of the cell is deranged. Hence arrive excessive cell activity and secretion, with resulting pathological products, and frequently cell death and disintegration. The discharges irritate, and the parts become inflamed and often thickened and indurated. Indurations of the womb thus produced may be mistaken for scirrhus. Fourteen kinds of cryptogamic parasites are described, belonging to the groups *Penicillium*, *Torulus*, *Botrytis*, *Zymotosis*, *Sarcina*, *Spharrotheca*, and *Crypta ;* and five kinds of animal parasites:—I. *Trichina cystica* (Salisbury), a small species of Trichina found in the human bladder. The author has met with it three times; the ova may be found in the urine; in one case ten to fifteen ova were found in a single drop. II. A species of *Vibrio* resembling that occurring in vinegar, to be found in freshly voided urine. III. *Trichomanas vaginæ.* IV. *Ciliaris bicaudalis* (Salisbury) consists of a single cell, slightly oval, having two hair-like caudal prolongations. The cell is covered with short, thickly set cilia. It is found in the secretion of the womb, vagina, and bladder. V. *Trichomanas irregularis* (Salisbury). The body smaller and more variable in size than *T. vaginæ.—J. H. Salisbury, M.D.*, *American Journal of Medical Sciences*, April, 1868.

The Pathology of Eczema Marginatum.—Dr. M'Call Anderson describes this disease as commencing on the inside of the thigh, where it is in contact with the scrotum, in the shape of a small round patch, which is red, elevated, itching, and which may, as the disease extends, become the seat of papules, vesicles, excoriations or crusts. The eruption heals in the centre, leaving the skin discoloured, owing to pigmentary deposit, while it extends at the edges in the shape of a circle, until it may reach the size of the palm or more. Similar patches often form in the neighbourhood, which coalesce with the circle first formed in such a way that a huge circle of eruption may result, which extends nearly to the umbilicus above, the knee below, and the sacrum behind. Inside this circle new circles often form, and similar patches may be detected in other parts of the body. The disease is most common to shoemakers and dragoons, owing to the moisture and friction entailed by their occupations. In India it is common and is known by the name of Burmese ringworm. Dr. M'Call Anderson differs from Hebra and E. Wilson as to the character and origin of this disease. He does not think it eczema, and he maintains with Köbner of Breslau that it is due to the same vegetable parasite that is found in tinea circinata, of which indeed he considers the disease a variety. He relates two cases, in which microscopic examination of the scales from the eruption discovered "spores, some of them isolated, some in chains and tubes of a fungous growth." Solution of bichloride (gr. ij to the ℥j) seems to cure the disease. The author notices, incidentally, that the use of bichloride lotion, followed by the use of solution of hyposulphite of soda, has the effect of dying the hair of a jet black colour.—*Dr. M'Call Anderson, Edinburgh Medical Journal*, May, 1868.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIÈNE.

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I.—Toxicology.

Chemical Constitution and the Physiological Action of Poisons.— We give precedence with sincere pleasure, in this report, to the researches of Drs. Crum-Brown and Fraser on the connection between chemical constitution and physiological action, and on the changes produced by direct chemical addition on the physiological action of certain poisons. The authors begin by stating that, as the chemical constitution of the majority of physiologically active substances is known, they investigate the subject by examining the physiological action of a substance before and after the performance upon it of a definite chemical operation introducing a known change into its constitution. There are two kinds of operation to choose between—replacement and addition. In their first inquiries the authors chose to select the effect of addition, their reason for such selection being that replacement does not produce nearly so great a change of physiological action as addition does. The following is a full abstract of their further argument and of their experiments:

"Comparing the action of carbonic oxide and carbonic acid, hydrocyanic acid and methylamine, arsenious and kakodylic acids, strychnia and brucia, and the salts of the ammonium bases derived from them, it may be seen that addition, in many cases at least, diminishes or removes physiological activity. This comparison leads to a suspicion that physiological activity is in some way connected with *chemical condensation*, by which term the authors mean susceptibility of addition, whether the addition takes place by the increase of the atomicity of an atom, or of a group of atoms. This suspicion receives some confirmation from the fact that such of the stable combinations of pentatomic arsenic and antimony as have been examined physiologically are stated to be inert, while all the soluble compounds of triatomic arsenic and antimony are active; similarly, the aromatic bodies are, as a rule, more active than the corresponding fatty bodies. The occurrence, however, of such poisons as alcohol, oxalic acid, and corrosive sublimate among saturated substances, and of comparatively inert condensed compounds, such as benzoic acid and salicine, shows that condensation is not the only condition of physiological activity.

"The statements of Stahlschmidt and Schroff, in reference to the action of the salts of methyl-strychnia, induced the authors to turn their attention, in the first place, to the effect of the addition of iodide of methyl to the natural alkaloids. As the iodides of the complex ammoniums thus produced are, in most cases, sparingly soluble in water, they have also examined the action of the corresponding sulphates.

"The poisonous alkaloids thus examined, and included in this paper, are strychnia, brucia, thebaia, codeia, morphia, and nicotia. The authors give details of the processes followed in obtaining the iodide and the sulphate of the methyl-derivatives of these bases, and they describe their physiological effects.

"Twelve grains of iodide of methyl-strychnium,[1] subcutaneously administered, produced no effect on a rabbit weighing three pounds. Fifteen grains were recovered from after symptoms, and twenty grains was a fatal dose. When exhibited by the stomach, twenty grains of this compound did not cause any symptom; while the same rabbit was rapidly killed by one tenth of a grain of strychnia, given in exactly the same way. Twenty grains of iodide of methyl-strychnium contain about fourteen grains of strychnia.

"The sulphate of methyl-strychnium, being a much more soluble salt than the iodide, was found to have a much smaller poisonous dose. One grain was fatal to a rabbit by subcutaneous exhibition. Eight tenths of a grain were recovered from, while five tenths did not cause any symptom. The rabbit that recovered after the administration of eight tenths of a grain of sulphate of methyl-strychnium, died shortly after one twentieth of a grain of strychnia was injected under the skin.

"Both the iodide and the sulphate of methyl-strychnium produced symptoms altogether different from those of strychnia. There were no convulsions, nor was there the slightest exaggeration of the reflex function; the symptoms were those of paralysis, and death was produced by the asphyxia that this occasioned. The authors further investigated this action by localised poisoning in frogs; and they have demonstrated that iodide and sulphate of methyl-strychnium paralyse the peripheral terminations (end-organs) of the motor nerves, and, therefore, possess exactly the same action as curare (wourali).

"Brucia and thebaia act in the same way as strychnia, and it was found that iodide and sulphate of methyl-brucium and methyl-thebaium have the same action as the analogous strychnia compounds. The fatal dose of iodide of methyl-brucium was found to be very much the same as that of the corresponding compound of strychnia; a larger dose was, however, necessary to produce death with sulphate of methyl-brucium than with sulphate of methyl-strychnium. Iodide of methyl-thebaium, being more soluble in warm water, has a smaller fatal dose than the iodides of methyl-strychnium and methyl-brucium. Six grains produced no effect when injected under the skin of a rabbit; eight grains caused symptoms, which were recovered from; and death occurred eleven minutes after the injection of ten grains. Eight grains of iodide of methyl-thebaium

[1] "The action of iodide of ethyl-strychnium was also examined, and found to be the same as that of iodide of methyl-strychnium."

contained about five grains and a half of thebaia, and, for a rabbit, the fatal dose of this alkaloid is one fifth of a grain.

"Among the opium alkaloids, codeia ranks next to thebaia in activity. It was found by the authors that six grains of iodide of methyl-codeium dissolved in warm water, and injected under the skin of a rabbit, caused no effect. Ten grains, however, was an almost fatal dose, and this contains about twelve times as much codeia as would kill a rabbit. It was also found that the fatal dose of sulphate of methyl-codeium is not very different from that of the iodide. Neither of these compounds possess the usual convulsant action of codeia; and as this alkaloid has but a feeble soporific action, it was difficult to determine how far this was modified by the direct chemical addition of methyl compounds. The authors also found that iodide and sulphate of methyl-codeium paralyse the motor nerve end-organs, an effect that is not produced by codeia itself.

"Iodide of methyl-morphium is a very insoluble substance. The largest dose that could, therefore, be administered subcutaneously to a rabbit was twenty grains, and this large quantity produced no effect. Eight grains of morphia were, some days afterwards, exhibited in the same way to this rabbit; the result was a decided soporific effect, followed by epileptiform convulsions and death.

"No effect was produced when iodide of methyl-morphium was administered to rabbits by the stomach, even in so large a dose as thirty grains.

"Recognising the possible fallacies connected with experiments with such a substance on rabbits, the authors determined to observe the effect on man. One of themselves accordingly took, on one occasion, half a grain of iodide of methyl-morphium as a powder, and on another one grain (containing about three fourths of a grain of morphia); but on neither occasion was there observed the slightest soporific or other effect.

"Four grains of sulphate of methyl-morphium produced decided narcotism on a rabbit, but no convulsive effect. Indeed, with this dose, and with various others that were given, paralysis appeared, and the authors have demonstrated that this symptom is due to an effect on the motor nerve end-organs.

"Iodide of methyl-nicotium was obtained in the form of crystals extremely soluble in cold water. When given to rabbits by subcutaneous injection, a dose of five grains was perfectly inert; one of fifteen grains produced serious symptoms, followed by recovery; and a dose of twenty grains was fatal. The symptoms were principally distinguished from those of nicotia by the absence of convulsions; but no paralytic action on motor nerve end-organs was caused.

"The authors have also investigated the action of iodide of methyl, and they obtained no evidence in support of the extremely improbable hypothesis, that some of the changes produced in the action of the substances they had examined might have been due to the addition of the physiological action of the methyl compounds. They conclude by discussing the possible causes of these modifications, by pointing out some of the practical applications of their results, and by promising to examine how far iodide of methyl may prove an antidote to the poisonous effects of these vegetable alkaloids, whose fatal dose it increases."—*Transactions of the Royal Society of Edinburgh*, January 6th, 1868.

On the Neutralization of some Poisons by bodies of the Methyl and Ethyl series.—We have ourselves been following a line of research physiological in character and running somewhat parallel, in its way, with the researches of Drs. Crum-Brown and Fraser. In 1860 we observed and recorded (in Brown-Séquard's 'Journal') the fact that in frogs the synthesis of cataract could be produced by injecting solutions of chloride of potassium and sodium, but that the iodides of the same metals would not give the synthesis. This led us to believe that the iodides, even in organisms of frogs, were changed in composition in the body. This same view was further confirmed by the known curative effects of iodide salts in some forms of disease in the human subject. The question, therefore, came to us whether the iodides within the organism would neutralize the action of some of the alkaloidal poisons. To test this the following research was made; it dated from the 24th of October last year. We made three solutions:—1. Consisted of two minims of iodide of ethyl and thirty of alcohol and water. 2. Consisted of the thirtieth of a grain of strychnia in thirty minims of alcohol and water. 3. Consisted of the thirtieth of a grain of strychnia with two minims of the iodide of ethyl and thirty of alcohol and water. A frog was injected with the solution No. 2. It became tetanic in one minute and a half. Another frog was injected with the solution No. 3, *i.e.* the solution of strychnia and iodide of ethyl. This frog also became tetanic in one minute and a half. The frog No. 1 was now injected with a solution containing five minims of the iodide of ethyl. Within ten minutes the spontaneous tetanus had ceased, and spasm, under the influence of irritation, was very much less. In twenty minutes there was entire relaxation, but with faint twitches when the skin was touched. The frog No. 2 was next injected with a solution containing one grain of iodide of ethyl. There was immediate relaxation of all the tetanic action, and irritation brought on no spasm. One hour after this the frog No. 1 still twitched when touched; while frog No. 2 remained relaxed and living, but paralysed. Both frogs died on the following day, retaining their symptoms to the end. It was clear in these two cases that the iodide of ethyl exerted an antidotal action to the poison, but as the animals died with different classes of symptoms a further research was made. A large frog was injected with ten minims of the iodide only. It seemed quite unaffected for some hours, but on the following day it died, presenting symptoms of general paralysis similar to the frog that had received the larger injection after the strychnia. Thus, the question had to be solved whether any precise formula of neutralization could be arrived at. In one experiment, it was clear, we had not used enough iodide to overcome the spasm, in another we had thrown in so much as to more than neutralize, in fact to kill by the iodide itself. Can, then, any known quantities for exact neutralization be introduced into a living body? Up to this time we have failed after the most careful study to find such quantity; we can certainly prolong life twenty-four and even twenty-eight hours after a terribly intense dose of strychnia, but ultimately there is death. Iodide of methyl acts in precisely a similar way as the iodide of ethyl, as do also the bromides of methyl or ethyl. Another series of experiments were about the same time made with nicotin. On October 26th (1867) two minims of nicotin were injected subcutaneously into a large rabbit. The animal died in twenty-five seconds. Another rabbit was injected with one minim of nicotin and ten of the iodide of methyl; it died in one minute and fifty-one seconds. A guinea-pig and a rabbit were treated with ten minims of the iodide only. It remained well for several hours, but both died the next day. Again, varied experiments were carried out to get at the neutralizing proportions of these two agents, and guinea-pigs were made to replace rabbits. But the point was never reached. Death from an intense dose of nicotin was frequently kept back for hours and the convulsive action was prevented, but the end was death. A point of inquiry still remained—to determine, namely, whether these neutralizing effects of the iodides were chemical or physiological in character. To approach a conclusion on this point we tested the action of the nitrites of the methyl, ethyl, and amyl series in a similar manner, and obtained results showing that the nitrites are also antidotal, but that the symptoms of strychnine poison returned when the antidote was allowed to escape from the body. We are bound, therefore, to affirm, in relation to the nitrites, that their neutralizing action is purely physiological, but we are not prepared to draw the inference from this that the iodides and bromides are to be placed, as antidotes, in the same position.—*Abstract of Report to the British Association for the Advancement of Science*, 1868.

Immediate Action of Hydrocyanic Acid.—Mr. Clay Hall reports to Professor Alfred Stillé the case of a gentleman who destroyed himself by taking about 100 drops of the diluted hydrocyanic acid prepared by Squibbs, of Brooklyn, of the strength of 2 per cent. of the pure anhydrous acid. The facts are of interest, from the circumstance that Mr. Hall was with the deceased within five minutes after the poison was taken, found him alive, and remained with him unto his death, which occurred twenty minutes after taking the acid. From the evidence at the inquest it was elicited that the deceased, a Mr. Pomeroy, went into a barn, poured into a goblet the poison (about 100 drops), recorked the bottle holding the poison, placed it on a shelf, and then going to the steps of the barn, placed his hat carefully on them; he now went back to the goblet, drank the fatal draught, placed the glass upon a ledge eight feet from the floor, laid himself upon the floor, placed his hands across his breast, and passed into unconsciousness. Mr. Clay Hall was the first person who got to Mr. Pomeroy, who had not then been in the barn five minutes. He (Pomeroy), says Mr. Hall, was lying extended upon the floor, unconscious. His muscles were relaxed and flaccid, with the exception of the muscles of the jaw, the jaw being firmly closed; his hands were folded across his chest, as in repose; the eyes were fixed, but lifelike, the pupils were in their normal condition; respiration was slow, but not laboured, although deep drawn; his pulse was about 50, becoming slower and less strong to the moment of his death. During the most forcible expirations not the slightest odour of the acid could be perceived in the breath. His respiration became slower and slower until intervals of one minute intervened, and in twelve minutes he breathed his last. At the moment of dissolution the pupil dilated, but there was not the movement of a muscle to indicate death; he simply ceased to breathe. The veins of his neck were strongly congested. Shortly after death the lifelike appearance of the corpse was surprising, and thirty-six hours afterwards the eye retained its brightness.—*Amer. Journ. of the Medical Sciences*, January, 1868.

Case of Poisoning with Rhus Toxicodendron..—Dr. W. R. Sanders records the following very rare instance of poisoning by the Rhus toxicodendron (poison ivy). Peter Doig, æt. 26, a gardener, was admitted into the Royal Infirmary, under Dr. Sanders's care, on July 25th, 1867. He was a strongly built man, of middle stature, and had evidently been in the enjoyment of robust health. He stated that after the diseases of childhood he had never suffered from any illness, with the exception of an attack of gastric fever, at the age of twenty, from which he perfectly recovered. The symptoms, on account of which he now sought medical treatment, consisted chiefly of an erysipelatous eruption on certain parts of the skin, of which he gave the following history:—About three weeks before admission, viz. on July 6th, he had, in company with other gardeners in Mr. Lawson's nursery, been employed in gathering the shoots of the Rhus toxicodendron for the purpose of supplying a homœopathic druggist in town. Doig continued at this occupation for about two and a half hours on that afternoon. Instead of pulling up the whole plant, as other workmen did, Doig plucked off the young shoots, and in so doing his hand came into frequent contact with the juice of the plant. This juice, which was white and milky when fresh, became dark on exposure, and concreted on the palms and wrists, forming dark scales, which adhered so closely that they were removed with difficulty by rubbing off the superficial layer of cuticle. At this time no inconvenience whatever was felt; but, four days afterwards, Doig noticed two blisters, each about the size of a threepenny piece, on the flexor surface of the right wrist. On the top of each blister there remained adherent a portion of the black concreted juice, and there was some redness around the vesications, but no pain. Four or five days subsequently the other wrist became similarly affected, and about the same time the redness began to spread slowly up both forearms. Nothing farther was noticed until the 23rd, when he was obliged to give up work in consequence of the swelling and stiffness of the forearms, accompanied by severe numbing and stinging pain, to relieve which he had rolled his arms in cold moist cloths. This was seventeen days after exposure to the poisonous juice. On the 24th, being now greatly alarmed at the progress of the eruption, he applied to the infirmary, where he was first admitted into the surgical wards, under Dr. Gillespie, who next transferred him to Dr. Sanders. On examining the patient, Dr. Sanders found the skin on the flexor surface of both forearms swollen, and of a florid red colour, like that of erysipelas, and the red surface was covered with small transparent vesicles, each about the size of a pin's head, closely set together. The vesicles resembled those of eczema, or the minute inflammatory vesications produced by the application of turpentine. Both forearms were considerably swollen, and felt stiff to the patient. Some of the black spots formed by the dried acrid juice were still seen on the palms of the hands and on the adjoining parts of the wrists. The skin of the (upper) arms was natural. The face, though less affected than the forearms and not vesicated, was swollen and erythematous, the eyelids being puffy and partially closed. The trunk of the body was untouched, but the skin of the penis and the scrotum was red, œdematous, and painful, and there were scattered spots of inflamed and slightly elevated skin on the inner surface of both thighs. The inflamed portion of skin was the seat of pain, sometimes of a numbing character, sometimes stinging like the irritation of nettles. The pain was worse at night, owing to the heat, but it was not severe anywhere except in the forearms. It was remarkable that these symptoms were unaccompanied by constitutional disturbances; the pulse was quiet and the appetite good. The tongue was somewhat furred and dry, but this was owing to constipation, which was easily relieved. During the course of the following day (26th) the red patches extended up the arms, and also down the sides as far as the knees, while some scattered spots appeared over the pubes. Next day (27th), the swelling and redness of both face and arms were diminishing, and the vesicles on the forearms were drying up into scabs, but the erythematous eruption on the thighs continued to spread downwards to the legs and upwards on the trunk of the body. On the 29th, at noon, the abdomen was found covered with irregularly shaped patches of inflamed skin, which had extended from the pubes upwards as far as the hypochondria. At the margin of the large patches there were numerous detached, small, reddish spots, like the eruption at the outset of measles, the larger patches resembling the continuous rash of scarlet fever. On the 31st the redness had extended to the back, while anteriorly the skin, from the pubes to the clavicles, was marked with inflamed patches and spots, the region of the sternum being alone unaffected. The legs were almost entirely covered with eruption. There was no vesication on any of these parts. But while the eruption was thus spreading on the lower half of the body, the upper half was recovering. Thus, the forearms were now nearly well, the redness and swelling were gone, only a few small vesicles remaining on the backs of the hands and between the fingers. The face had nearly the natural appearance, very slight desquamation being observable. At evening visit Doig complained of stiffness and rheumatic pains on the knees and elbows, but no swelling of the joints was present. These pains were ascribed by the patient to sitting up in a cold side room, and they were quite gone next day. Doig still presented no constitutional disturbance; with the exception of a tendency to constipation, all the functions were normal. On the 1st of August the eruption ceased to spread on the trunk, and soon began to fade. It had now completely disappeared on the face, arms, scrotum, and other parts first affected. There was very slight desquamation, the skin returning to its natural state. On the 2nd of August Doig left to go to the country, having been eight days in the infirmary. Dr. Sanders saw him again about three months afterwards, when he said that after going to the country from the infirmary the eruption, which had almost entirely subsided, reappeared on the trunk of the body, but it lasted only for a few days, and then completely and finally disappeared. The case required only ordinary care, never presenting at any time an appearance of danger. The patient, indeed, was at first in a state of extreme alarm; the fact of being poisoned and the persistent progress of the symptoms filled his mind with undefined terror. But on being confidently assured that his symptoms were free from danger he had the good sense to believe the statement, and the relief of his fears was perhaps the most important part of the treatment. Death has been caused by the poison in experiments on animals, but Dr. Sanders is not aware of any fatal cases in man. One circumstance deserving of notice in Doig's case is, that he alone was affected out of a number of persons employed in gathering the poison ivy. This might be ascribed to a special susceptibility of skin above referred to, but his own account suggested a more obvious cause in the fact that, while others gathered the entire plant, he alone plucked the young shoots, and in breaking off the young twigs set free the juice, which came in abundant contact with his arms and forearms. In Doig the irritant was long in taking effect, for on the wrists, which were most acted upon by the juice, the inflammatory action did not occur until after a considerable interval; and the face, which had received a small dose of the irritant principle, was only subsequently and less severely affected. The only circumstance requiring special explanation is the spreading of the eruption over the trunk of the body at a later period of the case, after all sources of irritation were removed. This, it appears, can only be explained on the supposition that some of the poison had at length been absorbed into the blood—or at least into the tissues of the skin—and this agrees with the observations of toxicologists, which have shown that the cutaneous symptoms may arise from the internal administration of the poison, or what is the same, from its absorption in the blood, as well as from its external local application.—*Edinburgh Medical Journal*, February, 1868.

Hygiene.

Defects of Building Materials in relation to Health—Saline Damp.—The author of this communication writes a separate essay without giving his name. He is "a metropolitan ratepayer,"—that is all he allows us to know of him. In a somewhat crude manner he presents us with a new subject belonging to public health, for which reason of novelty we notice him. He urges that the question of building upon an extensive scale for the accommodation of pauper lunatics and pauper patients afflicted with smallpox or fever is assuming very prominent proportions, and is becoming important in the social arrangements of the metropolis under the provisions of the Poor Law Act of 1867. In the construction of two of these extensive buildings (asylums) near to London, viz. at Leavesdon and Caterham, the author estimates that if bricks be used some twenty or thirty millions will be required for each, and that in bricks and mortar the third of the whole expenditure will be incurred. The legislature has assumed, says the writer, the responsibility of regulating the construction of buildings by the Metropolitan Building Act of 1855, and two amendment acts, modifying and extending the provisions of the original statute, have since received the sanction of Parliament. These acts contain provisions relating to the lines of walls, recesses, and openings, parapets and breast-summers, chimneys and flues, fireplaces and conveyance pipes for heated air, steam and other products of combustion, *but they make no reference whatever to the character and quality of the materials commonly used in the erection of such buildings.* There are no regulations enforceable to ensure the structure being perfectly dry in all seasons of the year. This matter of dryness of the building, as a necessity for the health of the occupants, is the point dwelt on by the author, who confines himself to an exposure of the error of using building materials charged with saline matters. Many of our public buildings give evidence of this error—ocular demonstration of inherent defects. The Hanwell viaduct of the Great Western Railway is adduced in proof of this fact. This, with its mildewed walls, indicates a process of absorption of water and giving up of water, according to the condition of the air, which means that in every part there is damp. If saline matter exist in bricks or in mortar, they act like sponge; they absorb moisture from the air, and they give it up again under the influence of heat. The new boundary walls of the Coldbath Fields' Prison, near to Mount Pleasant, is an instance in further illustration; and, adds the writer, "we should not be surprised if the costly erection of St. Thomas's Hospital, now in progress, should, in after years, awaken posterity to the fact that materials containing saline matter were used in the building." As to the source of this evil, the author traces it to the employment of sea sand, from which the saline matters have not been removed. "While the building acts, as before said, abound in rules applying to form of construction, they absolutely ignore the quality of building materials; hence, we have private dwellings, public schools, infirmaries, hospitals, workhouses, and prisons, built of bricks containing saline matter, kept together by means of mortar which has been mixed up with sea sand, or with Thames sand containing salt. Buildings so constructed never fail to show the tests of dampness. The walls mildewed on the outside frequently present a similar appearance inside through three or four coats of paint. The observant eye may notice on a flatted wall blisters of paint, which, if cut, will let moisture exude and trickle down. The board-room of the mansion in Spring Gardens, where the Metropolitan Board of Works holds its weekly meetings, and which is at present temporarily occupied by the Asylum District Board, affords striking proof of the correctness of these remarks." In many cases, the author believes, builders err in selecting materials from sheer ignorance, and that architects, highly educated professional men, from want of practical attention, have failed to detect the hidden causes which are silently operating in producing the most disastrous results. Sand is often dragged from the bed of the river too near to the sea or brought as ballast in ships from foreign ports, and being saturated with salt is unloaded into barges and sold to builders, who have every inducement to use seaborne sand, inasmuch as it makes beautiful white mortar, and may be mixed with lime in larger quantities than sand obtained inland. The soft Thames sand used in making bricks gives them a fine pale yellow surface; this kind of brick is pleasing to the eye, but if it be not "*weathered*" it renders the structure built of it open to all the defects which have already been stated. Owing to the fineness of the work it yields, architects often specify that all the outside work is to be done with Thames sand. The author points out further that the woodwork of buildings as well as the brickwork is constantly charged with saline matter when brought across the sea. Wood absorbs moisture rapidly. It first absorbs water in the course of its transit from its place of growth to its place of shipment; then it is exposed upon its sea-voyage to a further absorption of saline particles, and the author has often found timber unloaded from ships in the Commercial Docks and elsewhere nearly as wet before it was put into the water of the timber-basin as it would be after lying there in rafts. The timber thus brought over the sea is "well-pickled," and lying in fresh water does not abstract (we suppose in the time allowed) the whole of the saline matter it may have taken up. The author, we think, up to this point proves his case; he has shown that building materials charged with saline matter are in daily use, and he has further shown that buildings so constructed are of necessity damp buildings. But when he proceeds, as he afterwards does, to dwell on the "consequences," he proceeds beyond his knowledge. He opines that the "chronic rheumatism, ague, and intermittent fever" of our workhouses and many of our large hospitals originate from pervading damp; that the like obtains in the crowded dwellings of the poor; that the rich, whose residences wear a degree of splendour on their exterior, have "the same canker-worm of saline damp;" and that diseases are thus generated and matured when neither "medical men, family nurses, anxious parents, nor disconsolate children," ever dreamed of the agent that was creating the evil. In order to prevent the errors that he has pointed out, the author says it would be desirable, in so far as it can be done, to take all materials from the estate upon which the proposed buildings are to be erected. The bricks ought to be either slop made, or the mould sanded with the best quality of soft inland sand. The mortar should be made of lime mixed with inland sand, and to save sand a portion of burnt earth ground in a mortar-mill may be mixed with an equal quantity of sand. The mortar in all cases should be ground. If suitable stone can be obtained within a practicable distance, at an expense less than, or not exceeding that, of brick, it is preferable. We have given a faithful *résumé* of this short essay, and while we do not think the author has *proved* anything in respect to the production of disease, we admit he has given to our profession a good practical hint. We are becoming, especially through the labours of Dr. George Buchanan, keenly alive as to the influence of damp in the production of phthisis pulmonalis, and we are glad to know any important fact in respect to the cause of permanent damp in our dwellings. Saline damp comes to us, therefore, as an idea charged with interest and deserving our closest observation. It is our business to ascertain if a statement so positively made be really true.

Summary.

The Antagonistic Action of Opium and Belladonna. By J. T. Newman, M.D. ('Chicago Medical Journal,' November, 1867.)

Dr. Newman relates a case in which a woman who was an opium-eater, and who could take sixteen grains of morphia at once without injury, took a dose sufficiently excessive to produce coma, stertor, small pulse, contracted pupils, fixture of the jaws, and coldness of the extremities. She recovered after subcutaneous injection of nearly two grains of sulphate of atropia.

On the Melting and Subliming Temperatures of the principal Poisons, Organic and Inorganic. By William A. Guy, M.B., F.R.S. (Reprint from the 'Pharmaceutical Journal,' February, 1868.)

A brief but very useful extract of the varied and most interesting communications of Dr. Guy on sublimation of poisons. Deserves publication as a distinct essay.

Case of Mental Derangement limited to a single Moral Sentiment, occurring periodically, that sentiment being in a perfectly normal condition during the intervals. By Professor Samuel Jackson, M.D. ('American Journal of the Medical Sciences,' April, 1868.)

The case recorded by Professor Jackson establishes, he thinks, two facts :—the first the independence of the moral sentiments in a manner similar to that of the mental faculties, as demonstrated by the fact of a single moral sentiment being diseased for nearly four years; the second that in monomania there may be intermissions.

Researches on the Nature and Action of Indian and African Arrow-Poisons. By Hermann Beigel, M.D. (From the 'Journal of Anatomy and Physiology,' vol. ii.)

A very able paper, full of laborious experimental facts, and eminently suggestive.

On Adulteration of Sub-nitrate of Bismuth. By Prof. R. Redwood. ('Pharmaceutical Journal,' August, 1868.)

Dr. Redwood in this communication shows that sub-nitrate of bismuth is adulterated with phosphate of lime. The fact had previously been pointed out by M. Roussin, who found in one case as much as twenty-eight per cent. in a sample which presented the usual appearance, and answered to the ordinary tests of sub-nitrate of bismuth. Roussin's process is as follows for the detection :—Dissolve equal quantities of the sub-nitrate and of tartaric acid slightly diluted with water, and add to this a strong solution of carbonate of potash until all effervescence has ceased and the liquid is rendered strongly alkaline. If the sub-nitrate of bismuth be pure, the liquid will be clear and will remain so even after it has been boiled; but if the sample of sub-nitrate submitted to the test should contain phosphate of lime, even to the extent of but one or two per cent., this will form a white precipitate, which will not dissolve with long-continued boiling. To these remarks Dr. Redwood adds that the phosphate of lime, even when present in large quantity, is not precipitated in the first instance after the addition of the carbonate of potash, but its precipitation is immediately effected by boiling the solution. From one sample he obtained eleven per cent. and from another forty per cent. of this adulterant. He thinks both specimens were of foreign manufacture.

Indian Sanitation. (In the 'Public Health,' Nos. 5 and 6.)

An able article, doing credit to the new and useful publication in which it appears.

On Sewerage, with Remarks on the Best Means of House Drainage. By C. B. Nankivell, M.D.

In this address, published at the request of the Torquay Medical Society, Dr. Nankivell maintains the health side of the sewage question in preference to the mere question of utilisation.

Change of Molecular Structure during Cadaveric Decomposition. By C. Robin. ('La France Médicale,' October, 1867.)

M. Robin has conducted a series of researches on the change which takes place in the anatomic elements of the tissues after death. He has traced the changes from the first or progressive step to complete putrefaction, and states that when putrefaction is accomplished the tissues are reduced to a condition of molecular granulation, the granules being very minute, numerous, grey in colour, and having very active movement. The phenomenon of granular change does not appear until, by the smell, the substance is distinctly proved to have undergone putrefaction. Any partially solidified elementary homogeneous substances, fibres, or cells, if they do not present granulations at first, are ultimately resolved into the granular form, the change being complete throughout the whole of the structure.

Labour during Sleep. By Wendell Case, M.D. ('The American Journal of the Medical Sciences,' January, 1868.)

The question whether labour can occur during ordinary sleep without disturbing the mother has been answered affirmatively by Dr. Case. He relates that in the evening of December 16th, 1860, he was summoned to visit Mrs. B—, a lady from France, residing in the town of Hopedale, six miles from his residence. She was twenty-one years old, and was near the period of her confinement, but attributed her symptoms to over-fatigue on the previous day. Dr. Case found there had been severe pains in the lumbar region and slight nausea. The os uteri was dilated to three fourths the size of a half-dollar. At ten o'clock Dr. Case, having waited an hour for return of pains, suggested that they should all retire to rest, and that he should be called, if required. About 4 a.m. the husband of the lady, in great fright, summoned him, exclaiming, "Monsieur le Médecin, il y a quelquechose entre les jambes de ma femme;" and to his (Dr. Case's) great surprise, he found the head of the child had been wholly expelled during the profound sleep of the mother. In a moment the lady was delivered, and in less than twenty minutes the secundines were expelled. The patient said she had dreamed something was the matter with her, and awoke with a fright, probably the instant the head was expelled. She has since been confined, and with the usual amount of labour-pains.

REPORT ON SURGERY.

By John Chatto, M.R.C.S.E.

Treatment of Secondary Syphilis by the Hypodermic Injection of Corrosive Sublimate.—Dr. Lewin, after disposing of the non-mercurial treatment of syphilis as little better than a crotchet, proceeds to show that, of all means of employing mercury, that illustrated in this paper is the best. He has employed it for more than two years at the Berlin Charité; and both he, his colleagues, and his patients, are satisfied with the results. The cases here referred to amount to 500, viz. 356 women and 144 men, the symptoms in all being thoroughly well characterised. The details are minutely tabulated, so that the various facts, such as prior treatment and its nature, the absence of this, the occurrence of relapse, &c., are duly set forth and easily referred to. Dr. Lewin employs Luer's syringe, or in some cases a larger one, and, owing to the corrosive nature of the fluid, insists upon its being constantly washed out, and its point frequently sharpened. In private practice he keeps a marked canula for each patient. He prefers the back, lateral thoracic region, or buttock, as the place of puncture, because less irritative inflammation ensues; but in iritis the temporal region is preferable. In the great bulk of the cases a solution of 4 grains to the ounce was employed, which, supposing the syringe to hold 15 grains, would give $\frac{1}{8}$ grain each time. In the very sensitive, from $\frac{1}{8}$ to $\frac{1}{10}$ grain of morphia may be added with glycerine. The injections are best performed in the forenoon and afternoon, and, if a very rapid cure is sought, again in the evening. The patient need not be confined to his bed, or in warm weather even to the house, care being taken that he is not exposed to chills. Even when this precaution has been neglected ill results have seldom followed. The diet need not be much restricted, beyond being somewhat diminished in quantity; but alcoholic drinks should only be taken exceptionally. Great care should be taken in keeping the mouth clean, but moderate smoking may be allowed. The pain caused by the injection is sometimes considerable, especially if it be not performed adroitly, or the patient is very sensitive. In general, he soon becomes accustomed to it. The subsequent irritation, which usually soon subsides, sometimes goes on to inflammation, induration, or suppuration, especially if the injection be too strong or too freely used, some patients being far more susceptible than others. Dr. Lewin, in cases of slight venous hæmorrhage that have occurred among his many hundred injections, has never met with an instance of ill consequences supposed to be due to the introduction of the injected substances into the circulation. He found in his 144 male cases that the average quantity of $2\frac{5}{6}$ grains of sublimate were required to effect a cure, while in those of the cases which had previously undergone no other treatment 3 grs. were required. In the 356 women $2\frac{1}{4}$ grains sufficed, *i. e.* $\frac{3}{8}$ less than in the men.

Summing up his opinions, Dr. Lewin states that preference should be given to this mode, because (1) of the rapidity with which the symptoms disappear, this holding an exact proportion to the quantity of sublimate daily injected. Thus, two or three injections per diem of $\frac{1}{2}$ to $\frac{3}{4}$ grain cured numerous cases of iritis in from five to seven days. In these cases of very rapid cure the patient must keep indoors, and avoid all bodily or mental excitement. (2) The results, also, are certain and precise. In and out the hospital, the author during two years and a half has treated 900 cases, exhibiting every variety of symptom and group of symptoms; and in almost all of these, even in desperate cases, many of which had been fruitlessly treated by other modes, he has met with the most gratifying results. Syphilitic disease of the bones has offered the greatest resistance, for, although the nocturnal pains have been relieved, and the subperiosteal deposits removed, yet the bones themselves did not recover their normal volume. (3) The relapses are small in number and slight in character. The statistical comparison of the results obtained by this and by other means shows that while the relapses after the latter amounted to 81 per cent., those following the injection method were only 31 per cent. (4) Finally, the great convenience of the method, both for the patient and the surgeon.—*Annalen des Charité-Krankenhauses*, Band xiv.

Injuries of the Elbow.—Professor Bigelow observes that there is no class of injuries which so frequently gives rise to discontent and litigation, and these cases often turn out much less satisfactorily than they would have done had certain simple rules of treatment been adhered to. "The rule I would enjoin upon you is the following:—Ascertain first if the olecranon is broken, as this injury requires a special treatment. In all the other injuries, whether you are able to make an exact diagnosis, or are wholly unable to do so on account of the swelling, *treat them as though the forearm had been dislocated backwards*, and secure the arm at right angles to an inside angular splint. The propriety of this measure will not be doubted with regard to the more common dislocations of the arm. The very rare instances of the radius dislocated forwards, or the all but impossible dislocation of the ulna forwards alone, would doubtless declare themselves, and the bones would be replaced during the manipulation. Practically speaking, they are so rare that they need not be taken into account. But among the fractures, the transverse fracture of the lower end of the humerus, the T fracture into the joint, the fracture of the inner or outer condyle separately, the comparatively rare fracture of the coronoid process of the ulna, or of the radius or ulna near the joint, are all properly treated by the expedient above described; while the common injuries of the lower end of the humerus, including the fracture of the internal condyle into the joint, in most cases peremptorily demand it. In these cases it is sometimes difficult or impossible to make an accurate diagnosis; but the above treatment covers the whole of them, and does harm to none, while it is the omission of it, as I believe, that directly leads to deformity in a large proportion of them. . . . The patient being now etherized, the character of the injury is determined as far as may be without unnecessary harm from manipulation of the parts, and the elbow being placed at right angles, the wrist is drawn forwards, while the humerus is pushed backwards at the elbow. In this position it is forcibly maintained while the fragments are adjusted as far as may be, and an internal angular splint, padded by a folded towel, is applied by an assistant. To this the arm and forearm are now secured, the friction of the bandage of the forearm being relied on to prevent any backward displacement of the elbow. An outside straight splint may also be secured to the forearm if thought necessary. A few inches above and below the elbow may be left uncovered for cooling applications, and especially leeches if the swelling or superficial congestion make them advisable." Professor Bigelow also insists upon another point, viz. the injurious effect of the *passive motion* usually directed to be employed when the joint has become somewhat stiff from the above plan of treatment having been neglected or from the severe character of the injury. Such motion he believes does much harm by inducing pain and inflammatory action, the removal of which causes great delay. When the olecranon has been injured or the fragments of the humerus not properly replaced such motion may do especial harm. When the case has been properly treated and "the splint has been removed at the proper interval for repair (from four to six weeks), the arm can be flexed or extended through even a very small arc, not with that deceptive springiness and elasticity of the ligaments, but in a way to satisfy the surgeon that the cartilages are sliding one upon the other, however little. My rule is to leave the rest to nature with entire confidence in the result—allowing the patient to take off his splint daily, and as he pleases to flex and extend his arm as the pain and tenderness may allow him, encouraging him in his attempts to reach his forehead with his hand. I have also often advised a patient to bore holes in a soft board with a small gimlet, to increase the power of rotation. But if the cartilages do not slide through even in a small arc, and motion is restricted, elastic, and springy, owing to bony deformity, so much the worse for the patient, and so much the longer and less perfect the recovery. I do not believe you can accelerate it by passive motion, as the term is usually understood. . . . Exactly how far these remarks on passive motion apply to the knee and other joints and injuries, I will not attempt here to define, but can only say that I have seen more harm than good arise from forcible flexion of the knee after rheumatism and after fracture of the shaft of the femur. In simple fractures of the elbow, except of the olecranon, these remarks may be summed up as follows:—Always etherize the patient; go through the motions of reducing a backward dislocation of the forearm, and apply an internal angular splint. When there is bony deformity or projecting callus, passive motion does harm; and when the bones are in place and under supervision it is unnecessary."—*Boston Medical and Surgical Journal*, May 7.

Suture of Divided Nerves.—M. Blum, after a careful analysis of the numerous facts which have been published in France upon this interesting subject, arrives at these conclusions:—1. Experiments made upon animals do not suffice for the solution of this problem. Although upon them we can, up to a certain point, demonstrate sensibility to pain, it is impossible to pursue the different variations which the sensibility to touch may undergo. It is upon clinical observation we can alone rely. 2. The statements made by Eulenberg and Landois, that the suture has led to disastrous consequences, are ill-founded; but it must be admitted that in some cases these may be so violent as to oblige the surgeon to forego all attempts at bringing the ends together. 3. There is no well-authenticated instance of immediate union after suture of nerves. 4. The anatomical union produced by the suture of a nerve seems, however, to expedite the re-establishment of its functions; and in this point of view it is a good operation, worthy of being preserved.—*Archives Gén.*, July.

Pelikan's Modification of Pirogoff's Tibio-tarsal Operation.—Professor Heyfelder states that he has had ample opportunity of confirming the good opiuion he had formed of this operation in his own cases in Finland and St. Petersburg, by the experience derived from the Prusso-Austrian war of 1866. The procedure consists in making a curved incision, which, commencing above the posterior edge of the internal or external malleolus, passes along the dorsal surface of the foot and terminates at the other malleolus. After this incision, which divides only the skin, and the preliminary separation of the incised parts, the subjacent tissues are completely cut through down to the bone, and the epiphyses of the two bones of the leg are removed by the saw. The soft parts covering the os calcis are next divided, and the bone sawn in the same direction. The sawn surfaces of the bones of the leg and of the os calcis are easily brought in contact, which is impossible by the unmodified procedure, even after section of the tendo Achillis.—*Gazette Médicale*, August 29.

Reduction of Dislocations after the Subcutaneous Injection of Acetate of Morphia.—Dr. Thierfelder relates four cases in which reduction of dislocations, which had resisted the efforts made, speedily yielded after narcosis had been induced by the injection of from one half to one fifth of a grain of morphia. The cases he tried the plan in were examples of luxations of the humerus, the elbow, and the femur, and their narration bears out his statement of its efficacy. He says—"1. That the necessary degree of narcosis, producing muscular relaxation is much more certainly and readily obtained by the morphia than by the inhalation of chloroform. 2. This amount of narcosis produced by morphia, contrariwise to what is observed in the use of chloroform, is attended with little or no loss of consciousness—a circumstance of great consequence when we consider how disturbing the loss of sensibility is to the operator. In drinkers, too, chloroform gives rise to excitement rather than to anæsthesia, while in such persons (who are especially liable to this class of accidents) morphia thus employed forms a very certain means of treatment. 3. In luxations occurring in subjects suffering from organic disease in which chloroform is contra-indicated, the morphia is admissible. 4. The simplicity of the apparatus required enables the practitioner to always have it at hand. 5. The special assistance required in the administration of chloroform is no longer necessary."—*Ploss' Zeitschrift für Med. und Chir.*, No. 4.

On Accumulation of fluid in the Hernial Sac as an obstacle to reduction and on Petit's operation in relation to the Taxis.—Dr. Ravoth relates two cases as illustrative of the fact that accumulation of fluid in the sac not unfrequently prevents a reduction of a hernia, which may be easily effected after that has been discharged by puncture. He is a great advocate for Petit's operation, which he would rather have regarded as adjuvatory to the taxis than as herniotomy properly so-called. He thinks that this might lead to a much needed revision of the treatment of incarcerated hernia. In the great majority of cases it is with a simple mechanical obstacle we have to do, and it is surprising that the prognosis of incarceration should continue so unfavourable. The author believes, however, that the treatment of hernia is capable of as great improvement as that which has been brought about in the operation for cataract. In this last old prejudices have been cast away, and technical procedures have been vastly improved. Among the prejudices which still beset the management of incarcerated hernia is the abuse of the taxis and delay in the operation. Hours and days are lost in resorting to various means which an exacter diagnosis ought to have taught from the first would be fruitless. The fear of the operation has often delayed resorting to it, and Petit's procedure has had to contend with prejudices. Of course there are exceptional cases in which it cannot be resorted to; but in spite of these, Dr. Ravoth believes a valuable rule to be that after the taxis and its adjuvants have been tried in vain, Petit's operation should be resorted to without delay. Discharging the fluid the sac may contain is, as already stated, of great utility. Another point to be observed is the ease and safety with which the omentum even of large hernias which have existed for years may be returned, omental adhesions and degenerations being of far rarer occurrence than usually supposed. For this reason the separation and removal of the omentum must be avoided as much as possible, leading as it does to much delay in healing the wound. The external wound is also often made too large, and left unclean. The smaller and cleaner it is the greater will be the likelihood of primary union. There is great advantage in making the incision in part subcutaneously, and at all events at first it should be only small, enlarging it afterwards if necessary. An attempt should be made to unite the wound by first intention, or at all events to render the suppurative process as slight as possible. The chief danger of the operation is dependent upon the condition of the hernia and its treatment prior to this being undertaken. The less the patient has suffered from the duration of the strangulation and the consequent interruption to the normal activity of the intestinal canal, and the less the hernia and surrounding parts have been subjected to excessive injury from the taxis, the more favourable the prognosis and the more simple the after-treatment of the operation.—*Berlin Klin. Woch.*, No. 22.

Treatment of Bronchocele.—Professor Lücke, of Bern, observes that bronchocele in all its forms abounds in that canton, and that inflammation and suppuration in the cystic and parenchymatous forms are of common occurrence, as is the danger of suffocation from the growth of the bronchocele. In the ordinary parenchymatous form iodine has become the common popular remedy, which in many cases arrests the progress of the disease, confining it to the ordinary "thick neck," which from time to time, and especially in women after confinement, requires resort to be had again to the iodine. In the aneurysmatic form, when from the amount of pulsation present the ligature seemed the only remedy, small doses of the iodide of potassium in some cases have exerted a remarkable effect in causing a subsidence of the pulsation, and a diminution of the tumour itself. The cystic bronchocele is curable in all its forms. Frequently, puncture with subsequent iodine injection suffices; but we may have to resort to incision with suture of the sac. This in Dr. Lücke's cases has never been followed by bad consequences, although he has employed it in cysts which have attained the size of a child's head, with rigid, calcified walls. The only objection to it is the hideous cicatrix left behind. The seton recommended lately by Hamburger is correctly characterised by Patruban as a bad and dangerous procedure. When the soft parenchymatous bronchocele assumes a certain degree of consistency, which arises either from colloid degeneration, or, more frequently, from the formation of hard nodules within the substance of the glandular mass, the disease completely resists the external and internal use of iodine. Extirpation has been hitherto the only remedy, and this has been only applicable in small, movable, median bronchoceles, and quite exceptionally in those of large size. Alarming hæmorrhage and purulent infection have occurred so often as to leave the operation few defenders. Cauterization is also a dangerous procedure, and leads only to an imperfect recovery. The author relates an interesting case, in which, after persevering with the injection of tincture of iodine into the substance of the tumour, he succeeded in procuring its absorption. He has had other cases, but they are of too recent occurrence to report upon. In chronic enlargement of the lymphatic glands, which have resisted all other means, and have been on account of their number or position unsuitable for extirpation, he has also injected the iodine with success.—*Ibid.*, No. 25.

SUMMARY.

Amputation.—MacCormac. Amputation of the Thigh compared with Excision of the Knee. (Dublin Journ., August. With an Illustration.)

Aneurism.—Servier. On Gluteal Aneurism. (Gaz. Hebd., Nos. 21 and 22. Relates a fatal case, in which digital compression of the aorta and afterwards injection of the perchloride of iron were tried.)

Carotid Artery.—Lefort. On the Therapeutical Value of Ligature of the Common Carotid. (Gaz. Hebd., Nos. 28, 30, and 35. *Résumé* of a memoir communicated to the Academy of Medicine founded on 435 cases, all of which the author states he has verified by the original accounts.)

Dislocation.—Tillaux. Mechanism of Dislocation of the Femur backwards. (Union Méd., No. 82. With a Discussion at the Society of Surgery.)—Mathieu. Mechanism of Dislocation of the Lower Jaw. (Archives Gén., August.)

Ear.—Zaufal. Condition of the Vena Mastoidea in Caries of the Temporal Bone, with Otitis. (Wien Woch., Nos. 40 and 41.)—Politzer. On the Employment of the Tuning Fork in the Diagnosis of Diseases of the Ear. (Ibid., Nos. 42, 43, and 44.)—Schwartze. Mode of Perforating the Membrana Tympani. (Archiv für Ohrenheil, B. 3, H. 4.)—Schwartze. On the Caustic Treatment of Suppurative Otorrhea. (Ibid., B. 4, H. 1.)—Wreden. On Myringomykosis Aspergillina. (St. Petersb. Med. Zeit., 1867, No. 9. Under this name is described a parasitic growth of the membrana tympani. Fourteen cases given, and plates of the aspergillinæ.)

Enchondrosis.—Parisot. Treatment of Enchondrosis of the Phalanges and Metacarpus. (Gaz. Hebd., No. 31. Instead of amputating the part on which the enchondrosis is placed, Parisot removes the latter by gouging [*évidement*].)

Erectile Tumours.—Valette. Treatment of Erectile Tumours. (Gaz. Méd. de Lyon, No. 27. Employs a ligature in combination with chloride of zinc. Discussion at the Society of Medicine.)—Santesson. On the Danger of Injecting Nævi with the Perchloride of Iron. (Journal für Kinderkr., Mars. Death produced in a child apparently from some of the injection entering the facial vein.)

Excision.—Podrazki. Successful Case of Excision of the Wrist. (Wien. Med. Woch., No. 39. The author believes that this is the only recorded case after gunshot wound.)—Croly. Cases of Excision. (Dublin Journal, May and August. With Illustrations.)—Thompson. Excision of the Knee after Gunshot Wound. (Ibid., August.)—Maas. Excision of the Knee. (Berlin Klin. Woch., Nos. 26 and 27. Two cases occurring in Middeldorpf's Clinic.)

Exostosis.—Boekel. Case of Multiple Exostoses. (Gazette des Hôp., No. 88. These occurred in a girl seven years of age, implicating the bones of the extremities and the ribs on both sides. One on the forearm preventing pronation was successfully removed.)

Eye.—Galezowski. On Affections of the Cornea and their Treatment. (Union Med., Nos. 89 and 90.)—Jeffries. Three Cases of Retinitis Pigmentosa. (Boston Med. Journ., April 30. Also gives a ready method of recording the field of vision.)—Kügel. On

Nystagmus. (Annales d'Oculistique, Mai.)—Sichel. On the Operation of Strabismus. (Ibid.)—Von Graefe. On Conical Cornea. (Berlin Klin. Woch., Nos. 23 and 24. Describes a new mode of treatment by producing interstitial infiltration.)—Zehender. Retrospect of the Results of Ligature of the Carotid in Pulsating Orbital Tumours. (Klin. Monat. für Augenh. Relates to thirty-one cases already published.)—Taylor. Reports on Cases of Extraction by Linear Flap. (Edinburgh Medical Journal, July and August.)—Berlin. Foreign Bodies in the Vitreous Humour. (Archiv für Ophthal., xiv, A. 2.)—Leber. On Optic Neuritis. (Ibid.)

Fracture.—Berenger-Féraud. Ligature of Bones in Compound Fractures. (Bull. de Thérap., August 15 and 30. Adds a case to nine others already published. The treatment consists in connecting the fragments by means of metallic or other ligatures wound around them.)—Sonrier. Fracture of the Os Calcis by Compression. (Recueil de Méd. Militaire, June. The case is rare, as almost all others have been induced by the traction of the tendo Achillis. Autopsy and Woodcuts.)

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Ovariotomy.—Koeberlé. Statistical Results. (Gaz. Hebd., No. 32. Relates to 69 operations performed between 1862-68, 45 or two thirds recovering. The particulars are given in detailed tables.)—Greene. Eight Cases. (Boston Journ., April 30 and May 7. Six recoveries. Greene attaches great importance to the employment of artificial serum [recommended by Dr. Peaslee], the parts being kept constantly moistened in it. It should be used at blood-heat; and the author believes that in any operation where a large surface is exposed, hot water causes much less shock than cold.)—Decristoforis. Fatal Case. (Gaz. Med. Ital. Lombard., Nos. 25 and 26.)—Schroeder. Two Cases. (Berlin Klin. Woch., No. 21. One fatal from intestinal perforation twenty-two days after operation.)—Stilling. Case. (Deutsche Klin., No. 20. Fatal on the eighth day from peritonitis and trismus.)

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