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JULY, 1857.

PART FIRST.

Analytical and Critical Reviews.

Review I.

Of Nature and Art in the Cure of Disease. By Sir John Forbes, M.D., D.C.L. Oxon., F.R.S., Fellow of the Royal College of Physicians, Physician to the Queen's Household, &c., &c.—*London*, 1857. pp. 264.

The inquiry which Sir John Forbes suggests, as to the relative powers of Nature or Art to cure Disease, is one that involves a variety of questions affecting the science, the practice, the polity, and the ethics of medicine. The regiminal treatment of disease is by no means an invention of modern date, but its merits, as opposed to a more decidedly medicinal treatment, divided the ancients, as it has formed a subject of discussion in more recent times. The public have also ever been alive to the question, in evidence of which we would only quote one lay-author, who, in the 'History of a Foundling,' observes:

"If the number of those who recover by physic could be opposed to that of the martyrs to it, the former would rather exceed the latter. Nay, some are so cautious on this head, that, to avoid the possibility of killing the patient, they abstain from all methods of curing, and prescribe nothing but what can neither do good nor harm."

Fielding evidently thought Nature was not to be trusted, for he continues:

"I have heard some of these with great gravity deliver it as a maxim, that Nature should be left to do her own work, while the physician stands by, as it were, to clap her on the back, and encourage her when she doth well."

The subject is one that every tyro in the profession occasionally reflects upon; but it is essentially one upon which the opinion of men who rank highest in the profession, by the length of their experience, or the labours by which they have achieved distinction, deserves to be listened to with the most respectful attention. The book before us is the production of one who, after having been engaged in the practice of medicine for fifty years, feels, to employ his own words, that his profession has claims on him for much more than he has hitherto been able to give it, but that he is called upon now to communicate any information he may possess, if he is to communicate it at all.

"In doing so," he continues, "I cannot help being impressed with the feeling of solemnity which naturally accompanies any act that is to be the last of its kind. And in this mood I would fain regard the present work in the light of *A Legacy to my younger brethren*, which, slight as it is, may not be found altogether unworthy of their acceptance. I would indeed bequeath it in full confidence of its value, if I might reckon on its being received in the same way as the Legacy of the Pot of Gold in the fable was received by the rustic testator's sons. If my book—though, like the old man's vineyard, really containing in itself no gold—should only lead to the zealous cultivation of the subject of which it treats, the result could not fail to be of inestimable value to the cultivators. For, on the profounder, more critical, and purer study of Nature as manifested in diseases, rest, in my judgment, the best hopes of improvement in the medical art; and to this study the spirit of my book may, at least, lead the way and give the initiative, if its actual contents are found of lesser importance."

The error which the author regards as the great taint of medical science, and which he combats throughout this book, is a want of trust in the powers of Nature to arrest the processes of disease, and a consequent overweening faith in remedial agents as the sole means of cure. His main object is to endeavour to expose these misconceptions, so as to impress the minds of the younger and less prejudiced members of the profession with the truth and importance of the principles advocated, and also to prepare a work which might convey to educated members of the general public a juster knowledge of the real nature of disease, and the true characters and powers of the medical art. Dealing professedly in deductions of which the premises are supposed to be known to the reader, we fear that, however intelligible the author may render his views to professional readers, the unprofessional reader will fail to appreciate them, because the first elements—the actual observation of disease—will be wanting to enable him to judge of the justness of Sir John Forbes's remarks. The current theories of the day generally ooze out sufficiently to infect the lay public; a true theory of disease, one, to use Mr. Spencer's words, of which the negation would be both false and inconceivable, once established among our Asklepiadai, and the public would not fail speedily to be imbued with so much knowledge as would be good for them. Until our own banquet is secured, we do better not to invite strangers to the feast; but we may prepare them against the happy consummation, by placing before them such sound and wholesome fare as they can safely digest. Let the public, high and low, be instructed in the plain, well-proved facts of anatomy and physiology; let them learn the wonders of our healthy fabric, and the greater marvels of the functions that minister to life; and thus prepared, we will induct them into the inner shrine, when we ourselves have raised the veil. Our duties to ourselves and to our high calling demand an humble, earnest, and assiduous devotion to the study of Nature in health and disease; we may thus ultimately succeed in understanding, to the full of man's capacity, workings that are yet mysterious; we may thus obtain, at last, what is the very starting-point of Sir John Forbes's Theory of Therapeutics—a *Natural History of Disease.*

But, though we may be a long way from the goal, we must not ignore the fact that the whole tendency of the present age is to arrive at an interpretation of disease, such as the author shadows forth; Physiological Pathology, which forms the title of more than one work of distinction of recent date, has for its object the search into the relation of the morbid processes of disease to the analogous processes of health; or into the mode in which the normal changes of the body are perverted, in the disturbances constituting a deviation from health. What is the meaning of the term metamorphosis of tissues, now so often employed? Is it not applied equally in physiological and in pathological conditions, and do we not know that that metamorphosis passes through an endless variety of phases, both in health and disease; but that the transition is often imperceptible, showing the non-essentiality of disease as a separate entity, and proving that, in many cases at least, the same elements go to make up the sum of disease that previously, in a somewhat different order, constituted the normal state of health?

Let us suppose, for instance, that one of the essential features of pneumonia consists in the arrest of the chlorides of the blood in the lungs; physiology instructs us that a certain average of chlorides passes off by the urine in the four-and-twenty hours, and pathology informs us that in an individual labouring under certain symptoms, termed inflammation of the lung tissue, the renal secretion yields a much smaller quantity of this constituent; we conclude that by restoring the secretion of chlorides by the kidneys, we shall relieve the organs of respiration, and promote the restoration of the various disturbed functions to their healthy condition. The practical question, then, comes to be, whether we may or may not rely exclusively upon the natural powers to re-establish the proper balance; whether we are to employ artificial means to aid in its re-establishment, or whether we may trust Nature herself to effect it.

Again, let us take the case of a person wasting under a tubercular disease, in which hectic and colliquative sweats and diarrhœa are sapping the vital powers; we know that the metamorphosis of the tissues is going on so rapidly that, if left to herself, Nature will speedily wear out that thinning frame,—may we still place our faith in the tendency of disease to overcome its own evil, or in the unaided physiological powers to cast out the demon? or may we come to her aid with such helps as the study of Nature has proved to be conducive to an arrest of that excessive metamorphosis, and thus, if not save life, yet prolong its span? Or, to select a still more striking example, as capable of more positive synthetic and analytical proof:—in metallic poisoning, where all the tissues of the body are impregnated with the metal, and where disease is produced by the presence of the foreign substance in the muscles, the brain, the bones, what results are certain to occur if Nature, unaided, is required to cure the disturbance of function, the disease, that ensues? Will Nature eliminate the lead that causes the Burtonian line, or does she not rather wait for Art to come in with her sulphur or her iodide of potassium, to expel the enemy, who had effected so secure a lodgment that she alone would never have mastered him?

These are some of numerous instances that may be already alleged as indicative of the means by which we may rationally aid Nature in the treatment of disease; while especially the last two are brought forward to show that nature, or, in other words, the physiological forces of the system, do not suffice to restore health to the sufferer. We look forward with confidence to the time when the range of Art will be still further increased, and her relation to Nature still more accurately defined. But of one thing we are as certain as Sir John Forbes could desire us to be, that unless, in all our remedial administrations, we consult the laws of physiology, and seek to avail ourselves of the inherent powers of the constitution to remove disease, we shall ever fail to be masters of our art, and shall go about, ourselves blind, seeking to direct the blind. The real question in therapeutics is, what can Nature do unaided?—can she do it in the most expeditious way alone?—does she require some artificial assistance, or is she altogether unable to restore health without the intervention of art? In the above instances we have sought to illustrate some of the ways by which we may assist nature. Undoubtedly, in the majority of diseases, Nature—that is, the physiological processes—tend to remove disease by restoring the natural balance of the powers; in many she does so most expeditiously, if left to herself; in many the processes may be facilitated if we adopt the hints which the balance and the test-tube afford; in some she appears overcome by the noxious influence, and to succumb, unless a powerful antidote be administered, which, by neutralizing the poison, enables the physiological influences again to assert their power, and to restore the balance of health.

We are tempted to place before the reader a formula by which we have sought to make clear to ourselves and to students the various points of view under which we may regard diseased processes, and which has appeared to us to facilitate the *rationale* of our therapeutical applications.

If we represent the normal constituents of the body by a, b, c, d, e, and assume that in health they occupy the relation to one another of $a+b+c+d+e$, disease may be regarded as taking place in one of three ways, which may as readily be represented by an algebraic formula. Either the constituents are simply deranged, and they come to occupy a different mutual relation; or an elision of one or more of the elements may take place; or thirdly, a foreign element may be superadded. In the first case the different forms of disease may be as numerous as the changes that can be effected in the relative position of the elements, and instead of the formula, $a+b+c+d+e$, we may find them in the relation of $a+d+e+b+c$, or of $b+d+c+e+a$, and in many others. It is manifest that, if anywhere in the treatment of disease, this category will probably be the one in which we shall find the most numerous instances of what Sir John Forbes terms the power of Nature in curing disease,—that is to say, that the interference of medicinal agents will be less necessary, because, by placing the patient in a proper condition as regards noxious influences, by allowing the powers of his constitution to find their proper balance, the derangement of the constituents of health will be rectified. It becomes a question for further inquiry, whether it is not a due and legitimate prerogative of Art to ascertain that Nature is able to achieve certain results, when the conditions are granted, and to grant those conditions. We are willing at once to confess that since our first initiation into the mysteries of medicine we have abhorred the doctrine that our art consisted exclusively in the administration of pills and potions. To return.

Our second category—that in which an elision of one or more of the elements occurs—would be represented thus, $b+c+d+e$, or $a+b+e$, or let E represent the elements in their normal strength and order, the above two instances would stand thus respectively: $E-a$, and $E-(c+d)$. The inquiry to be made in this case in regard to therapeutics would be, whether the powers of Nature would be adequate to restore the deficiency, or whether it would be requisite to supply the element which we found wanting by artificial means, or, in other words, by drugs. There can be no doubt that here again the physican will often do little more than place Nature in a position to work out her own salvation; but at the same time he may, by the judicious administration of the deficient element, materially expedite the process. To take a familiar instance. The blanched lip and the pale tongue of an anæmic patient challenges us to prescribe a salt of iron, because we know that element to be wanting to restore our patient to health. By hygienic means alone, by fresh air, improved diet, and cold sponging, the system may be enabled to take up from the food the necessary iron without medicines; but we all know that either we cannot place our patient in circumstances in which the hygienic means are available, or that the process is very tedious, and will be much expedited by the administration of five grains of the ammonio-citrate of iron three times daily, half-an-hour after meals. Then, naturally, both the patient and the physician agree that the citrate of iron shall be taken.

In our third category we place those forms of disease in which we discover a new element superadded to the normal constituents. This can be nothing else than a poison, which may be of an organic or an inorganic character. The formula here would be $(a+b+c+d+e)+x$ or $+y$; or, representing the normal elements collectively as E, the formula would stand $E+x$ or $+y$. The duty of the medical man here would manifestly be first to recognise the presence of the x or y, and then to determine upon its best mode of elimination. The same question arises as was put before. Do the powers of Nature suffice to produce the desired effect, or do they not? No one, we think, will affirm generally that they suffice; while probably every medical man of experience will readily admit that in many instances the unassisted powers of Nature will secure the elimination of the x or the y. But we claim for the medical man the right to place Nature in the right position to develop her own powers, and we all know that the intervention of the physician for that purpose is often most essential, as Nature does not herself always assert her right to be heard in her own cause. Were it otherwise, probably neither Sir John Forbes's book nor this Review would have been written. On the other hand, it is unnecessary to quote instances in proof of the statement that unaided Nature—i.e., Nature without physic, *sensu strictiore*—is often inadequate to secure the elimination of x or y, and that without the eliminant, x or y will continue to infest our patient's tissues, racking his nerves, exhausting his vigour, turning his muscles into fat. There may be a medium between these two extremes; we may know that Nature will suffice to remove the poison, but we know also that in its passage through the system it will leave an indelible impression, or that much time will be wasted by the process; and we have an agent at hand which will accelerate the movement of the elements, and secure a more rapid evolution of x or y, shall we therefore not employ it because Nature might have carried out the work alone? Not to allow the employment of the drug under such circumstances would be tantamount to a wilful rejection of the boons accorded to us by Him from whom Nature has her source. *Omne simile claudicat*, and a limp may probably be discovered in any and every theory that was ever proposed; therefore we guard ourselves against the charge of representing all diseased actions by the above formulæ. Before concluding this brief exposition, we would merely add, for the benefit of any one who may be tempted to follow out our method more into detail, that the three categories would, even theoretically, necessarily often pass into one another; thus, whether an element were deficient, or a new one superadded, in either case the remaining elements might change places in a manner represented by the first category; or an element might be wanting, while a foreign constituent were superadded; it is clear that by going into a detailed comparison between individual forms of disease and the formulæ, we should often meet with very complex arrangements; these we have at present nothing to do with, as it is not our wish to dilate upon a mathematical representation of disease, but simply to show the point of view in which we regard the various demands made by diseased processes upon the therapeutical interference of medicine.

We think that by the above analysis we have rendered to ourselves more clear what may be expected of Nature in Sir John Forbes's sense, what of Art. Still, before proceeding to a further examination of Sir John's views, we would protest against that assumed antithesis being made a ground of accusation against the medical profession at large. Many there undoubtedly are who think that without medicine no disease can be well cured; but we feel satisfied that the great bulk of all educated members of the profession—and, thanks to modern progress, we may hope that that comprises the majority of all its members—consider themselves rather as ministers and interpreters of Nature than as Titans who are engaged in a constant warfare with her vicious manifestations. That interpretation may often be erroneous, but the attempt to discover it argues for the fact that the medical man does not desire to place his art in contrast with Nature.

If we are right in assuming these remarks to apply very generally to medical men of the present day, we by no means assert that such tendencies have always prevailed; the fact that they did not prevail some years ago, in this country at least, was proved by the animated opposition which the enlightened views of Sir John (then Dr.) Forbes excited when put forth in 1845, in an article On Homœopathy, Allopathy, and Young Physic. Twelve years are but a comparatively brief space of time in the development of a nation, or of an important integral part of a nation, yet any one who is conversant with the feelings, and views, and the general state of education of medical men, during the last twenty years, will, we think, bear us out in the opinion that, scientifically and ethically, great onward strides have been made. We cannot but believe that the courageous advocacy by Sir John Forbes of views much opposed to the prejudices of a large class in the profession, have contributed not a little towards the reformation of the *drugging system*. Most sincerely do we thank him as a benefactor of his profession and of mankind. At the same time we are constrained to admit that we are far from arriving at that goal of certainty which we all desire to attain; and we would willingly enlarge our knowledge of the relative powers of Nature and Art in the cure of disease. Sir John appreciates the difficulties that impede the attainment of this desirable end, but speaks more lightly of them than we think the case justifies.

"The main obstacles . . . . lie rather in the circumstances under which the subjects are presented to them than in the subjects themselves. When we have the proper field for investigation before us, there is very little difficulty in obtaining a positive and accurate knowledge of the power possessed by Nature in relieving and curing diseases. The phenomena to be observed are neither very numerous nor very complex; the facts are easily obtained, and the deductions are both facile and sure. All that is requisite to insure a positive and pure result is, in the first place, to take care that no artificial interference disturbs the organic processes going on; and, in the second place, to observe and chronicle progressive events. It is a case of simple observation throughout; no sifting of premises, no elimination of causes, no grouping or balancing of effects being requisite to insure a just conclusion. The just conclusion—the exact valuation or appreciation of the power under examination—is enunciated in the simple fact indicating what has been the issue of the organic processes constituting the disease. The sum total of beneficial modification of the morbid processes, whatever it may be, whether amounting to a complete or an imperfect cure, must be acknowledged to be the exclusive work of Nature; in other words, of the conservative powers inherent in the living body." (p. 25.)

The author admits that there are difficulties in finding the proper field for study; but this we do not regard as so serious a difficulty as the determination of where medicinal interference commences, and where it ceases. All writers on materia medica claim baths, hot and cold, as belonging to the *armamentarium medicorum;* the regiminal physician would probably claim them as coming within the pale of regiminal treatment, though we should be inclined to assert that the natural man abhors the cold bath, and only learns to love it by degrees. The application of ice externally and internally is a powerful remedy, but may belong to either party; poultices to the surface or the mucous membranes (in the shape of mucilaginous beverages) are instances of a similar kind; ripe fruits, raw or cooked, are regiminal, but their acids and neutral salts administered in solution would be eschewed, because they would come to the house by aid of the apothecary's boy. We quote these instances in no spirit of levity, but to show that the question is not in reality so much one to be settled by two opposing parties—the medicinal and the regiminal physicians—but that the inquiry to be prosecuted must and may be carried out contemporaneously by all who are anxious to elevate the science of medicine, and place it on a sure basis. No humane physician would consent to watch the progress of a case of neuralgia under purely regiminal treatment, when he knows that a grain of morphia will arrest the pain and give rest to the sufferer; nor would he allow a patient's health to continue to deteriorate because he did not choose to administer a drachm of the oil of male fern for the removal of tænia which infest his intestine. These, and numerous other instances of a similar character, would have to be eliminated before fixing either the diseases or the drugs which were to form the subject of study. Thus the question necessarily becomes more and more narrowed, and at last we arrive at the conclusion that what we have to investigate is rather how much or how little influence individual drugs are able to exert upon certain diseases than that we declare ourselves followers of this or that banner. When we examine the author's statements with regard to the "instruments of the medical art," we gather that, although so powerful an advocate of an essentially regiminal treatment of disease, he by no means despises or rejects the exhibition of drugs; thus, in speaking of alteratives, he says, "the class contains some remedies of positive and evident power;" he lauds opium and its products, as being "some of the noblest instruments of the medical art," and says that "the class contains a good many other agents of analogous though inferior value." Of genetica he says that the "class contains only three or four drugs, but they possess a positive power of greater or less extent." Scarcely a class is passed in review of which not something more or less favourable is said; we doubt much whether a physician who believes—may we not say knows?—that he has command of a remedy of "obvious and admirable power," would refuse to allow his patient all the relief the drug can afford, in order that he might test whether the unaided powers of Nature sufficed to remove the disease he labours under.

The reader will naturally inquire into the manner in which the author of the book under consideration reconciles the apparent contradiction involved in his all but unlimited faith in the powers of Nature to cure disease, on the one hand, and his laudation of drugs, as exhibited in our last remarks, on the other. We will quote his own words in reply, merely premising that Sir John terms the system he advocates, *Auxiliary or mild treatment*, *rational expectancy*.

"This modification of the indirect physiological method of treating diseases (more especially acute diseases), I regard as at once the most philosophical, the safest, the surest, and the most successful of all the forms it assumes in practice. . . . . In the first place, it completely recognises the autocracy of nature in the cure of acute diseases, and proceeds on the principle that it is not only useless, but injurious, to attempt to suppress or greatly to modify the morbid processes by strong measures of a perturbative or exhaustive kind.

"The indications which this mode of treatment seeks to fulfil are chiefly the following:—1st. To place the diseased body in the most favourable circumstances for the development and exercise of its own conservative powers, by the institution of a proper regimen, in the most comprehensive sense of that term. 2nd. To endeavour thereby, or through the use of medicaments, to remove such obstacles to the favourable action of the conservative and restorative powers as may be removable without the risk of checking or injuriously perverting them. 3rd. Applying these measures under a watchful supervision; not to attempt by any vigorous measures to alter the course of the morbid processes so long as they seem to keep within the limit of safety, and when they transgress, or threaten to transgress this limit, only then to endeavour to modify them by such mild measures as, if they fail in doing good, cannot do much harm. 4th. To be on the watch against possible contingencies, which may demand the employment of measures of exceptional activity, whether in the form of regimen or medicine, and, when required, to apply such measures with the necessary vigour." (p. 239.)

So far as this method combats the purely empirical proceedings and the treatment *à la Sangrado* that we all have seen, we most heartily concur with Sir John Forbes, and we think that if he had much familiar intercourse with physicians of less than twenty years' standing, and had the opportunity of judging of their practice, he would find that the prevailing fault is not so much a tendency to trust over much in drugs, as to be sceptical of their utility. There is a scepticism which leaves its holder in a slough of despond—a scepticism which, having no positive basis whatever, vacillates with the wind of public opinion or with the accidents of daily life. The medical man whose knowledge is of a calibre to allow him to become the prey of such scepticism, may be regarded as the most unfortunate man under the sun, for no professional act of his can be attended with any degree of moral satisfaction; he will be inclined to say with Dr. Cayol, "Les systèmes en médecine sont les idoles auxquelles on sacrifie des victimes humaines." But there is another scepticism, which is the characteristic of all men of science, which coexists with the warmest love of his profession and a full faith in the powers of his art, in the heart of the zealous and earnest physician; it is the scepticism which leads him to be suspicious of his own observations and of his deductions, until by repetition and by comparison with the results obtained by others, he has placed his conclusions on the firmest basis upon which a scientific fact can rest. Such is the scepticism which has ever distinguished the most elevated in our ranks,—we think that the general advancement of the profession, and the greater humility which that increased knowledge has brought with it, have spread more generally that form of scepticism which we would uphold as a laudable feature of our times. Still we are also satisfied that while many of the views advocated by Sir John Forbes are in a great measure the views held by the majority of physicians of the present day, we have a more positive knowledge of the extent and limits of the powers of Nature on the one hand, and of the real uses of drugs on the other, in the cure of morbid states, than has been possessed by our predecessors. If we keep to the path we are pursuing, and continue to seek to interpret Nature correctly in all her phases, making Physiology our main instructress in the manifestations of disease, but not disclaiming any aid obtainable from all the handmaidens of the physician, we shall pass safely through the Scylla and Charybdis of dogmatism and false scepticism. We shall retain our faith in the powers of Nature to cure disease, but we shall no less continue to believe that "the Lord hath created medicines out of the earth, and he that is wise will not abhor them."

In the preceding remarks we have sought rather to indicate the spirit that pervades Sir John Forbes's book than to give our readers a summary of its contents. With its spirit we cordially sympathize, for it is a spirit of earnest hatred of all quackery, of manly affection for the high objects of our common profession. We cannot deny that the author appears to us to underrate the value of medicines, but so long as with us the professional man and the tradesman are blended together, and Government takes quackery under its special protection, so long there will be little risk of drugs falling generally into disrepute; and we may say of Sir John, as of the Man of Ross, that

"E'en his errors leant to virtue's side."

With this reservation, we do not hesitate to repeat that we cordially agree with the author's general views, and recommend his "Legacy" to all medical men who earnestly seek for Truth in the daily practice of their surpassingly interesting profession.

Review II.

Gesammelte Abhandlungen zur wissenschaftlichen Medicin. Von Rudolph Virchow, Professor der Pathologischen Anatomie und Physiologie an der Universität zu Würzburg. Mit zahlreichen Holzschnitten und Tafeln.—*Frankfurt am Main.* 1856.

Collected Essays on Scientific Medicine. By Rudolph Virchow, Professor of Pathological Anatomy and Physiology at the University of Würzburg. With numerous Woodcuts and Plates.—*Frankfort.* 1856. pp. 1024.

The founder of the well-known 'Archiv für Pathologische Anatomie und Physiologie,' the editor of the newest and most elaborate 'Hand-book of Special Pathology and Therapeutics,' has presented us, in a handsome volume, with a collection of some of those researches by which he has exercised so great an influence on the progress of medical science in Germany, and has gained for himself, in a comparatively short space of time, a very high position among the reformers and promoters of our profession. The volume contains some of the author's earliest contributions, which were scattered through various journals, some of which have ceased to appear, and are not easily to be obtained, while, with a single exception, we find none of those essays which have been published in his 'Archiv.' Virchow's earlier memoirs are of particular interest, as through them we are enabled to understand more readily the views he has laid down in the 'Hand-book of Special Pathology and Therapeutics,' and are allowed at the same time to perceive the manner in which he advanced step by step. The individual essays are provided with new paragraphs, and notes are added, in which the results of later researches are recorded, and the memoir on the subject of thrombosis and blocking-up of bloodvessels (Virchow's embolia) which has created so much sensation, appears here for the first time in a complete form, the second and greater part having never been published before.

Each treatise bears the stamp of the powerful mind of an original inquirer, who examines his subject in every point of view and by all the means at his disposal; who draws his inferences in a logical manner, influenced as little as possible by existing theories, however long they may have been established, however great the authorities in their support.

We can do little more than give the titles of the greater part of the essays; but we propose to dwell especially and at some length upon those subjects with which Virchow's name is especially connected.

In the first section the author treats, in a philosophical manner, on the nature of man, on animal life, on medical science, on disease, and on epidemic disease (*Seuche*) in particular. His observations in these paragraphs abound in new ideas and sound criticism. We need only allude to those on pathological systems in general, and on the so-called ontological systems in specie; on cellular pathology, on metastasis and infection.

The second section (pp. 59–165) contains Virchow's principal essays on *fibrin*.

1. *Coagulation of Fibrin.**—The author agrees with Nasse and other physiologists, who teach that the coagulation of fibrin is effected by the juxtaposition of molecules, which are to be considered as perfectly invisible. The coagulated fibrin is a completely uniform gelatinous substance, which in larger masses appears always homogeneous, while in membrane-like pieces it assumes, by the formation of plaits and wrinkles, a fibrillating aspect. In 1854, the author adds, that in all his subsequent investigations he never met with any form of coagulated fibrin which could be regarded as of granular appearance. The coagulation of fibrin always commences with a gelatinous stage, in which the fibrin is invariably combined with a certain quantity of serum-water analogous to the water of crystallization. Some fibrin remains in this condition, but in general, after some time, the molecules that were hitherto invisible, approach each other more closely, the fluid between them is squeezed out, contraction—or coagulation, in the stricter sense of the word—takes place. While this process of contraction is advancing, the fibrillating condition becomes more evident, and as the last result of coagulation, the formation of true fibrils cannot be denied. Similar observations may be made on mucus. The gelatinous mucus is completely homogeneous, but by means of water, acids, or alcohol, coagulation is effected, and true fibrils become visible. Fibrin-like mucus in the act of coagulation alters the appearance of such bodies as may be imbedded in it, by making them oval, oblong, caudate, spindle-shaped, &c. This is particularly well seen in cellular formations of a viscid nature, as colourless blood and pus-globules, and the addition of acetic acid makes it still more evident.

2. *Physical Qualities of Fibrin.*†—Elasticity is considered as one of the most prominent peculiarities of coagulated fibrin; this peculiarity is attributed, as in caoutchouc, to the high degree of attraction between the molecules; its connexion with electric phenomena is denied, as only the dried fibrin exhibits, when heated, positive electricity, a character which it possesses in common with other proteinaceous substances. During the metamorphosis of the thrombus, the elasticity yields first to fragility, then again to toughness, a state which signifies the histogenetic transformation of the fibrinous coagulum into connecting tissue, the chemical transformation into gelatinous tissue. Viscosity, or the faculty of adhering to adjacent objects, is possessed by fresh fibrin only in a small degree. The opaque spots on the pericardium and peritoneum, the semi-cartilaginous plates in the coverings of the lungs, the spleen, the testicles, without adhesion to the opposite parts, may be quoted in favour of this assertion. It must be confessed, however, that the origin of these alterations is not in all cases due to fibrinous deposit from inflammation; but in some, at all events, we find unquestionable layers of fibrin on serous membranes, without agglutination to the other side. Although, however, fresh fibrin is not considered to be of viscous nature, yet it may become so by chemical transmutation within the organism some time after its extravasation; it may further appear to possess this peculiarity when mixed with other viscous substances, as albumen and colourless blood-globules.

3. *Chemical Qualities of Fibrin.*‡—In this essay Virchow endeavours, according to his own affirmation, to separate the proved facts from the probabilities and theories which abounded on this subject at that time still more than at present. 1. The *common fibrin.* Although it is true that blood containing fibrin does not coagulate when mixed with a solution of sulphate or carbonate of soda, yet we are compelled to admit "that the presence of fibrin can be recognised only by its coagulation; the coagulated fibrin in an approximative manner by the general characters of proteinaceous bodies, by its insolubility in water, but principally by its morphological and physical qualities." The *fat* which occurs in combination

* Froriep's Neue Notizen, No. 769, Sept. 1845.

† Zeitsch. f. rat. Med., Band v. pp. 213 ss.

‡ Ibid., Band iv. pp. 262 ss.

with fibrin the author found to be composed of 91·90 per cent. fatty acids, and 8 10 per cent. lime (and soda ?), free from cholesterine and serolin. These fatty acids of the fibrin appear analogous to those of the nervous tissue, by their containing nitrogen and phosphorus, by their swelling in water, and by their combination with lime. We may add, that Virchow has lately discovered the existence of a fatty substance, similar to that of the nerves and of fibrin, in the yolk of the hen's egg, in the corpus luteum of cattle, in the spleen, the lungs, in pus, &c., and applies to this body the name myelin (Markstoff).* The *salts* of fibrin do not exceed 1 per cent.; they consist of phosphate of lime and a small quantity of phosphate of magnesia, and are intimately incorporated with the proteinaceous substance. Virchow attaches a due importance to the presence of those salts for the process of nutrition, and of ossification in particular. The fibrin and albumen deposited in a certain organ may, according to his view, be so changed, that the proteinaceous substance, after having been transformed into soluble extractive matter or fat, is absorbed and carried to the organs of secretion, while the lime remains. This view gains in probability by the result of Schmidt's researches,† who found that the blood of some invertebrate animals contains a combination of albumen with caustic lime and phosphate of lime, which, under the influence of carbonic acid, is decomposed into carbonate of lime, soluble albumen, and phosphate of lime. 2. The *formed fibrin.*—We are assured of the presence of fibrin in the blood, chyle, lymph, humor aqueus of the eye, in exudations, and believe it to exist in the spermatic fluid. Besides this common coagulating fibrin, another variety has been admitted by some, which is supposed to exist already coagulated in definite forms. The existence of this variety is altogether denied by our author, and especially so in the glandular tissue, or in the muscle, except in the fluid plasma surrounding the primitive fibrillæ. The *résumé* of Virchow's researches on this subject is, "that the covering of the blood-globules is formed by a proteinaceous substance similar to fibrin, but that the existence of fibrin in the cellular or fibrous tissues as a constituent of their membranes, or nuclei, or contents, is chemically unproved." 3. Concerning the admission of *different varieties of fibrin* (of the arterial, venous, and inflammatory blood, Magendie and Rokitansky's "pseudo-fibrin," and Mulder's "oxyprotein," &c.), the author observes that he knows only of one kind—namely, the fibrin in the coagulated form.

4. *On the Disintegration of Fibrin.*‡—As far as the morphological changes in the breaking down of fibrin are concerned, Virchow agrees in general with Gulliver,§ but he never found them connected with any formation of new cells, but could discover only the remains of those previously admixed. With reference to the chemical processes, he observed the development of hydrosulphuric acid, ammonia, butyric acid, and a solution in some respects similar to albumen, but differing from it by the peculiar change of colour which it shows under the influence of nitric acid, gradually added, and which most resembles the erythroprotid of Mulder.

5. *On the Origin of Fibrin and the Cause of its Coagulation in Animal Fluids.*—In this section we meet first with the question on the pre-existence of fibrin in coagulating fluids. From his own researches and those of others, Virchow is led to the inference:

> "That in none of the animal fluids perfect fibrin pre-exists as such; but that both the blood, as also the lymph and lymphatic fluid, contain a substance nearly allied to it—that of the former more, that of the latter less so; which substances, by the contact of oxygen, become sooner or later transformed into real fibrin, and then coagulate." (p. 133).

With regard to the origin of this fibrin-producing material (Virchow's fibrinogenous substance), he considers it to be a product of the metamorphosis of tissue, and especially those structures which are more intimately connected with the

* Archiv. f. Path. Anatomie, Band vi. p. 562.
† Zur vergleichenden Physiologie der wirbellosen Thiere. 1845.
‡ Zeitsch. f. rat. Med., Band v. p. 226.
§ On the Softening of Coagulated Fibrin: Medico-Chirurgical Transactions. 1839.

lymphatic system (lymphatic glands, spleen, and particularly the connecting tissue). In these structures, and not in the blood itself, the fibrinogenous substance is formed. From thence it passes as well into the exudations as also into the lymph, being so to say washed out by the fluids transuding from the blood. According as, sooner or later, it comes in contact with oxygen or fluids rich in oxygen, it is sooner or later transformed into coagulable fibrin, the coagulation of which may take place within the tissue itself, within exudations, within lymph or bloodvessels. This, however, is a morbid process, for in health the fibrinogenous substance immediately undergoes another change, and is further decomposed. (p. 137.) It will be seen from this passage, that Virchow places the origin of fibrin not in the blood, but in the tissues themselves, a view which is not proved, but which at all events appears to simplify the mechanism of fibrinous exudations. Thus, the tissue in the state of irritation need not attract the fibrin from the blood in the capillaries, analogous to the secreting cells of some glands, nor need we suppose the coats of the capillaries to become more permeable by the irritation. In the normal state of things, the fibrin-producing substance formed in the tissues would be absorbed by the lymph-vessels ; but in some inflammatory conditions the surplus of fibrin, thus formed, would become accumulated within the tissues, or transude beyond their surface. The lymph, the blood, the exuded fluids, would in such cases become richer in the fibrinogenous material, and the fibrinous crasis (Hyperinosis) would be considered as well a product of inflammation as the exudation itself.

The third section is occupied by the author's essays on *Colourless Blood-Corpuscles and Leukœmia.* He published his first case on *white blood* in November, 1845.* Tumours of the spleen, œdema of the extremities, cough, diarrhœa, epistaxes, furunculous and pustulous eruptions, and predominance of the white blood-globules, were the principal symptoms. Already at that time Virchow made some remarks which show that he understood his case, and did not confound it with pyæmia, as Bennett, Rokitansky, and other observers had done. He quotes another case published a short time before in Vienna as pyæmia,† and vindicates it as one of leukæmia.

The essay *On White Blood and Tumours of the Spleen*‡ was written after the author had read Bennett's publication. The circumstance that the latter considers the change in the blood of a pyæmic nature, leads Virchow to the discussion on the subject of pyæmia. The latter maintains that there does not exist a distinct difference between the colourless globules of the blood and the pus-globules. He attributes the confusion on the subject of pus in the blood to the erroneous assumption that the so-called lymph-globules of the blood are identical with those of the lymph, and derived as such from the chyle ; to the imperfect knowledge of the different stages of development of the white blood-globules ; to the neglect of the influence of the media on the appearance of the globules ; finally, to the assumption of a certain normal pus (pus bonum et laudabile), which has served as a measure for all other cellular elements, again without paying attention to the different phases of the pus-globule itself.

"There are cells," the author says, "which are characterized by the existence of several nuclei. These cells are most frequently met with in pus, hence they have been called pus-corpuscles. But they exist also in the lymph and in the blood ; they are found among the epithelial cells of serous membranes, in the younger layers of the epidermis, &c. It was therefore wrong to call them pus-corpuscles. . . . . The pus-cell is formed in the exudation-plasma, as the chyle-cells ; and probably also the colourless blood-cells are found in the chyle and blood-plasma. The law of development is the same for the pus-cell and the colourless blood-cell ; both are relatively embryonic cells, which differ in so far as the former may become developed into connecting tissue (*Binde-substanz*), the latter into red blood-globules. If, therefore, we find cells with several nuclei in the blood, we must consider them as the foundation for new tissue elements of the blood, no matter whether they are in conditions favourable for further development, or whether they have become obnoxious to the laws

* Froriep's Neue Notizen, No. 780. Nov. 1845.
† Zeitsch. der k. k. Gesell. der Aertze zu Wien, vol. ii. p. 488. 1845.
‡ Med. Zeitung, Nos. 34–36. Aug. and Sept. 1846.

of retrogressive metamorphosis, before having reached their normal state of development. There is no cause for calling them pus-globules, but we may designate them colourless blood-globules, as the red globules form their highest development." (pp. 167, 168.)

It is well known that, several years after the publication of this essay, Lebert and Sedillot* have again adduced various points by which pus and colourless blood-globules are to be distinguished. Thus they maintain that the former are larger ($\frac{1}{100}$ to $\frac{1}{80}$ millimetre) than the latter ($\frac{1}{120}$ to $\frac{1}{100}$ millimetre); that the former are more yellowish, the latter white; the former spheroidal, the latter slightly lenticular; that the surface of the former is granular, mulberry-like, of the latter more smooth; that the former possess nuclei (of $\frac{1}{300}$ to $\frac{1}{200}$ millimetre) with a cup-shaped impression, the latter very small nuclei ($\frac{1}{500}$ to $\frac{1}{400}$ millimetre), almost like nucleoli, or fat-granules. Although we admit the correctness of the majority of these points, when we compare the usual white blood-globules with the well-developed genuine pus-globules, yet we constantly meet with pus-globules that have all the characters just ascribed to the white blood-globules, and *vice versâ*. If we further take into consideration, that both kinds of globules exhibit in the same individual important changes, according to their stage of development; that pus-globules, when mixed with blood (i.e., a more concentrated fluid), undergo, according to the laws of exosmosis and endosmosis, very marked alterations in shape (by becoming smaller, smoother, &c.), we cannot hesitate to assert that, with rare exceptions, the presence of pus in the blood must remain unproved, and especially so in the cases of white blood where no source for the purulent infection could be found.

Virchow therefore denied the pyæmic nature of the cases published in Edinburgh, and "vindicated for the colourless blood-globules a place in pathology."

In another article,† nine cases of pus-like blood, observed by Bichat, Velpeau, Oppolzer, and others, are examined, and the probability of their having been cases of white blood explained. Among the circumstances which appear to lead to an increased number of white globules, we find particularly mentioned—*a*, loss of blood (Nasse,‡ Remak,§ Henle)‖; *b*, chronic exhausting diseases (Gulliver's pus-globules); *c*, serious acute diseases, especially typhus, pneumonia, puerperal fever (Nasse). To the viscosity of these globules, already mentioned by Nasse and others, their motion along the walls of the capillaries is ascribed, as well as the fact that in retardation of the circulation they stagnate sooner than the red globules —a circumstance which has led to the idea of a new formation (E. H. Weber, Rokitansky). Piorry's mistake (Hæmatitis) regarding the explanation of a buffy coat, granular and uneven on its lower surface, which is sometimes seen in such conditions, is corrected. In the paragraph on the relation of the spleen to white blood, the author ascribes the function of sanguification, not to the spleen alone, but also to the lymphatic glands, thymus and thyroid, and liver (E. H. Weber and Kölliker). The splenic bodies are considered as closed capsules with a very minute ramification of vessels on their surface; their arrangement is compared with that of the placenta, where diffusion takes place between two fluids, separated from each other only by a permeable membrane. Similar may be the process in the conglobate glands. If the fluid passing from the glands into the blood exerts an influence on the transformation of the colourless into red globules, it is evident that morbid affections of these glands must be of the greatest importance for the development of the blood.

In a new chapter, *Die Leukämie*, the author alludes to the history of the development of our knowledge on this subject, and especially to the contested point of priority between Bennett and himself. Every impartial observer will admit that our thanks are due to both authors. Bennett has done much by the excellent cases he has published, and by the observations attached to them, as also by

* De l'Infection Purulente ou Pyoémie. 1849.
† Med. Zeitung. Jan. 3 & 4, 1847.
‡ Untersuchungen zur Physiol. und Pathol. 1839.
§ Med. Zeit. der Vereins f. Heilkunde in Preussen, No. 27. 1841.
‖ Zeit. f. rat. Med. 1844.

drawing the attention of others to this affection; but we cannot deny that to Virchow belongs the particular merit of having been the first who understood its real nature. Bichat, Velpeau, Oppolzer, and others, had published cases which, in all probability, belong to the same category; but they had not perceived their true bearing; nobody will therefore attribute to them the priority. Such was also the case with Bennett before Vichow's publication had appeared, although his observations had already been much more accurate and valuable than those of his predecessors. We should scarcely have entered into this question of priority did we not consider it justice to do so, as we find, in an excellent handbook of pathological anatomy touching on the subject of Leucocythemia, Virchow's name altogether omitted.

In reference to the name, Virchow is not inclined to sacrifice his "Leukæmia" to Bennett's "Leucocythemia:" he is of opinion that it would be more proper to apply the expression "Leucocythemia," or rather "Polyleucocythemia," to those states in which the white globules are physiologically (digestion, pregnancy) or pathologically (most inflammatory and typhous affections) increased, but only for a limited period; this polyleucocythemia exhibiting as great a difference from real leukæmia as chlorosis from the anæmia of carcinomatous patients. Leukæmia does not merely signify a state characterized by an increased number of white globules, but "an altered development of the tissue of the blood in its dependence on certain organs." There is, in reality, a less perfect colouration of the blood. Several observers (Uhle,* Griesinger,† De Pury,‡ among the latest) have made the observation, that the quantity of colourless globules is in some parts of the body much larger than in others; as in the splenic vein, which may be explained by a greater destruction of red globules within the tissue of the spleen, or also by increased formation of colourless globules in that organ. The accumulation of white elements in the vena cava and right side of the heart is attributed to the posthumous movements of the vessels during and after death, and to the inosculation of the thoracic duct, whose movements do not cease for several hours after death. The large proportion of white globules in the small vessels of the brain, as first pointed out by Bennett, has been repeatedly confirmed by Virchow; and we had the opportunity of observing it ourselves in two cases, as well in the brain as also in the lungs, the liver, and the kidneys.

The circumstances that, in some patients, the affection of the lymphatic glands prevails over that of the spleen, and *vice versâ;* that in the former cases the elements of the lymphatic glands (viz., "innumerable round granulated nuclei, generally provided with nucleoli, of the size of the usual nuclei of the lymphatic glands, here and there also cells consisting of such a nucleus surrounded by a membrane rather closely attached to it") predominate in the blood; in the latter those corresponding to the elements of the spleen;—these circumstances lead Virchow to establish two varieties of leukæmia, the lymphæmia and splenæmia. In a case observed by ourselves, where the lymphatic glands were enormously diseased, while the spleen was almost normal, we met certainly with many of the above-described lymphatic elements in the blood, but still more of the larger white globules which are attributed to the splenic variety. Scherer's qualitative analysis of the blood, in a case of the splenic variety, has exhibited, besides lactic, acetic, and formic acid, gelatin, a peculiar organic body, and 0·4 — 0·6 per cent. of hypoxanthin. The presence of the latter substance is of peculiar interest, as it will be remembered that it has been discovered by the same chemist in the pulp of the spleen.

With respect to the disease of the blood-forming organs, it is described as a hyperplasia of their constituent elements, first of the glandular cells, then also of the connecting tissue. As the results of Virchow's researches on this head do not materially differ from those of Bennett and other observers, we will not enter on them in this place; we must, however, allude to a very remarkable circumstance with which our author met in several cases of the lymphatic variety.

* Archiv f. Path. Anat., vol. v. † Ibid. ‡ Ibid., vol. viii.

The slightly enlarged liver contained numerous small whitish granules, generally of the size of a normal lobule of the liver, which exhibited under the microscope nuclear and cellular elements quite like those of the lymphatic glands. This infiltration of nuclear masses appeared to be in connexion with the portal vein. In one of these cases a similar alteration was witnessed in the kidneys. None of the other observers has discovered these formations, and we have ourselves looked for them in vain in the two cases that fell under our observation. We particularly recommend this interesting subject to further attention, as Virchow interprets it as a new formation of glandular tissue, analogous to the hypertrophy of the lymphatic glands, where the glandular tissue is likewise found transgressing the pre-existing boundaries of the glands. He considers this as the product of a lymphatic diathesis—i. e., a progressive inclination of the organs to the formation of lymphatic elements.

Virchow's view concerning the nature of leukæmia is, that it must be considered as a disease *sui generis*. With him, we expect further elucidation from more extended clinical observations: "Several times," he says, "the thought struck me, whether acute inflammatory processes may not lay the foundation of the disturbance." (p. 209.) The same idea, we should think, must have forced itself upon the mind of other observers too. The inflammatory character was very striking in the case to which we have alluded as having fallen under our own observation; the following are its main features: the patient was a man, twenty-nine years of age, tall and muscular, belonging to a family not quite free from scrofulous affections; healthy himself in his youth; never affected with ague; formerly an officer in the Polish army; he had endured privation during the last years before the commencement of the disease. This manifested itself, soon after having been wetted through on a cold winter day, by acute pain in the left axillary region, followed by considerable swelling, which increased within three weeks to the size of a fist. The pain then abated, the tumour seemed likewise to decrease, until four weeks later, when, while travelling, he contracted a cold, again suffered severely for a fortnight, and observed a rapid increase of the swelling. Then another remission during about three weeks, followed by a third exacerbation, which was accompanied by a considerable degree of pyrexia and an elevation of the temperature (more than 2° Fahr.) of the left axillary region over the right. A small quantity of blood drawn at that time exhibited, in a thin cylindrical glass, a layer of white globules above that of the red amounting to one-eighth of the latter. Towards the end of this exacerbation pain and swelling also manifested themselves in the right axilla. Then, again, abatement of all symptoms ensued during four weeks, with exception of the swellings and a moderate degree of paleness. Sixteen weeks after the beginning of the illness—namely, in April, 1852—there was cough, and fresh pain in both axillary regions. In May, swelling of the lymphatic glands of the neck, quickly increasing, without much pain; paleness; loss of strength. In June, swelling also of inguinal glands, of the glands of the arm and legs; diarrhœa, alternating with constipation; œdema of feet and face; glandular tumours of both axillæ and neck enormous; no perceptible swelling of spleen; layer of white globules, one fourth of red. Second week of July: Great dyspnœa; extremities cold; radial pulse imperceptible; heart's impulse very weak; 13th, muttering delirium; coma. Death on 14th of July—i. e., less than eight months from the commencement of the disease. *Post-mortem twenty hours after death.*—All the lymphatic glands of the body enormously enlarged, pale, moderately firm, exhibiting under the microscope the nuclear and cellular elements in an unusually developed degree; blood very pale; right heart filled almost entirely with a white coagulum, containing only few red elements; pulmonary arteries, up to their smaller ramification, filled with pale yellow coagula, some appearing drier and more firmly adhering to the walls than others. Spleen, eight ounces, tissue tense, rather pale; liver, four pounds and six ounces, very pale; all other organs normal, but highly anæmic; only the vessels of pia mater and the sinus of the head filled with pale blood and yellowish coagulum. The inflammatory character of the swellings was so great in this instance, that we considered it as a case of lymphatic adenitis; the alteration of the blood was so distinctly secondary that we hesitated in using the name leukæmia, as applying only to the secondary, not to the primary affection.

Regarding the course of leukæmia, Virchow says: "Little as we know at present of the origin of the disease of the organs, we may accurately survey the course and termination of the disease. As yet no well-proved case of cure is known." We may agree with this, as far as the developed cases of leukæmia are concerned, but further experience must show whether we may not be able to arrest the disease, while confined to the glandular or splenic affection, before the alteration of the blood has made much progress.

In another new article, On Colourless Blood-Globules, the author commences by considering three possibilities respecting their origin—1. Their formation in the blood itself; 2. Their introduction into the blood through the lymph and chyle; 3. Their being detached from the epithelium of the walls of the vessels. The first of these sources is limited to the division of the pre-existing cells; the third is regarded as unproved; but the lymph and chyle are conceived as conveying to the blood as well the developed, as also the undeveloped, globules, derived from the lymphatic glands, the spleen, and the connecting tissue. We cannot conclude our notice of this article in a better manner than by giving Virchow's own words regarding the nature of these mysterious bodies:

"I therefore must still maintain the view which I have repeatedly brought forward, that the colourless blood-globules which we find circulating in the blood are simple cells, without a specific character, whose transformation into red globules cannot take place; that they therefore form a relatively superfluous constituent of the blood—a kind of excess, or waste. The transformation of lymph-globules into red blood-globules takes place much sooner, and it appears that if a certain cell, when passing into the blood, has transgressed that stage of development, it is unfit to undergo its specific coloured metamorphosis. It then circulates for some time, and perishes finally by retrogressive metamorphosis. Thus it may be easily conceived that the larger the number of colourless globules in the blood, the smaller is the amount of red cells." (p. 218.)

The fourth section, occupying one-half of the whole volume, contains the essays on Thrombosis and Embolia, and on Inflammation of Vessels and Septic Infection. (pp. 219–732.) These are also subjects on which our knowledge has been considerably enlarged, and in some respects corrected, by Virchow's labours. Many cases of obstruction of arteries and coagulation of blood within the vessels had been published, but insufficiently interpreted, when Paget's excellent memoirs, 'On Obstruction of the Pulmonary Arteries,' appeared in 1844 and 1845. Almost at the same time we received an essay by Bouchut, touching the spontaneous coagulation of blood in the vessels in cachectic states and in chronic diseases. To Virchow, however, the merit is due, not only of having explained that many of the cases of obstruction of the pulmonary arteries are the effect of the lodgment of fibrinous plugs carried there from a distance, and having pointed out the places where, and the circumstances under which, the primary fibrinous coagula are formed, but to him also we must accord the priority in the question of the obturation of the systemic arteries in consequence of the detachment of solid substances from the valves of the left ventricle, &c. (1845.) Later we met with the publications of Pioch, Meinel, Doederlein, Rühle, Kirkes, Tufnell, Klinger, Simpson, &c., some of which have evidently been written without the knowledge of our author's researches, as Pioch's 'Cas de Gangrène partielle du Pied, attribué à un Caillot Détaché du Cœur;'* and, above all, Kirkes' masterly essay 'On some of the Principal Effects resulting from the Detachment of Fibrinous Deposits from the Interior of the Heart, and their Mixture with the Circulating Blood.'†

In his first publication, On the Obturation of the Pulmonary Artery,‡ Virchow distinguishes *primary* and *secondary* obturation. In the primary form the obstruc-

* Gazette Médicale. Août, 1847. † Med.-Chir. Trans. 1852.
‡ Froriep's Neue Notizen, No. 794. 1846.

tion commences in the artery, the alteration of the parenchyma being the consequence; in the secondary form the alteration of the parenchyma causes the obstruction. Many cases of obliteration of branches of the pulmonary artery in tuberculosis and pneumonia are attributed to the secondary obturation, but a large number of obturations are of a primary nature, and concerning these Virchow long since said:

"These plugs have been formed in some part of the vascular system, situated in the circulation anterior to the lungs—i. e., either in the veins or in the right side of the heart; and have been carried by the blood into the pulmonary artery." (p. 224.)

In proof of this, he adduces: 1. That plugs are met with in the venous system. 2. The plugs fill, when fresh, the whole lumen of the artery; they adhere only loosely to the walls, which exhibit no alteration; when old they adhere only to one side of the vessel. 3. The plugs do not usually commence from the capillaries, but extend only to the division of a larger branch, riding frequently on the bifurcation. 4. The age and condition of the plug are in general similar to those formed in the plug of the veins. 5. The thrombus of the vein, in which the blood is coagulated, extends some distance beyond the inosculation into the larger vein, which is still capable of carrying on the circulation. This prolongation of the plug takes place in the direction towards the heart, along the wall of the vein, where the plugged branch enters; thus the circumstances are given which are most favourable to the softening of the plug, a part of which may then break off and be carried away by the stream of the blood. This view gains strength by the irregular step-like appearance of the termination of the prolongated venous plug.

*Farther Researches concerning the Obturation of the Pulmonary Artery and its Consequences.**—The author's experiments on animals show that the venous stream of blood is able to carry off bodies of greater specific gravity than the venous blood through the right heart into the pulmonary artery; that the contact of these bodies with the internal surface of the heart does not cause any marked symptoms; that the plugs either ride on bifurcations or pass some distance into a branch; a consecutive coagulation of blood takes place before the plug, and another coagulation around it, if any space is left between its edges and the walls; but the walls of the artery behind the plug collapse if the lumen is completely obturated by the plug. The view of Rokitansky and older French pathologists regarding the spontaneous coagulation of the blood in the pulmonary arteries, in consequence of the admixture of products of inflammation to the blood, is rejected. Due justice is done to Paget's merits on this subject, but his hypothesis, that the presence of urea in the blood may increase the adhesion between blood-vessels and blood, and thus lead to spontaneous coagulation in the pulmonary arteries, is considered as still unproved. Retardation (or complete stoppage) of the circulation appears to be the principal condition favouring the coagulation of the blood within the vessels, while relative or absolute increase of fibrin is considered as of secondary importance. Retardation to such a degree as to lead to spontaneous coagulation is, however, almost only met with in the venous system—very rarely in the arteries either of the body or of the lungs; it is therefore, also, *à priori*, not likely that we should meet with spontaneous coagulation in the pulmonary arteries.

"The cases of obstruction of the arteries of the body by fibrinous plugs, cannot (he then already said) be adduced in proof of the just-reputed hypothesis, as they are likewise to be explained by blocking up through substances transported there from the left ventricle."

In order to elucidate the consequence of the mere obturation without complication, Virchow performed a series of experiments on dogs, which he relates under the following heads:—*a*, Introduction of animal substances into the jugular vein (fibrinous coagula, plugs taken from veins, pieces of muscle). *b*, Of pieces of the pith from the elder tree. *c*, Pieces of caoutchouc.

* This first appeared in Traube's Beiträge zur Experiment. Pathologie und Physiologie, Heft 2. Berl. 1846.

The results of these experiments show, that up to a certain period the consecutive alterations are similar in all cases of obturation, but that important differences are seen in the later development. In all cases we observe, first, coagulation of blood in the vessel round the plug, as already mentioned; later, inflammation of the coats of the artery, manifested by changes in the wall itself, without exudation into the cavity of the vessel. From this point great differences commence, no further alterations being witnessed after the intromission of the caoutchouc plugs, while the substances under *a* and *b* cause violent pneumonia, terminating either in suppuration or necrosis, pleuritis with extravasation into the parenchyma of the pleura, and abundant serous and hæmorrhagic effusion into the cavity, with predominant tendency to ichorous metamorphosis; the pleura over the affected part of the lung became necrotic, and by bursting, led to pneumothorax. The whole series of these phenomena developed itself in one case within less than five days.

We may infer from these results that the local affections following the obturation depend only to a small degree on the plugging up itself, but much more on the chemical and mechanical nature of the plug. The circumstance that the pulmonary tissue, after the complete obturation of the artery of an entire lobe, remains unaltered, corroborates the view that the bronchial artery is the nutritive vessel of the lung. Virchow's experiments further show the formation of a distinct collateral circulation through the bronchial and intercostal arteries after the obturation of the pulmonary artery. A very important experiment, in which a large number of pieces of muscle were introduced, exhibits the occurrence of death with all the symptoms of asphyxia, the heart having been found in the diastolic state. The ingestion of air acts likewise by the impediment it originates to the circulation through the lungs. Here, too, the heart is found in the diastolic state, as, in the whole, the most different forms of asphyxia produce paralysis of the heart, which again appears to be the effect of the regurgitation of the blood into the coronary arteries, through its accumulation in the right ventricle. The tetanic stretching of the voluntary muscles, the retardation of the respiration, the dilatation of the pupils, the protrusion of the eyes, &c., are the immediate consequences. Some of the author's experiments exhibit also very clearly the subsequent metamorphosis of the thrombi: 1, the organization, by vascularization and canaliculization; 2, the formation of detritus by simple softening or putrid deliquescence.

Regarding the secondary disturbances in man, the author shows that here, too, the greatest differences are met with, according to the size and nature of the obturating substance. Smaller obstructions remain probably without any urgent symptoms, as Paget had already mentioned; the fibrinous plugs become in general organized into cellular tissue, which sometimes contains vessels (usually also pigment), and adheres to the internal surface of the vessel. Sometimes they undergo the sinus-like degeneration. Larger thrombi exhibit symptoms that may at first more or less resemble syncope, but later assume the character of asphyxia. Thus we find in Case 8 the occurrence of several attacks marked by feelings of anxiety, by oppression, moaning, feeble pulse, coldness of the extremities, cold perspiration, at last death, with stretching of limbs, turning of eyes, jerking motion of thorax, and sighing.

On Acute Arteritis (pp. 380—450).*—In this chapter Virchow examines the results of the observations and experiments of other observers (Sasse, Bouillaud, Rigot and Trousseau, and Gendrin), from which no positive inferences can be drawn, and then relates thirteen experiments of his own, made on dogs. From these the author concludes: 1. That no exudation takes place on the free surface of the interior membrane of arteries. 2. That two circumstances may have led to an erroneous, opposite view on the subject—namely, the overlooking of the small collateral branches by which blood is conveyed into the empty artery, and the rupture of the internal membrane by which exudations collected between the coats may enter into the cavity of the vessels. 3. That necrosis of the arterial

* Archiv f. Path. Anat. und Phys., Band i. p. 272.

coats causes coagulation of the blood in the affected part. 4. Chemical and mechanical irritation causes inflammation of the external and middle layers of the coats of vessels. The alterations of the internal membrane are of a secondary and passive nature. 5. The phenomena of inflammation of the external and middle layers are entirely analogous to the common phenomena of parenchymatous inflammations. The fibrinous coagula in the cavity of arteries are therefore not to be considered as the products of exudation from the lining membrane, but as caused by coagulation of the blood. These coagulations may be considered under three heads: *a*, those only *attached to the wall*, and thus effecting a diminution of the lumen, occasioned either by local retardation of the circulation, or by roughness of the wall, or by a combination of both circumstances; *b*, *locally obturating coagula*, induced either by the preceding variety, through further coagulation of the blood, or by coarctation of the lumen, acting like a ligature, or by plugs detached from another point and carried by the circulation to the place of obturation. The proof for the embolic nature of an arterial obturation may be found in the locality of the coagulum, in the multiplicity of the coagula, in the co-existence of analogous bodies in the centre of the obturating coagulum and on distant points of the arterial system, in the suddenness of the appearance of the phenomena and in their constancy, in the condition of the arterial walls, and in the conformation of the secondary coagula round the embolus. A series of cases explain the author's propositions. The symptoms manifested during life depend on the ischæmia* of the part provided by the obturated artery, combined with collateral fluxion; they must therefore vary with the locality, the size, and the nature of the embolus. The paleness, coldness, and loss of turgor may be so intense as to justify Cruveilhier's term "cadaverisation." Amongst the functional disturbances of the extremities, the neuralgia stands foremost; hyperæsthesia, paræsthesia, anæsthesia, and paralysis, are likewise met with. Plugging of the cerebral arteries may produce the well-known symptoms of tying of the carotids. Soon the ischæmic symptoms become mixed with those produced by the collateral circulation, frequently leading to hyperæmia and its consequences—sometimes even to inflammation. In most instances, however, these secondary changes appear to belong to the retrogressive metamorphosis, or are of the necrotic nature. Thus we meet with softening of the brain (already compared by Rostan with senile gangrene), instances of which we have also in the cases of Rühle, Kirkes, and others.

c. Generally obturating coagula are combined only with necrotic processes. The coagulation in all the arteries of a certain district points to impediments of the circulation, and particularly of the capillary circulation. The impossibility of the entrance of blood into the capillaries acts like a ligature on the arteries; coagulation in the direction towards the heart is the necessary consequence. Thus the hæmorrhagic infarctus may cause coagulation in the arteries of the district, and at the same time necrosis of the part itself, by depriving it of its nutriment. Concerning the connexion of gangrene with the obturation of arteries, Virchow sums up his observations in the following manner:

> "Obturation of arteries may produce, but does not always produce, gangrene; gangrene may occasion obturation of arteries, but does not always occasion it; gangrene and obturation may be, but are not necessarily, joint effects of the same cause." (p. 450.)

Obturation of the Mesenteric Artery by an Immigrated Plug† (pp. 456–58).—Of particular interest in this case is the hyperæmia, and even fibrinous exudation in the parts supplied by the obturated artery. Virchow appears inclined to attribute this circumstance to the impaired nutrition of the vessels, which, according to this view, would be more easily distended, and even ruptured by collateral influx of blood.

* *Ischæmia*, a term formerly employed by Peter Frank, is applied by Virchow to states of local arterial anæmia, in which the blood is prevented from flowing into those parts for which it is destined. (p. 304.)

† Verhandl. der Würzburg Gesellsch., Band iv. p. 341.

Phlogosis and Thrombosis in the Vascular System (pp. 458–636).—In the essay on acute arteritis, the author was led, as we have seen, to the inference that the processes within the cavity of the vessels depend on thrombosis or coagulation, while the primary phenomena of inflammation are confined to the walls. Inflammatory, Virchow calls those active pathological processes which proceed from irritation, therefore the irritative disturbances of nutrition. Such pathological processes he describes: *a*, in the sheaths of the vessels (periarteritis and periphlebitis), leading to suppuration, to callosities, &c.; *b*, in the middle layers (mesarteritis, mesophlebitis); *c*, in the internal membrane (endarteritis and endophlebitis). Regarding this internal membrane, it will be remembered that the author, in his earlier publications, considered its alterations by irritation as of secondary and passive nature. At present, he still looks upon it as a kind of barrier against the changes of the middle coat in the acute forms of inflammation, yet he attributes to it also in these forms certain parenchymatous alterations; but much more important are the alterations in the chronic inflammation. He agrees with those pathologists (Bizot, Rayer, Tiedemann, Engel, Dittrich, and others), who derive those gelatinous and so-called semicartilaginous layers of the membranes of arteries from a chronic inflammatory process, as well as also those calcareous and fatty deposits, and the superficial ulcerations.

"It is of as inflammatory a nature," he says, "as the endocarditis, with which it is frequently in direct connexion, and as the so-called malum senile articulorum, the arthritis sicca s. villosa of some later authors; or as Burns proposed, the arthritis deformans. It may very well be named endarteritis deformans s. nodosa."

The author, however, urges the necessity of distinguishing from the inflammatory atheromatous process simple fatty degeneration, which may take place as well in the heart as in all the membranes of the vascular system, and is so well described by Paget, and later by Moosherr,* with respect to the capillaries of the brain. The atheromatous processes have their origin in the parenchymatous inflammation of the internal membrane. They commence in general with a slight swelling of the lining membrane (most distinct on the valves of the heart, the aorta and pulmonary artery), either in patches or in a more diffuse manner; the affected parts contain more fluid substance, by which the whole tissue may have a gelatinous appearance (the gelatinous or albuminous exudations of Bizot, Engel, Lebert, and others). Virchow is of opinion that this increase of substance is partly due to imbibition from the blood (analogy with tissues unprovided with vessels), but he proves, besides water, albumen, &c., the presence of another substance similar in reaction and microscopic aspect to fluid mucus, with many small round cells, often in the process of subdivision, and some larger ones spindle-shaped. The change consists, therefore, not merely in imbibition, but a morbidly increased metamorphosis with new formation, a species of hyperplasia. At the same time the fibres of the original tissue are frequently seen thickened, the cells enlarged with filiform ramifications, &c. Besides the gelatinous swellings, we meet often with harder "semicartilaginous" spots, which may be the product of a further alteration of the former (Lobstein's arteriosclerosis). The sclerotic patches usually undergo the atheromatous process; while the gelatinous swellings pass likewise through the medium of fatty metamorphosis, but terminate in softening and superficial ulcerations (*fettige Usur*); the latter is most distinctly seen in the pulmonary artery, the formation of the atheroma in the aorta. The calcification, which sometimes takes place in the semicartilaginous patches, is regarded as real ossification, on account of the analogous transformation of the cellular elements. Regarding the ætiology, Virchow does not follow Bichat and Rokitansky in ascribing any importance to the arteriality of the blood, but attributes the principal influence to mechanical movements (Rayer, Dittrich), without, however, altogether denying the existence of a dyscratic predisposing condition. The occurrence also of a chronic endophlebitis is another weight against the view that the arterial

* Ueber der Pathol. Verhalten der Kleinen Hirngefässe. Würzburg, 1854.

quality of the blood is the cause of the alterations in the lining membrane of the arteries; the endophlebitis is, however, of rarer occurrence, and terminates less frequently in atheroma than in ossification.

In endocarditis, too, the mechanical movements are of great influence, as Hope has already explained. On the endocardium, as on the lining membranes of the arteries, the phenomena of merely retrogressive metamorphosis are not to be mistaken for those dependent on inflammation. The latter are analogous to those met with in the arteries and the veins; here again we meet at first with the small gelatinous thickenings, principally towards the free edge of the valves; later on these are changed into more tense or semicartilaginous masses, which in a still later period may undergo the fatty (atheromatous) metamorphosis or ossification. The endocarditis, however, runs a more acute course, on account perhaps of the larger number of vessels, and the looser cellular tissue beneath the endocardium; through this medium there is here a greater tendency to the formation of warty excrescences, which may give rise to the deposition of fibrinous coagula from the blood, the origin of which is, of course, altogether different.

Concerning the composition of the thrombus, it differs from a simple blood coagulum by its distinctly stratiform construction, by its larger per centage of fibrin, by containing a greater number of colourless blood-globules. With regard to the circumstances under which a thrombus is formed, we find that retardation of the circulation is the condition which is common to all varieties of thrombosis; but how, in the retarded or stagnating blood, the change in the "fibrinogenous substance" is effected, without which the coagulation does not take place, is a further question which had not yet been answered to the author's satisfaction. Malherbe's theory of the superfibrination of the blood, Vogel's hypothesis of the inopexia (i. e., increased coagulability of the fibrin), Paget's view regarding the influence of urea, Engel's, Millington's, and Lee's, concerning the action of pus admixed to the blood,—all these suppositions appear not sufficient to explain the fact of the coagulation within the vessels. Referring to one of his former essays "On the Origin of Fibrin, &c.," Virchow repeats that the influence of oxygen is necessary to effect the coagulation of the "fibrinogenous substance;" this oxygen, when not admitted through lesions of continuity, must be developed within the blood itself, and he is inclined to find the source for this in the spontaneous decomposition of the blood-globules in the stagnating blood.

With respect to the relation between phlogosis and thrombosis in the vascular system, there is no doubt that they are mutually interdependent; but primary thrombosis is much more frequent than primary phlogosis. Phlogosis induces thrombosis principally in cases of endocarditis, when the roughened or ulcerated surface causes deposition of fibrin on the walls; and further, in suppurating inflammation or necrosis of the membranes.

In concluding this chapter, Virchow treats on some of the principal varieties of thrombosis. 1. The *marantic** thrombosis (the spontaneous, rheumatic, or metastatic phlebitis of some other authors) is the most frequent form, and may be induced by many debilitating diseases; in consequence of the diminished power of the heart the circulation becomes retarded in the most distant points, especially the veins of the extremities, the pelvis, and the cerebral sinus. The commencement of the nucleus of the thrombus is, in general, situated behind the valves, exactly in the angle in which these are attached to the veins. 2. Thrombosis through *compression* (ligature, dislocation of bones, &c.) 3. Thrombosis through *dilatation* (varices, aneurisms, teleangiectasiæ, &c.) 4. The *traumatic* thrombosis: *a*, thrombosis from *venesection*; *b*, from *amputation*. 5. Thrombosis of *new-born children*, analogous in some respects to that from amputation, and the forms of *fœtal* thrombosis. 6. *Puerperal* thrombosis. A moderate degree of placentar thrombosis is a physiological process, only by too great extension (incomplete contraction of the uterus) it becomes pathological, approaching in its origin the vene-

* μαρασμός=μάρανσις, the witheredness, tabefaction, atrophy, etc.

section-thrombosis; but the marantic form, that from dilatation and compression, may coexist. 7. *Secondary* thrombosis, through inflammation of the coats of vessels. The inflammation most frequently leading to thrombosis is the suppurative variety, during which, through the ruptured internal membrane, pus may enter into the cavity of the vessel; the thrombosis, however, is, in general, formed before the perforation of the internal membrane; it thus prevents at first the admixture of pus with the blood, but by degrees it breaks off, or deliquesces, or may undergo an ichorous metamorphosis, and may propagate the contamination to the blood itself. All these varieties of thrombosis, as also the other subjects treated of, are elucidated by a large number of well-described and well-adapted cases, which form an important element in the whole volume—a circumstance through which its value is greatly increased.

Embolia and Infection (pp. 636 ss.)—The discussion of the doctrine of pyæmia leads to the examination of the question, whether the principal symptoms are to be considered as produced by the mechanical (globules) or by the chemical action (absorption of the serum) of the pus? The theory of the irritating or obstructing nature of the pus-globules appears perfectly inadequate to explain, by itself, the phenomena ascribed to pyæmia; these must be divided into two series, the one depending on mechanical, the other on chemical influences, or, in other words, into the phenomena of embolia and infection; both are frequently combined, but more frequently separated. The theory of the mechanical metastasis of pus has lost much of its plausibility, since we know that the metastatic abscess is not formed by a mere metastasis of pus, but is originated by a lobular suppurating inflammation. Through this fact the purulent diathesis has become another form of inflammatory diathesis, characterized, however, by a tendency to suppuration. This inflammatory-purulent diathesis can, in many instances, not be explained by the admixture of pus globules—i. e., the mechanical element—as the phenomena of small-pox, of syphilis, and glanders clearly show; but these conditions point to chemical actions. The author here proposes three questions: the first, regarding the *diagnosis of the presence of pus* in the blood, has been treated of already in the section on leukæmia; the second, concerning the artificial production of the so-called purulent diathesis, induced him to try the injection of various fluids into the veins of dogs: *a*, admixture of putrid fluids (the products of simple putrefaction of animal substances, as water from putrescent fibrin) did not produce evident metastasis, and caused death sometimes without abscesses (Castelnau and Ducrest); but the author considers this subject as yet insufficiently examined; *b*, the injection of pus led to the following results:—1. Normal, fresh, not specific pus does not produce, when carefully injected, perceptible anatomical alterations, especially no metastasis—an inference which agrees with those of Dupuytren and A. Boyer. 2. The same operation performed with unfiltered, or very coherent pus, principally when a large quantity is injected in a short time, is followed by the formation of many centres of inflammation and suppuration. 3. When the injected pus is putrid, or specific, these secondary centres have the same character. 4. Injections into the arteries have the same effect as those into the veins. 5. A great part of the consecutive anatomical lesions is to be ascribed to thrombosis which may be caused by the ingestion of accidentally admixed blood coagula, and is not the necessary consequence of the injection of pus. The answer to the third question, namely, "Is the injection of real pus into the blood to be considered as the cause of the pyæmic phenomena in man?" is based on the result of the previous researches and on the analysis of some additional cases.

"The existence of pus in the blood," Virchow says, "cannot, as I have endeavoured to explain, be proved with certainty. The absorption of pus in substance we were obliged to refute; the aspiration of pus has been limited to few instances; the perforation of abscesses into veins has been designated as a rare occurrence. Finally, we have been able to demonstrate that suppurative phlebitis, as a rule, is the product of thrombosis, and that venous pus is the detritus of fibrin and blood-globules." (p. 665.)

On the other side it cannot be denied, after a careful examination of many cases of pyæmia, that they are caused by an alteration in the blood; it further cannot be doubted, that foreign substances may enter the blood from the primary centres of disease; it also appears probable that the principal part of this absorption is not performed by the veins situated in the diseased or wounded spot itself, as they are in general filled with a thrombus, but by the nearest of those ramifying in the neighbourhood in which the circulation continues, as also by the lymphatics. By this assertion, Virchow, of course, does not exclude the possibility of the absorption of fluid substances contained and formed in the thrombus itself. Regarding the chemical nature of the diffusible agent thus admitted into the blood, the author can give no definite explanation; every one, however, will agree with him in assuming that it is of different quality in different conditions; thus it appears inaccurate to speak of *putrid infection* (septicæmia or septhæmia) in those cases in which the symptoms become manifest before any putrefaction or suppuration can have taken place, as in some miasmatic and endemic affections.

"Here we have to deal neither with pus nor the common products of putrefaction, but with specific fluids, which are, no doubt, in a state of transmutation, which originate in the lymphatic fluids, under the influence of miasmatic or epidemic influences, and may infect not only the patient's own body, but also that of others. The chemical quality, by which the humours are altered, is unknown to us; we can, therefore, not use it for the appellation of the altered crasis of the blood. But vitiated the humours are which become admixed to the blood: we can, on that account, without hesitation, select a name from this circumstance; I propose, therefore, to call the condition *ichorrhæmia*, as already the ancients understood by *ichor*, corrupted humours." (p. 702.)

Virchow's ichorrhæmia differs therefore from septicæmia, or septhæmia, by the absence of the really putrid elements; both have that in common, that they are caused merely by infected fluids. The ichorrhæmia is further distinguished by being combined with a larger amount of colourless blood-globules and fibrin, thus approaching, in this respect, the inflammatory crasis (phlogæmia). Finally, the author agrees with Rokitansky in the view that there does not exist a condition which deserves the name of *pyæmia.* The ichorrhæmia, which is to supplant the pyæmia in many instances, possesses, as was just mentioned, the same inflammatory diathesis which had been ascribed to the old pyæmia. In the concluding part of this chapter Virchow gives a *résumé* on some points connected with embolia, on which, however, we are unable to enter. In the same manner we are obliged, for the present, to pass over the remaining sections, which contain contributions to *Gynaekology*, to the pathology of *new-born children*, to the *pathology of the cranium and brain*, and an essay on *cancroid* and *papillary tumours.*

All these essays will amply repay a careful perusal, which will not only convince the reader of the extended and varied knowledge of the author, but will also show him that Virchow's method of investigation is a truly philosophical one, combining observation, experiment, and induction, in a manner which we rarely meet with, but which must necessarily tend to the real advancement of science. We may, therefore, express our confident hope that the author will make good use of his present influential position as Professor of Pathological Anatomy at the University of Berlin, with clinical wards in the Charité, and a chemical laboratory at his disposal; he is the right man to show the way to the study of the phenomena of disease, as well during life as after death, without over-valuing the one or underrating the other; from him, we trust, his pupils will also learn not to depreciate the action of therapeutics, but to consider the art of healing as the last and highest aim of our profession.

Review III.

1. *Report on the Pathology of the Diseases of the Army in the East.* By Drs. Lyons and Aitken. ('Blue Book,' 1856.)
2. *Discussion sur le Typhus observé dans les Armées pendant la Guerre d'Orient.* ('Société Impériale de Médecine de Constantinople.')—*Constantinople.* 1856.

Discussion on the Typhus observed in the Armies during the War in the East. ('Imperial Society of Medicine of Constantinople.')

We observed in our number of July, 1856, that it would be desirable to delay the consideration of Dr. Lyons' Report until all the documents which may be published on the Diseases of the War are before us. But, as it is probable that the official publications which have yet to appear may deal with portions of the medical history of the campaign different from that which is discussed in Dr. Lyons' Report, and therefore will require special and particular consideration, we deem it advisable not to delay any longer our notice of the only pathological report which we are likely to receive. The second work, the title of which heads this review, is the first publication of a Society founded at Constantinople during the war, and which included among its members several of the most distinguished Surgeons of the French and English services. The appearance of this publication, and the interest attaching to the subject discussed by the Society, will lead us in this review to select especially for comment that portion of Dr. Lyons' Report which refers to the Fevers of the Eastern Force.

Dr. Lyons was sent out in April, 1855, with instructions from Lord Panmure to investigate the pathological anatomy of the diseases among the troops. A very able letter of instructions was drawn up for his guidance, and has already appeared in our pages.* Drs. Aitken and Doyle were sent out under him, and a very complete and efficient apparatus was provided.

The advantage of sending out to Turkey men experienced in morbid anatomy, who might institute a regular and systematic investigation, is so evident that it would be an insult to our readers to insist on the point. For want of such men during the winter of 1854–55 at Scutari and in the Crimea, we have lost the opportunity of acquiring a perfect medical history of the campaign. That several thousand men died,—that certain causes produced their deaths,—is the limit of our information. The precise structural lesions which were the immediate causes of death we do not, and shall never, know. We shall receive, no doubt, tables of the diseases to which these deaths are officially referred; but this meagre information is not equal to what the scientific precision of the day demands.

We cannot blame the medical officers of the army for not investigating the morbid anatomy of the diseases prevalent among the army in 1854–55. The labours of these gentlemen were overpowering and incessant, and they naturally threw aside that portion of their duty which could best be spared. Much better it is to have lost all the interest which the examination of those dead men would have given us, than to have taken from the living one moment of time, or one attention which might have aided in preserving a life dear and necessary to the country. The omission was inevitable, and must be inevitable in every campaign attended with unusual sickness, unless, as was done in the spring of 1855, and as is to be done for the approaching Chinese war, gentlemen are sent out for the single and exclusive object of examining the bodies of the dead, and of investigating the causes and the effects of diseases, instead of being occupied in treating them.

Unfortunately, however, in the Eastern campaign, the period of greatest mortality was allowed to pass by before the pathological inquirers were set to work, and their report is therefore by no means equal to what it would have been had their inquiries commenced simultaneously with the sickness. In April, 1855, when Dr. Lyons arrived at Scutari, the mortality of all the large hospitals aggregated there

* Vol. xviii. p. 279.

had fallen to four or five a-day, and soon fell even below this; and the types of disease had lost not only the intensity, but the characters which they had displayed in the winter. Subsequently, the diseases of the expeditionary force presented nothing specially remarkable or unusual; and in the English army in particular, various favouring circumstances combined to keep the health of the soldiers during the summer, autumn, and winter of 1855–56 in a condition unparalleled either in ancient or modern warfare. The French and Russians, indeed, as is well known, suffered greatly at the time when the English were most singularly healthy. "The English army," said one of the French surgeons, in the discussion of the Society at Constantinople, "is to the French what a rich family is to one less endowed with fortune's gifts;" and certainly no men were ever more zealously guarded from all sources of disease than were the survivors and successors of that heroic band whose sufferings and destruction will fill the saddest page of our military history.

After the arrival of Dr. Lyons and his assistants, the chief diseases among the English were typhus and typhoid fevers; and during the summer and autumn of 1855, it would seem clear that the latter disease constituted the great bulk of the cases, though in the previous winter and early spring there can be as little doubt that exanthematic typhus was much more prevalent. Scurvy had almost entirely disappeared, and though in the following winter it was again seen, it was in an extremely slight form, was easily checked by treatment, and did not influence in any great degree the progress of other diseases occurring in persons with this slight scorbutic taint.

The typhoid fever presented the deposits and ulcerations of the Peyerian glands, and deposits in the mesenteric glands, in the form so well known in Western Europe; Dr. Lyons says, "the enteric lesions were all but universally attendant upon it;" but he does not narrate the exceptional cases, in which, with all the other symptoms of typhoid fever, he found no ulceration of Peyer's patches. At p. 60 he gives a table of the chief morbid appearances in 50 cases of typhoid fever. In 31 of these cases there was ulceration of Peyer's patches; in 9 there was deposit in them, but the softened and ulcerative stage had not commenced; in 1 case only (No. 10, Pte. Hugh Love) Peyer's patches were unaffected; but this case was evidently one of double pneumonia, and not typhoid fever,* although it happened to be returned as "Febris Continua Communis." We have been unable to find any other evidence bearing out the inference to be drawn from Dr. Lyons' expression—viz., that the Peyerian glands were not invariably diseased.

The symptoms presented by the typhoid fever in the Crimea presented nothing unusual. Dr. Lyons refers to the frequent latency of its course; but this was not different from what occurs in a certain percentage of cases in France and England. Few things are more surprising than that practitioners will still look for strongly-marked febrile symptoms in every case of typhoid fever; these may or may not exist in a high degree; and there will always be a certain number of cases in which the febrile symptoms are extremely slight; a little elevation of temperature by 1° or 2° of Fahr., a moderate increase in the fulness and quickness of the pulse towards the evening, a little headache during the first five or six days of the disease, and scanty urine, may be the only symptoms of a case which is to terminate at a later date by hæmorrhage or perforation. Whether these cases were comparatively more numerous in the Crimea can only be known by proper statistics, and these are unfortunately not attainable.

It would appear from Dr. Lyons' observations that fatal cases at an advanced period were not uncommon, from continual progress of the intestinal lesion; and that frequently men returned to duty while this local affection was steadily advancing. This form of disease is a very interesting one, as it is comparatively seldom seen in civil life. At a certain period in typhoid fever, the specific disease

* "The left lung was condensed throughout, and was of a bright red colour on section, and non-crepitant, except a small portion of the apex. The lower and posterior parts of the inferior lobe (of the right lung?) were in a similar condition, and the texture of both was friable." (p. 64.)

of Peyer's patches ends, the mesenteric glands begin to lessen in size, and the nutrition of the body returns to its physiological condition. Under ordinary circumstances the intestinal ulcers rapidly heal; but from errors in diet, or from constitutional conditions unknown to us, they occasionally continue to spread in the mucous membrane of the ileum, just as dysenteric ulcers will do in the colon. Eventually the patient dies with obstinate diarrhœa and emaciation, or, much more rarely, by perforation. It can be well understood that this class of cases may have been very numerous in the Crimea; according to our observation, there was very little malingering among the men; there was almost always a great desire to return to duty, and this led many to report themselves as stronger than they really were; on discharge, therefore, from hospital with ulcers only partly healed, the coarse food and the exposure soon produced an increase of the intestinal ulceration; often, too, ulceration attacked also the colonic mucous membrane, and the case would have been termed "Dysentery following typhoid fever." Cases of this sort ended sometimes three or four months after the original attacks, and the men were out and in hospital two or three times during this time.

In addition to dysentery following typhoid fever, many cases were seen in which typhoid fever had followed dysentery. We had not ourselves much opportunity of seeing this, but a very competent observer, now unfortunately dead, informed us that the association of rather old, healed, dysenteric ulcers, with recent typhoid fever, was too frequent to allow him to suppose the coincidence was accidental. Dysentery, it is true, prevailed in the army, and a certain number of dysenteric persons would necessarily be afterwards attacked with typhoid fever, but during the summer, autumn, and winter of 1855–56, the dysentery and the typhoid fever were not so common as to lead one to suppose they would very frequently be found in the same person. It is possible, then, either that the same persons had a constitutional tendency to both dysentery and typhoid fever, or that the dysentery predisposed to the last-named disease.

Dysentery also accompanied the febrile stage of typhoid fever more commonly than it does in England and France, so that the affection of the large intestine, before, during, and after typhoid fever, may be considered to have been decidedly more pronounced in the Crimea than we are accustomed to see it here.

General tuberculosis occasionally followed the Crimean, as it will do the English typhoid fever.

The so-called "Crimean fever" was simply the typhoid fever. There was no special and distinguishable Crimean fever; there were intermittent, remittent, and relapsing fevers (probably), and typhus and typhoid fevers, but there was no disease to which, scientifically, the term Crimean fever should be applied.

The mortality of the typhoid fever cannot be known, as the correct diagnosis was frequently not made, and as in the army returns there is only one general heading of Continued Fever. Not infrequently cases of typhoid fever appear to have been returned as diarrhœa. At page 3, Dr. Lyons gives a table of 16 cases, which were returned as being fatal from "diarrhœa." These cases were really—

Typhoid fever	6
Typhoid fever, with dysenteric ulceration . . .	3
Dysentery	3
Pneumonia	1
Peritonitis	1
General tuberculosis	1
General serous inflammation	1
	16

Among Dr. Lyons' 50 fatal cases (pleurisy, pericarditis, meningitis, peritonitis), we notice only 2 cases of perforation. We conceive there is no disease which the army medical officer should study more carefully than typhoid fever. Its frequently insidious course, its duration, and its sequelæ, render it a most difficult disease to treat, unless the diagnosis is made early. Then all becomes clear, and the patient has the full benefit of what we know respecting treatment. It is certainly surprising, considering the way in which the subject has been discussed of late years, to find how ignorant men still are of this most common disease. Out of the 16 cases just referred to, no less than 9 were of typhoid fever, and yet this grave fact was never suspected. Dr. Lyons refers to another case, in which a man was discharged from hospital, after "a short fever" of seven or eight days, and was then in a few days readmitted, and speedily died with extensive ulceration in the ileum. The fact simply being, that the decline of headache and fever—which in mild cases of typhoid often occurs at eighth or tenth days—must have been mistaken for full convalescence, and the unhappy soldier was therefore thrust out of hospital, and was compelled to perform his heavy duties during the height of a disease, which had been made dangerous and fatal by an unpardonable mistake. How many cases of typhoid fever do we see in civil practice, in which life is imperilled, and sometimes destroyed, by the indiscriminate use of purgatives, the disease receiving all sorts of names but the right one, and being treated in all kinds of ways but the proper one? Hæmorrhage or perforation is sometimes the first symptom which startles the practitioner out of his dream of a "bilious seizure," or a "slight bronchitis," or something of that kind. In Dr. Lyons' description of the morbid appearances of typhoid, we notice nothing unusual.

The typhoid fever, although most prevalent during the summer and autumn of 1855, was seen more or less till the complete evacuation of the Crimea. During the winter, however, cases of typhus became more frequent, although in the English army they never became very numerous.

In the French and Russian armies, however, soon after the capture of Sebastopol, typhus, which had existed all the summer, began to assume an epidemic character, and from this time till May, 1856, it ravaged these armies with a fury unknown since the great epidemics of the imperial wars.

The reason of its spread was almost always attributed by the French and Russians to two causes: viz., a general scorbutic condition of the men, and an immense amount of over-crowding both in the barracks and hospitals. The Russians, retiring from Sebastopol after the eighth of September, were concentrated in the valleys, where they suffered greatly from intermittents, and the hospitals became much crowded.

"As the result of this over-crowding" (said a Russian physician, M. Alferieff,* to the Constantinople Society), "the typhus appeared. At Simpheropol, at Odessa, and at Nicolaieff, there was also over-crowding, which was evidently the cause of typhus in these different cities. The unfavourable season may have had an influence, but in all cases over-crowding (encombrement) must be recognised, if not as the unique, yet as the essential and most active cause of the epidemic. The description of typhus, as given by Hildenbrand, corresponds perfectly with this disease. . . . . The eruption which, like the papules of measles, appeared generally on the thorax and abdomen, extended sometimes over the whole of the body, and even to the palms of the hands. . . . . The duration was seven or fourteen days, or longer. The most usual complication, when the disease lasted for any time, was pneumonia, such as is characterized by M. Piorry as hypostatic. But this pneumonia, sometimes lobular, sometimes lobar, was not always limited to the posterior and inferior parts of the organ; it extended sometimes to the summit, it had not any very pronounced symptom, there was scarcely any cough, a slight dyspnœa, the characteristic expectoration was wanting, and without auscultation it would have been most frequently undetected." (pp. 126, 127.)

His observations have "led M. Alferieff to reject the opinions of those who admit that the typhus and the typhoid fever are the same disease." (p. 127.)

Further particulars as to this Russian typhus were given by M. Moering, who was charged by the Russian Government to examine the chemistry and the microscopic anatomy of the blood and organs. M. Moering had made about two hundred dissections. The chemical results were negative; he found albumen in

* This gentleman, the Professor of Pathology at Kiev, and M. Moering, Professor of Hygiene at Kiev, were sent by the Russian Government at the beginning of the autumn to Odessa, and then to the Crimea, to inspect and report on the health of the troops. After the peace, they proceeded to Constantinople to survey the French hospitals.

the urine towards the end of the disease, but it does not appear that he examined the urine otherwise. The analysis of the blood led to no special result, except that the absence of ammonia was ascertained. The anatomical conditions were hyperæmia of all the organs and of the muscles, during the first week, and increased epithelial formation on all the mucous surfaces. The hyperæmia, thus general, "had no special seat of election." In the second week the membranes of the brain were more particularly affected; the arachnoid was opaque; the depending parts of the brain were softened; the posterior parts of the lungs were engorged, while the anterior were a little emphysematous. In the intestines there was increased mucus, and the follicles were even a little swollen. At the end of this period there was often parotitis. In the third and following weeks the lesions were very variable; the lung and the intestine, especially its inferior part, were the organs most affected.

"But the lesion of the intestine was not that of the typhoid fever; it was in fact only a softening of the mucous membrane. Twice only were ulcerations found in the small intestine; but these individuals laboured under tuberculosis, and these ulcerations were evidently due to this general disease, since they had none of the characters of the dothinenteric lesion described by M. Louis." (p. 130.)

The amount of mortality caused by this fever in the Russian army is not known, but we have reason to believe it to have been enormous. Although not mentioned by MM. Alferieff and Moering, it was attended by scurvy; and we have it from a competent witness, that at Odessa, at any rate, every scorbutic person attacked with typhus died. It was perhaps fortunate for the English army that the advance so ardently longed for was prevented by the unexpected peace; the Russians, no doubt, more than decimated by disease as they were, would not have resisted the attack of the English troops, who were in the highest state of vigour and endurance; but in the Russian positions the English would have met a foe more deadly and more tenacious. As it was, the repose of the winter, which strengthened the English, exhausted the Russians; and if the elements fought against us in the previous year, we gained an ally in the following winter, which silently sent more victims to their rest than the most deadly volleys of the red artillery.

Nearly at the same time that the Russians retreated, the French began to suffer. All the hard work during the autumn and winter of 1855–56 fell upon them. They were écheloned in vast masses along the marshy valleys of the Tchernaya, and suffered like the Russians from malarious fevers and from typhus, which spread rapidly in the regiments; their ambulances soon became overcrowded, and were emptied into Constantinople, where they had hospitals capable of holding 14,000 sick. On board the hospital ships the fever always increased; the Constantinople hospitals were soon overcrowded, and the terrible state of things with which we are all familiar, commenced. Despite the organization and the admirable foresight of the French, they were for a time overwhelmed; the fever spread from bed to bed, the intensity of the propagation being in proportion to the overcrowding (Jacquot); a considerable proportion of medical officers and 600 male attendants were attacked in two months; the Sisters of Charity and the priests largely suffered; the culinary and laundry arrangements could not meet the pressure, and finally (at the end of March) the transit of sick from the Crimea to Constantinople was stopped, and the army in the field was for the time compelled to provide for its own sick. The mortality at Constantinople, from November to April, has been variously estimated at from 15,000 to 40,000. The mortality of the fever itself at some of the hospitals was 35 per cent. In these secondary hospitals, in fact, the disease was more general and more fatal than in the Crimean ambulances.

With respect to the distinction between this typhus and the typhoid fever, a fierce discussion was carried on among the French doctors at the Imperial Society. On the one side, M. Cazalas, the principal medical officer of the Hôpital de l'Ecole, represented the indefinite school, and asserted that:

"1. Identical in reality, typhus and typhoid fever differed only in form.

"2. The typhus, properly so called, common in the Crimea and in certain hospitals at Constantinople, existed at the Hôpital de l'École only in isolated cases, which did not generally differ from the typhoid fever.

"3. The diseases which declared themselves among the convalescents at Constantinople were sometimes gastric fevers, the typhus or typhoid fevers; sometimes a cerebral congestion, a meningitis, an active or passive hydrocephalus; sometimes a remittent or intermittent affection.

"4. These diseases were all complicated with scurvy, and very often with chronic diarrhœa.

"5. These diverse affections seldom ran their course without complication of remittence or intermittence, and those which had the intermittent or remittent character had a great tendency to continuity.

"6. All these affections tended equally to take a typhic or typhoid character.

"7. The pathological states resulting from the *mélange* of all these elements were very complex. The scurvy is, perhaps, always the foundation; and the gastric, intermittent, and typhic elements enter most often into their composition.....

"8. These morbid states are for the most part neither typhus nor typhoid fevers; they are the complex accidents in which the typhus has only a secondary *rôle*, and are determined in cachectic or sickly men by the access of fever, or reactions too violent for the organs enfeebled by scurvy or by a miasmatic infection, either animal or vegetable.

"9. These states have only exceptionally a regular course and a constant symptom, stupor with delirium, like typhus; they offer in general the form and the marks of typhoid fever; and at the autopsy the lesion which characterizes this malady." (p. 19.)

We omit six other statements made by M. Cazalas, as the above quotation sufficiently expresses his opinion. We need only observe that he afterwards says, that "the lesion of the typhoid fever is wanting when the invasion has been sudden and the death rapid." (p. 20.)

It will be seen, on a critical examination, that two questions are raised by M. Cazalas, although they are not distinctly put. He alleges, first, that any special fever which could be called typhus was uncommon in his hospital, the cases in which were of a very complex nature, since the men had been acted upon by various causes, which profoundly influenced nutrition—viz., scurvy (which implies bad food), miasmatic influences, and fatigue. That cases do occur in which such profound lesions of nutrition give us forms of disease so complex as to render it difficult to refer them to any particular nosological heading, is certain; and it is a mere question of evidence how many patients in any given hospital are affected with such cachectic conditions, and how many with a disease to which a definite name can be given. At the other hospitals of Constantinople, the proportion stated by M. Cazalas was not found to be correct; and there prevailed, as a principal disease, a definite fever with a certain course, and which had characters so marked as to call for a separate and distinguishing name.

M. Cazalas's other proposition is, that this fever, when it did occur, was in reality the ordinary typhoid fever, only it did not present the definite intestinal lesion when its onset was abrupt and its fatal issue rapid.

On the opposite side to M. Cazalas we must place M. Jacquot, the chief of the French hospital at Pera. While this able physician fully recognises the possible coincident occurrence of typhus, of scurvy, of marsh fever, he insists much "on the necessity, to avoid confusion, of disengaging these morbid states from each other." (p. 29.) The great epidemic disease which ravaged their hospitals was not, he insists, a *mélange* of typhoid states superinduced on other diseases; it attacked sound individuals, it was highly contagious, it had definite symptoms; a special eruption, quite distinct from the typhoid rash, existed, and there was not the intestinal lesions of typhoid fever. By way of exclusion, M. Jacquot proves that it could be neither a meningitis, nor an encephalitis, nor a cerebral congestion, nor a marsh fever, nor a typhoid fever, nor a *mélange* of typhoid states occurring in diverse maladies. With respect especially to dothinenteritis, M. Jacquot asserts that the autopsies made by himself and MM. Hospel, Ganam, Valette, Barudel, Gauderax, Tholozan, and Lallemand, amounting to 160 in number, prove that "the dothinenteric lesion never exists in the typhus." (p. 149.)

M. Jacquot concludes finally:

"1. The reigning epidemic was the contagious typhus of armies.

"2. The typhus showed itself—*a*, solitary, as when it attacked the persons composing the hospital staff, or in the Crimea the healthy men; *b*, it attacked convalescents arrived at such a state of reparation that it pursued its ordinary course; *c*, it attacked persons cachectic, scorbutic, or already affected with other profound maladies; the affection was then complex; the typhus was profoundly modified in its symptoms and in its march; its distinctive characters were obscured; the anatomical lesions were numerous and diverse; the curative means were multiplied; the prognosis was graver, the diagnosis difficult.

"3. Typhus and typhoid fever are distinct maladies, and they can be distinguished with facility at the bedside by the symptoms, the course, and the commemorative circumstances. The absence of the intestinal lesion of the typhoid fever verifies the diagnosis on the dead body.

"4. The Constantinople typhus had no special fatal period, nor any determinate and invariable duration.

"In addition to the true typhus, solitary or complex, there existed accidental or typhic states grafted on other affections." (p. 149.)

We need not now prolong this discussion. Few of our readers, we fancy, but will find M. Jacquot's opinions more consonant with their views than those of M. Cazalas. Like M. Jacquot, we found no difficulty, in 1855–56, in at once diagnosing the typhus and typhoid fevers among the English. The distinction, which in this country is one of the most facile clinical problems, was not more difficult in Turkey. It is true, however, that our men did not present the scorbutic and cachectic conditions which among the French masked the new disease, and rendered typhus occasionally difficult of diagnosis. These underlying conditions constitute the great difficulty in the management of disease in times of war. To recognise, appreciate, and treat them, is no easy task for men who have been accustomed only to the simpler features of disease in times of peace. A full description of these complex states has never yet been given, but we know no greater service that could be rendered to military medicine than a comprehensive modern work on the 'Diseases of the Camp and Army,' if it were written with the honesty and vigour of Pringle, and with the quick intelligence and keen insight of Robert Jackson.

If the French were for the time oppressed, and even overwhelmed by this terrible epidemic, their admirable organization soon began to regain ground; during the months of April and May, the fever rapidly declined, and it is a good example of the excellent method of our allies, that the French troops were conveyed home without any great outbreaks of typhus on board the transports, and without the introduction of the disease into France. To prevent the chance of this latter event, every sanitary precaution was taken, both in embarking the men in the Crimea and at Constantinople, and in landing them at Marseilles, where large camps and hospitals were formed, in which the men were placed in quarantine. The medical history of the war shows indeed in a striking way the advance of sanitary science since the Imperial wars of the first Napoleon. At that time, each army carried with it the spotted fever, and the ravages of the sword were not the only curse it inflicted on the countries it marched across. In this war the most careful steps were taken to prevent such a catastrophe, and the highest intelligence of the nation was employed in devising means for ensuring to the French troops a safe transit, and to the French people a safe intercourse. A successful retreat is said to be the most honourable thing after a decisive victory, and the French surgeons certainly retreated from the terrible disease which assailed them in Turkey with a skilful adaptation of means which does them great honour.

But we must no longer desert Dr. Lyons. His chapter on Typhus Fever is a short one, and presents no new feature of interest, and no post-mortem examinations are recorded.

In addition to typhus and typhoid fevers, Dr. Lyons describes intermittent, remittent, and relapsing fevers, and what he calls "simple continued fever"—a disease, the description of which is too brief to enable us to pass any opinion on its nature, and of which we personally saw no examples.

The relapsing fever, as described by Dr. Lyons, occurred during the summer, and would seem to have presented the characters with which we are familiar here. The early symptoms were severe, and rapidly reached their acme, while on the fifth or sixth day there was sudden subsidence (crisis), with sweating. This apparent convalescence was followed by return of febrile symptoms after "two, three, four, or more days." No cases were fatal.

We had not ourselves any opportunity of accurately studying this form of disease, but we have been informed that it was seen in the Secondary Hospitals in the autumn and winter of 1855–6, and presented sometimes three or four relapses. It was probably this fever which was attended so frequently with jaundice, either during or after its course.

Passing from the consideration of fevers, we find that Dr. Lyons discusses at considerable length both cholera and dysentery. Six post-mortem examinations only are given of the former disease, and we observe no new facts which need detain us.

The chapter On Dysentery is written with very great care, and is illustrated with a table containing a summary of fifty post-mortem examinations of acute and chronic dysentery. The morbid anatomy of the Turkish and Crimean dysentery appears to be completely identical with that of the Indian disease. There was great exudation on and among the coats, sloughing ulceration, commencing sometimes in the follicles, at other times in the membrane, or even, Dr. Lyons thinks, in the exudative layer which had become organized. This last observation (of ulceration commencing, not in the mucous membrane, but in lymph thrown out upon it and organized) is new to us, and we should have been glad to have seen it illustrated by some minute dissections. The implication of the solitary glands is described by Dr. Lyons very exactly, although he does not appear to have traced their changes beyond the early period of commencing ulceration.

"That the vesicular glandular apparatus of the large intestine often participates immediately in the dysenteric process is not only very probable, but we think that such participation is more common than is usually supposed." (p. 47.)

The fact is, however, that this change in the solitary glands has been considered by some writers as the proper anatomical character of acute dysentery, and as a prior change to that intense hyperæmia and enormous exudation of lymph, which constitutes the better known anatomical characters of the disease. As far as they go, Dr. Lyons' observations support this view.

Coincident disease of the small intestine appears to have been more common in Turkey than in the Indian dysentery. The following table, drawn up from Dr. Lyons' summary, illustrates this:—

Headings in Dr. Lyons' Report.	No. of cases.	Ulceration or exudation in small as well as in large intestines.	Small intestine healthy, or more often with congestion, enlargement of glands, or atrophy of membrane.	No note of small intestines.
Acute dysentery	7	2	3	2
Chronic dysentery	9	1	2	6
Complex dysentery	34	11	14	9
	50	14	19	17

The affection of the small intestines, in the cases of "complex dysentery," consisted in several (four certainly, probably six) cases of ulceration of Peyer's patches, and in several other cases these glands were "infarcted." As in Indian dysentery these glands seem never specially attacked, and suffer only where there is general disease of the whole iliac mucous membrane, it must be admitted that Dr. Lyons is quite correct in referring these cases to a complication of dysentery and typhoid fever. But apart from, and making every allowance for, this fact, the disease of the small intestines would appear to have been more common than in India, probably from the presence of special constitutional cachectic conditions, as it is under such circumstances, and especially in scurvy, that the small intestines are engaged, and show, not usually ulceration, but diphtheritic exudation.

The secondary affections of dysentery are treated rather shortly, and we are not very clear how closely Dr. Lyons associates them with the primary disease. They consist of changes in the liver, spleen, kidneys, and especially the lungs (" vesicular bronchitis and lobular pneumonia ").

Returning from this very interesting section on Dysentery to the commencement of the Report, we may observe that Dr. Lyons devotes a few pages to "Diarrhœa," a disease which, according to the official returns, is often fatal. On post-mortem examination, however, all the so-called cases of diarrhœa turned out to be typhoid fever, dysentery, tuberculosis, or pneumonia! and Dr. Lyons has never been able to meet with a pure fatal case of diarrhœa. It is quite time, indeed, that this term should disappear from our tables of deaths. The diarrhœa which was so common among the troops, but which was never really fatal, is arranged by Dr. Lyons under three heads—

1. Atonic diarrhœa, or lientery.
2. Bilious diarrhœa.
3. Congestive diarrhœa.

The lientery is described at some length as a disease in which the food appeared to pass through the stomach and intestines with little alteration, as if there were an arrest of "the digestive, assimilative, and absorbent functions." There appears to have been little pain, and, as far as we can gather from Dr. Lyons' account, there was no pouring out into the intestines of bile, or the intestinal fluids which constitute the stools of common diarrhœa. We did not ourselves witness any form of this disease, but a grave affection somewhat corresponding to it, but often very fatal, was seen in the Chinese war of 1840–42, and after death the mucous membrane of the small and large intestines was, we believe, pale and softened. In Turkey it appears to have been extremely slight, and many medical officers denied its existence.

After considering at some length, in the first half of his Report, the various diseases we have now shortly noticed, Dr. Lyons gives us, in a second part, a *résumé* of the pathological anatomy of the various organs. This is an important chapter, and is very well done. We should, however, have been glad to have had a statement of the number of cases from which the general conclusions are drawn. A rather novel feature in it is that the specific gravities, not only of the organs but of membranes, are given. These observations were made by Dr. Aitken, and reflect great credit on him. As a general result, it would appear that exudation into the intestinal mucous membrane, both in typhoid fever and in dysentery, raised the specific gravity of the membrane; and even in atrophy of the membrane, the specific gravity was above the natural standard. The point is sufficiently interesting to induce us to lay before our readers a portion of the table:

Specific Gravity of the Mucous Membranes.

	Maximum.	Minimum.	Mean.
Small intestines:			
Peyer's patches	1·044	1·032	1·039
(*a*) Parts with glandular and exudative deposit	1·040	—	—
(*b*) Parts in atrophic state	1·038	1·030	1·035
Apparently healthy	1·036	1·030	1·032
Great intestines:			
(*a*) Colon with dysenteric exudation	1·050	1·037	1·043
(*b*) Rectum	1·044	1·038	1·041
Atrophic degeneration	1·040	1·037	1·038
Apparently healthy	1·038	1·028	1·033

We are not, however, told from how many examinations these figures were derived.

We have now given as much space as we can afford to Dr. Lyons' Report. Our remarks will show the favourable opinion we entertain of much of its contents. It is written extremely well, is clearly expressed, and is in many parts very descriptive. The introduction, which contains a general statement of Dr. Lyons' opinions on the pathological origin, progress, and treatment of the diseases of the Eastern force, is a very important document, and may probably come again under review on a future occasion.

Yet, with all these undoubted merits, we most frankly say that the Report has in some degree disappointed us. The absolute number of post-mortem examinations is not great, and they are communicated very briefly, and in many cases very imperfectly. More might surely have been done in this direction; and, considering that this Report will represent on the Continent the opinions of the most advanced British School of Pathology, no consideration should have prevented the fullest detail of all the morbid appearances. The chemistry of the fluids has been left untouched, and the microscopical notes are short and unsatisfactory. We have looked in vain for even a microscopical examination of the blood; and the absence of these inquiries is not compensated by any researches carried on at the bedside of the patients, for the histories of the diseases are even more meagre than the accounts of the post-mortem examinations.

Much, no doubt, has been done by Dr. Lyons and his assistants, and great were evidently the difficulties they had to encounter. Taking into account the real and great merits of the Report, and the difficulties the Reporters laboured under, some may think our remarks too severe. We can only assure Drs. Lyons, Aitken, and Doyle, that we should have had much greater pleasure could we have closed our review without any qualification of the favourable opinion we have generally expressed. We can assure them, too, that their Report is a gain to science, and will always be a document of interest and authority for those who study the medical history of the Crimean campaign.

Review IV.

On the Constitutional Treatment of Female Diseases. By Edward Rigby, M.D., Fellow of the Royal College of Physicians, Senior Physician to the General Lying-in Hospital, Examiner in Midwifery at the University of London.—*London.* 1857. pp. 324.

The appearance of a new work from the pen of so eminent a physician-accoucheur as Dr. Rigby, cannot fail to interest the profession; for, notwithstanding the abundance of recent publications on obstetric medicine, there is still room for practical information on a subject which is daily gaining in extent and importance.

Dr. Rigby describes his book as strictly practical, and prepares the reader in his preface for the due appreciation of constitutional treatment in diseases of the female generative organs.

The experience of every observing physician demonstrates the fallacy of attributing much curative power to merely local means of treatment, and proves the utility of embracing large views of general or constitutional management. Abernethy displayed this in vivid colours to the surgeon; and the physician will also reap an ample reward in the cure of disease, if he devote himself earnestly and perseveringly to the discovery of those constitutional defects and disorders which, hidden it may be from the sight of superficial observers, so constantly lie at the root of most of those phenomena the local manifestations of which are too often looked upon as the essence of the complaint, and the sole objects of treatment.

We are glad to find that Dr. Rigby has publicly enlisted himself amongst the number of those physicians who place their chief reliance, in the treatment of the diseases peculiar to the female, upon constitutional measures. The first three chapters of the work before us comprise the subjects of amenorrhœa, dysmenorrhœa, and menorrhagia; and although we cannot say they contain much that is new either in description or mode of treatment, they well deserve attentive consideration. With regard to amenorrhœa, like other functional derangements of the uterine system, it is, in fact, a *symptom*, an effect of general derangement; and, as Dr. Rigby justly observes, it behoves the practitioner to look beyond the mere local affection, and carefully investigate the abnormal or defective actions of the system upon which it essentially depends. It is undoubtedly a great mistake to imagine that the absence of this natural secretion is the *cause* of the numerous ills that accompany its retardation or suppression, although such is a very prevalent and convenient doctrine. Nothing is easier than to convince an anxious mother that her daughter's deteriorated health and sickly appearance depend upon this irregularity; it is, indeed, so natural to her to think so, that the practitioner needs no other explanation to satisfy her anxious inquiries. Does not this facility of accounting for diseased appearances often lead to an equally superficial method of treatment? And does not the young sufferer sometimes fall a sacrifice to the inroads of constitutional and organic diseases, whilst attempts are vainly being made to excite the appearance or reappearance of the menstrual discharge; its absence simply depending upon the demands made by other diseases upon the system, rendering it unable to establish or continue those functions of the uterus which otherwise ought to exist? In vain shall we under such circumstances administer savine, borax, cantharides, and other special emmenagogues, as they are called; until the constitutional powers are restored, no good effect will be produced; and thus we find the best emmenagogues are fresh air and exercise, and such medicines as are most calculated to regulate the bowels and improve the general health.

Dr. Rigby says:—

"The two most valuable emmenagogues which we possess, and which exert a special action on the uterus, are the preparations of iodine, and the secale cornutum. The iodide of iron is, perhaps, the best form for administering iodine to obtain its emmenagogue effects, and may be given in the form of pill or syrup two or three times daily. The secale cornutum is best given in the fresh powder, suspended in water with a little mucilage."

Dysmenorrhœa is referred to the following separate heads:

1. It may be connected with derangement of the digestive organs.
2. It occurs in a gouty or rheumatic habit of body.
3. Or it may be of an hysterical or neuralgic character.
4. It occurs in connexion with some inflammatory action of the uterus, usually the os and cervix; and,
5. It arises from ovarian irritation.

The presence of one or other of these causes must of course influence the treatment in each particular case, and general principles must be our guide. The symptoms attending that particular form of dysmenorrhœa in which the ovary is the chief seat of pain, are well described in the following extract:

"The organ becomes highly congested or actually inflamed; it swells considerably, and becomes intensely sensitive. The pain is of the most agonizing character, and is frequently attended with severe nausea, or obstinate and most distressing vomiting. The patient describes it as being different to any other pain she ever experienced, and dreads a return of the attack; its peculiarly unbearable, sickening character apparently resembling the sufferings from orchitis, or from any injury to the testicle in the male. This is decidedly the severest form of dysmenorrhœa, and, moreover, is remarkable for another peculiarity—viz., the formation of fibrinous exudations from the uterus."

The author further remarks:

"That a slight amount of ovarian irritation, although it will be accompanied by ovarian pain at the menstrual period, may not be of sufficient duration to produce the uterine exudations; hence, although we may have ovarian pain without exudations, we cannot have exudations without ovarian pain."

The chapter On Menorrhagia occupies thirty-two pages of well-written, practical matter, but we search in vain for anything that is not already familiar to the experienced accoucheur. The various causes of menorrhagia are usefully commented upon; and in cases where the hæmorrhage is very profuse, we are glad to find the means we have on several occasions practised and recommended, approved also by so good an authority as Dr. Rigby—viz., plugging the vagina with a sponge dipped in vinegar or alum-water, and throwing up a large enema of cold water. This, he says,

"Acts beneficially in many ways. By thus applying cold immediately along the posterior wall of the uterus, we not only produce a considerable check upon the activity of its circulation, but stimulate the organ to a firmer state of contraction, which will exert a powerful control on the profuseness of the discharge; and though last, not least, it will effectually clear the rectum of any fæcal accumulations which may have existed, and necessarily tend to aggravate and keep up the discharge."

With respect to the concluding remark about fæcal accumulations, we would strongly impress upon all students and practitioners who may be called upon to deal with functional or even organic diseases of the uterus, the absolute necessity of removing daily the solid contents of the large intestines. Of the *functional* disorders, fæcal accumulations are perhaps the most fruitful source, and they never fail to aggravate those which depend upon organic change; the enormous quantity of solid material which lies packed up in the colon in some of these cases is almost incredible, and the removal of it often alone effects a cure. This, however, is not to be accomplished by means of strong purgatives, but by the daily exhibition of some mild aperient for a length of time. Dr. Rigby recommends sulphate of iron and sulphate of magnesia, but we have found nothing answer better than a dinner-pill composed of one or two grains of watery extract of aloes, and the same quantity of extract of rhubarb; or this failing, a combination of compound colocynth pill and extract of henbane, followed, if necessary, by an occasional enema of warm water or gruel.

As an astringent in *leucorrhœa*, Dr. Rigby highly extols the infusion of red bark (*Cinchona oblongifolia*) with nitro-muriatic or sulphuric acid.

"I know of no astringent tonic so powerful as the recent infusion of this species of bark; and if the liver have been previously well roused to active secretion, and the bowels effectually cleared, a rapid improvement, not only of her general health, but also as regards the diminished leucorrhœa, will soon be evident."

On the subjects of inflammation and ulceration of the os uteri, the constitutional origin of these affections, in a majority of instances, is strongly, and we think justly, insisted upon; and ulceration unconnected with malignant disease of the uterus is stated to be a rare affection.

"Its presence can doubtless produce much irritation and corresponding local symptoms; but to assert that it is a cause of general derangement in the system, and to propound the postulate (for I can call it nothing else) that it is a most frequent primary cause of impaired health in women, argues either a singular ignorance of the fundamental laws of pathology, or great indifference to truth in the attempt to propagate and maintain certain doctrines in justification of an improper and dishonest mode of treatment."

Our own experience is strictly in accordance with the statement that many appearances denominated ulcerations were simply abrasions or excoriations, and that such cases can be speedily and effectually cured by general restoratives and such simple local means as will ensure entire cleanliness.

In reference to *displacements* of the uterus, a good description is given of one of the most troublesome we meet with; and it would be a great boon to those who suffer from it, and to the profession, to discover a more effectual and painless mode of cure than the introduction of an instrument into the cavity of the uterus.

"In examining a case of the retroversion of the unimpregnated uterus during life, the finger can frequently reach a firm, globular mass, like a walnut, situated behind the cervix uteri, and evidently posterior to the vagina. At the first touch, or to one unacquainted with this condition of the womb, it seems like a lump of scybalous matter in the rectum, for in many—perhaps in most instances—the finger cannot reach sufficiently high up to distinguish the continuity of this mass with the cervix, the point of flexion being usually in the body of the uterus, close above its junction with the cervix. In other cases, where the fundus is low down, being either on a level with, or even lower than the os uteri, the curve in the posterior wall can easily be felt and traced by the finger from the cervix to the fundus. On examining per rectum, we feel the same hard lump through the anterior wall of the intestine; and by being able to reach higher up in this direction than with the finger per vaginam, we can frequently verify or correct our first impression."

In speaking of *uterine polypi*, they are said to

"Vary as to their size, structure, and the part of the organ from which they grow. The large polypi, which are usually of a fibrous tissue, arise from the sides or fundus of the uterus, while the soft polypi, which consist chiefly of fibro-cellular tissue, more or less condensed, and covered with mucous membrane, arise from the os and cervix. . . . . .

"These larger polypi are usually solitary, but the smaller ones, which have been commonly called *mucous polypi*, and which have their attachment to the edge of the os uteri, or just within the canal of the cervix, generally occur two or more together, or sometimes like a fringe around the greater part of the os uteri." (pp. 163–4.)

In corroboration of the opinion of the late talented Dr. Gooch, that it is advisable to remove portions of malignant disease when it assumes a shape fitted for the application of a ligature, we quote the following paragraph, being strongly convinced that such a course is often extremely desirable, and, owing to the uncertainty of diagnosis, sometimes entirely successful:

"A polypoid mass of malignant growth is generally considered as unfitted for the ligature; but having treated many cases of malignant disease in this manner with good effect, I can affirm that it is not only safe, but capable of producing great relief to the patient. I deny that this relief being necessarily only of a temporary character, is a contra-indication to the use of the ligature; and in many inveterate cases of fungoid uterine disease I have had reason to be thankful that such a means was in my power for lessening the constant discharge, the frequent hæmorrhages, and severe sufferings of the patient (although I was well aware that it was but for a while), and that she has thus gained a few weeks more of ease and comparatively improved health. Hence, I have never hesitated, in every case of malignant uterine disease, to apply a ligature, if the shape of the tumour rendered it possible."

The following observations on the teatment of *cancer* of the uterus are particularly deserving of the attention of practitioners:

"I find considerable difficulty in stating what ought to be the treatment in its early stage, or stage of induration, not only on account of its incurable character, but also because its commencement is so insidious, that we can rarely have an opportunity of investigating the case until extensive mischief already exists. Even when examined at an early period, the practitioner, it is true, easily recognises the solid feel and alteration of the part, and the darting pains and cachectic looks of the patient probably confirm his suspicions; but he dreads to decide the point at this early stage, and hopes on, naturally wishing to give the patient the benefit of every doubt. I have felt the cervix uteri in other and apparently similar cases, as hard, or even more so, and probably more tender; and yet when I had removed the source of irritation, or allayed the inflammatory action of the part, it has assumed its natural characters. I cannot but think that this would be the reasoning of most practitioners in examining a suspicious os and cervix uteri at this stage. In those cases where the suspicious part consists merely of a little isolated tubercle, not bigger than a small pea, and which is probably an indurated muciparous gland, but which is becoming tender and irritable, and the patient complains of lancinating pains, which she distinctly refers to this point, we can successfully obliterate it by holding a piece of lunar caustic against it for about a minute; the darting pains cease henceforth, and on examination two or three weeks afterwards, the tubercle will have nearly, if not entirely, disappeared. With this exception, I have no hesitation in declaring, that the application of caustic in either stage of the disease is mischievous; if applied during the first, or stage of induration, ulceration is liable to be brought on, where, but for this cause, the disease might have continued in abeyance even for years. In the second stage, viz., of ulceration, I have repeatedly seen the process greatly accelerated by caustic; the sore quickly assumes a corroding character, and spreads with a destructive rapidity which soon exhausts the patient." (pp. 214, 215.)

The diagnosis between cancerous ulceration and the corroding ulcer of the os uteri is well described:

"1. The patient does not suffer the acute darting pains which are commonly so remarkable a character in cancer of the uterus.

"2. Nor does the touch of the finger produce the severe pain which is frequently the case in cancer, but a sensation of soreness.

"3. There is no induration of the surrounding parts, as in cancer; on the contrary, they are soft and natural to the touch.

"4. The uterus is quite moveable.

"5. The disease, commonly, does not extend beyond the uterus." (pp. 242–3.)

And in the present day, when the application of caustic to almost every variety of uterine ulceration is so fashionable, we cannot but think the following remarks especially appropriate:—

"The more I consider the causes and character of phagedenic ulceration in other parts, the more do I feel convinced that it is in general, and not local, treatment that we must chiefly put our trust. It must be by the appropriate use of alterative, laxative, and tonic medicines that we hope to produce such salutary changes in the unhealthy circulation as shall be incompatible with the morbid action which has been set up in a part; and that so far from irritating the slumbering mischief into rapidly destructive action by the senseless use of escharotics locally applied, our endeavour should be to retard its progress, as far as possible, by such applications as shall soothe its irritability, and, at any rate, keep it in a dormant state. By far the worst cases which have come under my notice have been those where caustic has been applied. The ulcer, till then, had been advancing slowly; but, immediately after its application, it seemed to assume a new character. It had spread as much in two or three days as it had done before in a month. The hæmorrhages became more frequent and profuse, and soon exhausted the patient." (pp. 243–4.)

The three concluding chapters, On Ovarian Affections, contain much useful information, and well deserve a careful perusal; but we cannot help expressing regret that Dr. Rigby has given us so little of his own personal experience, and quoted so largely from other authors. That which he has published is instructive and good, and so far not to be found fault with; but there is not much that we can fairly call original; and this, considering the high position and large practice of the writer, disappoints the expectations we had formed on first seeing a new work from such experienced hands. Before concluding our remarks, we feel it right to notice two circumstances with respect to which we differ; they are not, however, stated on Dr. Rigby's authority, although he appears to sanction them. The first is the statement that the multilocular ovarian tumour must be placed as colloid disease by the side of goitre (p. 285), and that there is a close relation existing between it and bronchocele. That it is connected with the strumous constitution we have no shadow of a doubt; but its analogy with bronchocele is surely disproved by the almost certain curability of the latter, and the constant fatality of the former. The second is the statement quoted from Mr. Safford Lee, that

"Whatever may be the appearance of the complicated structure of ovarian tumours, we never have them producing the effects of malignant disease; nor can they be recognised by their symptoms. There is not a case on record where the colloid-looking portions of the cyst have spread to, or communicated disease to, neighbouring tissues." (p. 291.)

We are acquainted with a case in which the abdomen was occupied by a very large cyst, connected with the right ovary by a thick fleshy pedicle, and adherent over its whole anterior and upper surface to the peritoneum, particularly at and around the umbilicus. The intestines were pressed into a very small space on the left side, in the situation of the spleen. The cyst contained almost a pailful of dark chocolate-coloured fluid, which in the lower part of the sac was thicker, of the consistence of paste, or even putty, and adhered in masses to the lining of the sac. The walls of the sac were of various degrees of thickness; thinner anteriorly, but posteriorly to about the size and shape of a placenta, very thick, and covered with glistening fibres like the expansion of tendinous structure. These thicker portions, when cut into, were found to contain numerous smaller cysts, in various degrees of development, containing gelatinous fluid. Behind the sac on the right side, adherent to it, but not forming an integral part of it, was a mass of cerebriform tumour, soft, and easily broken up with the finger, situated in front of the right kidney. The subject of this disease was a young woman, twenty-two years of age. Her complaint began with pain in the right side, about a year before her death, quickly followed by dropsy. Her body was greatly emaciated. Before death the abdomen was very much distended with fluid; fluctuation was everywhere remarkably distinct. On the right side a hardness could be felt through the parietes of the abdomen. Her aspect was unhealthy, similar to what is observed in malignant disease, and she died in the unusually short period of a year from the commencement of her attack.

Although we have expressed a desire for more originality, we are quite satisfied of the practical utility of the work before us; we strongly recommend it to those who are studying the diseases of which it treats, and shall look forward with much interest to a second edition, in which we hope to be supplied with a greater amount of the personal practical experience of one so qualified in all respects to communicate it, on the subject of ovarian disease.

Review V.

1. *Report from the Select Committee on Adulteration of Food, Drinks, and Drugs.* Ordered by the House of Commons to be Printed, July 22d, 1856.

2. *Paddington Sanitary Report for the year* 1856. By J. B. Sanderson, M.D., Medical Officer of Health. 1857.

3. *Adulteration Detected; or Plain Instructions for the Discovery of Frauds in Food and Medicine.* By A. H. Hassall, M.D., &c.—*London*, 1857.

4. *Dictionnaire des Altérations et Falsifications des Substances Alimentaires, Médicamenteuses et Commerciales, avec l'Indication des Moyens de les Reconnaître.* Par M. A. Chevallier, Pharmacien-Chimiste, Membre de la Légion d'Honneur, &c. Deuxième Edition.—*Paris*, 1854.

Dictionary of Adulterations of Alimentary, Medicinal, and Commercial Substances, with Directions for their Discovery. By M. A. Chevallier.

Whoever adopts Mr. Froude's recommendation to study English history in the Statutes at Large,* will find, first, that John Bull has from time immemorial been in the habit of quarrelling with his bread and butter; and secondly, that his complaints have always attracted the attention of the Legislature. An Act passed in the fifty-first year of Henry III., professes in its preamble merely to confirm previous laws, when it condemns fraudulent bakers to the pillory, adulterating vintners and brewers to the cart's tail, and to a like penalty forestallers and butchers selling meat unfit for food, or which has been killed by Jews. That the Parliament had a precedent for the punishment is confirmed by Domesday Book, by which it appears that from the time of Edward the Confessor any one convicted of making unwholesome beer was at Chester enthroned on a tumbril of dung, or heavily fined.† Statutes of this nature accumulate as Parliaments are more frequent; and it gives us a right notion of the business-like habits of our ancestors, to see that no political strife, no national revolution, turned their attention aside from the importance of social and material comfort to the kingdom. The turbulent peers and

* On the Best Method of Teaching English History: Oxford Essays. 1855.

† "Malam cerevisiam faciens aut in cathedrâ ponebatur stercoris, aut IV solidos debat præpositis."—Quoted in Selden, Titles of Honour, ii. 5, 8.

burgesses who in Richard II.'s reign were passing contradictory acts of attainder against each other, and were foolish enough to vote that any one should be held a traitor who proposed their repeal, could yet find time to make sensible laws concerning the cleansing of malt, to superintend the exportation of worsted, the breeding of salmon, to alter former acts oppressive to the weaver, &c. The single Parliament of Richard III.'s anxious reign seems to have been more interested with the prevention of "devil's dust" in cloth, or short measures in oil and wine, and the protection of our infant manufactures, than with the succession to the throne. An invasion, Bosworth Field, and the introduction of a new dynasty of kings, intervene during the recess; yet, when the members meet again after a few months, many with bloody hands, many in deep mourning, we find them passing a sensible navigation act, gauging wines, defining the duties of tanners, and persecuting poachers. From this time the statute book is choked with the multitude of long-repealed acts to restrain imposture in the prepared food and manufactures, which were daily growing in importance. They are duly recorded by Burn and Hawkins, and remain to attest the fact, that attention to the physical comforts of the people has always been viewed by the British Legislature as a duty admitting of no evasion.

To match fossil evidences of the care of our forefathers for these things, there are still in force several most stringent acts, designed by our own generation to secure the purity of food and drink. We have laws punishing severely the adulteration of bread, flour, milk, meat, tea, coffee, tobacco, sugar, wine, beer, spirits, and other excisable articles. The Report of the Committee of the House of Commons, which heads this paper, cites especially the "Alehouse Act" (the 9th of George IV.), as applying to beer, porter, cider, and perry; and the "Bread Acts" (the 3d and 4th of George IV., and the 6th and 7th of William IV.), for farinaceous foods. These give power of search on a magistrate's warrant upon information for that purpose, and allow of summary conviction, with considerable penalties, cumulating on repeated offences. As respects general adulterations of other articles, any public body—such as a vestry, or a hospital, or a gaol, or a barrack—has its remedy in indictment under the Statute of Frauds; and individuals may and do proceed by action against the adulterating dealer for obtaining money under false pretences.*

Of late an opinion has begun to prevail that the Imperial Government does not do all that it ought to guard its countrymen from imposition; and that it is its duty to exert more force to stop an evil which presents to the public a very alarming aspect. Before they act, Parliament has been at some trouble to inquire by committee into the circumstances, and the Report here quoted is the result. But at the very threshold of the question, the Committee have to confess themselves puzzled by the very conflicting evidence as to the extent of the evil: some highly respectable and experienced witnesses pronounce that the amount of adulteration is really insignificant; others, equally respectable, that it is universal: some say that a great part of our daily food is rendered poisonous by the alum which all bakers put in it; others say that the alum does not remain there as alum, but is decomposed, and if it did remain, would be innocuous; some that no large brewer adulterates, and another that the beer he got from a public-house produced very curious effects; and so on.

Whence this discrepancy? We think it may be explained as follows. The articles last named in the list of the dietary protected by the law come under the cognizance of the collectors of Customs or Excise, and the exceptional power of visit possessed by these authorities enables them easily to obtain evidence sufficient for the conviction of fraudulent adulterators; and, in fact, it appears that the large manufacturers are prevented as a rule from tampering with any goods of which the component parts contribute to the revenue. But further the gauger's hands do not stretch. It is not his duty or interest to interfere often with the retailer. So long as Sir John Grains and Co. have made so much pure gin and

* Mr. Goodman says he has successfully carried through the Central Criminal Court an action of this sort for sanding sugar. Report, &c., Answer 3931.

paid tax thereon, it matters little to the Chancellor of the Exchequer that over Jack Smith's counter it is sold mixed with water and cardamoms. The consumer swallows in the end more of the diluted luxury, and in this particular case lives all the longer to pay his taxes; while, on the other hand, the cost of the prosecution of petty offenders would be very great. Hence, a witness acquainted principally with a wholesale trade may easily be sceptical about the existence of any adulteration; while another, who has purchased from retailers of all grades, may, among the enormous number of small struggling shopkeepers, find an alarming list of knaves.

Balancing, however, one evidence against the other, it is impossible to avoid the conclusion that a large number of the articles in daily use for food, drink, and clothing, are sold to the consumer in a state far removed from purity. And this, too, is generally the result of design, and is not declared by the name under which the article is sold. But a question may fairly arise whether it is right to call this deed always by the hard name of *Adulteration*—a word derived from the most degrading and anti-social sin denounced in the Decalogue; and which always rouses the public to indignation, and the perpetrator to denial. Is it not in many cases a *bonâ fide dilution?*—in many cases an improved mode of manufacture?—in many careless or stingy methods of perparation?—in many more a harmless catering to the whims of customers? Yet all these get classed together in the minds of the public along with the introduction of substances decidedly injurious or nauseous. Many a publican would hesitate to put strychnine or cocculus indicus in his beer, who yet daily waters his tap. And if a man sells milk cheap, he may illogically argue he has a right to dilute it. Such various opinions are expressed about the use of alum in bread, that we are bold enough to confess we think it an improvement to at least half the flour used in England. Those who bake at home are constantly condemned to eat a bad batch of bread because its use is forbidden in their kitchen, and without the employment of it a great deal of the wheat now turned into human food would have to be given to animals. By the use of alum a light friable bread can be made from inferior flour, which without it forms a tough, indigestible substance, like the French country bread so dreaded by the dyspeptic. Careless or stingy modes of preparation scarcely amount to a sin in a competitive country like England; they lower the price of the article, though they supply it nasty, and the customer soon learns to suspect it is not worth more than is asked for it; if in the habit of buying cheap those eatables where carelessness really introduces a poison, as in the case of pickles, he rapidly acquires a simple test of its presence. As to the whims and prejudices of the public, woe to the legislation which does not respect them. "Surely we may have our cheese coloured with annatto, our sugar-plums with the contents of our colour-box, our simnels with saffron, if it pleases our eye." Before any new principle of legislation for the evil can be generally applied, it must be carefully defined where adulteration begins and ends; the four cases above instanced must be eliminated from it; and it must be clearly laid down whether the individual impurities (such as those cited above—water, alum, dirt, and paint) are to be classed under them, or are to be made a heavier fault.

There must exist some means of preventing that which is intended as a check upon decided fraud, being made an oppression on individuals or a chain upon trade. How is this now done in the laws already existing on the subject? By simply adhering to the principle expressed in the legal maxim, "*caveat emptor.*" For example, by the statute of Richard II., the mayor or bailiffs are to proceed against the dealers in sophisticated malt on the information of the consumers; and such is the intention of most subsequent legislation on similar points. The good sense of our countrymen is left to determine whether each individual case amounts to a fraud or not, and the plan is on the whole successful. The ninety-nine bakers out of a hundred who use alum, or put a small quantity of potatoes or rice in their loaves without marking them with an M., know they are liable to punishment; but they also feel confident that no customer would be foolish enough to bring the case forward unless the quantity was so great as to be a real injury to his stomach or pocket.

The Committee, then, seem to act discreetly when they recommend an adherence to what has been found suited to the English character, and quote the Bread Act as comprising "much that may be useful in framing a measure applicable to adulterations generally." By this Act, information must be laid before a magistrate, who grants a search-warrant at his discretion, imposes certain penalties for the proved offence, and, if he pleases, makes it public by advertisement. An appeal lies to Quarter Sessions, and no person can be convicted unless complaint is made within a reasonable period. It is probably impossible to go farther than this without the generalization of measures repugnant to English feeling, and only tolerated in the exceptional cases of the excise and customs from the obvious necessity for raising money by some means.

But several of the Committee's witnesses, whose opinions we are bound to respect, advocate the introduction of a repressive force much stronger, much more imposing, and more likely to exhibit striking results—but at the same time completely novel to the English executive: at least, as a general measure. "*Caveat venditor*," Dr. Hassall says, in one of his answers, should be in future our commercial maxim; that the tradesman should not be allowed to sell, even if the customer were willing to buy, and that encouragement should be given by Government to articles now avoided on account of their expense—such as crystallized instead of coarse sugars. Dr. Taylor suggests that there should be established authorities, with a power to search all shops, to take samples for analysis, seize what they consider noxious, and impose fines. Dr. Carpenter proposes "detective inspectors" of food and drink. Mr. P. Mackenzie, editor of the 'Glasgow Reformer's Gazette,' would place a wider power of visit in the hands of the excise. Mr. Postgate would spread through the kingdom well-educated "custodiers of the public health," with fixed salaries of about 500*l.* a year, who should employ detectives to purchase articles for them to submit to analysis. If these were found adulterated, then information should be laid before a magistrate by a public prosecutor, and that summary conviction should follow; that all tradesmen should be licensed, and a renewal of licenses refused after repeated offences; that all persons accessory to "pernicious" adulteration should be amenable to the criminal law; and that the faulty nomenclature of an article should be an offence. He would have these custodiers in sufficient numbers to make any analysis which a customer should lay before them, not only of food, and drink, and drugs, but of linen, wool, silks, &c., and would place them under a central bureau like the French Conseil de Salubrité. Mr. Wakley suggests somewhat similar measures to be carried out through the Board of Health, its local medical officers, and a corps of visiting purchasers appointed by them. He would not impose heavy fines, but would advertise in periodicals and placard on church doors and other public places the names of tradesmen who had been searched, so that all might know not only which was a bad shop, but which was a good one. He estimates the expense of such a machinery at about 10,000*l.* a year for London, not, indeed, expecting for that sum to inspect all the retail shops, but trusting to effect much by the terror inspired. Dr. Waller Lewis and Mr. Calvert also praise and recommend for imitation in this country the spy system pursued in France. Mr. Wallington advises that tradesmen should be forbidden by a Treasury order to deal in certain materials of adulteration; that they should be liable to have interrogatories filed against them, which they should be compelled to answer on oath, admitting or denying their guilty possession of these goods; that their denial should be followed by "inspection," their admission or proof of perjury by an "injunction," disregard of which should be a contempt of court, and involve imprisonment. It is needless to say that Mr. Wallington is a solicitor.

It is evident that there is here proposed a *general* employment of a machinery hitherto only *exceptional*, and the confessedly exceptional and unpopular nature of which has caused it to be less and less brought into play as legislation grows more perfect. A detective police has always been a hateful thing from the earliest ages, and never more so than when it has dealt with articles of diet. The Greek for an inspector of food (συκοφάντης) was used by the shrewd Athenians as a term of reproach for the worst of fair-seeming sinners, and means in Aristophanes something more odious than our translation "sycophant." Even in the strongest continental government, the long establishment of a detective force, or the necessarily high education of a censor of the press, cannot prevent the executives of these offices being the objects of avoidance and scorn. The same feeling would grow up with tenfold strength in such a country as England. Nor would matters be mended by the simple expedient advised by Mr. Postgate, of calling them "custodiers," or by any newly-invented nomenclature. The Puritans substituted the word "trier" for "inquisitor," without gaining popularity for their measures of investigation, and all similar evasions are soon found out. The worst of this would be, that a sufficient number of respectable persons could not be found to execute the office, as soon as it was found that "custodier" became as much a term of scorn as "*sycophant*," "*delator*," "informer," "trier," "inquisitor," "gauger," or "inspector." Certainly, no medical men, with whom the love and respect of their fellow-countrymen is the chief reward, could be induced to place themselves in so opprobrious a position as the employers of these means would occupy.

But even if a respectable body of officers could be got together sufficient to carry on the design of a detective repression of the evil, a doubt may be fairly expressed whether it would be so effectual as the present plan. If *caveat emptor* ceases to be the maxim, the buyer will learn to shift his responsibility on to the detective, and the occasional analysis of an expert will take the place of the constant supervision of customers. The latter are made as acute by interest as the former by science. An artisan may not be able to tell that his butter contains "Black Jacks" and "Bosh," or the exact percentage of its excess of water, but he soon finds out when it is nasty; he may be ignorant of the amount of solid material in his milk, but he knows when it is watery, and if so, makes a rough calculation whether it is worth while to give a higher price for a better article or not. Even those whose special vocation is chemistry and scientific research acknowledge that the palate is a more sensitive and readier detective of imperfection in food, than the manipulations of the laboratory or the microscope. In a lecture on baking at the Polytechnic Institution, on the 31st of March last, Mr. Pepper said that the taste was a much better way of knowing good from bad bread, the presence of alum or the excess of water, than chemical or microscopic examination. The police of common sense, stimulated by interest, is preferable to that of educated detection.

Then again, the most frequent proceedings would of course be against the most numerous—the small shopkeepers. The sufferers would soon get up a cry that it was an attempt of the capitalist retailers to put down the "poor man's shop;" and it is not impossible that sometimes the law might in this way be really made an instrument of oppression.

To carry out such a system partially, as, for instance, in the metropolis, would be suicidal to its ultimate success. It would be purifying the drawing-room at the expense of the passages and the kitchen, and would be merely shifting the evil. This is what actually happens in France. As regards bread, for example, Mr. Blyth tells the Committee how well the Parisian loaf agreed with him. And it *is* good, though with some dyspeptics we have found London bread agree better. But how is this goodness brought about? Simply because the strictness of the police prevents the bakers using any alum or mixed flours, and consequently the very best wheat alone can be employed. Hence the inferior article is thrown upon the provinces, and there the bread is almost universally bad; not adulterated perhaps, but nasty. Throughout Gascony it is musty, tough, and innutritious; and at a town no less important than Caen, we once found a French fellow-traveller searching all the shops in the hope of obtaining some eatable bread for an invalid son. He was unsuccessful, and we were obliged to share with him the London biscuits which we always carry when travelling in France. Such a thing would not have happened in the most neglected English village. But even in

Paris the strictest police supervision does not secure in some articles so good a quality as the vigilance of customers with us. Milk, for example, in the French metropolis, is found by M. Becquerel to contain of water from 84·9 to 97·2 per 100, a variation of 12·3; while in 32 specimens analysed by Dr. Sanderson for the Paddington Report, the range is only 9·7, namely, between 86·8 and 96·5. None is quite so good as the Parisian, but none is quite so bad—an evident proof that the French article is the most artificially diluted. And it must not be supposed that even in continental governments a detective police always finds it easy to punish acknowledged fraud; for instance, the tribunal of Sarreguemines decided, a few years ago, that selling rancid butter enveloped in a thin layer of fresh, could not be brought under the wording of the law. It is true that the "Procureur" got the decision reversed on appeal, but he had some trouble to do so even in France.* And at Bordeaux, Besançon, and Brussels, the police have had to complain that their exertions in punishing the sale of poisonous bonbons were not seconded by the tribunals; a fine of two francs in one case, and of six francs in another, is an obvious insult to the prosecutor; and if chromate of lead does come under the notice of a judge appointed to prevent it, we do think the user of it should have more than a sixteen franc fine and a week's imprisonment.† It appears that these are typical examples of the working of the law on sugar-plums, for M. Chevallier cites no others. No police at all is better than such an inefficient one.

It is scarcely necessary to say that any systematic control over modes of manufacture, and the quality of articles allowed to be sold, is quite inconsistent with fair play to the principles of free trade. If we raise the price by ordering them to be made good and in less quantity, we must either protect the British merchant or ruin him. And even those who hesitated about the introduction of our present commercial system of free trade are generally anxious at least to give it a just trial.

The advocates of detection may, perhaps, argue, *extremis morbis extrema remedia*—that the evil is of such magnitude as to justify even the most objectionable modes of repression. But the most trustworthy evidence—evidence resting on the personal experience of competent persons—does not by any means show this. Indeed, it proves the negative, as far as it is possible to do so. The vagueness of Mr. Accum's statements, which enumerate on hearsay the number of substances used for sophistication, has made the public suppose, for the last quarter of a century, that the poisonous and nauseous adulteration of food is just as common as the most harmless dilution. Hence there has been naturally a great alarm and outcry. And if Dr. Hassall had stopped short with the long tabular list of the names of articles ascertained by himself ‡ to be fraudulently employed, some considerable alarm, though not so great, would be justified. But the carefully prepared details of his examinations, if really read, are calculated to dissipate much of the panic. For instance, beginning with the letter A, we find "Annatto" to be adulterated with flour, turmeric, and plaster of Paris, and on referring to the analyses,§ we find that ponderable quantities of these substances, neither noxious nor nasty, were found. Then we read of red-ochre and red-lead. The former, one would rather be without, for it is a paint; but then we find there are only two grains of it in the hundred, while of the really poisonous lead there is only "a trace" in some few specimens. The "sulphate of copper" enumerated in the list is not mentioned in the analyses at all, and appears to have been the oxide. Considering, then, that annatto itself is only used to tint cheese, and to give to some milk the very slight orange colour it possesses—in fact, exists in our food only as "a trace," of which we should be a very long time in eating 100 grains—we feel quite willing to abide the consequences of our homœopathic doses of iron

* Chevallier: art., Buerre. † Ibid.: art., Bonbons.

‡ The liberty is here taken of substituting the singular number for the plural; for though Dr. Hassall modestly shrouds his identity by speaking of "ourselves," it is very evident that to him alone we owe the admirable researches contained in his book. The occasional dilution of them by the observations of others, we look upon as an adulteration

§ Adulterations Detected, p. 465.

and lead. Next comes "Arrowroot," which we read without sorrow is often made of potatoes, tapioca, and sago. Then "Anchovies," which, instead of being from Gorgona, are sometimes "Dutch and French fish." Most of us would as soon eat them as the true *Engraulis encrassicolus*. However, it appears that the red colour of the sauce is due to art, so that to prevent our eyes being offended with a dirty tinted condiment, we are eating paint. But here Dr. Hassall comforts us with the information that it is only ochre, or harmless iron rust, and we reflect that, were it copious enough to hurt us, we should immediately detect it, for we know the taste of iron well enough. He has never found even a trace of red-lead.

Next in alphabetical order comes "Brandy," which sometimes contains water and burnt sugar. Then "Bread," in which the only suspicious article is alum; but in spite of its universal use to lighten the baking of second-rate flour, no single instance of injury to health can be found. And so on through the list of forty-four, amongst which the only articles designedly* sophisticated with deleterious substances are, "Cayenne pepper" with red-lead and vermilion; "Egg-powders" with chrome yellow; "Snuff" with chrome yellow (in nine of forty-three samples), oxide of lead (in three out of forty-three), and a suspicion of powdered glass; and the "coloured confectionery" of twelfth-cakes, &c., with external application of noxious pigments. Now of Cayenne pepper and snuff, the quantity swallowed even by the most inveterate is small; "egg-powders" are known to few, and eaten by fewer; and really the only one against which evidence of bad effects can be brought is the "coloured confectionery." But we have all been taught from our earliest days, that these "ships in full sail," "dogs," "ducks," and "sailors,"† were not made to be eaten, any more than tin soldiers and tops are to be sucked. An Act of Parliament which would deal with them must go deep into the toy question, otherwise it will be evaded by their being sold as "*Jouets d'enfants*."

Dr. Sanderson's special examination of the milk, bread, and flour sold in Paddington exhibits the acknowledged facts that there is very often water in our milk-cans and alum in our loaf, but that society's life is not in any danger. The ease with which the amount of these adulterations can be detected excuses us from pitying those who submit to them against their will, and renders unpardonable any guardians who allow them to proceed to such extent as to injure the poor persons entrusted to their care. The mere reputation of having occasionally put milk in a glass to try the quantity of cream in it, is often sufficient to render you safe; at St. Mary's Hospital the milk supplied is habitually richer than an average cow's yield (containing 13·2 of solid matter *vice* 12·98 per cent.), merely because some years ago it was tested by the galactometer. Dr. Neligan's evidence about the milk and bread of Dublin goes to prove the same point of the innocuous nature of the adulterations.‡

But even the occurrence of a quantity of water very much above the average does not necessarily show that the seller is the guilty party. We all know how wet nurses vary in the aqueous contents of their milk, and the same is more strikingly the case in different breeds of cows, and as a consequence of different nutriment. M. Chevallier§ quotes an instance in which the adulteration of bran with sawdust in Paris was detected through the thin milk yielded by the cows fed upon it; when to have fined the milkman would have been most unjust. The same difficulty in fixing on the real guilty party, and the same injustice in applying the principle of *caveat venditor* to the retailer, would be experienced in the case of all articles of food.

It is clear that it is through the pocket alone that society's health really suffers by the adulteration of food; and all readers of political economy by the light of history are, or ought to be in the middle of the nineteenth century, convinced that

* The copper in pickles is an accident from the use of saucepans of that metal. It is frequently found in them, but, according to Dr. Neligan's evidence, not in quantities injurious to health. So little alarmed is this analyst at copper vessels, that he has not interdicted their use in his own kitchen. The detection is immediate, from the precipitation of the copper on the steel knife, and is so obviously against the interest of the pickle-maker, that it may be omitted from the list of intentional adulterations.

† Adulterations Detected, p. 487. ‡ Report, March 12th, 1856. § Art., "Son."

any attempt of the central government to interfere for the advantage of their people's pockets is a failure. It may *control* or arrest for other ulterior objects, but it cannot *promote* commerce.

On the whole, then, it may be concluded that the detective interference of a central authority with the vast masses of food which form the chief bulk of our country's merchandise, would only partially accomplish the end desired, would interfere with free-trade, raise the price of provisions (thus perhaps causing the starvation of thousands), and would establish a bureaucracy always odious, and more especially so when it meddles with domestic interests. At the same time it would be one step towards teaching us to rely for protection upon Government, and to a remission of the sleepless attention to his own interests which is the best education for being able to watch the interests of others, and which makes an Englishman not only a good tradesman, but a successful ruler at home and abroad.

In adhering to the principle of "every purchaser his own detective," it will be right, certainly, to give him every facility for being so. Those are his best friends who afford him such facilities. We can devise no way of doing this so little objectionable as that of vestries selecting (or perhaps being obliged by law to select) officers of health capable of making analyses of food and drink, and agreeing that they shall make them at a fixed price for all inhabitants of the district. In populous places this work might of itself bring in a good income to the officer. Dr. Hassall says that he could, by employing assistance for the rough work, guarantee between two hundred and three hundred examinations a week. At two shillings apiece this would give 1,000*l.* a year to the analyst, and allow 250*l.* for laboratory expenses—enough to secure the services of a highly-educated man. If there was not sufficient work to occupy and support him, the ordinary duties and salary of officer of health might be added, or he might be a medical man and allowed to practise, or to take pupils. It may be observed, that while Dr. Hassall was the Lancet Commissioner, he was a hospital physician and lecturer, practised in private families, conducted analyses for other persons, and continued his literary labours as well; so it is evident analytical work may easily be associated with that of another description.

When the party interested had got his analysis, he might use his own discretion as to whether the importance of the adulteration would justify him in getting a summons, and following up the case in the method which the application of the principles of the Bread Bill to other objects would enable him to do.

It would be perhaps desirable to enter the results of the analyses in a record open to the inspection of the parishioners: but except where the case had been proceeded with, it may be questioned whether the publication of them in the public journals, as advised by several witnesses, would be judicious. There is nothing in it discordant with the spirit of English legislation, for it is simply a modern application of the exposure contemplated by our forefathers. But still there is danger of the punishment pressing unequally; and it is also excessively severe, amounting in some cases to undeserved ruin. The "Statute of the Pillory and the Tumbril," quoted at the beginning of this article, was followed shortly by another, cautiously prescribing the mode of application, lest the vertebræ of the king's lieges should be overstretched; so, probably, it would be necessary to take immediate care that the pillory of the periodical should not be *too* straight, the dung-cart of the daily paper not *too* stercoraceous.

It may be said—and the argument appeals to our best feelings—that it is impossible for poor persons to spare the time and money, or to gain the information requisite for this attention to their interests. A *reductio ad absurdum* might be made, by picturing a labourer at two shillings a day spending the whole of it in getting analysed the loaf which he has obtained on credit, and starving while attending the magistrate's leisure. But what is to prevent the union of the working classes into committees, clubs, and benefit societies for this object? Their combinations have often hitherto been led by ignorance to do most foolish things;

but the best way to prevent this, would be to give them right objects, not to try and put them down. The latter attempt must always be a failure, so long as "man is by nature a clubbing animal."* Time and civilization have done much, and it is comforting to see the improvement that has taken place even in the aim at utopian schemes. Compare, for instance, Wat Tyler's agitation against taxes, or the bread rioters of the present century, with the delegates from the last strike meeting to confer at the Society of Arts. And that these men can unite also for the very purposes now under discussion, is shown by the Blue Book in our hands. Among the witnesses examined are the foreman of the "People's Flour Mill" at Leeds, and the book-keeper of the "Rochdale Corn Mill." Both are societies set on foot and directed entirely by artisans for some years, with the object of securing good bread for their own consumption. And if they could unite to manage the large capital embarked in such undertakings, they could surely subscribe and administer the few pounds required for analyses and prosecutions. Self-reliance is the best lesson we can teach our countrymen, and he is a truer patriot and more conscientious statesman who bids us put our own shoulders to the wheel, than he who earns temporary popularity by himself lifting our cart from the rut.

We come now to the adulteration of DRUGS, and have at the outset to regret that this question should have been by the Parliamentary Committee mixed up with that of food. They stand on quite a different footing. If food is noxiously contaminated, the offended taste and smell give warning; but of drugs the natural qualities are abhorrent, and that patient must be a very unfortunate one who is by experience a competent judge of goodness. If food is diluted, our appetite prompts us to compensate the fraud by eating more; but with drugs dilution is death. If the specific quantity ordered is too weak for the contemplated purpose, it may be too late to rectify the error, when it is found out by its consequences. It is therefore of vital importance to parties concerned, that extreme accuracy and equality of strength in active remedies should be attained, and purity here is no pedantry.

We are dealing with a trade of very limited extent as compared with the supply of food; and exceptional measures applied to it are quite consistent with a protest against a more general interference with commercial principles. Again, an augmented cost in the raw article affects essentially the retailer of food; it is a large part of the payment he gets, because his labour is but slightly skilled; but to the highly educated compounder of medicines the price of the drugs matters little, it is a very small fraction of what he charges for his presumed knowledge and trustworthiness. Again, a slight rise in the price of food would be the death of thousands; a considerable increase in the value of drugs would seldom even be known to the consumer.† It is clear, then, that an interference with the trade in drugs could not appreciably affect the purses of either retailer or customer.

On these grounds a detective inspection of chemists' and druggists' shops may be fairly advocated, if a case is made out of either diluent or noxious adulteration existing to any important extent. For it has been shown that dilution and danger to life are here convertible terms. And the case does appear to be fairly made out by the universal consent of all who have examined the subject. It is true that some highly respectable large drug dealers deny before the Committee their cognisance of adulterations being now carried on by wholesale houses, and everybody believes the witnesses. But even they confess that a few years ago, when they were young and struggling, the practice was common (evidence of Messrs.

* "ἄνθρωπος φύσει πολιτικὸν ζῶον."—Aristotle's Politics, Book I.

† The slight degree to which the price of drugs affects the interests of the sick may be illustrated by a few simple facts. In the four years and a half ending Dec. 31, 1855, there were treated at St. Mary's Hospital, 48,405 patients, at an expense of 1602*l.* for drugs, herbs, leeches, and dispensary sundries—that is to say, for an average of less than ninepence a-head. And this was in spite of medicine of a most costly kind being used when requisite, although "atropine," at four pence a grain, appears as one item, and "Boudault's pepsin," as another, in the last bill. Now such of these as were in-patients cost the hospital an average of more than two pounds for food alone, exclusive of wine, beer, nursing, lodging, medical attendance, and other things, amounting to about three pounds more. It is evident that doubling or quadrupling the cost of drugs would make less difference in the expense of an illness than adding a farthing to the price of a quartern loaf.

Baiss, Bell, Allen, Atkinson, Bastick); Mr. Drew, though he thinks the prevalence of adulteration has been exaggerated, believes that inferior drugs find their way to the *provincial* wholesale houses, and through them to the retailers and public; and Dr. Bingley confirms the fact of small dealers practising the fraud. But the revelation made by Mr. Wakley, of what took place when he was articled pupil to a druggist in Somersetshire, is alone enough to alarm a patient to his danger in a country where such practices are possible, and where, in witness's belief, they have been increased among the smaller traders since the period alluded to, along with the increase of science and the publication of the processes. It appears that the chief wholesale business of this sadly immoral house was to send sham drugs to London. For example, "———'s Verdigris" consisted entirely of sulphate of copper and acetate of lead; "Annatto," of turmeric, lime, red paint, and soap; "Calomel," of white precipitate and sulphate of lime; "Burnt sponge," of charcoal and salt. One cannot be surprised at Mr. Wakley leaving the drug trade, and the world must be congratulated on his having the boldness to reveal the secrets of his former prison-house.

Now, if such things can be done by even "provincial" or "inferior" wholesale houses, what is likely to be the practice among the thousands of small straggling country retailers? Are they more likely to purify their drugs, or to save a few pounds out of their quarterly accounts by further diluting their stocks?

Even in London, Dr. Hassall found, of twenty-three samples of gum opium, nineteen adulterated; of forty samples of powdered opium, thirty-nine adulterated; of twenty samples of scammony, two only were genuine, one entirely fictitious, and the rest variously diluted with sand, wood, gum, and flour, to the extent of from 18 to 65 per cent. Among thirty-three specimens of jalap, fourteen were impure. In the same number of ipecacuanha powder, eighteen. In this last case decisive evidence existed that the sophistication was not mere dirt or carelessness, for the utterly extraneous matter, *tartar emetic*, was found. Colocynth contained chalk, rhubarb and squills contained flour, &c., &c. The danger to life involved in the uncertainty of dose thus entailed is so obvious and pressing, that we cannot be surprised at the demand for immediate coercive measures over a trade where the temptations to fraud are so strong, and are actually extensively yielded to.

A hope is expressed by some witnesses, and by the Report of the Committee, that the laudable efforts of the Pharmaceutical Society to diffuse scientific knowledge among chemists and druggists will prevent fraud by introducing a better class into the trade. It has effected some improvement already; and there is no doubt that if a customer understands how to choose a tradesman, he will find in most large cities a member of the Society from whom he may be pretty certain of getting pure drugs. But too much must not be expected from mere education as a preventive of fraud; the criminal records of the last twelve months have given the country fair warning on this point; and the impression is gaining ground, that the more a man's intellect is sharpened, the more necessary it is to subject it to either the natural restraint of public opinion, or the artificial chain of the law.

It is right that the advocate of the inspective or detective system should propose some body capable of being entrusted with such an exceptional power—a power of which Englishmen are so justly jealous. Should it be given to the Pharmaceutical Society? Gratitude is due to them for their diffusion of science, by which they have advanced and are advancing the position of their trade, till it stands almost on the level of a profession. But the experience of modern Europe is against the once prevalent "guild" system, by which each trade was entrusted with the police of its own members. Its interests are too much to diminish numbers and increase profits, to the detriment of commerce. Though, as a conservative people, we retain the system in a few cases under restrictions, there would be an unwarrantable anachronism in creating a powerful "guild" in the present day.

Another proposed body is the Board of Health, or some other Government bureau, on a footing similar to the "Conseil de Santé Publique," consisting of eminent scientific characters appointed by a Home Secretary. Such an authority would be quite new, and not entirely unexceptionable. It would have a strong temptation to a kind of pedantic purism, which would interfere as much with "mint and anise and cummin," as with the "weightier matters." Sugared honey or woody liquorice would be as severely visited as chalky calomel or starchy quinine. This is what actually happens abroad. M. Chevallier tells of a Spanish druggist who was fined 9000 ducats and deprived of civil rights for a year, for putting sugar and starch into manna. Among the counts in the indictment against the French pharmaciens in 1850, was one for adulterating *onguent de peuplier* (a mere scented pommade) with balsam of Peru. In 1852, at Paris, Madame R. was condemned to be imprisoned three months, to the costs of the suit, and a fine of fifty francs, for selling syrup of capillaire containing, some indeed, but "too little," of that inert plant. In 1850, a chemist was fined 1000 francs and imprisoned eight months for making orange-flower water with oil of neroli. The selling of artificial seidlitz water in stone bottles, even though the label was not imitated, was punished by a fine in 1847. But the favourite subject of prosecution in Paris is *Sirop de Gomme;* twenty victims of heavy fines exacted in 1849, '50, '51, '54, for making it of glucose, are quoted by M. Chevallier, who says he could add largely to the list of convictions for the harmless substitution of potato-sugar for cane-sugar.

Yet in spite of this prudery, some really powerful drugs are commonly sold in France of strengths more dangerously various than in this country, and no notice appears to be taken. For example, scammony, as analysed by M. Thorel, contained from 6 to 84 per cent. of its active resin; whereas, in Dr. Hassall's examinations, the range was only from 13 to 79. Mention is made by M. Chevallier of modes of imitating jalap, ipecacuanha, rhubarb, &c., but no prosecutions seem to have been instituted on their account, and only one instance of a fine exacted for falsifying opium. The fullest details for the purpose of comparison are found about scammony, and judging from that, it may be surmised that all the adulterated drugs of which samples are analysed by Dr. Hassall, might have been sold with perfect impunity across the Channel. So adhesive is the *sirop de gomme* to the fingers of the police, that they appear incapable of handling more important substances.

No great advantage can be hoped from an inspection of the drugs at the outports, as proposed by some. The principal places of adulteration seem to be the grinders' mills and the provincial dealers, which of course would not be interfered with by this plan. Besides, it would prevent the importation of many inferior descriptions of drugs, which are useless indeed, and improper for sale in their impure state, but which contain a certain quantity of the active principles, and may be made available for pharmaceutical purposes. Again, the expense of the reintroduction of strict examination at the outports would be enormous. It must be remembered that we have introduced free-trade, remitted the duties, and disbanded most of the collectors who would take cognisance of these articles; whereas the United States, from whence it is proposed to imitate this system, are still in the enjoyment of protection, and have ready made a protective army of custom-house officers. They have got a machinery which our economists have been labouring for some years to break up bit by bit, and it would be a pity to reconstruct it without necessity.

The most practical scheme seems one of which the embryo already exists in London and Dublin—namely, the placing a power of visit in the hands of a medical corporation, altogether unconnected with the drug trade. It is their interest that drugs should be plentiful, pure, and reasonable in price; they can have no object in hampering commerce, and as each article is valued in their eyes solely in proportion to its medicinal activity, no over-busy purism need be feared from them. Besides, as they are already viewed by the retailer of medicines as the guardians of the consumer, there would be less jealousy of their inspection than would be the case with any other authority. The embryo alluded to in London is the power possessed by the College of Physicians of inspecting chemists' shops in the City. Its censors exercise this authority twice a year, by selecting some retail establishments whose drugs they examine, and, if they are adulterated, throw them into the gutter. But as the censors are not paid for the time so spent, and have plenty of other engagements, the good done must be very partial. At Dublin, a similar power resides in the Irish College of Physicians of visiting shops where medicines are compounded, and of publishing the names of offenders against purity. According to Dr. Neligan, the local Act which gives them this power is to a certain extent efficient and popular. But want of remuneration again paralyses the executive.

It will evidently be necessary to provide a fund of considerable amount to pay the expense which would be incurred by a medical corporation becoming responsible for the due visitation of chemists and druggists throughout the kingdom. Probably no fairer mode of doing this can be devised than to throw the burden on the parties to be benefited—the consumer and the shopkeeper. The former would receive the advantage of a more valuable article, and the latter be freed from the competition of dishonest rivals. Such a distribution of the expense would be equitably effected by making all compounders of medicine pay a small tax for license, renewable annually. This sum he would of course be repaid in the price charged for the goods. Whether the visitors should themselves hold a court for the granting of licenses, or should give certificates for that purpose to Quarter Sessions, is a matter of detail of no moment; but a plan grounded on these principles would certainly more than any other secure the confidence of the medical profession and their patients.

A very important aid towards the proposed object would be given by a revision of the Pharmacopœias, with the special view of securing, not so much superior quality as accuracy and equality of strength in active remedies. One great and not difficult improvement would be the expulsion from prescriptions of articles known to be of various qualities in commerce. Why, for example, should the oft-quoted raw scammony be used at all? Why should not *scammonium* be defined to mean the active resin of the plant, while the English *scammony* remains as the commercial designation? Then those who wished their patients to find certain results follow the medicine, would write *scammonium ;* those who wished them to take the chance of having sometimes 6 per cent. and sometimes 80 per cent. of activity in the powders, could order in English. The same remark would apply to jalap and several other vegetable matters, found, by all parties who inquired, to be sold of most various strengths, not by the wilfulness of the dealers, but because they are so produced by nature.

The carrying out fairly and consistently these and a few other simple expedients, which would naturally suggest themselves to a medical corporation, would in a few years secure to England a purity of drugs, not only equal to other countries, for that she appears to have already, but equal to her civilization in other respects.

Before we conclude this Article, it is proper to make a few remarks on a fear that has been expressed, and among others by Mr. Wakley, that the agitation of the subject, and the diffusion of knowledge about it, has a tendency to increase adulteration. He urges this as a reason for some immediate legislation. Now we should not object to immediate legislation, if cautious; but we must demur to the grounds on which it is asked. The general diffusion and the popularization of knowledge, such as is contained in the books here quoted, is most useful, and conduces to arrest the evil. Does a publication of the tricks of card-sharpers and thimble-riggers augment their profits, or guard the simple against them? Surely the latter. Manuscript books of modes of adulteration appear from the statements of several witnesses to have been very recently kept by manufacturing druggists; these of course were secretly handed over with the good-will of the house; and very likely still more precious secrets were transferred *vivâ voce* from father to son, and from partner to partner. But the revelations of traitorous science, the true Zaphnath-paaneah, have now put the customer equally in possession of the knowledge, and it is his own fault if he does not use it to his advantage.

It must not be supposed, even, that the publication in print of modes of adulteration is a new thing, the evil consequences of which should require us to be in a hurry to guard against them. Beginning with Colin at Tours, in 1513,* and Lodetto at Brescia in 1569,† M. Chevallier enumerates eleven authors on the subject before the present century. And from this list, probably, a good many are omitted whose works, valuable when first published, have been obscured by advancing science. Cordus, for instance, is not named, whose 'Dispensatorium,' published in 1598, contains a complete list of the adulterations of drugs and some articles of diet (such as sugar), with practical directions for detecting them by their physical properties. During the French revolutionary period, the dreadful mania for commercial swindling, so graphically described by M. Goncourt,‡ gave an unexampled activity to every kind of fraudulent sophistication. The consequence, not the cause, was the translation of Remer's work by Bouillon Lagrange, in 1816, and the appearance of many more French writers, of whom a list is given in the preface to the 'Dictionnaire.' The most valuable, previous to the work under review, seems to be the 'Traité des Moyens de Reconnaître les Falsifications des Drogues simples et composées,' by MM. Bussy and Bourtron-Charlard.

So that even if he did not come into MS. knowledge by inheritance, any manufacturer who could get a foreign book translated for him, need never have been at a loss to know what articles to use for adulteration, and what his customers are likely to find out. The advantage of the modern works over the older is, that while the latter simply enumerate the methods of fraud, and give a customer data by which he may detect it, the former put him in possession of accurate means of ascertaining not only the presence, but the amount also, of the enemy—an important consideration in distinguishing the real guilty adulterations from the innocent. Instead therefore of being frightened at the recent multiplication of that literature, of which there are quoted in the heading two of the most commonly known examples, we cannot but consider it a safeguard of continually increasing value. M. Chevallier's volumes have been translated into German, with valuable additions by Dr. Westrumb at Göttingen, and it is probable that an English version would be well received both in this country and in America, especially if it were brought up quite to the scientific knowledge of the present time.

Review VI.

1. *History of Medicine from its Origin to the Nineteenth Century; with an Appendix, containing a Philosophical and Historical Review of Medicine to the Present Time.* By P. V. Renouard, M.D. Translated from the French by Cornelius G. Comegys, M.D., Professor of the Institutes of Medicine, Miami Medical College.—*Cincinnati*, 1856. 8vo, pp. 719.
2. *The Medical Profession in Ancient Times: an Anniversary Discourse delivered before the New York Academy of Medicine, November 7th*, 1855. By John Watson, M.D., Surgeon to the New York Hospital. Published by order of the Academy.—*New York*, 1856. 8vo, pp. 222.

That an acquaintance with the chief particulars in the History of Medicine, from its birth or origin onwards, should be regarded as an indispensable complement of the well-informed physician or surgeon, probably all will be found willing to acknowledge. When, however, we consider what means have been employed, and what facilities provided, to enable the earnest student of his profession to

* Histoire des Drogues, &c. A second edition is in the library of the College of Physicians.
† Dialogo de gl' Inganni d'alcuni Malvaggi speciali. In the library of the Medico-Chirurgical Société is also a later edition.
‡ Histoire de la Société Française pendant la Directoire. Paris, 1854.

overcome the difficulties, and to assist him in the research and observation so requisite for obtaining a satisfactory knowledge of this subject, we find few landmarks to guide him. In it our English literature has been hitherto far from varied or accurate, while, complete as the opportunities for the instruction of students of medicine by lectures have become in every other essential department, in this they have been at all times acknowledged as deficient. To a certain extent the neglect of the History of Medicine, as an important subject of professional instruction, has prevailed in other countries than our own; but, considering the fame which Great Britain has acquired in the cultivation of Scientific Medicine, it is impossible to suggest this as an excuse for an essential element of the physician's and surgeon's education being overlooked. Strictly speaking, indeed, the great medical schools of our country are in this particular very greatly behind those of the Continent; for though the study of the History of Medicine is nowhere among the latter prescribed as imperative, in Paris, as well as Berlin and other German cities, lectures upon it are delivered; while even at Athens, in no degree famous as a medical school, the History of Medicine forms the subject of a distinct course of prelections. Apart altogether from the interest which necessarily pertains to a subject extending, as the History of Medicine does, from the very earliest to the present time, occupied with almost every country and nation of men, not excepting the rude and uncivilized, a knowledge of Medical History is calculated to secure the establishment of what alone is true, or at all events well founded; and that physician or surgeon who has undergone the additional mental culture it implies, is undoubtedly in a better position for the avoidance of error and for the rejection of unsound views. The consideration of the doctrines, theories, and practice of former races of physicians and surgeons, interesting as they must be, will also be eminently serviceable; and for the physician and surgeon now, we can conceive no duty more agreeable, and none more profitable, than the contemplation and comparison of the views of their predecessors. Such was the habit of the illustrious and the learned Scarpa: "Fu mai sempre mio costume nell' esercizio della chirurgia di confrontare le mie osservazioni con quelle dei più accreditati Maestri dell' arte che in ogni età fiorirono."*

We have spoken of the deficiency of our English literature upon the subject of Medical History, and, compared with that of France or Germany, it is indeed limited. We possess no such large or complete works as those of Le Clerc, Portal, Sprengel, Hecker or Haeser. In our language, the smaller works of Friend—which in some respects may be regarded as a continuation of Le Clerc's—Hamilton, Walker, William Black, and Moir, are those hitherto chiefly known and consulted. The absence of any complete and altogether trustworthy guide to the English student of Medical History must not, however, be considered to argue the want of individuals from time to time qualified for the duties of its authorship. Though we possess no extended History of Medicine, there are many admirable works in our language upon particular branches of the subject, greater or smaller, while the recent appearance of Dr. Wise's learned work on Hindoo Medicine,† and the various highly interesting contributions of Dr. Simpson, in illustration and elucidation of Greek and Roman physic, prove that with us an increased attention is being directed to it. Of late years, too, the indefatigable labours of Daremberg and Littré, in France, of De Renzi and the late Professor Vulpes at Naples, throwing light upon previously dark and obscure periods of the History of Medicine, and bringing to light in all their excellence the treasures of its ancient and forgotten heroes, have been worthily followed by Greenhill, Adams, and others, in our own country.

It is expressly from the conviction of the deficiency of the English language in works on the History of Medicine, that we are indebted to Dr. Comegys for the excellent translation of the comparatively recent work of Renouard, the title of which is placed at the head of this article. With it we have associated the very

* Trattato delle principale Malattie degli occhi.
† Termed by Haeser, in his 'Lehrbuch der Geschichte der Medicin,' "eine sehr gediegene Arbeit."

able discouse on the Medical Profession in Ancient Times, by another American physician, Dr. John Watson—an essay which we have perused with much pleasure, and which is calculated to shed additional lustre on the distinguished body before whom it was delivered. It is not a little remarkable that for these, the two most recent additions to our historical medical literature—one original, the other a translation from the French—we have to thank our American brethren. Aided by the labours of Dr. Watson and M. Renouard, and appealing to the writings of others, we shall endeavour to signalize what, in our opinion, are some of the chief points of importance and of interest in one of the most instructive portions of the History of Ancient Medicine, whether regarded by the enlightened practitioner or the student of medicine. Our readers may perhaps be inclined to start at the magnitude of the task we have thus proposed to ourselves, but in the present article we have no further object than to enlist their sympathies in the scheme of making instruction in the History of Medicine to be regarded as an essential of professional education, in something of the same light as the History of the Church is looked upon in schools of divinity. How much is due to the early cultivators of our science—how great the debt we owe to Hippocrates, for example—can only be truly appreciated by him who has made the History of Medicine, antecedent to its great father and since his time, the subject of careful study.

It is interesting to observe how in all ages the question of the probable origin of medicine has occupied the attention of those who have themselves advanced the science. That, indeed, may be traced back to the very infancy of the human family—to a period regarding which we have no historical account, and possess only what Sprengel terms *fabelhafte Ueberlieferungen.** Into the speculations which this inquiry has given rise to, we shall not enter, though at many times it has been discussed with very great learning, talent, and acuteness. Van Helmont, in his remarkable work, entitled 'Ortus Medicinæ,' &c., and Le Clerc, in the work already alluded to, 'Histoire de la Médecine ou l'on voit l'origine et les progrès de cet Art, de siècle en siècle,' &c., may with much advantage be consulted; and not less so the remarks in the treatise 'On Ancient Medicine,' one of the Hippocratic collection. That the origin of Medicine may legitimately be traced to motions of the human intellect, and need not be attributed, as some have attempted to do, to a direct divine communication to man, is rendered probable from the circumstance that a variety, rude indeed, and oftentimes most uncouth, of medical and surgical practice has invariably been found to exist in newly-discovered countries, even though at the period of their discovery these have been sunk in the most degraded barbarism. Judging from this fact alone, we should feel inclined to adopt the opinion that medicine in its origin was coeval with man, or, as Le Clerc has observed, "Le premier homme a été en un certain sens le premier Médecin."†

We cannot in this article devote space to the consideration of Primitive Medicine—a subject which, though possessing much intrinsic interest, has only that charm to reward our speculation, as it cannot be maintained that the progress of mythological medicine among the Hindoos, Chinese, and other nations, has any intimate relation with the real advance of our science. If we turn to ancient Egypt, a country in which we know the arts of civilized life to have been very early cultivated, there can be little doubt that the profession of medicine existed in it. The first physicians of Egypt were the priests; of their practice, and indeed of the constitution of their order, the knowledge we possess is limited; but this we do know, that medicine in Egypt was blended with superstition, and mixed up with all kinds of religious and fanatic observances. It was practised by a sect of the priests known as παστοφόροι, on account of the long vestures which they wore, and because they were employed on certain ceremonial days in carrying the bed of the goddess Venus. These medical priests, we have reason to believe, were treated with great respect, and this we can the more readily understand when we consider that the occurrence of diseases was viewed by the Egyptians, just as it

* Versuch einer Pragmatischen Geschichte der Arzneikunde. Erster Theil, p. 19.
† Histoire de la Médecine. Première partie, p. 7.

was afterwards by the early inhabitants of Greece and Italy, as a direct manifestation of the displeasure and interposition of their deities, and being thus assured they would naturally look to the ministers of these deities, the officiating priests of their temples, to be the means, if only indirectly, of procuring their removal. In this way it probably was that the priests of Egypt became Egypt's first physicians. That a subdivision of labour existed, that something resembling the specialities of the present day obtained among the Egyptians, we learn from Herodotus.

"The art of medicine (he says) is thus divided amongst them; each physician applies himself to one disease only, and not more. All places abound in physicians; some physicians are for the eyes, others for the head, others for the teeth, others for the parts about the belly, and others for internal disorders."*

The embalmers, whose handiwork has itself made Egypt famous, and has astonished and perplexed modern nations, probably were included among the medical priests. Of them Dr. Watson remarks, that—

"Their occupation must have rendered them familiar with the internal structure of the body, and furnished them with useful insight into the nature, causes, and results of diseased action." (p. 16.)

For our own part, we confess that the account Herodotus† gives of the operation of embalming—the brains being drawn through the nostrils by an iron hook, and the incision in the side being made by means of a sharp stone—leads us to the belief that notwithstanding the frequency with which the operation of embalming was performed, little satisfactory knowledge could be gained from it. Indeed, it appears to have partaken much more of a religious than of a medical character, and was very probably not performed by the same class of priests who were charged with the treatment of disease. It is true that we read in Genesis, fiftieth chapter, "And Joseph commanded his servants, *the physicians*, to embalm his father: and *the physicians* embalmed Israel." But various learned commentators on this passage in the Old Testament, and among others, Bishop Kidder, have regarded the Hebrew word translated physicians to signify those merely to whom the care of embalming belonged.‡ Contemporaneous with the Egyptian there was another nation, of whose history in many important particulars the sacred writings authoritatively inform us, concerning whom and whose institutions, therefore, we are not left in the same degree of doubt. We know that civilization, with its attendant improvements and advances in the arts and sciences, had made very considerable progress in Egypt before the family of Israel, compelled by famine, were driven to take refuge there. And such being the case, we would naturally expect that the Jewish people, from their contact and intimate relationship with the Egyptians, during fully four hundred years, would receive from them much of that knowledge and skill with which they were endowed. In the writings of Moses we find abundant proof that, as in the case of the Egyptians, the Jewish priests were originally the physicians. It was to the Levites the people applied when affected by leprosy; from them the infected sought a cure; it was the priests who determined what individuals and families were to do.

"When a man shall have in the skin of his flesh a rising, a scab or bright spot, and it be in the skin of his flesh, like the plague of leprosy, then shall he be brought unto Aaron the priest, or unto one of his sons the priests; and the priest shall look on the plague in the skin of the flesh; and when the hair in the plague is turned white, and the plague in sight be deeper than the skin of the flesh, it is a plague of leprosy, and the priest shall look upon him and pronounce him unclean."

In this passage of the thirteenth chapter of Leviticus, and in what follows in that and the succeeding chapter regarding leprosy, we have a striking example of the power committed to the priests: they decided as to the nature of the disease—leprosy or not; and consequent on that decision was the course which they

* Herodotus, Euterpe, cap. 84. † Euterpe, c. 86. ‡ D'Oyley and Mant's Bible.

again ordered to be followed and observed. Nor is it in connexion with the disease of leprosy merely that the medical functions of the Jewish priests is exhibited. Regarding the precepts contained in the twelfth and fifteenth chapters of Leviticus, Renouard truly observes, that after their perusal, "one cannot repress a sentiment of admiration for the wisdom and foresight which made such salutary regulations a religious duty." (p. 33.) How long the Jewish priests continued to be physicians also, we are unable accurately to determine. In the Apocryphal book of Ecclesiasticus, 38th chapter, which has, from its style, been attributed to Solomon, but which, in any case, was written two hundred years before the birth of Christ, mention is for the first time made of the office of physician apart from that of priest.

"Honour a physician with the honour due unto him for the uses which ye may have of him, for the Lord hath created him. For of the Most High cometh healing, and he shall receive honour of the king."

Passing now from this rapid glance at the condition of medicine in Egypt and among the Jews, we have to view it in Greece, still mythical and priest-ridden, antecedent to the Trojan war, and to that period in which, among the Greeks, we find materials the most ample and the most instructive for the history of medicine. Here it is unnecessary to follow the example of Le Clerc and Sprengel, and trace the history of the medical mythology of Greece, or even to mention the names of the numerous gods and goddesses, heroes or heroines, who were regarded as the inventors or cultivators of the various branches of medicine. Some of these—as Thoth, whom the Greeks called Hermes, and Isis—were borrowed from the Egyptians, others were of purely Grecian origin.

Leaving the period of mystic and primitive medicine, and in order to form a proper estimate of what the genius of Hippocrates effected for our science, we may now briefly inquire into the condition of medicine antecedent to the birth of him who has in all ages been truly regarded as its father, limiting this inquiry still further by directing our present attention to the temples of Æsculapius, which, as Dr. Watson observes, "notwithstanding the speculations of philosophers, and the trainings of the Palestræ, were the first great foundations of medical knowledge among the Greeks." (p. 35.)

The priests of the temples of Æsculapius, or Asclepiadæ, knew well how to take advantage both of natural situation for their erection, and of the respect—amounting, indeed, to a feeling of veneration—with which they were regarded by those who sought their precincts. Cabanis has well observed, that—

"Many cures must have been accomplished by the diversion which the patients experienced in their journeys to these temples, by an exercise to which perhaps they had been but little accustomed; by the invigorating effects which an elevated situation produces on man, and indeed upon the generality of animals; and lastly, by the still more invigorating effects of hope."*

But besides such methods of securing or promoting health as have now been referred to, and which may be classed under the head of Asclepiadæan hygiene, there is reason to believe that the priests of the temples or Asclepions resorted to various means of cure, according to their notion of the particular ailments under which the patients who consulted the divinity laboured. The Asclepiadæ we know prescribed bloodletting in certain cases, purgation or vomits in others, while friction, sea-bathing, and the use of mineral waters, were other remedies they often employed. Beyond all this, and exercising as it no doubt did a most powerful influence on the sick and on the sick folks' friends, was the mental influence which the doctor-priests of the temples knew only too well how to produce. Admission to the temples was forbidden to such as had not previously undergone certain means of purification; and when entrance was effected, the interrogation of the oracle was frequently delayed. Sometimes a day or a night, or two nights, were first spent, the patients meantime lying in the temples. Abstinence, prayers, fasting, sacrifices, followed. After all these preliminary rites had been gone through,

* Revolutions of Medical Science, p. 53.

and after the immolation of a ram or of a fowl, or, as at Cyrene, a goat, or at one of the other Asclepions any animal save this last, the will of the oracle was craved, and the response communicated by the priests.* At this time but mysterious and uncertain information was conveyed. Sometimes the divinity deigned to appear under the form of a serpent devouring the cakes upon his altar; more frequently the eyes of the faithful and wondering sick were not treated to so close a manifestation of his presence. At certain times and on certain occasions the will of the god was communicated in dreams, and these were interpreted by the priests. The importance attributed to dreams, even at a later period, may be judged of by a perusal of the Hippocratic treatise "περὶ ἐνυπνίων."

The chief, and at the same time the most celebrated temples of Æsculapius were those at Epidaurus in the Peloponnesus, at Pergamos in Asia, at Rhodes on the island of Cos, at Cnidos, and at Cyrene, a city of Lybia. Besides these, there were numerous others, both in Greece proper and in the Grecian dependencies. Schulze, under the head of "Notitia Asclepiorum," mentions and describes alphabetically little short of a hundred.† It was customary for the priests in the Asclepions to report to new comers the history of the extraordinary cures which had been effected for former invalids, and particularly to signalize those cases which appeared in any degree to tally with theirs. The walls and pillars of the temples—and this is especially known to have been the case in those of Cos and of Cnidos, which was burnt in Hippocrates' time—were covered with inscriptions, detailing in shorter or longer terms the history of the diseases and the nature of the remedies which had at the advice of the deity been employed in these cases. Metal, marble, wood, stone, may all have been used for this purpose, according, probably, to the circumstances of affluence or poverty in which the benefited parties were placed. Those who have visited the parish churches in the different Roman Catholic countries of the Continent, and more especially Southern Italy, will call to remembrance the manner in which the walls and pillars are covered by the so-called votive offerings, and will at once recognize in the ancient practice of the Grecian temples the quarter from which the latter may reasonably be assumed to have sprung. Scanty as the information was that these tablets conveyed, and better calculated as they no doubt were to fortify the piety of the faithful, than for any great end in the advancement of science, still we feel disposed to agree in the reflection of the learned author of the "Revolutions of Medical Science," that—

> "However imperfect these descriptions of diseases and of their methods of cure may have been, their collection was nevertheless very valuable. They formed, as it were, the first rudiments of the art, and discovered some faint traces of the method of observation and experiment which alone is capable of placing it upon a solid basis."

Antecedent to the time of Hippocrates, to which we have now to turn, a great revolution had been effected for medicine by the first or early philosophers, and of these Pythagoras, Heraclitus, and Democritus were the chief. Into the consideration of their particular views we cannot here enter, but it is only due to these philosophers to accord to them the merit of having secured in great measure for medicine its release and freedom from superstition. "They," says Cabanis, "transformed an occult and sacerdotal doctrine into a popular science, into a common art." In the four hundred and sixtieth year before the birth of Christ, Hippocrates, according to very general belief, and expressed opinion, was born. Of the personal history of Hippocrates we know but little; that little, however, is so familiar to all, as to render it unnecessary here to recount it. As regards not a few of the particulars of his life and education, as well as of his doctrines and practice, differences of opinion among those who have made such the subjects of a peculiar and attentive study has prevailed;—to these Dr. Watson and M. Renouard allude, and we conceive that it would be the duty of a lecturer on the History of Medicine, after a careful investigation of the views entertained by all com-

* It was a cock that Socrates (according to Plato), in his last interview with his friends, requested them to offer for him to Æsculapius.
† Historia Medicinæ a rerum initio ad annum urbis Romæ DXXXV., p. 118.

petent authorities, to unfold them; for assuredly it may at once be conceded that anything relating to the history of Hippocrates—"that divine old man," as Sydenham has expressively named him—is worthy of attentive consideration. When we consider the age in which Hippocrates lived; that at the period in question anatomy was scarcely practised; that physiology was virtually unknown; that with the exception of a few articles, in the vegetable materia medica of Greece alone was he able to find remedies wherewith to combat diseases, though it was in the operations of Nature herself that he chiefly confided; in the contemplation of his life and labours, surely we find abundant proof of his genius and true greatness. What was known before his time was small indeed, not entitling medicine to be dignified by the name of a science. The consideration of what, during subsequent ages, has been added to the stores of knowledge he collected—what has been taken from these stores as inaccurate and unimportant—leads to the conviction which has in all ages been acknowledged, that by Hippocrates an era in medicine was formed. It was the power of observation which he so largely possessed, and so conscientiously employed, which caused Hippocrates to differ from all who preceded, from many physicians who came after him; joined to his high intellect, his exalted morality, and kindness of heart, it was this observing power that made him what he was. It is impossible to read a page of the genuine writings of Hippocrates, to peruse a single case, without being struck with the truth of this remark. The observation of individual cases of disease, the recording of these, marking the changes undergone daily, sometimes oftener, may have been, indeed very probably was, suggested to Hippocrates by the votive tablets deposited in the temples of Æsculapius; but upon this, as Dr. Bostock* has well observed, "he so far improved as to be entitled to the merit of an inventor." And what was his invention other than the discovery of the method by which medicine is best studied, the patient is best ameliorated, the knowledge of the physician most extended? In all subsequent ages, too, this method, gradually ripening to perfection, which it seems in our own day to have actually attained, has been regarded as the best for teaching an acquaintance with medicine. Clinical medicine—the study of individual cases of disease at the bedside—had, in reality, its origin in and from Hippocrates. Again, Hippocrates was the first who carefully watched the "juvantia et lædentia," as they have since been termed. He narrowly noticed the effects, good or bad, of his remedial applications, and endeavoured to remove or palliate individual symptoms. In this particular he was no less a discoverer than in the former, and what he did then, the wisest and the best informed physicians in all ages since have continued to follow him in doing.

Such were the chief improvements which Hippocrates introduced into, or effected for medicine; he pointed out that the first and great aim of the physician is to watch the operations of nature. He demonstrated the worthlessness of crude theories, and established incontestably that observation is the sole basis or foundation of medicine. The healing art in the hands of Hippocrates was, by his genius, and his genius alone, raised to the dignity of a science of experience and of facts. That distinguished position once acquired for medicine has never been lost. Regarding medicine "as a principle of humanity, and not merely as a means for attaining profit and glory," Hippocrates was not content to instruct those of his own family alone—the plan followed by the Asclepiadæ—in the precious truths he had himself acquired, and therefore he earnestly desired and eagerly sought to communicate his knowledge to strangers, and to those who had no claims of kindred to interest him in them. In this respect we may regard Hippocrates as the first and the greatest of medical reformers. Actuated by his genius no less than by his humanity, he soon saw the propriety, the necessity, indeed, of breaking through the system of uhphilosophical exclusion which confined the physicians of Greece, as it were, to a single family. Can we doubt that the bold determination he then formed, and the means he must have taken to carry it into execution, would procure for Hippocrates much odium, and array against him many enemies,

* See his admirable article, prefixed to the Cyclopædia of Practical Medicine.

particularly among the Asclepiadæ, whose hereditary, and as they fondly imagined, inalienable rights, he had so ruthlessly assailed. And when he had effected the reform he aimed at, when not only the Greek, free from the trammels of an Asclepiadean oath, though rejoicing to subscribe the Hippocratic declaration contained in the Ὅρκος, but the stranger, it mattered not from where, could avail himself of his instructions, and following these out could devote himself to the practice of the healing art, we can only faintly imagine what may have been the thoughts which occurred to the mind, what the emotions which animated the breast of Hippocrates, not yet aged, and we can in some degree only comprehend the zest and the energy, as Le Clerc says, "le ferment," that would be thereby diffused among his pupils. Some of the pupils of Hippocrates became his most distinguished successors, and various members of his own family, more especially his son-in-law, Polybus, worthily supported the character he had acquired. To most of his contemporaries, perhaps, Le Clerc is warranted in concluding that the words of Hippocrates himself were appropriate, "Qu'il y avait plusieurs médecins de nom, mais peu qui le fussent en effet."* There can, too, be little doubt that the lustre of his great name very nearly, if not completely obscured theirs. This period in the history of medicine differs in no respect from that of any other, for, as has always happened, not only in it, but in other departments of human knowledge, the advance in the science and the improvements which he introduced into practice seemed, as it were, sufficient for centuries, and for such a period in no considerable degree were these advanced beyond the limit of perfection to which Hippocrates had brought them. Mr. Moir has well remarked:

> "It happened with Hippocrates in medicine, as with Chaucer in English literature, that he not only far outstripped the age in which he lived, but left many succeeding generations without the hope of rivalling his excellences."

Into an examination of the Hippocratic writings it is foreign to our purpose to enter, but, as affording the ablest and most complete view of these, we beg to refer our readers to what has been written upon the subject by Dr. Greenhill,† Dr. Adams,‡ and M. Littré.§ Dr. Watson only does the latter justice, when he speaks of his examination of the whole collection as "most careful and searching."

Thirty years after the birth of the father of medicine, and just at the time when his fame was beginning to spread abroad, a great philosopher was born whose name was destined to be known till the end of time; this was "the divine Plato," who, like Pythagoras and Democritus before him, and Aristotle shortly after, though not embracing the profession of medicine, applied himself to the study of various things concerning the theory of medicine, and more particularly the economy of the human body, and the elements of which it is composed. No distinct treatise on medicine was written by Plato, but numerous references to it exist in several of his works. It is evident that he diligently studied the writings of Hippocrates, though in so far as the advancement of medicine by any new observation, or the suggestion of any reasonable theory is concerned, he cannot be said to have profited much by the study. But at present we cannot consider the doctrines of Plato and Aristotle, any more than those of Pythagoras, of whom we have already spoken. Regarding Aristotle, there is no doubt that the original character of his mind, the special advantages which he enjoyed, and the pursuits in which he engaged, rendered him better qualified than was Plato for conferring advantages on the science of medicine. The connexion of these two and other philosophers with medicine may be traced to the circumstance of a knowledge of its science having at that time been looked upon in Greece as one of the requirements of a polite or general education. In so far as anatomy and physiology are concerned, it will now be pretty generally admitted that they should form branches of that general stock of knowledge towards the attainment of which the liberally educated ought

* Histoire de la Médecine, chap. xxxiii.
† Dictionary of Greek and Roman Biography and Mythology. By William Smith, LL.D. Article, Hippocrates.
‡ His admirable edition of the Works for the Sydenham Society.
§ Œuvres Complètes d'Hippocrate. Traduction nouvelle.

to aim; and it may not without reason be presumed that, were some general ideas in regard to the nature and treatment of diseases included in the category, we should find that the gratifying result of an increased reliance upon the skill of the thoroughly educated and competent physician, and of a gradual decline in the hideous refuge of quackery to which so very many presently resort, would follow.

As the father of medicine founded the science of medicine, so may his immediate descendants, more especially Thessalus, Polybus, Draco, and Prodicus, be said to have founded the first medical school based upon rational principles. It has been indifferently called the Hippocratian or the Dogmatic School, the sect of the Dogmatists. Belonging to this school was Diocles, of whom Galen frequently speaks in high terms, and who, along with certain other distinguished cultivators of medicine, is thus alluded to by Celsus:—"Post quem Diocles Carystius, deinde Praxagoras et Chrysippus, tum Herophilus et Erasistratus, sic artem hanc exercuerunt ut etiam in diversas curandi vias processerint."* The two last named physicians were the most distinguished of those who adorned the Alexandrian school of medicine. The death of that renowned king, whose name the city received and still bears, occurred at the early age of thirty-two, about seven years after its foundation, and 324 years B.C. After this event the vast empire of the Macedonian conqueror was dismembered, and in the year 321 B.C., Ptolemy the First, surnamed "σωτήρ," half-brother of Alexander the Great, because son of Philip, became king of Egypt. He was the first of that dynasty of Greek kings in Egypt who conceived

> "Truly a royal idea," says Renouard, "and worthy of the successors of Alexander, that of collecting together all the intellectual riches of the universe, and placing them at the disposal of studious men, who were desirous to use them for their improvement and the advancement of science." (p. 166.)

The library and museum of Alexandria, founded by the first Ptolemy, received under the patronage of his successors numerous valuable additions. The literature of all nations was to be found in the former. The library of Aristotle, at that time the largest private collection in the world, was acquired for it by purchase, till at length, according to Eusebius, the Alexandrian collection contained no fewer than one hundred thousand volumina or rolls. The Ptolemies, owing to the extensive commerce in which they engaged, were enabled to gather together, chiefly from the Indian Ocean and its shores, a multitude of plants and animals previously unknown and undiscovered, and these were submitted to the observation and investigation of naturalists. In this Alexandrian school, which the munificence of the Ptolemies founded and sustained, the science of medicine flourished; and it cannot be doubted that for several improvements we are indebted to its professors.

Anatomy and physiology received an impetus, which, undreamt of previously, must have filled with amazement those who witnessed the change effected, when Herophilus and Erasistratus, the two most renowned of the Alexandrian school, first openly practised the dissection of the human body. Upon the testimony of Celsus, it has been believed in all ages that vivisection was pursued by these two famous physicians. Celsus's words are, "qui (Herophilus et Erasistratus) nocentes homines à regibus ex carcere acceptos vivos inciderint"† By some they have been praised; by not a few, as by Tertullian,‡ who in a curious passage styles Herophilus "ille medicus aut lanius," condemned on account of the barbarity of their procedures. For our own part, we are unwilling to believe that, under the sway of so enlightened potentates as the Alexandrian rulers, such enormities were ever practised. There appears no occasion for regarding a statement resting on the authority of Celsus alone, though mentioned by Galen, and quoted by Cœlius Aurelianus and other authors, as an historical fact. It must be kept in view, that the performance of vivisection was brought as an accusation by Celsus against Herophilus and Erasistratus, leaders of the Dogmatic school—he himself evidently

* Liber 1, Præfatio. † Ibid. ‡ De Anima, c. 10. Ed. F. Oehler. Tome ii. p. 571.

tending to the support of opposite opinions—and that, at a period long subsequent, similar accusations were repeated in the case of more than one of the earlier cultivators of anatomy. In the case of the latter, such accusations were without the vestige of a foundation to rest upon, and if not due to the malice and malignity of enemies, can only be ascribed to the scandal and tittle-tattle of busybodies, whose speeches, even though nowise meant, too often end in the propagation of what is slanderons and untrue. It apperrs to us as by no means improbable that Herophilus and Erasistratus, just as happened to the anatomists in more recent times, may, by the performance of some heroic operations, have given rise to the belief of their being engaged in proceedings which we naturally shrink from laying to the charge of men so deservedly famous in their profession. Concerning the personal history of these, the most distinguished of the Alexandrian professors, we know little that can be regarded as really accurate, but frequent allusions to their investigations, opinions, and practice are met with in Galen, and in Cœlus Aurelianus, who was nearly the contemporary of Galen. The latter speaks in the highest terms of what Herophilus did for anatomy; and while Erasistratus also appears to have been an accomplished anatomist, the fame of his colleague has excelled in the practical part of their common profession.

No period of the history of medicine is more interesting than that of the establishment of the Alexandrian school. An important event occurred in the history of medicine soon after its institution.

"At this time (writes Celsus) medicine was divided into three branches—one which cured by diet, *victu;* another by medicines, *medicamentis;* a third by the hand, *manu.* By the Greeks the first was named *dietetic,* διαιτητικὴ; the second *pharmaceutical,* φαρμακευτικὴ; the third *chirurgical,* χειρουργικὴ."

For the future, these divisions were distinct, and exercised by distinct parties, separate individuals. This was a great change. Previously, the same persons had acted in the capacities of physician and surgeon alike.

Shortly after the establishment of the Alexandrian school—that is, about three hundred and twenty years before Christ—the formation of the two rival sects, the Dogmatists and the Empirics, arose. "Hanc mox secuta est medicorum in sectas secessio," says Blumenbach, in his admirable synoptical introduction to the literary history of medicine, "præsertim in *dogmaticam* quod in medicina exercenda rationi multum tribuebat et *empiricam* quæ fere unice experientiæ confidebat." The real matter of dispute in this controversy, at the time of its origin, and as, somewhat altered, it has occupied men's minds since, and occupies them now, was, "how far is theory to be permitted to regulate practice?" Celsus, in an elaborate passage,* has very candidly stated the case for both parties, though he evidently, as has already been noticed, favoured the empirics. To it, and to the admirable account which Le Clerc has given of the rival sects, we must refer those interested in the inquiry; and further to two very elegant essays, entitled 'The Empiric and the Dogmatist,' by Dr. Thomas Percival.† The establishment and flourishing career of the school of Alexandria forms a very important chapter in the history of medicine. In it, during several centuries, a succession of learned men, not devoted to medicine alone, but to the other sciences as well, was produced, and thus the school became subservient to the advancement of knowledge and learning, and at all events prevented the decay into which, after the decline of Grecian literature, they were in no small danger of falling. But though during centuries the principal seat of medical learning, Alexandria was not the only school. Smyrna, Pergamus, and Epidaurus, during the same period attracted students, and these cities were visited by not a few of the more distinguished pupils of the Alexandrian teachers.

We must now pass very shortly to consider the rise and progress of medicine in Rome. Pliny informs us in terms the most distinct that for more than six centuries Rome was without physicians,‡ an expressian which, while it cannot be

* Liber 1, Præfatio. † Essays. ‡ Liber xxix. chap. 1.

supposed to indicate what it literally means, that no attempts were made to mitigate or to cure diseases, may be accepted as a very explicit notification, on the authority of one very well informed, that during six centuries there were in Rome no individuals eminent for their learning or skill in the healing art, or perhaps, that during that lengthened period, medicine was in no part of the vast territory of Rome regarded as the object of a distinct art or occupation.

By the famous Roman historian, Livy, who was born fifty-nine years B.C., and died A.D. 17, as well as by others, references are made which clearly indicate that in medicine, as in nearly every other subject relating to the arts of life, the Romans copied the Greeks. No better proof of this exists than the history Livy gives us of the introduction of the worship of Æsculapius into Rome. "Anno 234 ante Christi natum," says Blumenbach, "medicina cum reliquis scientiis e Grecia Romam migravit." The first physician who settled and practised at Rome was Archagatus, the son of Lysanias. He was born in the Peloponnesus, and appears to have entered the Eternal City during the consulate of Lucius Æmilius Paulus and Marcus Livius Salinator, in the year 535 from its foundation, or about 219 years before the birth of our Lord. Little is known of this, the first Roman physician. Pliny informs us that the privileges of citizenship were conferred upon him. He is alluded to under the title Vulnerarius, from which we may conclude that he was more devoted to the practice of surgery than that of medicine. Another title, but one by no means so complimentary, was conferred upon Archagatus—namely, Carnifex, murderer or executioner, on account of the unfeeling and even barbarous manner in which he is said to have treated his patients.* The performance of some bold operation may in the first instance have led to the latter epithet being applied to Archagatus, a conjecture all the more likely to be correct when we reflect how common it has been in recent times, and even in our own day, to regard distinguished surgeons as necessarily cruel, or at all events, less humane than other mortals. That their being adepts in surgery should necessarily lead to such an issue, we need not say appears to us a complete *non sequitur.* We learn from a study of the early history of Rome, that the inhabitants regarded with feelings of envy, and oftentimes of hatred, the Greeks who had settled themselves down in Italy, and chiefly in the capital, no doubt like our modern emigrants, for the purpose of "bettering themselves." In the intensity of his dislike to the Grecian settlers, Marcus Porcius Cato, the censor, was at this time particularly distinguished. By Scipio Africanus, on the other hand, they are said to have been encouraged and protected. It has even been affirmed that by Cato the expulsion of the Greek physicians from Italy, just as in modern times the Jesuits have been banished the Eternal City, was meditated, if not actually put into execution.† About a century elapsed before we meet the name of any other physician in Rome who distinguished himself; but about that time we do find one whose influence was widely exerted and proved most beneficial. "Primus vero medicorum Romanorum fama et meritis præcellens Asclepiades erat."‡ This physician was born 91 years B.C., was a native of Bithynia, and originally settled in Rome as a teacher of rhetoric, but being unsuccessful in this walk, at a comparatively late period of life he applied himself to the study of medicine. From what Galen and Cœlius Aurelianus have recorded of Asclepiades, it is evident that he was a man possessed of very considerable talents and understanding. Like not a few practitioners of our own times, however, who, thirsting for professional success, are not very delicate in the means they employ, if only they obtain it, Asclepiades commended himself to public confidence by the very general condemnation—not unfrequently the unmeasured abuse—with which he assailed the practice of his contemporaries, and the disparaging manner in which he spoke of the doctrines of his predecessors—even those of Hippocrates himself. This was

* Plinii, op. cit.

† See Agrippa's Variety of Arts and Sciences, p. 297. For a view of this philosopher's hatred of physicians, see Mr. Moorley's admirable Life of Cornelius Agrippa.

‡ Blumenbach.

the method Asclepiades adopted, and whether or not his success was due to his sagacity, of the fact of his great popularity there can be no doubt. He was totally ignorant of anatomy, and was foremost in proclaiming the inutility of anatomical and pathological investigations. His merits have been very differently estimated by different authors.* There can, however, be no doubt that he was instrumental in advancing the boundaries of medical knowledge. Of him Celsus says, "Asclepiades officium esse medici dicit, ut tuto, ut celeriter, ut jucunde curet," a phrase which may be said to have become proverbial in the profession. He employed the lancet in the acute diseases of the chest which were accompanied by pain, but in those only; he incised the tonsils, and was the first to perform the important operations of laryngotomy and tracheotomy, of which Cœlius Aurelianus nearly two centuries thereafter writes in terms indicating his idea of the extreme rashness of Asclepiades, and further characterizes the operation as one which should never be performed. He it is who first divided diseases under the names of acute and chronic. Another important doctrine of Asclepiades is thus well expressed by Dr. Watson, "the self-limitation of diseases, asserting that the principal cure for a fever was the disease itself." (p. 101.)

The immediate successors of Asclepiades were Themison, the originator of the Methodic school, who held views midway between the Dogmatists on the one hand and the Empirics on the other; Antonius Musa, to whom Pliny† refers; and Cassius, whom Celsus‡ styles "ingeniosissimus seculi nostri medicus." Of these and others we cannot now speak, and pass to a brief notice of him who has been justly designated the Latin Hippocrates. Aulus, or Aurelius Cornelius Celsus, the first native Roman who wrote on medicine, for previously to his time the physicians in Rome who had attained to eminence were Greeks or Asiatics, is most generally considered not to have been a regular member of the medical profession, but as having devoted himself to the study of many things connected with the theory and practice of medicine. Judging from his writings, 'De re Medica,' it is indeed difficult to believe that Celsus was a mere dilettante in physic; but it must be remembered that by his contemporaries his works on rhetoric and agriculture seem to have been equally prized,§ while their author was respected as a most learned man—one who studied natural history in all its varied departments, and attained an excellent understanding in each.

Celsus was, in all probability, born in Rome, though by some Verona has been considered the place of his birth. That event occurred within the first few years of the era, and most likely in the year 4. We have the authority of Galen for stating that Celsus received an excellent education. By the attentive student of his writings, however, no such authority is required: the perusal of these forces the conviction. By some authorities it has been supposed that Celsus acted as secretary to the Emperor Tiberius when on his expedition to the east, and they imagine this view to receive confirmation from the circumstance of Horace, in his epistle to Julius Florus, mentioning the compilations which a certain Celsus made from the library of Mount Palatine:

> "Quid mihi Celsus agit? Monitus multumque monendus,
> Privatas ut quærat opes, et tangere vitet,
> Scripta, Palatinus quæcumque recepit Apollo."¶

They further endeavour to establish the existence of an intimate relationship between Celsus and Ovid. It is greatly to be regretted that all the works of this distinguished author have not been spared to us. At the same time we may truly congratulate ourselves on the possession of the treatise, 'De re Medica, libri octo,'

* By Cacchi (in his Discorso supra Asclepiade, Firenze, 1758) he is styled "Uno dei più excellenti e più fortunati uomini dell' antichità."

† Historia Naturalis, lib. xix. chap. 8.

‡ Celsus: lib. i., præfatio.

§ It has not always been considered so, however. Quintilian makes the following foolish and ill-natured remark: "Quid plura? cum etiam Cornelius Celsus mediocri vir ingenio non solum de his omnibus conscripserit artibus, sed amplius rei militaris et rusticæ etiam, et medicinæ præcepta reliquerit? dignus vel ipso proposito, ut eum scisse omnia illa credamus." Lib. xii. ch xi.

¶ Epistolarum lib. i. 8, l. 15.

and reasonably regard it as a *chef d'œuvre.** Into a discussion of this work, so well known and so thoroughly appreciated, it is here altogether unnecessary to enter. The most interesting portions are undoubtedly those devoted to surgery. Hippocrates and Asclepiades are the two authors to whom Celsus is chiefly indebted. In his system of prognostics,—which, in ancient times, was not employed in the limited sense in which we now use the term prognosis, but was significant of all the phenomena of disease, as the master mind and observing power of Hippocrates had traced them,—Celsus was at one with the father of medicine, also in respect to the nature and treatment of surgical maladies. Many passages on these subjects are literal translations from Hippocrates; and it very probably was from the known respect he entertained for the Coan physician, and the frequent allusion to his writings and quotations from them, that the title "Hippocrates Latinorum" came to be applied to Celsus. The references to Asclepiades, particularly in the earlier books, are frequent. Not unfrequently he differs on important topics from him; but Celsus applies the title of a good author to Asclepiades, and his opinions he evidently respected as those of a sagacious physician. Next to these, Celsus most frequently alludes to the views and practice of Themison, his contemporary, and of the learned and distinguished teachers of the Alexandrian school, Herophilus and Erasistratus. But though evidently an earnest admirer, Celsus was no blind follower of Hippocrates. Like Asclepiades, he rejected the theory regarding critical days—a most important part of the Hippocratic doctrine as respects fevers and other acute diseases; and after giving a short but comprehensive and decided statement, in the third book,† of his own views as opposed to those of the Father of Medicine, he thus sums up:—"Adeo apparet, quacunque ratione ad numerum respexerimus, nihil rationis sub illo quidem auctore (i. e., Hippocrates) reperiri."

At the period when Celsus flourished, surgery had made very considerable progress. Many of those operations which we denominate the grand or capital operations of surgery are minutely described in his pages, and were evidently practised with success in his time. Of late years an additional amount of interest has been thrown upon the subject of the surgery of ancient Rome, from the circumstance of the discovery of numerous surgical instruments in the excavations at Herculaneum and Pompeii. When it is remembered that these Roman cities were at a distance from the capital which cannot have exceeded a few days' journey at the time, and that at the very period of their overthrow (A.D. 79) Celsus was alive in Rome, very probably engaged in writing the treatise 'De re Medica,' it is highly probable, if not absolutely certain, that the same surgical instruments which were in the hands of the practitioners of Rome would be possessed by their brethren then exercising their art at Herculaneum and Pompeii. In illustration of this—a subject to which neither M. Renouard nor Dr. Watson have had their attention called—we must, for the present, merely refer our readers to the labours of the late Professor Vulpes, of Naples, who with untiring dilligence examined all the discovered instruments, and by his well directed efforts has conferred a substantial benefit on the history of surgery.‡ Of the practitioners in Rome contemporary with Celsus, and those who flourished during the first and second centuries, we cannot now write in detail. During that period the capital of the world gave rise to no native author of distinction on any of the branches of the art of healing. The knowledge we possess of the condition of the Roman empire, and of the state of the medical profession in it, as well as more precise information regarding individual physicians and surgeons of the period, is due in no small measure to the illustrious Caius Plinius Secundus, Pliny the elder, who, in his immortal work, 'Historia Naturalis,' has especially made us acquainted with the medicinal plants then in use, the medicines derived

* For a long period the treatise 'De re Medica' was regarded as complete: the celebrated Morgagni, towards the commencement of last century, had the merit of discovering that the fourth book was incomplete, and that the hiatus was of a considerable extent.

† Chap. iv.

‡ Illustrazione di tutti gli Strumenti Chirurgici scavati in Ercolano e in Pompeii e che ora conservansi nel Real Museo Burbonico di Napoli. For notices of this work see Dublin Quarterly Journal of Medical Science, August, 1852; and Edinburgh Monthly Journal, August, 1853.

from the animal kingdom, and further, with very interesting particulars regarding many diseases, commencing, as Book twenty-sixth does, with the affection lichen, or mentagra, which is supposed by some to have been identical with the venereal disease.

Regarding the interval of time which extended from Asclepiades to Galen, about two hundred and fifty years, Dr. Watson well observes that it was "one of the most active periods in the whole history of our art."

> "As such," he continues, "it is more worthy of notice, from the fact that the native Romans were never seriously devoted to the cultivation of the sciences. But quick discoverers of the useful, they knew how to improve upon the suggestions or discoveries of the Greeks. Their immense cloacæ for the drainage of the city,—their public baths,—their care in the selection of sites for new towns, villas, and private residences,—their improvements in architecture, and the domestic arrangements of their dwellings, as set forth by Vitruvius and others, are sufficient to show that the lectures of their Grecian masters on the rules of health had been properly appreciated, and the information thus diffused among them turned to good account." (p. 142.)

During the same period distinguished men were born in various provinces or dependencies of the Roman empire. Of these none has enjoyed a greater amount of posthumous fame than Aretæus, surnamed the Cappadocian, from the place of his birth. Little is known of the personal history of this distinguished physician, Galen, strange to say, being entirely silent regarding him; there can be little doubt, however, that his literary and medical fame was not acquired among the mountains of his native Cappadocia, nor that a great part of his life was passed in Egypt, as Dr. Watson supposes; but that, like Archigenes born in Syria, and Galen in the city of Pergamos, prompted by a laudable ambition, he had early hastened to the capital, "there," as Dr. Adams, his most recent biographer,* has observed, "to try his fortune in the great seat of empire."

As an author, Aretæus is distinguished for the combination of elegance and simplicity. By the moderns his works have been most highly esteemed. Haller seems to assign him even a higher niche than that he considers due to Hippocrates; Sprengel reckons him immediately after the Father of Medicine; and our countryman, Dr. Friend, seems to rank them together. Aretæus was an able defender and supporter, the most able, indeed, of the doctrines of the Pneumatics. The sect of the Pneumatics, originally founded by Athenæus of Attaleia, in Asia Minor, but a practitioner in Rome, centred their belief in the body being constituted of solids, fluids, and pneuma or spirits. Upon the due correspondence and proper relationship of these three constituent elements depended health. Like Aristotle and the Stoics, whose doctrines in the times of Aretæus, patronized by the Roman Emperor Antoninus, were held in high repute, he regarded the "pneuma" as passing from the lungs to the heart, and thence by the arteries distributed to all parts of the system. The heart he regarded as the focus, or central point of the vital force and of the soul. The qualities of the pneuma he looked upon as mainly determining the nature of most diseases—a dense pneuma producing organic obstructions. Thus, in the first book of the treatise on chronic diseases, and in the chapter "περι Σπλῆνος," Aretæus observes, "Even to its upper parts the abdomen is filled with a pneuma, thick, misty, humid in appearance, but not in reality."

About the same time as the Cappadocian, there flourished the author of the only complete treatise on the Materia Medica which has been handed down to us from antiquity—Pedacius Dioscorides. It is true that before the birth of this eminent man there were many physicians and others who had occupied themselves with the examination and investigation of the substances used in medicine; but comparatively little was known before his day, and no exact information had been collected. Prior to Dioscorides there had been, among others, the two Andromachi, father and son, to whom a remarkable prominence is given by Galen. The father was physician to the Emperor Nero, from whom he received the designation ἀρχιατρός,

* See an interesting sketch in the Works of Aretæus, edited for the Sydenham Society.

a term afterwards used in the days of the Emperors to denote a physician of unusual eminence. He must have lived very shortly before Galen, as we find the latter thus writing in regard to him: "Andromachus vir mehercule memorabilis nec multo ante nos natus."* Anazarbia, a town in Cilicia, was the place of the birth of Dioscorides—an event which probably occurred in the latter half of the first century. In all ages the works of Dioscorides have been read and admired. Galen frequently refers to and quotes from them; by Oribasius, a learned physician of the fourth century, they are spoken of in the highest praise. Modern authors, too, have all contributed to do Dioscorides honour. As his works now exist, they contain five books on the Materia Medica, and two separate treatises, in which poisons, wounds inflicted by venomous animals, and their appropriate treatment or antidotes, are discussed. No ancient work on medicine or its allied sciences is mor entitled to our study or regard, and certainly there are none which will more fully reward the attention bestowed upon it.

The condition of medicine in Rome during the second century is best judged by a consideration of the writings of that illustrious man whose genius and fame have secured for him the title of "the Prince of Physicians." Claudius Galen was born in the one hundred and thirty-first year of the Christian era, during the reign of the Emperor Hadrian, in the city of Pergamos, in Asia Minor, celebrated in many respects, but more especially on account of the temple of Æsculapius.

For our knowledge of the personal history of Galen, we are chiefly indebted to himself; he has been his own biographer, and materials ample enough are supplied —not always judiciously,—throughout his writings, to enable us to acquire a tolerably circumstantial view of his life. Into this we cannot now fully enter; a somewhat meagre account of it is given by Dr. Watson. It appears that by his father, Nicon, the dawning of genius in the youthful Galen was very early discovered, and that from the first no means were spared for quickening that promise which was afterwards so gloriously fulfilled. It was from the mere accident of a dream that Nicon changed the profession he had intended the son to pursue, from that of philosophy to medicine, and thus was secured to our science the possession of one of the greatest names which has adorned it in any age or clime. The professional education of Galen was of the most liberal and extensive description; originally conducted in the city of his birth, he seems to have passed from Pergamos to Smyrna, thence to Corinth, and finally to Alexandria, at that time still deservedly eminent in the world of science. After a residence in Egypt, which must have extended over several years, Galen made a journey to Cilicia, Phœnicia, and Palestine, visited Scyros and Crete, and in his twenty-ninth year (A. D. 158) he again reached his native Pergamos.

On the occurrence of a revolutionary movement, probably judging that his position and means of professional advancement would be seriously impaired, and no doubt attracted westward by the fame which certain of the Greek physicians in Rome had acquired, he bent his course to the then capital of the world. Settled there, his reputation immediately began to rise, and during the four years he remained, we find Galen to have been regarded by small minds with that degree of enmity and malignity which has ever been seen in this world's history to be directed by them against those who, whether deservedly or otherwise, have rapidly attained to professional success.† In the work of Galen which we have referred to, abundant particulars regarding his life in Rome are afforded. It was, no doubt, by his successful lecturing and writing, and particularly by the success of his practice, that the envy and ill-will of the Roman physicians were drawn down upon him, and it was their determined hostility which induced him to escape from their machinations and from Rome at the same time. It has been said, indeed the charge has been frequently made, that Galen fled from Rome to avoid the plague, which, originating in the neighbourhood of Antioch in the year 166, and in its pro-

* De Theriaca.

† The language employed by Galen himself in his treatise 'De Prænotione ad Epigenem,' being "Medicorum et Philosophorum in urbe malignitate."

gress ravaging many parts of the Roman empire, finally reached the capital.* There can be little doubt that in the main this accusation, though perhaps receiving some sort of countenance fron what Galen himself says in the 'De Libris Propriis,' cap. i., is groundless. Surely the whole of his after life and the character of Galen sufficiently contradict it. But at the same time, the fact of his leaving Rome just as the pestilence reached it, clearly shows that Galen regarded his life as very insecure in that city owing to the eager rage of his enemies; and further, that his anxiety to revisit Pergamos, now that the revolution which had originally driven him from it was over ("seditione in patria mea sedata"), was also very great.

It was in 167 that Galen left Rome, but he had scarcely settled down to his usual mode of life in Pergamos, before he was summoned to attend the Emperors Marcus Aurelius and Lucius Verus, then at Aquileia in Venetia, the chief bulwark of Italy on the north-east frontier, whither they had gone to superintend the preparations for war with the northern tribes, and had resolved to spend the months of winter. Passing through Thrace and Macedonia, a journey which he performed on foot, for in the 'De Simplicium Medicamentorum Facultatibus,' we read "eamque pene totam pedestri itinere pertransivi," Galen, towards the close of the year 169, reached the camp of the Emperors, but to find it abandoned by both, owing to the pestilence, already referred to as so wide-spread, having broken out with redoubled violence. The Emperor Verus died in the Venetian territory, of apoplexy; Galen accompanied Aurelius to Rome. Again rising high in public favour, and retaining the confidence of Aurelius, he was appointed by the Emperor physician to his son Commodus, then a boy, when he himself left Rome to prosecute his wars on the Danube. About this time two of his principal works formerly commenced were completed—viz., 'De usu Partium Corporis Humani,' and 'De Hippocratis et Platonis Decretis.' Galen once more revisited Pergamos, and finally died at an advanced age; when and where this event occurred is not, however, accurately known. Many authors, and among others Chartier, have stated that Galen, convinced by the miracles performed by the disciples of Christ, embraced the Christian religion, and that animated by the desire to visit the scene where these miracles were performed, he had set out on a voyage to Judea, but that owing to shipwreck or sudden illness, he never accomplished his design.† Such are some of the best authenticated circumstances with which we have become acquainted in regard to the personal history of one of the most illustrious physicians, as he was one of the ablest and most renowned men, who ever lived. Galen did not confine himself to the acquirement of professional knowledge alone: it is evident from his writings that he was a philosopher no less than physician, and he particularly insisted on the necessity of all physicians being acquainted with other branches of knowledge in addition to medicine. Of his personal character, as exhibited in his writings, there is much to admire him for, and there are also some defects, otherwise he had not been mortal, to point out. He was certainly a most accomplished and very learned man, and seems to have been habitually deeply impressed with the gravity and high responsibility of the office of physician. But no doubt he had too high an opinion of his own great merits, and like all such as over-estimate their own good qualities, he expressed himself with a bitterness and contempt, which frequently became untrue besides unjust, of his personal opponents and contemporaries. For this failing on the part of Galen, living at the time he did, and unsoftened, as during the greater part of his active life he no doubt was, by the amenity and genial influence of Christianity, let us not forget that some apology at least is to be found.

The works which Galen bequeathed to the profession are as numerous as they are valuable, amounting to not fewer than two hundred, while the information they contain and learning they display are such as to make it abundantly evident,

* This was the epidemic which Gibbon in his forcible language describes as "attacking with indiscriminate rage every rank and profession."

† See Portal: Histoire de l'Anatomie et de la Chirurgie, vol. i. p. 92.

that of his contemporaries there was no one who equalled him; that of the second century Galen was the *facile princeps* of medical writers and of physicians. The severity of our judgment may be extenuated regarding a man who so vehemently and systematically condemned the opinions and practice of others, when we reflect that, as was the case with Galen, he was immeasurably superior to all his contemporaries, of the "futility of whose reasoning" and "deficiency in their information" he was profoundly convinced. The result of all this was, that Galen attained to a rank in the medical world, and swayed the opinions of physicians and of the public on all points connected with medicine, in a manner before and since unknown. Hippocrates, it is true, had been recognised during his lifetime as the Father of Medicine; but in his day learning was confined to the few, and it required the development of man and his inventions, such as the establishment of the empire of Rome brought about, before a Galen could exist. For many centuries after his decease, the doctrines and tenets of Galen were, as Dr. Bostock well observes, "regarded very much in the light of oracles, which few persons had the courage to oppose."

According to Choulant,* who has made the so-called works of Galen a subject of special study, these consist of eighty-three treatises acknowledged to be genuine, nineteen whose genuineness has with more or less reason been doubted, forty-five undoubtedly spurious, and fifteen commentaries on different works of Hippocrates, while more than fifty short pieces and fragments (many or most of which are probably spurious), are still lying unpublished in the different libraries of Europe. Besides the works now referred to, many other treatises were written by Galen, of which nothing but the titles have been preserved. Dr. Greenhill believes that the total number of the Galenic writings cannot have been fewer than five hundred.† The first edition of the collected works of Galen was published at Venice in 1521, in five volumes folio.‡ The best edition of the works of Galen is that of Kühn, in twenty volumes octavo, 1821–1833.

In judging of the particular views entertained by Galen, it must be remembered, that at the period when he began to study, the profession of medicine was divided by several sects, these all disputing with one another. The Dogmatists and Empirics had long existed, but still the particular views of these two rival parties were upheld with an equal degree of tenacity and keenness to what had manifested itself in the days of Celsus, and before his time. About a century B.C. had arisen the Methodic sect, while very shortly before the time of Galen there had been established the Eclectics, Pneumatics, and Episynthetics. What is important in connexion with Galen to note is, that, unlike all the physicians of Rome who were his contemporaries, unlike all who had preceded him, he attached himself to none. "Nullius addictus jurare in verba magistri," he chose from the tenets of each what he believed to be true and most generally useful, but in no way did he connect himself with the Pneumatic or the Eclectic school, with the Dogmatists, Empirics, or Episynthetics. So far, however, as his general principles of professional action are concerned, Galen may truly be considered more of a Dogmatist than as belonging to any of the other sects. Of Hippocrates, Galen was a great admirer, yet he was no blind copyist, frequently differing from him, generally in no empty or vain spirit, assigning a reason, and in the majority of instances a legitimate one, for the variance. He despised those persons who attached themselves to any particular master. If we judge from the meagreness of his anatomical descriptions, and from the circumstance of his never referring to the dissection of the human body, we are entitled to conclude that this was a branch of professional inquiry which Galen had no opportunity of practising. Monkeys and other animals he did frequently examine anatomically.

Erasistratus and the disciples of the Alexandrian school conceived that the arteries contained no blood, but air; Galen, by experiments happily contrived and

* Handbuch der Bücherkunde für die Aeltere Medicin.
† See his most interesting account of Galen, in Dr. Smith's Greek and Roman Biographical Dictionary.
‡ See Blumenbach, op. cit.; Haller, Bibliotheca Anatomica, tom. i.; and Dr. Greenhill, as above.

executed, determined that the arteries contained blood, and blood alone. "Ubi funiculo dissectam arteriam utrinque ligavimus, et quod in medio comprehensum fuerat incidimus, sanguine plenam ipsam esse monstravimus." This, then, was a most important discovery, and equally important and interesting is the account Galen gives of the function of respiration. Mons. Flourens has well observed in regard to it: "l'idée de Galien était un progrès, et tellement un progrès que, sur ce point, la physiologie tout entière n'a pu en faire un autre que par le secours de la nouvelle chimie."* The pathology of Galen though by no means so perfect as his physiology, is still worthy of attention; like the Father of Medicine, he supposes that in the fluids the primary cause of disease existed. As his foundation of reasoning, he adopts the doctrine of the four elements. The causes of disease he regarded as remote or at hand, and of these he gives a description not unlike our predisponent and excitant causes. The superabundance and the degeneration, but especially the putridity of the humours, Galen looked upon as the grand first causes of diseases. His practice was founded upon his pathology, and partakes of its excellences and errors.

Many of the cases recorded by Galen, though falling short in the interest excited by the simple and genuine manner of those detailed by Hypocrates, are instructive in no small degree. In particular diseases Galen adopted special modes of practice; but as a general rule his regimen, diet, and great part of his employment of medicines, resembled the plan followed by Hippocrates. At Pergamos, Galen appears to have acted both as physician and surgeon; but "in Rome he did as Rome did," and in conformity with the usage of the physicians of the Capital, abstained from the performance of surgical operations. There is something extremely interesting in observing how very similar our notions and actions in this repect are with those entertained and practised sixteen hundred years ago.

In regard to Galen it may be said, as was truly affirmed of Hippocrates, that his amazing superiority to his contemporaries for a very long period repressed any attempts at further improvement in the science and art of medicine. The names of some of those who flourished at the same time as Galen have reached us; and the learned Le Clerc, and in some measure too the erudite Sprengel, deserve no small amount of credit for rescuing from oblivion those of not a few which would otherwise have perished. But in judging of the merits of men—and this is perhaps truer of our profession than of any other, though true of all—we are too apt to consider those only who have been great authors, while unfortunately it has not unfrequently happened, and happens still, that the man who has seen most, and is best able to write because he has most to write about, is summoned hence ere posterity are favoured, or even contemporaries have profited, to the extent they might have done; this is a point which should not be forgotten by the student of the History of Medicine.

With a brief notice of the Greek writers subsequent to Galen, and an interesting chapter on the Laws and Customs of the Roman Empire in Relation to the Medical Profession, the work of Dr. Watson is for the present brought to a close. From his preface we are led to expect a further contribution to the History of Medicine—this time among the Arabs and in the schools of the middle ages. We can only say that we shall be glad to welcome any additional fruits of his labours, feeling satisfied that these will be the result of a careful and reflective study; for though in the volume before us there is no distinct manifestation of a profound acquaintance with the ancient authors themselves, there is abundant evidence of Dr. Watson's familiarity with the writings of both ancients and moderns who have devoted attention to the History of Medicine.

In noticing a work of merit and excellence such as Dr. Watson's it is a pleasing duty to conclude without a single word of adverse criticism, and we shall not deny ourselves that gratification; at the same time we take the liberty of counselling a more exact revision of his proofs by Dr. Watson in the event of a second edition being called for. The references to authors are oftentimes inaccurate: for

* Histoire de la Découverte de la Circulation du Sang, p. 5.

example, at foot of page 13, "Plinii Historia Naturalis, lib. xxix., cap. vi–viii." is cited; but in the 29th book of Pliny there are only six chapters! the reference should be lib. xxix., cap. i. Again, at page 149, it is annoying to find Satyrus spelt *Satyrius*, and the well known-instructor of Galen at Corinth, Numesianus, appearing as *Normisianus*; while worse than either, Polybus, the distinguished son-in-law of Hippocrates, is throughout the book styled *Polybius!* and might thus be confounded with the eminent historian of that name.

Of the book of Renouard, and the manner in which Dr. Comegys, the translator, has executed his task, we have already expressed a very favourable opinion.* Had our space permitted it, we should have been glad to have entered upon a fuller consideration of some of the more important doctrines which at various times were supported in the different schools of medicine, and to which in this article we have scarcely been able to do more than to allude. As regards these, the most suitable place for their discussion is no doubt the lecture-room; and we hope before long to find that in every important school of medicine in this country, opportunities will be afforded to students whereby they may be enabled to attain some knowledge at least, of the history of that profession to the practice of which their lives are to be devoted.

REVIEW VII.

Clinical Lectures on Certain Diseases of the Urinary Organs: and on Dropsies. By ROBERT BENTLEY TODD, M.D., F.R.S., Physician to King's College Hospital. *London*, 1857. Small 8vo, pp. 435.

IN our day much is done, laudably done, and well done, by aspirants. The goddess who presides over medical affairs in London will not be approached by any who bring not in their hand some votive offering, as a testimony of their ability and diligence. It may be a heart, a lung, or a kidney, nor will she disdain an uterus, liver, or stomach. Something her worshippers must do to attract her regard, and obtain at least permission to be distinguished from the *ignobile vulgus*. And for their future it is much the same. The same efforts, the same offerings, are needful to obtain her further and higher favours as were required for the first. Ἐργάζεσθε is her response to all the petitions that daily beset her. And of a verity the oracle is obeyed, and on all sides work is done, and by very many right good work, that bears well-deserved golden fruit. But apart from these, though once of them, are a few, the elect (not those of the College of Physicians), who are accepted and privileged. To them the gates of the golden shrine are open, they enter when they will, and on them the goddess sheds her choicest favours. They have attained above the region of strife and contest, and walk (or rather *drive*) in calm and secure dignity!

> "Perfecto munere Divæ—
> Devenere locus lætos, et amœna vireta
> Fortunatorum nemorum, sedesque beatus."

Now, to the praise of some of these, it is to be said that they still pursue their labours, though for their own sakes unneeded. They use their advantages, and improve them, more for the good of others than their own. Their work is from the purest motive, and can scarcely fail to be of the highest order.

Among these distinguished worthies, Dr. Todd holds his place most deservedly. He has steadily laboured for the promotion of rational medicine, not only by his personal exertions, but by calling forth and encouraging those of others. It is no

* Dr. Comegys also has allowed many errors of the press to escape his notice. At page 39 there is a very stupid one. Cleyer specimen *Medicæ Siniæ*, should be Cleyer specimen Medicinæ Sinicæ: and at p. 42, the author's name Cleyer appears as Fleyer. Many others might be noticed.

small merit that the 'Cyclopædia of Anatomy and Physiology' owes its existence to him: it required no little endurance and effort to bring this great work to a conclusion. The Todd-Bowman (as Germans call it) Text-book of 'Physiological Anatomy' was a work of love for one of the principal sciences on which rational medicine relies, which does its authors the greatest credit. To it the rising generation of medical men will owe in great measure a familiar acquaintance with all the chief truths respecting the healthy structure and working of the frames which are to form the subjects of their care. The possession of such knowledge will do more to make sound and able practitioners than anything else. For his share in these two undertakings, now happily completed, in which self-interest could have been little, if at all, concerned, we think Dr. Todd deserves the best thanks of his generation. Of his other well-known labours we cannot say anything now, but must hasten on to the especial consideration of the last, whose title forms the heading to this Article.

Remembering that a *preface* is really a *postscript*, we shall defer the consideration of some admirable suggestions contained in this part, until we have given our readers some idea of the contents of the volume. The first two lectures are devoted to the subject of Hæmaturia; the third and fourth to that of Renal Disease attended with Albuminuria and Dropsy; the succeeding seven to Dropsy of various kinds; and the remaining five chiefly to Gout, its various manifestations and treatment. It seems to us that Dr. Todd has done very wisely in taking such prominent and visible symptoms as hæmaturia, dropsy, and gout, as texts for his discourses. The object of clinical teaching is to impart to the student facility in dealing with the problems of disease which are ever coming before him. It is therefore a great advantage to him to have large and striking phenomena so opened out and explained to him in all their possible causes and relations, that whenever and wherever he meets them, they may have to him all the significance that really belongs to them. The mind is so prone to take partial and narrow views of the objects it contemplates, that it needs frequently to have them set out before it in a comprehensive manner. It is the privilege of genius, especially when aided by experience, to put common and trite things before our mental view in a fresh manner, so that we come to see in them more than we did before, and to see in clearer light what we saw before more obscurely. The good old parable of Eyes and no Eyes, is continually verified in medical life. One man sees in disease only dim, misty images, representing to his mind some name about which he has read and been taught. To another, each form of morbid action stands out sharp and clear—if not in all its lesser features, at least in the main outlines. It is a great benefit to the student to be led by one who himself sees clearly.

Dr. Todd notices the occurrence of hæmaturia in connexion with rheumatic fever and pericarditis; in acute renal dropsy; in an inflammatory state of kidney: in scarlatinal dropsy; as vicarious of catamenial flow; as concurrent with phlegmonoid inflammation of the kidney; as dependent on renal calculus; as associated with inflammation and ulceration of the ureter; in connexion with erysipelas of the fauces; as dependent on fungoid disease (cancer) of the bladder; and as an endemic affection of the Mauritius. Other conditions giving rise to hæmaturia are incidentally referred to also, so that on the whole the two lectures afford a very good guide to all the various diseases in which this symptom may arise. The diagnosis between renal and vesical hæmorrhage is well stated, and the difficulty of determining in many cases whether a calculus is the cause of the bleeding or not. Dr. Todd states that "by far the most common cause of bloody urine is the disturbance of a calculus formed and lodged in the kidney." This we incline to doubt, as well from the results of various cases in which the symptom has disappeared under treatment, as from the following positive evidence:—In 100 post-mortem examinations, where the history of the symptoms during life was obtained, hæmaturia is noticed as having occurred in 5 only. In one of these it was from purpura, in a second from diffuse cellular inflammation, in a third from morbus Brightii (large kidney), in the fourth from cardiac and renal disease, and in the fifth from scrofulous renal disease. In not one of the hundred was there renal calculus. Of 18 cases occurring under our own notice, in which hæmatura was a notable symptom, the attendant conditions were as follows:—In 4 scarlatina; in 4 chronic renal degeneration; in one of these, pregnancy acted as a special promoter of the hæmaturia; in 1 purpura and debility; in 1 the patient suffered from the endemic of the Mauritius, passing the so-called chylous urine; in 1 the urine was of high specific gravity, and deposited oxalates copiously: there had been severe aching in the loins for years; very great benefit was obtained by the steady use of nitro-muriatic acid and liq. opii. It is possible there was calculus in this case, but it is certainly doubtful. In 1 case the hæmorrhage came on after a fall on the back, and continued more or less for many weeks, in spite of the recumbent posture and remedies. There did not appear to be any sufficient ground for believing in the existence of a calculus; it seemed more probable that there was renal degeneration in progress, and that the tone of the Malpighian tufts was notably impaired. In one case the attendant symptoms were those of nervous depression, evening chills, &c. Under gallic acid, quinine, and opium, the hæmorrhage speedily ceased, and the general health improved. In one case orchitis was the first affection, which was replaced by an inflammatory affection of the kidneys, and this again was subdued easily by cupping and salines with antimony. The urine did not lose its smokiness, however, until after quinine had been administered. In one case there were vesical symtoms, and reason to suspect strongly the existence of malignant disease; the patient had had a tumour removed from the left breast four years previously. In one case there was great vesical irritation, but a cure was effected by copaiba and gallic acid; the disorder appeared to be catarrhal. In one case the hæmatura, which had resisted astringents, became immediately very much diminished as soon as calomel and colchicum were given, so as to produce a cholagogue effect, and entirely disappeared for some time under the additional use of nitro-muriatic acid and liq. taraxaci. In the last case the hæmorrhage was decidedly renal. It ceased under the use of strychnia, and subsequently chlorate of potassium with pulv. jalap. co. at intervals. There was some dropsy and a patch of erythema nodosum, but no material impairment of the general health. The hæmaturia appeared to result from a congestion of nerve origin—paralysis of the renal plexus. The conclusion from the above evidence must, we think, be counter to Dr. Todd's. Renal congestions of an acute kind are evidently the principal causal conditions of hæmaturia. Considering the peculiar disposition of the blood-vessels of the kidney, one would expect *à priori* that bleeding from this organ would be very liable to occur—that, in fact, it would be more common than epistaxis. No doubt both hæmorrhages may result from similar causes.

Dr. Todd frequently alludes to the elimination of various morbid matters through the kidneys, and the irritation of these organs thereby, thus explaining the congestions of acute and scarlatinal dropsy. We wish to take some exception to this popular doctrine of poisons being carried to special organs, which then undergo irritation in their efforts to carry them out of the system. That a poison is received into the blood in such cases is pretty certain; that it causes inflammatory irritation in various parts is also certain; but that this indicates a necessary and real eliminative action seems to us very doubtful, and rather likely to mislead us in our practice. Turpentine passes off very positively by the skin and lungs, which it does not irritate, as well as by the kidney, which it is apt to irritate. Cantharides act quite as much on the bladder and genital organs as on the kidneys. Are we to suppose that the pulmonary inflammations of measles and hooping-cough, or the cutaneous eruptions of small-pox and scarlatina, or the intestinal exanthem and ulcerations of typhoid fever, are in any more than a faint and superficial resemblance eliminative actions? Are we to think otherwise of the serous exudations of pemphigus and eczema? Surely, if so, then much more the sweats of an ague are eliminative, and ought to be greatly promoted as a means of getting rid of the malarious poison. In our belief, the various inflammations of the exanthemata are simply reactions of the tissues in which they occur against the poison, and if they did not occur, so much the better; so much the more chance would there be of the poison being quietly eliminated through some other channel. Take the case of a family among whom scarlatina makes its appearance. All have been alike exposed to the poison, all must have imbibed it: but in some it produces slight, in others deadly effects. What is this, but the tissues of some resenting the poison violently, of others but little? Some persons have an immunity towards one or other of the exanthemata, though the poison enters their blood when exposed to contagion (as by the laws of gaseous diffusion it must), the system does not succumb under it; it is, in fact, not a poison to them. This is particularly the case with infants as regards scarlatina. How striking is the now ascertained fact, that eight grains of quinine daily will preserve a man in health amid the deadly miasmata of African rivers! From all that we know of the nerve-toning action of quinine, and the similar beneficial effect of generous diet, and a cheerful animation, in preserving soldiers and sailors from sickness, it seems impossible to doubt that these and all like means act by increasing the resisting power of the system, not by neutralizing the poison which surrounds it. To the same purpose is the striking case quoted by Dr. Watson (vol. i. p. 708).

The practical lesson of all this is very important. It tells us that we are not to think of getting this mischievous agent eliminated, as the only means of restoring our patients, but that we are just to obviate as far as we can the morbid action that the poison has set up. If the system is labouring under variola, we do not encourage the eruption on the skin, and congratulate our patient the more abundant the pustules are. On the contrary, Dr. Watson says, "The object is to prevent if possible a copious eruption, upon which, as we have seen, the severity and peril of the disorder entirely depend." The treatment, then, of all such like cases is to be simply that of inflammation and congestion, modified according as the affection is sthenic or asthenic. An excellent instance of judicious management of the latter state is given at p. 50, where a man with severe erysipelas of the fauces and hæmaturia, is restored to convalescence in ten days—not by sweating or purging, but "by the free administration of quinine and beef-tea by the rectum, wine, and the local application of nitrate of silver to the throat."

Dr. Todd regards the instances of dropsy concurring with albuminuria as naturally dividing themselves into two great classes: those in which dropsy is an urgent and prominent symptom, and those in which it is much less so. The causes giving rise to the first class are exposure to cold and wet, and scarlatina. Those producing the second are various, but are chronic in their action. The following table, given at p. 104, sets forth the author's views most clearly:

"A. Cases in which dropsy is urgent and acute, and albumen abundant.	Acute dropsy. Dropsy after scarlet fever.	Acute enlargement of kidney.
B. Cases in which dropsy is not a prominent symptom—is very variable in amount—chronic—and may be absent. Albumen variable.	Chronic enlargement of kidney.	Fatty disease (Bright's kidney). Waxy disease.
	Chronic contraction of kidney.	Chronic nephritis, or chronic wasting kidney. Gouty kidney."

After giving the histories of a case of fatty, and one of waxy, kidney disease, the diagnosis of these states is thus described:

"The contracted state is much more frequent than that of enlargement, whether fatty or waxy. The fatty condition is generally attended with dropsy, much greater in amount and of a more persistent and obstinate character than (in?) either of the others, which, unless accompanied by some diseased or enfeebled state of heart, are frequently quite free from dropsy, and generally have it only to a slight amount. The presence of abundant fat-cells, fatty casts, and free oil, with albumen in large quantity, in the urine, would indicate fatty kidney, although, for a time at least, such a state of disease may exist without these appearances; whereas, in the contracted kidney, fat-cells or fatty casts are either not present in the urine, or exist in but small number, and only occasionally, and in its more advanced stages, and the albumen is never by any means so abundant as to render the urine nearly solid under nitric acid and heat.

"The waxy kidney exhibits clinical phenomena sufficiently distinct from those of the fatty disease. Instead of the white anæmic complexion, with puffy face, which accompanies the latter malady, you will find the patient looking sallow, and, generally speaking, free from any swelling of the face. Dropsy either does not exist at all, or is very trifling. It does not show itself until the disease has advanced considerably, and it rarely, if ever, is so prominent and chronic a symptom as in the fatty disease, nor is it often as much as in the contracted kidney. . . . In most of the cases the peculiar waxy degeneration is not limited to the kidneys, but affects the liver and spleen, causing enlargement of these organs. The increased size of these viscera, therefore, becomes an aid to the diagnosis of this affection, in addition to those signs which may be obtained from the altered urinary secretion.

"The condition of the urine resembles that found in the waxy (?) disease as regards the quantity of albumen, which is generally large. But fat-cells are not found, nor the fatty casts; transparent fibrinous casts and the *debris* of epithelium are the most common appearances. But these may be absent: and in both forms of enlarged kidney this absence of all sediment is not uncommon." (pp. 105–107.)

With regard to the contracted kidney, Dr. Todd remarks:

"The different varieties of contraction of the kidneys are due, so far as our knowledge at present enables us to state, to one and the same pathological condition, ... and the *rationale* of the morbid process by which the contraction is effected may probably be explained in some such manner as the following:—Some causes or other come into operation which excite disturbances of the nutritive processes to a greater or less degree, and interfere with the normal development of the blood, this fluid becomes contaminated, and some or all of the contaminating ingredients are conveyed to the kidneys to be eliminated by these organs. In their passage through these glands, these poisonous elements create a highly disturbed state of their nutrition—a state, possibly, in some degree inflammatory, but chiefly atrophic, the tendency of which is to cause the organs to waste and shrink. The kidneys, thus injured, are rendered unable to carry off in due quantity some of the elementary constituents of the urine; and these, accumulating in the blood, become a further source of mischief, in fact, a further source of poisoning, not only to the kidneys, but also, secondarily, to almost all the other organs of the body.

"Now, one state in which this train of symptoms is very apt to occur, is that condition of the system which we call *gout;* a peculiar state, in which some morbid material—uric acid, perhaps, or some compound of uric acid, or, at all events, something very nearly allied to this substance—becomes developed in abnormal quantity in the blood, operates as a poison upon the joints, and likewise irritates the kidneys, and thus tends to keep up a gradual process of retention of morbid matters in the system, which ultimately leads to the destruction of these organs. You will not suppose that I limit the causation of this contracted state of kidney solely to gout: there are many cases in which we find no trace of gout; yet there is a general constitutional condition, analogous in many respects to that which gives rise to gout, where the assimilative processes are much at fault, and where the blood is ill-supplied and poor." (pp. 107–111.)

The term "waxy," we think, is not the best to apply to the form of disease it is intended to designate. We are familiar with the appearance of the so-called material, but are quite unable to imagine in what respect it is like wax. It is very much like bacon, cooked, the fat being then translucent; and therefore we have always used the German term *speckig*, or bacony, in speaking of it. The characteristic of this deposit is, that it is always unorganized: is shows no cells, no fibres, but looks under the microscope like fragments of clear stiff jelly, with a vitreous fracture. It always indicates a grave deterioration of the nutritive processes, and may be produced by various debilitating causes, of which the scrofulous habit is one. After a good deal of study of degenerative disease, we have been led to classify the various forms in the following way. There appear to be two principal ones: the first in which organs enlarge by the deposition of cacoplastic matters in their substance; the second in which they simply atrophy and shrink. Of the first, we have examples in hypertrophy of the brain, in scrofulous enlargement of lymphatic glands, in hypertrophic cirrhosis of the liver, and in bacony-deposit affecting the liver and spleen. Of the second, we have examples in certain softenings of the brain, where there is local decay of the tissue, in the contracted kidney, and the small fatty heart. In some cases the stomach-tubes are involved in a quantity of nucleated fibroid tissue, amid which they degenerate; in others they undergo simple wasting of themselves. The character of the first of these two forms of degeneration is *perversion* of the normal nutritive force of the part, so that it either turns good plasma into abnormal structure (low fibroid), or allows deteriorated plasma to be deposited, and to accumulate. The essential character of the second is simple *decay* and *loss* of assimilative force. In both of these forms of degeneration the wasting part may contain more or less of oily matter. Either the cacoplastic deposit may change into oil, by adipocerous transformation, or the normal tissue as it perishes may be replaced by the same. No doubt the presence of oil indicates a difference in the nature of the morbid change, but not, we think, a very important one.

It does not very clearly appear to us why it is necessary to assume that, in the case of the contracted kidney, the first step is contamination of the blood, poisonous matter from which seeking to be eliminated by the kidneys disturbs their healthy nutrition. May not failure of the vital power of the kidney be simply the whole of the evil? may it not be quite possible that the organ undergoes atrophy and wasting, just as the muscular tissue of the heart often does, or the supra-renal capsules more rarely? We have seen patients die of mere anæmia—of a condition which appeared, as far as one could judge, to be simply degeneration of the blood-cells. Why in such like cases should we go beyond what we are sure of, and advance into the regions of hypothesis without having adequate reason for so doing? Each organ has a life of its own, and there seems no room to doubt that this life may fail or deteriorate as a primary change, not due to any prior disorder elsewhere. To take the case of gout,—Dr. Todd says that uric acid, or some compound of it, "becomes developed in abnormal quantity in the blood," irritates, and ultimately disorganizes the kidneys. But why does it come to be in abnormal quantity in the blood? We greatly doubt that it is because it is formed too rapidly in this fluid, in consequence of errors in the diet, &c. Its amount in the urine is so small, and is liable to vary so much, that it cannot be thought improbable that the kidneys, *if sound*, might easily do a little extra duty in the way of excreting it. Dr. Garrod tells us that uric acid accumulates in the blood, because its quantity is diminished in the urine, because it is not excreted by the kidneys. Before the gouty paroxysm, the amount in the urine was not more than one-twelfth the healthy mean, and in chronic gout, with tophaceous deposit, the uric acid was always deficient in the urine, both absolutely and relatively to the other organic matters, and was always present in the blood. Surely, then, in the case of the gouty kidney, it is not the morbid blood that spoils the kidney, but the failure of the renal function that spoils the blood.

Lecture V. contains a good *résumé* of the known facts relating to dropsy. We shall only remark on one point respecting which we can offer some observations of our own. Dr. Todd justly observes, that "the limb in hemiplegia which has suffered most in its nervous power, is in general that which exhibits the greatest amount of dropsy." In a case of general paralysis now under our care, there was very notable dropsy of all the limbs, the urine not being albuminous, nor the heart diseased. This dropsy very materially diminished under the use of iron and quinine. In another case, that of a lady who had long been the subject of chronic aguish disorder, with innumerable neuralgiæ and neuroses, there was most marked puffiness of the hands and feet, so much so that sometimes she could not put on her laced boot *in the morning*, though she could later in the day. Here also there was no renal or cardiac disease. The only cause for the dropsy in both these cases appeared to be debility of the vaso-motor nerves.

Lectures VI. and VII. are devoted to scarlatinal dropsy. The author expresses his opinion that the dropsy is not fully developed without the concurrence of the three following conditions:—1st, a peculiar irritated state of the kidneys; 2nd, an analogous morbid state of the skin; 3rd, a certain depravity of blood, which is not only deficient in its proper constituents, but likewise contains morbid poisonous ones. "If any one of these is absent, you may have a threatening of the dropsy, but the full result does not follow." Granting this, though we are not prepared to give our entire assent to it, and how shall we reconcile it with the theory above enunciated, of the production of this dropsy? If when the peculiar conditions of the blood and skin are present, the kidneys *can* be healthy (see p. 157), then surely the passage of the scarlatina poison through these organs cannot be the cause of their inflammatory condition, and the consequent dropsy. In his directions respecting the treatment of this affection, Dr. Todd's practice is dominated by his view of the necessity of eliminating the poison, we think unduly. He objects to local bloodletting because the poison is there irritating the kidneys, and detraction of blood will not take it away. Purgatives and diaphoretics are his main confidence, and diuretics of an unirritating kind. His treatment is "not antiphlogistic, but calmative and eliminatory." The use of port wine is frequently attended "with most signal benefit" (of course in cases that need a stimulus). The directions given by Dr. West, in his excellent lectures, differ materially from those of Dr. Todd, and in some respects we prefer them. He thinks tartar emetic a very valuable remedy, and the abstraction of blood from the arm in severe cases indispensable. Dr. Copland also is in favour of bleeding, and says that it "is not unusual to find the sequelæ of scarlatina to require, and the patient affected by them to tolerate, the bleeding more than in any of the previous stages of the malady." In our opinion, antimony given decidedly and early, will generally control effectually the morbid action in the kidney when it is of sthenic type. When this state has passed away, or when it is asthenic from the outset, we shall find the ferri pot. tart., combined with potass. acetat., or small doses of tinct. ferri. muriat., efficient remedies. They tone the relaxed vessels, improve the quality of the blood, and act as diuretics at the same time. The principles of treatment are in fact those of inflammation generally; the local afflux of blood is to be stayed, and subsequently, if hyperæmia persist from relaxation of the arterial coats, their tone is to be aroused by the usual agents. We quite agree with Dr. Todd as to the injurious influence of scarlatina on the blood globules: the anæmia of the dropsical from this disease is certainly very marked.

Lecture VIII. is on the subject of acute renal dropsy. We are glad to see that Dr. Todd does not consider this, or scarlatinal dropsy, or any similar state, as any *necessary* precursor of either form of degenerative disease, as Frerichs does. In a note at p. 110, he disallows the term chronic nephritis, applied by Dr. Johnson to the pathological state connected with the wasted granular kidney. He observes correctly, "the evidence of an inflammatory process having any share in the production of this state of kidney, appears to me very unsatisfactory:" and so it has always seemed to us.

Lecture IX. illustrates cardiac dropsy. We could have wished that Dr. Todd had here laid stress on a point which seems to us of the very highest importance, but which is very much neglected or uncared for. It is the absolute need there exists for maintaining a steady, upholding, tonic treatment in all cases of cardiac disease, where there is a tendency to dropsy, and where the power of the heart is inclined to fail. It is lamentable to see a patient leave a hospital just cleared of his dropsy, and feeling comparatively well, and to know that no attempt is to be made to invigorate the system, and especially the enfeebled organ whose imperfection constantly tends to reproduce the symptoms. It is now nearly four years since we took under our care a discharged soldier, who could not walk more than a hundred yards without being exhausted from dyspnœa. He had hæmoptysis, cough, and the physical signs of considerable cardiac dilatation, mitral and tricuspid insufficiency. Under treatment by tinct. ferri. mur. and ol. morrh., long sustained and repeated at need, he continued, though in very poor circumstances, to act as a messenger, and afterwards as a light porter in a shop, till January of this year. He has walked three or four miles a day, carrying a pretty heavy load.

Lectures X. and XI. treat of ascites. Cases are related illustrating its production by enlarged and by contracted livers (Glisson's capsule being thickened in both), by omental cancer, subacute peritonitis, and renal disease. Dr. Todd thinks it is "highly probable that the enlarged liver with thickened Glisson's capsule, is a different disease from the contracted liver." According to our observation, the difference is merely one of more or less fibroid formation in the portal canals and interlobular spaces. There may be a very great quantity of new-formed fibroid tissue, and then the bulk of the liver is enlarged, or there may be very little indeed; but that little may contract strongly, and compress the small portal veins. The directions respecting the operation of paracentesis are highly judicious—they relate to the adoption of the recumbent posture, the exhibition of opium, and the leaving the bowels quite quiet for several days. Dr. Todd advises that the operation should not be postponed too long, as there is then less probability of the system being able to "resist inflammation or withstand exhaustion."

Lecture XII., on the gouty kidney, contains several interesting cases well worth perusal, but not requiring any special notice.

In Lecture XIII., seven pages are given to direct us how to distinguish pus in the urine from other deposits. Surely it would have been better to say in as many lines that microscopic examination and the nitric-acid test, with heat, are fully sufficient. In this lecture and the succeeding, the subject of gouty inflammation of the bladder is very ably treated and illustrated. The remarks which Dr. Todd makes respecting the various modes in which gout affects the bladder, are very instructive and important. 1. It may cause inflammation of the mucous membrane. 2. It may render it very irritable, and so cause incontinence of urine. 3. It may affect the muscular coat, so as to paralyze it and occasion distention of the bladder. 4. It may cause violent pain in the region of the bladder. We feel some doubt whether the last mode can always, or often, be very decidedly attributed to gout. It has certainly happened to us on several occasions to meet with a very similar state in persons of a neuralgic habit, in whom we regarded it as a local affection of the same kind, whose exact locality could scarcely be determined, whether in the abdominal parietes, or the peritoneum, or the viscera. Of the second mode we met some time ago the following marked instance:—A gardener, aged fifty-seven, who had suffered seven or eight times from gout, but who had always had, as he stated, "the best of health," complained of complete incontinence of urine, and great emaciation. The bladder was not distended; the urine was pale, not albuminous, over acid, depositing uric acid in plenty spontaneously. A great variety of treatment was tried without effect, until the right hand became affected by gouty inflammation, whereupon the vesical disorder immediately ceased. Several interesting cases of sacculated, pus-secreting kidneys, conclude Lecture XIV.

Lectures XV. and XVI. treat of gout. Dr. Todd cruelly destroys the solace of those who are thus afflicted, by tracing the origin of gout chiefly to *beer*—"that ignoble source!" Alas! that Sydenham's goodly list of "magni reges, dynastæ, exercituum, classiumque duces, philosophi, aliique his similes," should come to be put in the same category with "coalheavers, bakers, brewer's-draymen, house-painters, and others of the working classes." Verily gout, like "Pallida mors, æquo pulsat pede pauperum tabernas, regumque turres." This at least in England; but it seems that in non-beer drinking countries the lower classes escape. The features of distinction between gout and rheumatism are delineated in a masterly manner; we would, however, that Dr. Todd had dwelt rather more on the means of distinguishing between the more obscure and chronic forms of these diseases, than between the more strongly marked and febrile. We cordially agree with Dr. Todd in his condemnation of active antiphlogistic treatment of gouty affections—at least in Londoners; it creates, says he, asthenia, and "asthenia gives to both rheumatic fever and gout what I may call *the shifting character*, which in both diseases is most perilous, but in the latter especially so." With regard to treatment, it is rather refreshing to find some novelty in the proceeding recommended by one of so much experience, even though we may hesitate to agree with it. Dr. Todd is decidedly opposed to the use of colchicum; not that he doubts its power, "in sthenic cases in young subjects," of relieving pain and hastening the removal of the paroxysm, "but at the same time experience leads him to subscribe to a belief very popular among gouty patients, that if it shortens the duration of the attacks, it likewise shortens the interval between the attacks. We are far from believing that colchicum acts in any way of neutralizing the gouty poison, any more than quinine the paludal. The latter tones and fortifies the nervous system, so that it can resist the malarious influence; and the former acts as a contra-stimulant on the articulations, nullifying acute inflammatory afflux when set up in them by rheumatism or gout. At the same time, colchicum tends to increase the bile-flow and the intestinal secretion, and if judiciously combined with purgatives, it seems to us highly reasonable to expect good results from its use. These, however, more particularly relate to its use in the paroxysms, the averting of which must depend on the kidneys and other excreting glands being aroused to function properly. Small local blisters are the best means, according to our author, of relieving the articular inflammations, and they may be used even during the height of the disorder. Nothing is said of the application of pure spirits of wine to the parts in the acute paroxysm, as practised by Dr. Goolden, or of the old remedy, soda poultices, in the more chronic form.

Lecture XVI. relates a case of chronic and asthenic gout, with chalk-stone deposit in the joints, and bronchial and gastric gouty affections. This needs no particular observation, except that we should like to have heard if Dr. Todd had verified the observations of Dr. Budd relative to the conversion in many cases of uric acid into urea in the vicinity of the inflamed joints. The alcoholic extract of blister serum was found to become a solid mass of nitrate of urea when treated with nitric acid.*

Having passed the lectures in review, we would now return to the consideration of the preface. The remarks contained in it are most sound, manly, and needed. They are the voice of a man who has strong sense, and the courage to speak out. They relate to the very defective manner in which clinical teaching is given in the majority of the London hospitals, and point out the chief causes of this shortcoming. The period of the day is ill chosen; the student's time is absorbed, as well as his attention, by unnecessary lectures; and the visits of physicians and surgeons are so arranged that unless a student could be in two places at once, like Sir B. Roche's bird, he must omit attendance upon one or the other. Dr. Todd proposes a morning clinic (he has established one at King's College Hospital, medical and surgical on alternate days), and the curtailment of the courses of lectures required. With regard to the latter point, we do thoroughly agree with him that long courses of lectures are unnecessary "on any subject not requiring demonstration and experiment." What does a student want with lectures on physiology, when he can read, and must read, Carpenter and Todd-Bowman, or Kirkes? Must he not learn his osteology with the bone in his hand, and Ward before him; and his anatomy in the like way? How much more knowledge, even in chemistry, would a student acquire who worked with a good text-book to guide him, in a laboratory, performing various experiments himself, testing, weighing, measuring, &c.? Would not a student get a far better knowledge of materia medica by being put into a well-arranged museum, and set to read Pereira there, than by listening to the best course of lectures that were ever given? We doubt that the mode of teaching by lectures is the best calculated to impart sound knowledge. The professor is obliged to pass over so much ground, to go from one subject to another so quickly, that no time is afforded for careful, digesting thought. Suppose any of us to go and hear Faraday lecture on a subject quite new to us, or nearly so, how much real knowledge should we have acquired, unless we studied the subject carefully afterwards? And if the subjects must be thus studied, and if, moreover, there are excellent treatises in our hands, is not the

* See Medico-Chirurgical Transactions, vol. xxxviii. p. 241.

time given to lectures so much lost? One very useful aid the student might have, not alluded to by Dr. Todd, is the assistance of younger men, who should act as tutors in the several branches of study. In dissecting-rooms the presence of such men is found essential; but we feel sure the same aid would be of great use in other departments. At the Universities, the private tutor is the real essential help to the working man, and the public college lectures are, or used to be, felt rather as an hindrance and interruption. Some plan of the following kind it appears to us would be a most material improvement on the present system. Let all lectures, except those on chemistry, and clinical, be abolished. Let text-books on the various subjects be assigned to the student. Let him have tutors, men a little above his own standing, to refer to for explanations, for assistance in dissection, and demonstration of specimens contained in well-stocked museums. Let there be yearly examinations instead of one, as at present; and let the subjects for the first be the more elementary and collateral sciences, and those for the last such as more exclusively relate to practice. Let each student in the final examination be required to examine and prescribe for cases of disease. The junior students should be discouraged from running after operations, which they cannot observe in any manner that will do them the least good. The out-patients at hospitals and dispensaries might with the greatest advantage be attended by some of the more advanced students, under the inspection of the medical officers. To something of this common-sense kind we shall in due time, no doubt, come, but the sooner the better.

Review VIII.

1. *Report from the Select Committee on Medical Department (Army); together with the Proceedings of the Committee, Minutes of Evidence, Appendix, and Index.* Ordered by the House of Commons to be printed.—*London*, 1856. Folio, pp. 379.

2. *The Crimean Commission and the Chelsea Board; being a Review of the Proceedings and Report of the Board.* By Col. Tulloch, late Commissioner in the Crimea.—*London*, 1857. 8vo, pp. 189.

Public attention having been directed to the organization of the Medical Department of the Army, by the reports of the disgraceful condition of the hospital at Scutari in the winter of 1854–5, and of the inadequate supplies of medicines and medical comforts furnished to the army in Bulgaria and in the Crimea, the House of Commons ordered, on the 15th of April, 1856, "That a Select Committee be appointed on the Medical Department of the Army," with power to send for persons, papers, and records. It would probably be difficult to find a tribunal less qualified to conduct a searching inquiry into the working of a professional department,—its defects, its requirements, and the changes necessary to render it efficient,—than a Committee of the House of Commons. The members, with very few exceptions, do not bring to the investigation a practical knowledge of the duties required of the department, of the qualifications essential to their discharge, and of the difficulties which the officers have to encounter in the varied spheres in which they must labour. They are consequently left in a great measure at the mercy of private, and therefore irresponsible, advisers, who, it is to be feared, often lead the inquiry towards details in which they are personally interested, instead of pointing to great measures which would benefit the whole profession, and tend to raise it to that position in the army which it undoubtedly deserves, and by which its services to the soldier would be greatly enhanced. Such a Committee also carries on its labours without any instructions as to the nature of the investigation, its objects, or its limits. It has, in fact, a roving commission to inquire *de omnibus rebus et quibusdam alliis*, and this power is not unfrequently abused.

The Report before us is not free from the faults adverted to. With the exception of Mr. Stafford and Colonel Boldero, it is not too much to say that the Committee was not conversant with the subject into which it was to inquire. In the course of its proceedings, too, the object for which it had been appointed was more than once forgotten,—questions were put as to matters wholly foreign to it,—and the examinations assumed a personal character, neither very edifying nor very creditable to the good taste of the members by whom they were conducted.

Dr. Andrew Smith appears to have been the individual upon whom the heavy artillery of irrelevant questions was chiefly brought to bear. He was examined, somewhat unfairly, as to his own previous services, and the circumstances under which he was appointed Director-General; as to his refusal to employ Dr. Davy, a *retired* Inspector-General, as chief of the department in the Crimea; his reasons for appointing Dr. Hall to supersede Dr. Burrell in the East; the grounds on which he appointed Dr. Menzies to the medical charge of the camp, at Shorncliffe, and the reasons he assigned for the subsequent removal of that officer,—all of which questions, it appears to us, were irrelevant to the subject on which the Committee was to report. With the single exception of the case of Dr. Menzies, the explanations of Dr. Smith were, upon the whole, satisfactory; but we regret to say that he did not assign any good reason for having placed in so responsible a situation an officer who, though zealous and hard-working, had signally failed at Scutari from want of administrative talent, and who in the present case laboured under the very serious disadvantage of not understanding the language of the German officers, whose hospitals he was to superintend. Of his own unfitness, Dr. Menzies seems to have been fully sensible, and repeatedly applied to be relieved from the charge, but was refused; and when at length he was removed, he appears to have been treated with harshness by Dr. Smith, whose explanations on this subject before the Committee were far from satisfactory. We deeply regret this, because in other respects the Director-General appears to have acted throughout with energy and honesty, and to have been animated by a desire to promote the welfare of the soldier, and the interests of the Medical Department.

It would be impossible, in the space which we can afford, to give a summary of the evidence taken before the Committee, because of its very discursive character, and the great variety of opinions expressed by the different witnesses on many of the subjects. We shall therefore content ourselves with adverting to what appear from the Report, to be the principal grievances complained of, and the remedies suggested.

A memorial to Lord Panmure, from the Military Medical Officers serving with the British Army in the East, is printed in the Appendix, with observations by Dr. Smith, to whom it was referred by his lordship. These are well deserving of careful perusal, and so clearly point out the injustice with which the department is in many respects treated, that we are sure Dr. Smith cannot but feel uncomfortable that he has so long neglected to inquire into these matters, and to advocate the claims of the officers who are under his jurisdiction, and of whom he may be fairly deemed the official representative.

The first subject in the memorial to which we shall advert is *The Rates of Pay.* These were admitted by all the witnesses examined to be insufficient to secure the services of the best class of men, and there seemed to be a unanimous feeling that they must be increased. We have said this was the opinion of *all* the witnesses; but perhaps we ought to have excepted the Earl of Cardigan, for though his Lordship had, as he informed the Committee, commanded a regiment for eighteen years, he professed that he "did not know anything about the pay" of the assistant-surgeons! But the whole of the noble lord's evidence was pretty much of the same stamp—characterized by ignorance even of the organization of the department, and by supreme indifference to the feelings and claims of the officers. For instance, although Lord Cardigan commanded the Light Cavalry Brigade in the

Crimea, and ought to have been fully cognizant of the mode in which the sick are supplied with their diets and hospital comforts, and to have ascertained that this very important duty was satisfactorily and efficiently performed in his brigade, he stated to the Committee, "I never heard of a man called a purveyor, and I never saw one in my life." Again, his lordship's enlightened views, and liberal, kindly feelings towards the medical officers, are well exemplified in the following answers to questions put by Colonel Boldero :

"*Q.* 4120. It appears that the Medical Department of the Army have no full pay retirement; do you think that it would be for the benefit of the service, as well as beneficial to the gentlemen themselves, if, after twenty-five or thirty years' service, they were allowed to retire upon full pay?—*A.* Are they not allowed to retire now on full pay after any service?

"*Q.* 4121. They get half pay, but they are not allowed to retire, as every other branch of the service is, upon full pay after a given period?—*A.* Of course it would be advantageous to them; but I cannot say that I could give my opinion in the recommendation of it, because if one is to begin to recommend about the increase of pay, there is no saying where it will end."

Truly, the noble lord's rule of action is *not* "Fiat justitia, ruat cœlum." To those who are curious in the study of character, we cannot recommend anything more amusing or instructive than a perusal of his evidence.

In returning the memorial with his observations for the information of Lord Panmure, Dr. Smith remarks:

"The subject involved in the documents herewith returned, I felt was one of a comprehensive nature, and not to be treated hastily; I therefore resolved to consult the views of certain officers of the department whom I might see in the course of a few weeks, and obtain from them such information as they might be able to afford me."

We are not informed of the names or number of the counsellors whom *chance* thus threw in his way, and in the absence of such information must therefore treat the suggestions as those simply of the Director-General. We have condensed into the following abstract his recommendations on the subject of pay, and for the purpose of comparison have also stated the rates as at present authorized.

Daily pay.	On appointment.		After 10 years' service.		After 20 years' service.		After 25 years' service.	
	Present rate.	Proposed rate.	Present rate.	Proposed rate.	Present rate.	Proposed rate.	Present rate.	Proposed rate.
	s. d.	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*
Assistant-surgeon	7 6	10 0	10 0	13 0	—	—	—	—
Regimental surgeon and second class staff	13 0	15 0	15 0	20 0	19 0	25 0	22 0	—
Staff-surgeon, first class	19 0	25 0	—	—	22 0	28 0	24 0	30 0
Deputy inspector-general	24 0	30 0	—	—	28 0	30 0	30 0	40 0
Inspector-general	36 0	60 0	—	—	38 0	60 0	40 0	60 0

Dr. Smith strongly recommends the abolition of a most unjust rule at present in existence, by which an officer, when promoted, however long he may have been in the army, is compelled, under certain circumstances, to serve two years on a lower rate of pay than his length of service entitles him to; the practical effect being, that those who have been unlucky in their promotion are punished for this misfortune!

He also points out the injustice done to surgeons of regiments in deducting $8\frac{1}{2}d.$ per day for forage, while the lieutenant-colonel, major, and adjutant receive it without any charge. If all require horses for the discharge of their duties, it is unreasonable that one should be compelled to pay for what the others receive free.

Half-pay and Retired-pay.—It is suggested with reference to all ranks, that—

"When placed on half-pay, owing to reduction of establishment, or from being incapacitated by reason of ill-health contracted in and occasioned by service, or when disabled by wounds, the officer should be granted three-fourths of the rate of pay he is receiving when he is placed on the retired list, or is reduced."

The present and proposed rates are shown in the following scale:

Retired pay.	Under 10 years' service.		After 10, but under 20, years.		After 20, but under 25, years.		After 25, but under 30, years.		After 30 years' service.	
	Present rate.	Proposed rate.	Present rate.	Proposed rate.	Present rate.	Proposed rate.	Present rate.	Proposed rate.	Present rate.	Proposed rate.
	s. d.	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	
Assistant surgeon	4 0	7 6	5 0	9 9	6 0	—	7 0	—	7 6	No increase after 25 years' service.
Regimental surgeon	6 0	11 3	8 6	15 0	11 0	18 9	13 0	—	15 0	
and second-class staff	7 6	—	—	—	—	—	—	—	—	
Staff surgeon, first-class.	7 6	—	10 0	18 9	12 6	21 0	15 0	22 6	17 0	
Deputy inspector-general	8 0	—	10 6	22 6	14 0	22 6	17 0	30 0	20 0	
Inspector-general.......	—	—	—	—	20 0	45 0	25 0	45 0	30 0	

It is likewise proposed that medical officers of all ranks should have a right to retire after twenty years' full pay service, instead of twenty-five years, as at present, but that when they do so, they should receive only five-tenths—or if they have completed twenty-five years' service, seven-tenths of their pay, instead of three-fourths, as recommended when their retirement is caused by reduction of establishment or by impaired health. Dr. Smith is of opinion that but few will take advantage of the right of earlier retirement.

"By the time a man has served twenty years, his attachment to the army is generally strong, and he finds himself unsuited to embark in private practice, and consequently is inclined to cling to the life and kind of society to which he has been long accustomed, rather than encounter the *ennui* which is experienced by men without regular occupation."

We doubt the accuracy of this conclusion, and cannot but think that a considerable number, and these the most intelligent and hardworking of the officers, will avail themselves of the opportunity to get out of a service with whose unsettled mode of life they have become pretty well sickened. The difference in the rate of retirement, however, may probably influence the results as regards the twenty years' service.

Under the head of "General Observations," Dr. Smith remarks:

"In addition to what I have stated in the preceding memoranda, I may farther observe that many medical officers consider that a certain number of individuals of each of the grades in the department should be permitted to retire on full pay under the same regulations as is observed in the case of military officers; and further, that a certain sum of money should be granted to be distributed as good service-money."

We regret that Dr. Smith should have felt himself justified in thus faintly recording what we believe to be a very strong and very general opinion of the medical officers. Colonel Boldero, in his examination of Lord Cardigan, already quoted, brought out the injustice of the case, that "every other branch of the service" has a full pay retirement except the medical; nor is there any class by whom a reward of the description of "good service-money" can be deemed to be more fairly earned. Promotion, honours, rewards, and pensions are bestowed with a liberal hand upon the military officers for gallant conduct in the field, or for bringing to a successful termination an expedition against an enemy, but the unflinching courage displayed by medical officers in their frequent encounters with pestilence, and their valuable and unceasing labours to maintain the soldier in a state of efficiency and consequent fitness for the fatigues of active service, are unheeded, unrewarded, and even unacknowledged. In the Continental armies this is not the case, and we trust that ere long the feeling in favour of the labours of our medical brethren in the army which was so strongly evinced by the English people, may be developed among the military authorities and those who have the power to reward courage and merit, even when shown in another—we may add a nobler—form than that of physical daring.

With a view to prevent the imputation of favouritism in the recommendation for such good service-pension, it would be advisable to adopt the practice followed in the case of military officers, to publish in the army estimates, or, if thought preferable, in the 'London Gazette,' the specific grounds on which the officer has been selected for this reward.

Relative Rank of Medical Officers.—The only alteration proposed in this is, that assistant-surgeons, after ten years' service, should have the rank of captain; surgeons above fifteen years' service, that of major; staff-surgeons of the first class, above twenty years, that of lieutenant-colonel; and deputy inspectors-general, above twenty-five years, that of colonel; but in all these cases as junior of the respective grades. But there is a matter connected with the rank of medical officers which has given rise to a great deal of annoyance and unpleasant feeling in the department. It has been stated to be given to them merely for the purpose of defining their position in the choice of quarters and in sharing prize-money; but when they are ordered to sit as members of boards or courts of inquiry, their rank is over-ruled, and they are called upon to sign last and to give their vote first, whatever their rank may be, as if they were junior to all the other members. Dr. Smith recommends—

> "That they should be detailed in orders, sit, and sign proceedings in the order of precedence attaching to their relative rank and seniority, and on no occasion should a medical officer be required to sit on any board or court under the presidency of a military officer of inferior relative rank. * * * If medical officers are regarded eligible to sit on boards, courts of inquiry, or courts-martial, they should have the consideration which they expect their relative rank should ensure them."

The present practice cannot but have the effect of depreciating the status of medical officers, and lowering them in the eyes of the soldiers; it is an abuse similar to that by which the naval assistant-surgeons were so long kept out of the ward-room, and like it, will, we trust, be got rid of, either by relieving medical officers from all such duties, or by allowing their rank to be what it professes.

A most erroneous notion seemed to exist in the minds of some of the Committee on the subject of the rank of medical officers; that they were desirous of obtaining military rank because it would confer on them military command. Thus Sir H. Davie asked Lord Cardigan, "With regard to the military position of the medical officers, would you recommend their having any military command at any time?" And again, "Would you allow him to command a parade, if he happened to have the senior choice of quarters?" (in other words, to be senior officer.) Now, we venture to affirm that such a notion as that suggested by Sir H. Davie never entered the minds of the medical officers. Their complaint is, that they are called upon to perform military duties, and are denied while doing so the advantage of military rank; and they ask either to be entirely relieved from such duties, or if required by the military authorities to perform them, that they should take their position, and enjoy all the advantages and privileges attached to the corresponding rank, but without the power of military command. We believe there are no officers in the service more alive to the necessity of military subordination, or who will more fully concur in the opinion expressed by Lord Cardigan in reply to the following question by Colonel Kingscote:

"*Q.* 3987. Your lordship thinks that the commanding officer of the regiment should have supreme military control over every person in, or attached to, the regiment?—*A.* If he has not, I consider that it is impossible to carry on the command of the regiment."

Honorary Distinctions and Rewards.—The non-participation by the medical officers in the distribution of honours for service in the field has long been a source of just complaint in the department. It is only within a few years that they were declared eligible to receive the military decoration of the Bath, and it has been conferred on them with a very sparing hand, compared with their more fortunate brother officers. Nor has this been compensated for by the grant of the civil Order of the Bath, which for many of their services would be an appropriate reward, and though not conferred, has been often merited. It is to be hoped that in this respect a more liberal feeling may be hereafter shown by the authorities. A suggestion is thrown out that a limited number of the most meritorious officers of the department might be appointed honorary physicians and surgeons to her Majesty—a reward analogous to that conferred on military officers who are made aides-de-camp to the Queen. A step of honorary rank on retirement from the service is also pointed out as a suitable and inexpensive reward to medical officers, which might be of use to them should they afterwards desire private practice.

But Dr. Smith is of opinion that the Director-General should have it in his power to reward special and meritorious services. He suggests that

"When a medical officer is specially recommended by the senior military and medical officers under whom he is serving, on account of his having rendered highly valuable and important services, either in the field or during the prevalence of an epidemic, or on an occasion when more than ordinary exertions were required, and were made in an unmistakeable manner, such recommendation should entitle him to reckon one year's service towards promotion and increase of pay, and the same for every subsequent recommendation. To warrant these advantages being conceded, the recommendation should be in most decided terms, and should contain a clause to the effect that it is offered with a view to secure to the officer recommended the reward which they know is conferred under certain circumstances."

This proposition appears reasonable, but some guarantee, such as the publication of the recommendations, with a full detail of the grounds on which they are made, in the "Gazette" or in General Orders, would be necessary to prevent abuse. Loud complaints reached this country, of injustice and partiality in the distribution of honours among the medical officers in the Crimea; and these were in some instances so general as to remove any suspicion that they were the mere effusions of disappointed men. To make rewards beneficial to the department, it is essential that they should be fairly won, and impartially distributed. The relative rank of the regimental surgeons unfortunately rendered them ineligible for the Order of the Bath; and although it was upon them that the hard work of the disastrous winter of 1854–5 chiefly fell, they had the mortification of seeing the honours conferred exclusively upon the staff, who had been comparatively exempt from the privations and hardships which they endured.

By the regulations of the service, the funeral honours paid to the medical staff are limited by no firing taking place over their graves. Dr. Smith says—

"This treatment has always been regarded by staff medical officers as an act of injustice; and not a few of them, when dying, from a dislike to have it supposed by soldiers that they were inferior to other officers, have requested to be buried privately. I think there are fair grounds for discontent in this respect, and I feel it my duty to recommend that every medical officer should be buried with the military honours that attach to his relative rank."

The sooner any invidious distinctions of this kind are abolished the better will it be for the army. They engender feelings which may naturally be expected to lessen the zeal and efficiency of a most useful branch of the service.

Leave of Absence.—One of the grievances most felt by the medical officers, and acknowledged by all the witnesses examined on the subject, is the difficulty, almost amounting to an impossibility, of obtaining leave of absence. When an officer is sent home by the recommendation of a medical board, the period is stated in their proceedings which will probably be necessary for his recovery, and he is usually granted leave for that time; but in the case of a medical officer the leave is cancelled immediately on his arrival in England, and unless he is able to join at the end of two months, he is in most cases placed on half-pay. The result of this is, that officers are frequently sent to Fort Pitt when they ought not to be doing duty, and the service consequently suffers. When a medical officer at home applies for leave of absence, "he is compelled to arrange with some qualified civil practitioner to discharge his duties, and thus in many instances incurs considerable expense." Dr. Smith recommends that

> "Each medical officer should be granted yearly six weeks' leave of absence, provided the circumstances of the service should admit; and regimental medical officers should not be required to pay a substitute to discharge their duties during the period."

A slight increase to the strength of the medical staff would enable the Director-General to carry out this arrangement at a very trifling cost to the public; and it is surely not too much to expect that an act of such obvious justice should at once be sanctioned by the Minister of State for War. Such an increase is indeed the only suggestion for the improvement of the department which was made to the Committee by the Adjutant-General.

Promotion.—The next point of importance to which we shall refer is comprised in the following paragraph in the Surgeons' Memorial:—"With regard to promotion, we trust your Lordship will not consider us presumptuous if we take the liberty of pointing out to you the necessity and expediency of some fixed principles being established whereby it should be regulated." In his observations on this paragraph, and also in a letter to Dr. Hall, dated the 17th of August, 1855 (p. 298), Dr. Smith asserts that the promotion is conducted on a definite plan; he says, "A fixed principle is, as far as practicable, observed; seniority is the general rule, and is only inoperative when the interest of the service, or of the department appears to render another practice necessary;" and he proceeds to detail what these circumstances are. This statement, however, of the general principle on which promotions are said to be recommended, does not accord with that subsequently given in his evidence before the Committee. For in his examination on the 1st of May, 1856, he was asked,

> "What is the general rule in recommending an assistant-surgeon to be made a full surgeon? —*A.* The senior is promoted invariably, unless there is something in the senior that renders it improper that he should be advanced.
>
> "*Q.* 109. Have you observed that rule also in other cases of promotion in the higher ranks?—*A.* No; the established rule with regard to them is selection."

So that it appears that in the case then under consideration—for it was with reference to the *Surgeons'* Memorial that the first statement was made—the "fixed principle" of promotion was the very reverse of that which he stated for Lord Panmure's information. In a subsequent examination, Dr. Smith detailed at some length the mode in which promotions had been made since his accession to office, and stated that he had "exercised to a very small degree the principle of selection;" and that only in very strong cases. But it also appeared that the regulations on the subject of promotion were among the *leges non scriptæ* of the department; that they were contained chiefly in letters extending over forty years, and that they did not exist in any codified form. This is to be regretted, for so long as they remain undeclared, officers will continue to think themselves subject to the caprice of the head of the department, even when they are treated in accordance with rules laid down by the higher authorities for his guidance. The Director-General would act wisely if he were to have the rules drawn out clearly, and promulgated for the information of all whom it may concern.

But the question of the principle on which promotions are made, brought under the consideration of the Committee another subject of great importance to the medical officers, although unnoticed in the Memorial and in Dr. Smith's Observations. It is that of

Confidential Reports.—It appears that the Director-General

"Receives yearly, sometimes oftener, from the principal medical officers of large hospitals or commands, reports in reference to the medical officers who have done duty under them, stating those that had been efficient and those that had not; those that had been indolent and those that had been active; also the qualification of all in every other respect"—and that he has been in the habit of making selections for promotion in accordance with the tenor of these reports. (*Q.* 110.)

But these confidential reports are not communicated to the officers against whom they operate so injuriously, and an opportunity is not afforded them of explaining matters which may—very probably undesignedly—have been misrepresented to the Director-General. It is true that Dr. Smith says—

"If an officer was to complain to me that he was suffering seriously in consequence of some reason or other, in a way that he did not think he deserved to suffer, and if he claimed consideration or advancement, I would consider it then my duty to tell him why he was suffering. But I do not make it a rule to communicate the confidential reports."

But there are many officers who would submit to what they deemed an injustice without remonstrating, and who would probably attribute to personal feeling a course of conduct arising out of these reports. When an officer is not afforded an opportunity of refuting charges thus brought against him, the confidential reports, if incorrect, become anonymous slanders. This is not the case with the reports made by the inspecting general officers to the Commander-in-Chief. Lord De Ros states, that in every instance in which an officer is reported upon, he is informed of it, and that no charge can be brought against an officer in the Inspecting-General's report which he has not an opportunity of answering. The Adjutant-General says:

"It is the invariable practice, when any unfavourable report is made of any officer, that that report is brought to the officer's notice through his commanding officer." (*Q.* 3270.)

And H.R.H. the Duke of Cambridge observes:

"I think that if a man is reported against for such neglect of duty as would bar his promotion, he ought to be informed of it; I think that no man ought to be put in that position without being informed of it." (*Q.* 3678.)

We trust that after the very strong expression of opinion against the existing practice, the designation "confidential" will no longer be deemed synonymous with "secret" reports. It is quite incomprehensible how such a system could ever have been tolerated.

Service in the Field.—The surgeons, in their Memorial,

"Suggest that active employment in the field should reckon as equivalent to three times the amount of ordinary service, and that colonial service should also receive some consideration above that passed in Great Britain."

But Dr. Smith objects to this, and as it appears to us with much justice, that service in some of our colonies is more destructive to health and more fatal than many campaigns. Again, if colonial service were to receive the consideration thus recommended, it would be necessary to establish a sliding-scale according to the morbific character of the command. But Dr. Smith observes truly:

"That in the army, officers are compelled to serve where they are ordered; therefore the accident of some being on service in the colonies or in England, whilst others are in the field, is not the result of any act of their own, and therefore ought not to operate to their prejudice, the more especially because there are few of them who would not, if they had their choice, be with the army in the Crimea."

Among the subjects inquired into by the Committee, was that of the

Appointment of Dispensers; and from the time and care bestowed upon this portion of the investigation, it was evidently deemed a question of considerable importance. By the regulations of the army, medical officers are forbidden to employ the hospital sergeants or orderlies to compound the medicines, and are required to certify in their reports that they have not done so. A serious practical evil results from this: during periods of excessive sickness, or when there are many serious cases under treatment in hospital, the surgeon is obliged to devote a considerable portion of his time to this duty, when he might be more profitably employed in the wards; and from this, of course, the patients suffer. It has also the natural tendency to lead the surgeon to prescribe remedies which cause little trouble in compounding, in preference to those which he might otherwise select. The former of these evils was severely felt in the Crimea, and with a view to obviate it, Dr. Smith recommended the appointment of dispensers, with the rank of ensign, and the pay of 7*s.* 6*d.* per diem. They were mostly young men who had been employed for some time in chemists' shops, and passed an examination as to their fitness before a Board of Medical Officers. Such of them as were appointed to regiments were gazetted, and received commissions, while those employed on the staff were merely warrant officers. The plan as carried out seems to have met with general condemnation, both from the military and medical officers, Dr. Smith alone, of all the witnesses, expressing a favourable opinion of it. But even he did not approve of them being gazetted as officers, though it does not appear by whom that measure was adopted, contrary to his opinion; it certainly could not be any one practically conversant with military matters. The objections to these dispensers were, that they were not taken from that class of society from which officers are usually obtained; that from their previous habits, education, and position, they were not suited to take a position among the officers, and that, from the want of qualification, they could not hope for any further advancement in the profession. There cannot be a question that it must prove injurious to the department to create in it a rank having a low educational, professional, and civil status, and the members of which are without the greatest incentive to exertion —hope. The Guards are exempt from the operation of the general rule above quoted, and have a non-commissioned officer attached to each of their hospitals, whose sole duty appears to be to compound and dispense. It seemed to be the general feeling of the witnesses that this system might be extended to the Line, and that a sergeant, receiving a moderate additional pay, would be much more suitable for duties of this nature than a commissioned officer. If we may judge from the very creditable appearance made by the Guards' sergeants when examined before the Committee, they are a class of men well qualified for such situations. We trust that the suggestions on this subject made to the Committee will receive due consideration, and that a superior class of non-commissioned officers will be created, fit to be intrusted with, and suitably remunerated for, the discharge of this important duty.

Another subject incidentally noticed by the Committee was the organization of *The Medical Staff Corps.*—This body was raised to supply a want greatly felt in the early part of the war—that of hospital sergeants and servants, and if properly carried out would be a most valuable addition to the department. But, in the first instance at least, it seemed to share in the mismanagement which disgraced our military system; for Captain Bunbury, the officer appointed to raise it, says that he had no written orders to guide him; that his instructions were "merely to organize, clothe, and equip the corps as recruits came in." The result was, that out of nine hundred and ninety-three men enlisted, five hundred and twenty-four were found ineligible "because they could not read and write, and because of their general appearance." Dr. Mackenzie, who had to examine some of these recruits at Manchester, says:

"I was not informed whether they were required to be able to read or write, or whether they were to be physically more or less fit for the service than other men. When I asked them for what regiment they were intended, and they told me it was for the Medical Staff Corps, I looked up in amazement, never having heard of it before."

Sir B. Hawes, the Deputy Secretary-at-War, admitted that

"At first there was some misunderstanding, but latterly they have been required to read and write well, and to present general testimonials of character."

He did not inform the Committee who was to blame for this "misunderstanding," but we think it fair to state that Dr. Smith repudiated all responsibility in the matter.

The suggestions of the Duke of Cambridge as to the constitution of the corps and the mode of recruiting it, are those of a practical man acquainted with the subject on which he speaks, and deserve the serious attention of the authorities who are responsible for this branch of the service. The importance of training an efficient body of intelligent hospital-sergeants and dispensers, and of steady, reliable orderlies, cannot be over-estimated, whether as regards the welfare of the sick soldier or the comfort and usefulness of the medical officer.

The preceding appear to be the chief deficiencies and grievances in the medical department of the army, and we trust the authorities, military and medical, will lose no time in having them removed. It is gratifying to find that, notwithstanding the injustice with which the medical officers have so long been treated, they have proved themselves, according to the testimony of the highest military authorities who were examined before the Committee, to be zealous, efficient, and unflinching in the discharge of their duties, and humane and kind to the soldier. Lord De Ros, who was Quartermaster-General in Bulgaria, says—

"From what I saw of the department when illness broke out in the fearful manner it did at Varna, I was strongly impressed with the idea that it was impossible for any body of men to do their duty better, or with more zeal and kindness." (*Q.* 2559.)

Sir Richard Airey, who held the same important appointment in the Crimea, corrobates this statement:

"I do not think it possible that there could have been a more energetic, anxious, or active class of men in the world than they were." (*Q.* 4392.)

And his Royal Highness the Duke of Cambridge, in confirming the testimony which he had formerly given before the Sebastopol Committee as to "the great exertions and philanthropy of the medical officers generally," again observes—

"I had every reason to be well satisfied with the great exertions of the medical officers." (*Q.* 3644.)

In Colonel Tulloch's review of the proceedings and report of the Chelsea Board, there is no direct allusion to the army medical department. Sir John Hall appears to have deemed it more prudent not to appeal to that tribunal against the animadversions contained in the report of the Commissioners. He declined the opportunity offered him of explaining the charges brought against him, and thus virtually admitted the justice of the censure passed upon him by them. Under these circumstances we should not have considered it necessary to notice this book, but in it Colonel Tulloch has been permitted to publish "The summary of information relative to the sickness, mortality, and prevailing diseases among the troops serving in the Crimea," which Lord Panmure decided, when the original report was presented to him, "was not comprised in the instructions of the Commissioners." The information now published has been so ably condensed already, that to give a correct idea of it would merely be to reprint the whole; we shall therefore only notice a few of the general results which corroborate in a most striking manner the conclusions at which we arrived in reference to the causes of the excessive mortality in the Crimea, from a careful study of the evidence taken before the Sebastopol Committee and the Crimean Commissioners.*

The total mortality *by disease* in the army in the Crimea during the seven months from the 1st of October, 1854, to the 30th of April, 1855, amounted to 35 per cent., including the deaths at Scutari and in the hospitals on the Bosphorus. The loss in the ill-fated Walcheren expedition amounted only to 10½ per cent. in six months, and during the Peninsular war it did not exceed 12 per cent. for a whole year. The proportion of deaths varied greatly in the different arms of the service in the Crimea. Thus, in the Cavalry, it was 15 per cent., in the Ordnance

* See British and Foreign Medico-Chirurgical Review, Oct., 1855, and July, 1856.

18, in the regiments of the Line it amounted to 39 per cent.; while in the Naval Brigade," which took a very prominent part in the operations of the siege," it was under 4 per cent. But in the Line regiments there was a striking diversity in the rate of mortality

"The average loss of four regiments which arrived in and about January, and did not, for nearly a month, take any part of the duties in the front, was only 7 per cent. The average of four other regiments which arrived in December, and was sent immediately to the front was 27 per cent. In the Highland Brigade stationed at Balaklava, the average was 24 per cent.; while in the regiments employed in front, on which the duties of the siege chiefly devolved, the average was 45 per cent.; and in eight of these corps which suffered most, it was 73 per cent."

In marked contrast to these, is the loss sustained by a detachment of 154 men of the 68th Regiment, "stationed at Lord Raglan's headquarters during the winter, and exempted in a great measure from the various heavy duties, exposures, and privations which affected the other part of the regiment," and which amounted only to 2 per cent.!

Various peculiarities attaching to the different arms of the service, explain in a great measure the exemption enjoyed by some of them from the excessive mortality which almost annihilated several Line regiments. Thus, the Naval Brigade had

"From three to four nights in bed for one on duty; their cooking was well arranged, and hot meals were always ready for them when they came from the trenches; they were well provided with boots, stockings, and clothing; and in addition to their rations they made good soup of ox-heads which they bought of the commissariat butchers for that purpose."

They also had arrangements by which, on their return from the trenches, they got their clothes and blankets thoroughly dried. The Cavalry "was entirely exempt from the labours of the siege; they had but little night duty; and being in the vicinity of Balaklava they had greater facilities for getting supplies." As regards the Ordnance,

"Two troops and one battery of artillery being constantly at Balaklava, were exempt in a great measure from trench duties; the men in the field batteries in front did not remain all night in the trenches. . . . . The siege train companies remained in the trenches, but in a smaller proportion than the men of the line; and the batteries, having their wagons, were provided regularly with rations and other supplies, and were thus spared the fatigues they would otherwise have undergone for that purpose."

Each man also had an oil-cloth to lie upon, and was thus protected from the damp ground. The Sappers and Miners had two nights in bed for one on duty, and they had also

"An officer at Balaklava who purchased all kinds of groceries, flour, and other food for them from the shipping, whenever they could be obtained, and had them conveyed to the front on fifteen mules belonging to the corps, which were maintained effective throughout the winter."

It would thus appear that the exemption from mortality in the different arms was in proportion to their exemption from night duty, and to their facilities for obtaining supplies of warm clothing and nutritious food. The same principle will account for the difference in the various infantry corps, the loss having been smallest in the corps which arrived after the period of great privation and exposure had passed; and next to that, in the Highland Brigade, which was much nearer its supplies, had less trench duty, and was hutted at an earlier period than the regiments in front.

These facts fully confirm the conclusions we formerly expressed,* as to the causes which gave rise to the fearful mortality which nearly annihilated the Crimean force, and justify the question with which Colonel Tulloch closes the introduction to his very able book—

* British and Foreign Medico-Chirurgical Review, July, 1856.

"With the graves of ten thousand of their countrymen before their eyes, with the mouldering remains of Britain's choicest cavelry beneath their feet, and with an overwhelming mass of evidence in their possession, to show how much of this loss might have been averted by a proper application of the supplies, could the Commissioners be expected to arrive at the conclusion of the Board of General Officers, that for all this no one in the Crimea was to blame?"

After all that was said of the deficiencies of the Army Medical Department in the early part of the war, it is satisfactory to find by Lord West's evidence, that the medical officers "are not in any way responsible for the great sickness that prevailed in the Crimea." The committee of the House of Commons also report, that having "had incidentally brought before them the admirable manner in which the army and civil surgeons have performed their duties in the East, your Committee are glad to take this opportunity of recording the high opinion they entertain of their merits."

Review IX.

The Census of Ireland for the Year 1851. Eleven Vols. Presented to both Houses of Parliament by command of Her Majesty.—*Dublin*, 1856.

We propose in this article to give some account of the last Census of Ireland, that of 1851, in its bearings on the diseases of the country and on medical science, so far as it tends to illustrate the one and to aid in the advancement of the other.

We are apt to associate with the Census of a people little more than an array of figures in a tabular form, restricted to the numbering of souls and the distinction of sexes. Such was its primitive limit. In progress of time, and especially in modern times, its character has changed; it has become developed, and expanded into a complex system, tabulating great general facts, and displaying the condition of society in most of the particulars capable of being expressed in numbers. This character is specially that of the Irish Census, so remarkable for its comprehensiveness, the labour involved in its details, the judgment displayed in its arrangement, and the collateral science and research brought in aid by which it has been enlightened, and made so interesting and instructive.

The comprehensive nature of this great work is well displayed by the parts into which it is divided. They are six, and the following: 1st, The Townland Census, in four volumes, detailing the Electoral divisions and the several townlands (of which latter there are 86,700 in Ireland), the area, population, and houses in each, distinguishing the inhabited, uninhabited, and those building, in 1841 and 1851, according to provinces,—altogether constituting the most minute subdivision of the kingdom that has ever before been attempted. 2nd, The Agricultural Statistics for 1851, and in addition those of 1852, in two volumes, showing the number of holdings, divided into nine classes according to the area of the farms, the average under crops, and the quantity of live stock on each class of holdings, with sundry tables bearing on the rural economy of Ireland. 3rd, A report in one volume on the Status of Disease in Ireland, giving the results of the first attempt ever made to ascertain in a single day of the year the amount of sickness, and the diseases of a temporary and permanent nature, by which the people at that time were affected, —a part to which the Commissioners justly attach much importance, as showing the numbers of the principal maladies, approximate and proportional, prevailing at the same time,—the numbers of the deaf and dumb, of blind, of the lunatic and idiotic, &c., of zymotic and sporadic diseases, distinguishing those found in hospitals, workhouses, &c., and specifying the more important by name, according to a nosological chart introduced with their synonyms, popular, local, and in the Irish language. 4th, A Report, in one volume, on Ages and Education in the several provinces, counties, baronies, and towns, giving the number, age, and per-centage of those who could read and write, read only, and who could neither read nor write, in 1841 and 1851,—also the number of schools and of scholars, with a table showing the number, by counties, of the Irish-speaking population. 5th, Tables of Deaths, and a Report on the Deaths, in two volumes, so far as could be ascertained in the absence of a general registration of births, marriages, and deaths, of which Ireland is the only country of the United Kingdom—nay, of any European kingdom—so destitute, a deficiency much to be regretted, and to which the Commissioners, with great propriety, urgently call the attention of the Government. Included in the same part is a tabulated abstract of cosmical phenomena, of epizootics, famines, and pestilences, extending from the earliest records to the year 1851. This, we understand, is the special labour of the Assistant Commissioner, Mr. Wilde. It is a new feature in a work of this kind, and great credit is due to its author for its completeness. We shall presently have to recur to it. The 6th, and last part, is the General Report, containing the remarks of the Commissioners on the condition of the Irish people, under the several heads comprised in the preceding parts, with additional tables in further illustration, especially relating to the inmates of public institutions, their pursuits and degrees of education, house accommodation, condition as to marriage, Irish-speaking population, the occupations of the people, and the very important subject of emigration. These several parts are comprised in eleven folio Blue Books, the thinest extending to 149 pretty closely printed pages, the thickest to 780, and altogether amounting to 4820.

We state these commonplace particulars the better to give some idea of the extent of the work, and the labour entailed in its elaboration. From its nature it is essentially one of reference. The short notice we have given of its contents may serve as a guide, or a brief expositor to those who may wish to consult it on any of the many subjects it comprises appertaining to medical science and research.

Our further notice of it must necessarily be very limited. We shall begin with its most attractive and novel portion—that which constitutes the Reports on the Tables of Deaths, with the appended table of Cosmical Phenomena, &c., to which we have already referred. In this table we find briefly sketched the most memorable events recorded in history relating to unusual phenomena, meteorological especially; to epizootics, famines, and pestilences; showing on the whole a remarkable similarity, as if the past prefigured the future, and the almost present were a reflex of the past. The whole time to which the record extends is divided by the Commissioners into three principal epochs:

"The first or præchristian period, extends from the earliest times to which tradition refers, —when the first colonization of Ireland is dimly shadowed forth in the Bardic and legendary annals of the past, and before fixed history and chronology existed among the natives of this kingdom,—to the reception of Christianity by the inhabitants of Ireland, about the middle of the fifth century. The second, which may be styled the Historic period, in which the notices of plagues and famines become more distinct, and derive authority from contemporaneous writings, extends from the Irish Christian era, dating from the arrival of St. Patrick, A. D. 432, to about the middle of the seventeenth century, when the adoption of a scientific nomenclature, the extension of medical knowledge, and the more general diffusion of literature through the art of printing, helped to dispel the mists of superstition and ignorance; when historians, both professional and general, began to describe with accuracy the history and symptoms of various maladies which affect the animal creation, and when authentic records of disease commenced to occupy the place of the barren historic annals which had hitherto merely related the circumstances of the wet, the drought, the plague, or murrain. The third period, which we (the Commissioners) have nominated the scientific, extends from the year 1650 to the present time." (Part V., vol. i., p. 2.)

The larger middle period the Commissioners have subdivided according to marked political events, such as the early Danish invasion, the later Anglo-Norman conquests—events materially affecting the people, by opening intercourse with foreigners, and favouring the introduction of new diseases. In the formation of this epitome, in which events are only noticed, authorities for the record are in all instances given, showing much curious and learned research, especially as regards the earlier periods, when it would appear from clear evidence that Ireland was in advance of Britain, and bore marks of a remote colonization by a people not destitute of the arts, literature, and science, such as belonged to the more civilized nations of the ancient world. In glancing over this collection of events, and in passing from one period to another, we are reminded of a museum in which objects of art and science are arranged according to ages, and where, even by a rapid survey, some well-marked idea may be formed of the several stages of history to which the objects that meet the eye belong, one period in its occurrences well illustrating its antecedent, and the three together instructively contrasted. We may adduce in proof an example or two from the cosmical or meteorological phenomena in the way they are described. In the early period we find showers of blood noted down as not of unfrequent occurrence, and other showers not less extraordinary—showers which in our own time, with the same appearance, lose their marvellousness, their true nature being determined by the methods of science,—those called showers of blood being resolved, under such scrutiny, into either rain, coloured by red impalpable dust, or sand, the product of a volcano, or drifted from the desert by a hurricane, or the droppings of swarms of insects, their urinary excrement, consisting chiefly of lithic acid of the same colour. In like manner the reported, and in the olden time credited, turning of lakes and standing waters into blood, is now read and understood to be a phenomenon of colour, depending mostly on the appearance, in countless numbers, of red microscopic algæ, and of animalcules belonging to the class of rhizopodes. To give another instance: in 1695, a dew like butter, and so called, is reported to have fallen on the grass in low marshy places, and again in a subsequent year, and to have been used by the natives for the purpose of greasing the axles of their carts,—a phenomenon resolving itself, according to the observations of science, into the occurrence of a fungus of wonderfully rapid growth, the *othalium flavum* of Link, which occasionally, under peculiar states of atmosphere, suddenly appears, and, somewhat like butter in appearance, looks as if it had fallen from the air. In all times, let it be remembered that the ignorant are the representatives of the dark ages, and of the periods of fable and superstition, and that their accounts of phenomena almost always stand in need of enlightened interpretation.

To the medical inquirer, this tabulated epitome of events is chiefly interesting as marking the occurrence of epidemics in connexion with meteorological phenomena and a fluctuating state of society, especially as influenced by desolating war, or more desolating famine. The similarity of scourges to which the country has been subject is very remarkable, and is strongly displayed in these annals. The chief of them, even from the remotest historical period, have been the same as those which have desolated the country in our own times—viz., epidemic fevers of various types, dysentery and cholic, or cholera, but more especially the two first, and traceable to similarity of causes. Famine, fever, and dysentery, it would appear, have been as constantly allied in Ireland at all periods, as they were in the Crimea in our gallant and ill-provided army before Sebastopol in the dreadful winter of 1854–5. For information on these important and deeply-interesting topics, this table of events is invaluable to the medical inquirer, and no doubt will often be referred to by him; and let us hope that, escaping the ordinary fate of blue-books, it will have, as it so much deserves, the attention of statesmen studying the causes of the well-being of a people, and the influences opposing that well-being. As the record approaches our own time it increases in fulness and completeness. The account of the last great famine and of the potato disease, and of the consequences of these on the population, is given in ample and impressive detail, and in a very lucid manner; and such also, only in a less degree, in proportion to their degree of importance, is the account of some preceding potato failures, and of the years of scarcity resulting, accompanied by epidemic diseases of like kind.

It is curious to see how the fates of nations are sometimes influenced by apparently trivial causes. The steam engine is one example of the kind as regards the potential and material prosperity of our country; the power-loom another. In Ireland, an exotic plank holds the same place as to its influence, but in an opposite line from the prosperous. Who, *à priori*, could have imagined that a tuber brought as a curiosity from the New World could, in the short historical time of little more than two centuries, have become what it has proved to be in Ireland—the main cause, when flourishing and yielding a good crop, of an increase of population exciting surprise and apprehension, and, when subject to disease and failure, creating a famine almost without parallel in the history of mankind for its destructive effects? In 1652, when this tuber was coming into use, the population of the whole of Ireland, we learn, was estimated as under a million—viz., 850,000; it rapidly advanced with the extention of the root, so that in 1841 it had reached 8,747,400! And it has been calculated that, had it not been interrupted in its progress, it would, in the present year, instead of being reduced to about six millions, have reached nine millions. Truly the potato is a vegetable of remarkable properties for good and for evil; under culture yielding more food in the same space than any other plant suitable to our soil and climate, per acre,—for instance, twice or thrice as much nutriment as wheat—and that nutriment, from the varied composition of the potato, more wholesome, when used by itself, and sustaining, than perhaps any other article of diet, with the exception of milk, and, we would add, the oat—and so easily cooked as to require only the simplest culinary art and means, and so palatable as, without addition, or the mere addition of a little salt, to be grateful to almost every taste; but on the other hand—to turn to its dangerous qualities—above the majority of plants subject to disease. In a single week in 1846, the first of the famine years, we are assured that almost the whole crop was destroyed, occasioning a loss computed at little short of 16,000,000*l.* Moreover, strongly contrasted with wheat and other grains, the potato is of so perishable a nature, as to preclude the laying it up in store beyond a few months. With its introduction, and probably in part attributable to its use, cutaneous disease, especially leprosy, formerly so prevalent in Ireland, disappeared. And, we would ask, may not the little prevalence of calculous and gouty complaints, so remarkable in that country, be owing to the same cause? Of its deleterious influences, which are now so well known, we need not enter into particulars—influences passing into effects in these latter years, and so strongly exemplified in the recent history of the country, especially in those districts of it in which the potato had become the staff of life, the almost exclusive food of the people, and that people, owing mainly to their food, in the lowest condition, wretchedly housed, ill clad, ill paid, every way ill off—low in mind as well as in corporeal vigour. So great indeed, and potential has been the influence of the potato on the people of Ireland, that from the time it came into general use and took the place of a more varied and substantial diet, the quality of the potato crop alone may be received as an idex of the condition of the people, as much so as the barometer is of the pressure of the atmosphere. We are assured that when the dependency on the potato was at its maximum, "it was not merely the food of the people, but supplied the place of capital and of a circulating medium"—that money-wages were then almost unknown—and that then the coarsest kind, the lumper, was planted in preference, because most productive.

Not the least valuable part of these annals is that recording the state of the weather and seasons, drawn up from various sources, but till very recently only from very incomplete meteorological observations, the barometer, it would appear, not having been mentioned till 1744, and the thermometer not till even later—viz., 1750. Situated as Ireland is, at the north-western extremity of Europe, exposed to the full influence of the northern branch of the gulf stream which sweeps its shores, its climate is marked by peculiarities. Professor Lloyd, in his "Notes on Meteorology of Ireland," states that its winter temperature is as high as that of the southern shores of the Euxine, while, on the other hand, the great precipitation of vapour due to the same cause, gives it a summer heat as low as parts of Finland." This peculiarity is well marked in the tables of the Census, showing the temperature of the seasons, and the mean quantity of rain in the several seasons. In Dublin, according to the Ordnance observations, extending from 1831 to 1852, the mean temperature of spring is 43°; of summer 54·9°; of autumn, 53·2°

of winter, 42·7°; the maximum mean summer heat being only 74·3°, and only in one year so high as 80°. According to the same observations, extending from 1837 to 1852, the average fall of rain in spring was 5·21 inches; in summer, 6·25; in autumn, 7·62; and in winter, 7·66. The number of days in which rain fell, more or less as to quantity, being, in spring, 53; in summer, 51; in autumn, 58; in winter, 58; making an average total for the year of 21·54 inches, and of 220 rainy days; whilst in London the yearly average is about 25 inches, and 175 days. These figures, both as to temperature and rain, seem to denote great equability; yet in respect to both there is a capriciousness. It would appear that changes are sudden from wet to dry in excess, and in occasional years probably exceeding even the proverbially mutable climate of England—the tendency, however, being to excess of moisture, and danger of failure of crops from such excess. The character, too, of the country accords, a large proportion of the whole area of the island being raised only a few hundred feet above the level of the sea, with its great extent of inland lakes and of bog, and of uncultivated land but little removed from the state of bog. From the Census it would appear, that of the total face of the country, equal to 20,811,774 acres, no less than 631,210 were of water in 1851, and 5,023,984 were waste, chiefly, it may be inferred, bog. In casting the eye over the annals, one cannot fail of being struck by the very frequent record of destructive storms and floods, and of inauspicious seasons productive of famine, referrible, in some of their worst effects, to the peculiarities of ground as well as of climate—any unusual fall of rain having so much more effect where so large a portion of country is constantly saturated with moisture, and where the general surface is so little favourable to drainage. This condition of climate and surface it is important to keep in mind,—both more favourable to pasturage and green crops, to the rearing of cattle, than to the growing of grain; and of the kinds of grain, less unfavourable to the hardy oat than to any other of the cerealia, especially wheet. It is well too, to keep in mind, that whilst as a crop the oat is subject to fewest causalties in growth, it has this other great advantage, that both its grain and straw afford most substantial and sustaining food to man and beast, and that the former is most admirably adapted to supply the place of the potato,* if the Irish people are to continue to experience improvement and advance, and not fall back again on a total dependency on that root, with all its consequent evils.

Another element bearing on the public health is the dwellings of the people. These in the Census have been divided into four classes, according to their quality:

"In the lowest, or fourth class, are comprised all mud cabins having only one room; in the third, a better description of cottage, still built of mud, but varying from two to four rooms and windows; in the second, a good farm-house, or in towns a house in a small street, having from five to nine rooms and windows; and in the first, all houses of a better description than the preceding classes."

A comparison of the dwellings of the inhabitants, as returned in the Census of 1841 and 1851, is very instructive, and most significant of the events of the period, as affecting the condition of the people and the change in progress. In 1841, the total of inhabited houses was 1,328,839; in 1851, it had fallen to 1,046,233—an enormous decrease, and happily confined to those of the fourth or worst class. Persons who have any knowledge of Ireland, and especially of its wilder parts,—those shaded most dark in the chart accompanyiug the Census showing the degrees of education, and most light in the other showing the density of the popula-

* The following are the results of the latest analysis of this grain made by Professor Norton ,and Mr. Furnberg, selecting of the four kinds examined by them (the four differ very little in the proportions of their constituents), the Hopetoun, Ayrshire: Starch, 64·80; sugar, 1·58; gum, 2·41; oil, 6·97; casein (avenine), 16·27; albumen, 1·29; gluten, 1·46; epidermis, 2·39; alkaline salts and loss, 1·84. See Journal of the Bath and West of England Society for the Encouragement of Agriculture, &c., recently published by Ridgway, in which, in a valuable paper by Mr. Pratt. On the Cultivation of Cereal Crops, the advantages of the oat are well pointed out, in relation to soil and climate, and comparative nutritive power. It is worthy of mention that the potato contains a vegetable acid; the quantity indeed is minute, yet its presence may be of importance. Both in its raw and boiled tuber we have detected it by litmus paper: its species we have not attempted to ascertain.

tion, parts in which the people are most scattered and least educated,—can well understand the significance of the change as regards the well-being of the people. By those who have not visited the country, and are only acquainted with the cottages of our agricultural labourers, hardly a notion can be formed of the extreme wretchedness of an Irish cabin—a damp earthen floor within, a fetid pool without, cattle and the human inmates under the same roof, a hearth without a chimney, and no egress for the smoke of the peat fire but through the door and the ventilating crevices in the mud walls and roof; in brief, the same kind of hovel as that described by Sir William Petty nearly two hundred years ago, neither better nor worse, and even then constituting the great majority of the dwellings, 160,000 out of the then total 200,000.

"Wretched nasty cabins, without chimney, window, or door-shut, even worse than those of the savage Americans, and wholly unfit for the making of the merchantieth butter, cheese, or the manufauture of woollen, linen, or leather."

On the diseases of Ireland, past and present, we have already said that the information given by the Commissioners, and especially by the Assistant Commissioner, is invaluable for reference. Limited as we are as to space and time, we must confine our notice chiefly to a small part of the whole, comprised in the "Status of Disease," and the "Analysis of Tables of Pestilences and Tables of Deaths."

The volume bearing the first title is designed, we have seen, to exhibit the existing diseases of the whole people, as collected in the first attempt ever made to form such a record—viz., on the night of March 30, 1851. The diseases enumerated, divided into permanent and temporary, are given under nine heads in sections: 1, the deaf and dumb; 2, the blind; 3, the lunatic and idiotic; 4, the lame and decrepit; 5, the sick in workhouses; 6, the sick in hospitals; 7, the sick in prisons; 8 the sick in asylums; 9 the total sick in Ireland. This total, including those labouring under permanent disease, was 104,495, or about one, it is estimated, in every sixty-two and a-half of the population, the proportion of the sexes being 100 females to 95·53 males. Satisfactory data are wanting for comparing the state of health of Ireland, as indicated by these numbers, with that of other countries. Judging, however, from the data we have, such as they are, the number of sick thus given for the entire population appears to be very small, and we can hardly avoid the conclusion that it has been under-rated. Amongst our troops on service in Ireland, from 1797 to 1828, the average daily sick was 51 per 1,000, or one in every 19·6. Referring to the tables of the Scotch and English Benefit Societies, and the returns of the East India Company's labourers in London, of the age of twenty to forty, we find the average daily sick of the whole to be 14·3 per 1,000, or 1 in 70. This last number denotes less sickness, indeed, than that assigned above to the whole population of Ireland, but in how small a proportion in relation to the differences of ages; on the one part from twenty to forty, nearly the healthiest period of life, on the other, the entire period, with its extremes so prone to disease!*

Among the permanent diseases, the deaf and dumb hold the first place. Those congenital and becoming so after birth, are stated to amount to 4,747, of whom 2,688 were males, and 2,059 were females; the dumb, not deaf, are returned as 433, of whom 259 were males, and 174 females. The details respecting these unfortunate persons are deserving not merely of perusal, but of careful study, were it only for the physiological bearing of the subject. Tables are introduced illustrative of particulars of most importance. In the tables of occupations of the deaf mutes, 1,917 of the whole are entered as known to be following some occupation, the remaining 2,830 not being specified. The employments in which they are engaged are very various; sixty different ones are named; of the men the greater number are found following the occupation of labourers (636), and of servants (100); of the other sex the greater numbers are servants (160), and milliners and seamstresses (111). Respecting the degree of health in this class and

* See Army Statistical Reports, p. 16. 1839.

the duration of life, there does not appear to be any well marked peculiarities, excepting that judging from the limited number of fatal cases tabulated, pulmonary consumption seems to be rather more fatal to them than to the population generally, being in the proportion of one to 3·26 of their entire number, confirming the opinion that as deaf mutism itself is frequently a variety of struma, so the persons afflicted therewith are more particularly predisposed to diseases of a scrofulous character in after life."

The total number of blind in Ireland is stated to have been 7587, of whom 3588 are males and 3999 females, being in the proportion of 1 in 8·64 of the entire population, which, with the exception of Norway, would appear from the most recent observations to be higher than the average for Europe generally, and America. Comparing the town and country population, the former affords the highest number, which in the report is referred, and we doubt not justly, to a denser population, a condition increasing the risk of infection and the spread of the worst kind of ophthalmia, when once introduced. Much interesting information is furnished respecting the occupation of the blind, their education, &c. More than half of them are married, and about the same number of each sex—viz., 1992 males and 1994 females. Their length of life, it would appear, exceeds the average, the number of the age of forty and upwards being to those under that age as 5010 to 2577. According to history, Ireland has always been remarkable for the large number of its blind. Giraldus Cambrensis (we quote from the Report) says, that "so many persons born blind, so many lame, so many deformed, so many wanting some of Nature's gifts, I never met with in any other land;" and the older writings of the physicians of the country, still preserved in the Gaelic language, are said to be confirmatory of this, as well as the fact of the many recorded occasional outbreaks of epidemic ophthalmia.

The number of lunatics and idiots in Ireland was in all 9980, of whom 5074 were lunatics (2500 males, 2571 females) and 4906 were idiots (2666 males, 2240 females). The details on this subject to be found in the Report, with the tabular statistics and accounts of lunatic asylums, will amply repay the inquiring reader. We shall do little more than advert to the classes of persons affected, and their occupations. It is very worthy of remark that the proportion of insane in the town population, and in the professional class and the educated, exceeds that of the country, of the working and of the uneducated class. Reviewing the total of the insane, excluding idiots, it would appear that the educated are to the uneducated in the proportion of 100 to 61, and dividing the whole number according to occupations into ten classes, it further appears that no less than 404 belong to the professional class, which amount exceeds by a considerable number all the other classes specified, with the exception of the large agricultural one, giving rise to the remark that "this preponderance of mental disease among the professional and upper classes, shows how much more education and habits of thought tend to produce aberration of intellect than ordinary manual labour," qualifying the remark, as we believe truth requires, by prefixing unsound or ill-directed to education, and substituting irregular habits of life for habits of thought. Ireland now is honorably distinguished for the care taken of the insane, and for the many excellent asylums provided for them. It is a distinction, however, gained of late years; even no longer ago than 1817, according to a report of a committee appointed to examine into the state of the pauper lunatics—

"When a strong young man is thus afflicted, the only way they have to manage him is by making a hole in the floor of the cabin, not high enough for the person to stand up in, with a crib over him to prevent his getting out—the hole about four feet deep. They give the wretched being his food in it, and there he generally dies."

Of the last class of those labouring under permanent diseases, the lame and decrepit, the total number returned was 4375, of whom 2320 were males and 2055 were females. The class is too miscellaneous, judging from the remarks in the

Report, to allow of any useful deductions, especially as the return of the persons so affected is described as incomplete.

To those whose attention is directed to paupers, workhouses, and hospitals, valuable information will be found in the Report on all these subjects, with copious statistics. It is curious to find that at a very remote period, anterior to the introduction of Christianity, buildings were set apart for the reception of the sick and wounded:

"We read (it is stated in the Report) that when the regal residences of Tara and Emania existed, there was attached to the latter 'the House of the Crimson Branch,' where the warriors of old hung up their arms and trophies; and near to this stood the *Broin Bearg*, or the 'House of Sorrow,' where the sick and wounded were provided for."

It is also remarkable, as pointed out, considering how leper hospitals and monastic hospitals had been early established in Ireland, indicating unusual regard for the infirmities of our fellow men, that no record of any civil hospital in the capital of the country is found to exist previous to the eighteenth century,—a city now so amply provided with institutions of the kind, and with a medical school attached and dependent on them, of a high character and well-earned European reputation.

Of the great division, that of temporary diseases, of which the total has already been given—viz., 104,495—the sources assigned are the following:

Reported at home (rural)	41,836
" (civic)	6,455
(Sexes nearly equal.)	
In general and fever hospitals, lunatic asylums, and others, for the blind, aged, and infirm, &c., and hospitals of gaols	9,078
(100 males to 77·65 females.)	
In workhouses and workhouse hospitals	47,126
(100 females to 84·54 males.)	

The tables given in the appendix to the Report on the status of disease are ample and instructive, considering them merely approximative, allowing for underrating. Our limits forbid our dwelling on them. All that we can venture is to make a few selections. They are returned under two great heads—zymotic (epidemic, endemic, and contagious diseases) and sporadic diseases, and the latter are again subdivided into ten groups, according chiefly to the organs affected.

I. Zymotic diseases	34,998
II. Sporiatic diseases—	
Of the brain and nervous system, and organs of sense	24,522
" circulating organs	534
" respiratory organs	10,509
" digestive organs	4,511
" urinary organs	289
" generative organs	693
" locomotive organs	8,822
" tegumentary organs	7,167
" uncertain seat	10,394
Accidental causes, such as burns, poison, effects of cold and starvation, &c.	1,224
Causes not specified	832
General total	104,495

We shall select from these groups a few particular diseases for brief comment. As their localities will be one point of comparison, and this according to provinces, in may be right to state *in limine* the population of each, as determined by the Census. Of the four into which the whole of Ireland is divided, Leinster, forming a considerable portion of the eastern coast, had in 1851 a population of 1,672,738; Munster, constituting the southern portion, 1,857,736; Ulster, the northern portion, 2,011,880; and Connaught, the western, 1,010,031; the chief local difference, apart from direction of aspect, being that the province of Leinster possesses a smaller portion of sea-coast than either of the others, and is almost entirely without those deep inlets of the sea for which the others are remarkable.

The diseases which were found throughout Ireland most to prevail on the night the reckoning was made were, as of old and in all times, those of the epidemic kind, especially fevers, dysentery, and diarrhœa, amounting to 34,998, or one in three of the total sick.

> "Great variety (it is remarked in the Report) existed in the provincial summaries for this class in proportion to the population; thus in Leinster and Connaught one person in every 209 was returned as sick from some of the causes specified under the head of epidemic disease; in Munster, as one in 106; whereas in Ulster the proportion was only one in 432."

They add:—

> "In examining more minutely into the distribution of the epidemic class of diseases, we find a remarkable difference in particular localities, being greatest in the city of Kilkenny and the counties of Clare and Kerry, the city of Waterford, and the town of Galway, in which lacalities the proportion varied from 1 in 55 to one in 54 of the population; and least in the counties of Antrim, Down, Armagh, Donegal, and Dublin, and also in Belfast town, showing in the former instance the effects of poverty and destitution in the production and maintenance of epidemic diseases, and in the latter of comfort, industry, and cleanliness, in maintaining a comparative immunity from diseases of an epidemic or contagious character."

Amongst the sporadic diseases, those of the respiratory organs are conspicuous, and especially consumption. On the 30th of March, 4182 cases of this malady were returned, and, according to provinces, as follows: Leinster, 1326; Munster, 1260; Ulster, 1104; Connaught, 492; or, in proportion to the population in the first provinee, about 8 in every 10,000 of the inhabitants; in the second very nearly 7; in the third little more than 5; and in the fourth, Connaught, a little under 5, about 4·88. Taking certain counties in these provinces, as Mayo in Connaught and Kildare in Leinster, the proportional difference in the prevalency of the disease is even more striking—in the first little more than 3 cases occurring in 10,000 of the inhabitants, whilst in the latter there are as many as 9·7. On referring to the map, these counties are seen lying on the very opposite sides of the island—Mayo projecting into the Atlantic, fully exposed to the west; Kildare, inland, lying low, the bog of Allen forming a part of it, yet at no great distance from the sea, and but little sheltered from the colder winds, the east and north-east. Besides difference of situation, difference of diet may be concerned in favouring more exemption from the disease in the western province and county than in the eastern. In the former it is probable that more fish is used, and general a more plentiful diet than in the latter—sea-fish, which, containing iodine in its composition, there is reason to believe, checks the formation of tubercles, and tends to keep under the low scrofulous diathesis. Other facts have come to our knowledge favourable to this conclusion. We shall mention one: it is the remarkable exemption of the island of Lewis from phthisis—an island, the climate of which is peculiarly equable, and the inhabitants of which consume little animal food, excepting sea-fish, but of that a plenty.

Referring to the table of ages in the Census of persons labouring under consumption, it is curious to see the wide range of increase from infancy up to ten, and beyond up to twenty-five, when at about the maximum, and the diminution even more irregularly occurring with advancing years after twenty-five; and it is worthy of remark that up to sixty-five the proportional diminution is not considerable. Through the majority of ages, especially after puberty and till an advanced period of life, the disease shows a preponderance in the female sex, being in the total in the instance of the male 1798, and in that of the female 2384. The latency and persistency of tubercles, indicated, we think, by the large proportion of elderly persons returned as labouring under phthisis, constitute a subject of inquiry which has not, we believe, received the attention it deserves. That such a state is not uncommon, we are satisfied; many a chronic catarrh we have known associated with tubercles in the lungs in those past the meridian of life; and many fatal cases—suddenly fatal owing to accidental violence—have come under our observation, in which tubercles existed in these organs, without having been suspected, or sensibly impairing the general health.

The rarity of some diseases, as shown by the Census tables, is remarkable, such as ague (201 cases, probably a large proportion of them imported), delirium tremens (9), syphilis (824, chiefly occurring in garrison towns), gonorrhœa (79, to which the same remark applies), stone (32), other diseases of the urinary organs (257), gout (51). The moderate prevalency of certain other sporadic diseases is not so remarkable, such as rheumatism (3953), ulceration (2616), itch (1193), scrofula (2654), dropsy (1464), and need no comment.

We pass now to the Analysis of Tables of Pestilences, and Tables of Deaths, and with a feeling of regret that the notices we shall have to offer must, in relation to the importance of the subjects, be commensurately short.

Of the total deaths returned for the decennial period between 1841 and 1851, amounting to 1,361,051, as many as 553,801 were owing to epidemic diseases, equal to 40·6 per cent. of the entire mortality—a circumstance not surprising, considering that the famine period is included in the decennium, arising out of the destruction of the potato in 1846, and the following years. The most destructive of all these diseases were fever, dysentery, and diarrhœa, the almost invariable and dire accompaniments of famine. These together swept off 366,584 of the population—fevers 222,029, dysentery 93,232, diarrhœa 41,323. The other diseases belonging to the zymotic or epidemic class next in degree of fatality were, cholera and small-pox, measles, scarlatina, hooping-cough, and croup. Cholera occasioned 35,989 deaths; small-pox, 38,275; measles, scarlatina, hooping-cough, and croup conjointly, 100,141: of these scarlatina, the least destructive, was fatal to 20,175.

Of the diseases or causes of death not belonging to the zymotic or epidemic family, but in part arising out of the famine, the more remarkable were those included under scurvy, infirmity, debility, old age, and starvation. The mortality from these reached the vast amount of 155,693, of which total deaths 21,770 were attributed to starvation. To these might be added 15,000 more, partly of like origin—viz., marasmus (6805), dropsy (662), ulceration (3634), mortification (3901).

Of the more ordinary diseases, pulmonary consumption, as usual, holds the highest rank as to fatality. The deaths from it are no less than 153,098—75,240 males, 77,858 females. The information respecting this malady contained in the several tables of mortality, accords, as nearly as could be expected, with that afforded in the tables already referred to of existing diseases, collected on the night of the 30th March. And making a like comparison as to localities, the results before obtained are tolerably confirmed. Thus, whilst the per-centage of deaths from phthisis in Connaught was only 7·6 of the whole mortality of that province, in Leinster it was 14·7; and whilst only 5·9 per cent. of the whole in the county of Mayo, it was as high as 19·8 per cent. in the county of Kildare.

Of deaths from other diseases, we shall mention only a few, and these—with the exception of one, convulsions, from their comparative unfrequency, not needing special notice—such as the folowing:—Gout, occasioning 272 deaths; diabetes, 158; stone, 35; aneurism, 187; empyema, 93; delirium tremens, 91; but convulsions, 43,167! In the instance of this last disease, it is worthy of remark that as many as 27,914 died under twelve months—viz., 16,017 males and 11,897 females.

Deaths from violence, and sudden deaths, have received, as might have been expected, the careful attention of the Commissioners. In tabulating them, two divisions have been made—one including those on which inquests were held, the other those on which no inquests were held—taken from the police reports made to the Inspector-General of Constabulary. Under the first, 29,265 deaths are returned, 20,866 males and 8399 females. Under the second, 333—of whom 277 were males, 56 were females. The deaths on which inquests were held are subdivided into three classes: 1st, deaths by violence, neglect, evil intent or design, amounting to 2374—males 1589, females 785; 2nd, suicide, amounting to 841—males 573, females 208; 3rd, accidental deaths without design or intent, amounting to 12,717—males 9158, females 5021. The following excerpta are very significant, and are given as examples:—

	Murder. M. F.		Infanticide.		Desertion & exposure.		Suicide. M. F.		Suffocation by limekilns. M. F.
Leinster ...	94+41=135	...	50	...	369	...	191+85=276	...	71+3= 74
Munster ...	127+44=171	...	107	...	52	...	149+66=215	...	129+6=135
Ulster ...	22+ 6= 28	...	123	...	63	...	165+83=248	...	7+4= 11
Connaught	118+46=164	...	60	...	24	...	68+34=102	...	3+3= 6

The details recorded in the Census tables as to sex, age, locality, whether civic or rural, afford important data bearing on the history of man in his social relations, and the influences to which he is exposed. Merely considering the general numbers, we see in the return of deaths on which inquests have been held, how the male sex preponderates. The same preponderance, it is worthy of note, appears more or less marked in the tables of deaths from diseases of an acute kind: even in infancy it is shown under the head of convulsions. Is not the inference, then, fair, that in the male sex the tendency is to morbid action in excess, or to the sthenic diathesis, and that the opposite, or asthenic, rather belongs to the female sex?—as seems to be indicated, also, by the diseases to which they are in a higher ratio subject, such as phthisis and marasmus. As regards crime and acts of violence, we do not offer these remarks anywise apologetically; we would suggest them rather to the reflecting as a reason for exercising control and keeping in subjection that *vis insita* so apt to be in morbid excess, and, acting mentally, productive of crime and insanity, and bodily, producing disease and shortening life. In the instance of murders returned for the several provinces, it is satisfactory to find how comparatively small is their numbers, especially contrasted with suicides; and also that the smallest number has been committed in the one, Ulster, the population of which is reputed to be best educated, most industrious, and prosperous. Less satisfactory is the fact of the large proportion of suicides in the same province; but this is in accordance with another fact already alluded to, as denoted by the Census returns, that the [ill?] educated are more subject to insanity than the uneducated. We have alluded to the greater average age of the female sex: it is a curious fact, and seems to be well established by the Census tables, that comparing the persons of very advanced age in Ireland and England, notwithstanding the great disparity as to the amount of the population in the two countries, there is a larger number of and above 100 in the former than in the latter—viz., 711 in Ireland to 319 in England.* May not this, too, be owing to the same cause, a less demand on the *vis vitæ*, in connexion with a lower kind of diet, and that productive of less action, less injury of the organization, and clogging (if the expression may be allowed) of the vascular system, produced by atheromatous and ossific deposition?

One topic more that we must notice, regretting it must be so briefly, is that which has been well illustrated in the Census tables—viz., the seasons in connexion with disease. The freedom, with occasional and rare exceptions, of the climate of Ireland from malaria—that is, from the cause productive of ague—is perhaps as remarkable as the frequent presence and intensity of those causes which are productive of endemic and epidemic fever not of the intermittent kind, and of dysentery and diarrhœa.

"We attribute (say the Commissioners) the comparative immunity which the inhabitants of Ireland have had from ague, to the circumstance of the almost total absence of marsh or fen."

Adding:

"The extraordinary rapid growth of mosses, &c., while they have by accumulation of vegetable matter produced bog to so great an extent in Ireland, are not subject to those annual

[* We may be allowed to suggest that this is in part accounted for by the absence of sufficiently authenticated records.—Ed.]

decompositions affecting the atmosphere, which have by other plants in all countries [we would rather say in so many countries] and in all times assisted to taint the air and produce disease either in the natives or strangers residing in the vicinity of the marshes in which they grow. Moreover, our bogs, from containing so large a quantity of tannin, may prevent their acting injuriously on animal life." "And thus," observes the writer of the 'Reports upon the Tables of Deaths in 1841,' "I am led to believe that the bogs of this country—the water of which thousands drink, and on the borders of which thousands live with impunity—do not in any way conduce to the propagation of ague, and much less than is generally supposed to that of other epidemic affections."

A like and more remarkable immunity from ague is described as experienced in the pine swamps of Virginia.* It is worthy of remark also, that wet seasons in Ireland are considered generally, though not without exceptions, the most healthy, especially as regards those formidable and ever-recurring diseases, fever and dysentery. As to the climate of Ireland, in relation to the production of these diseases, there seems to be a good deal of obscurity. Etiologically we are disposed to infer that it is rather negative or passive, or at most at times merely predisposing, than in any of its peculiarities positively active ; and that the engendering circumstances of both diseases are rather to be sought for in the unwholesome, crowded state of the dwellings of the inhabitants, and a poor, and in a famine period unwholesome, diet, than in any noxious element existing *per se* in the atmosphere. But the subject is too large to be treated summarily. An opinion is quoted in the Report, that for the last one hundred and fifty years fever has raged decennially in Ireland, without any very obvious cause. The epidemics enumerated following each other so rapidly, hardly accord with the opinion ; they accord better with the commonly-received conclusions, that want, squalor and fever are more associated in the way of causation—a conclusion clearly adopted by the Commissioners of health, who, in their Report on the last great epidemic, state—

"It is impossible not to be struck with the coincidence between the scarcity and consequent high price of potatoes and the prevalency of fever. In 1846, the scarcity was first felt, and fever began to show itself, and as prices still continued to rise in the winter of 1846 and spring of 1847, the effects of a want of food were seen in an alarming increase of fever; and during the years 1847 and 1848, and the greater part of 1849, the prices continued so high as to deprive the poor of their accustomed food, and notwithstanding all the long-continued and benevolent exertions of the Government and individuals, an epidemic of an unparalleled severity and extent continued its ravages."

In accordance with this, the tables of death show that the mortality from fever, as well as from the other great epidemics, was remarkably least in that province in which, from the estates being larger and manufactures affording an aid to agriculture, the distress of famine was least experienced—viz., Ulster. For the sake of comparison, we insert the following:

Deaths from	Leinster.	Munster.	Ulster.	Connaught.
Fevers . . .	47,405	87,741	41,818	45,055
Dysentery . .	11,306	43,930	12,384	25,612
Diarrhœa . .	8,193	16,404	9,321	7,405
Starvation . .	11,027	9,346	1,165	10,232
Totals	77,931	157,421	64,688	88,314

The connexion of the weather, the seasons with proportional mortality is far better determined. From the summary of these, as given by the Commissioners, it appears—reviewing the whole decennial period, that the maximum of deaths from all diseases was during the harsh and uncertain weather of spring ; that the number diminished with the warm weather of summer, and still more with the mild and more constant weather of autumn, increasing with the cold and greater variability of winter. The precise number of deaths returned under each season is the following :—Under spring, 443,182 ; under summer, 373,748 ; under autumn, 192,005, or 25,177 less than in spring ; under winter, 340,787. Even dysentery and

* A Journey in the Seaboard Slave States, with Remarks on their Economy. By F. L. Ormsted. London, 1846.

diarrhœa are not exceptions; indeed, on examining the whole of the great groups in which all the deaths from disease are arranged, we do not find—and it is very remarkable—a single exception, not even in the class of violent and sudden deaths.

We must now, however unwillingly, draw to a conclusion, necessarily passing over many important subjects—important even in vital statistics—in the folios before us. The period included in the Census, short as it is—a single decennium—will always be memorable in the history of the country, and the future historian cannot fail of having a grateful feeling towards the Commissioners, Dr. Donnelly and Mr. Wild, for the vast amount of valuable information which, with so much industry and ability, they have brought together, and more especially relating to the great events of the time,—the failure of the potato crop, suddenly stricken with disease and rot—the famine and pestilence ensuing—and the emigration, with their accompaniments, many of them so terrible, and their consequences, some of them, we rejoice to think, so auspicious. We have already expressed our opinion of the records of these events, as detailed in the Census; we can honestly say that we have never read, whether in history or romance, descriptions of wretchedness, suffering, and degradation so afflicting, and politically so instructive, as those we find in these annals. Here is one picture taken from these records:

"December, 1846.—A terrible apathy hangs over the poor of Skibbereen; starvation has destroyed every generous sympathy; despair has made them hardened and insensible, and they sullenly await their doom with indifference and without fear. Death is in every hovel; disease and famine, its dread precursors, have fastened on the young and old, the strong and the feeble, the mother and the infant; whole families lie together on the damp floor, devoured by fever, without a human being to wet their burning lips or raise their languid heads; the husband dies by the side of his wife, and she knows not that he is beyond the reach of earthly suffering; the same rag covers the festering remains of mortality and the skeleton forms of the living, who are unconscious of the horrible contiguity; the rats devour the corpse, and there is no energy among the living to scare them from the horrid banquet; fathers bury their children without a sigh, and cover them in shallow graves round which no weeping mother, no sympathizing friends, are grouped; one scanty funeral is followed by another and another. Without food or fuel, bed or bedding, whole families are shut up in naked hovels, dropping one by one into the arms of death." (*Cork Examiner.*)

And such, as this picture, there are many more we have marked, of famine-horrors not less distressing and heartrending, which we will spare our readers the infliction of reading. Let us pass to the results—the happy consequences, which are hardly less remarkable than their deplorable antecedents, and we would fain hope will more than compensate for them, in conducing to a healtheir state a more prosperous and less precarious future.

To recapitulate very briefly, even at the risk of some repetition on one side—the disastrous side—we find a diminution of population, from excess of deaths over births, and from emigration, to the enormous amount of 2,466,414; on the other—the prosperous—side we find, using almost the words of the Commissioners, a great advancement of the country; the extent of arable land and the value of farm-stock increased, with a decrease of the very small holdings and an increase of the larger; the worst class of houses in course of being replaced by a better, a smaller proportion of families dependent on their own mere manual labour for support; and the education of the people in favourable progress with increased means, more schools, and augmented Government aid. It may be well to show these ameliorations numerically. 1st. Of the dwellings, the indications of the comfort; the decrease of cabins has been to the amount of 355,689! the increase of dwellings of the better class to that of 73,073. 2nd. Of means of livelihood. In 1841, 42·96 per cent. of the entire population were engaged in some occupation or profession; in 1851, the proportion had increased to 43·37 per cent., whilst those depending on their own manual labour had decreased to 18·9 per cent. 3rd. Of education. In 1851, 5 per cent. more of the population, of the age of five and under sixteen, were at school, than in 1841; and there was a decrease of the proportion of those who could neither read nor write of 4 per cent. males and of 8 per cent. females.

Coincident for the most part with these ameliorations, and mainly connected with them as causes or effects, are some others, such as the opening of inland communications by railways, a closer union with England, and freer intercourse by steamers; the exchange of landed property from insolvent to solvent hands, due to the Encumbered Estates Act; the introduction and diffusion of more capital; a better and more active agriculture, denoted not only by the increase of arable land and the increased value of stock, but even more by the diminution of waste land—viz., from 5,209,492 acres to 3,851,793 acres, and the augmented number of agricultural societies; the better condition and means of life of labourers, and it is believed a better quality of labour from money-payment of wages, and increase of wages from 6*d.* and 8*d.* a day to 1*s.* and 1*s.* 6*d.*; and, through the operation of the New Poor-Law, and increasing attention and regard on the part of the rich towards the poor; lastly, not to omit an important element, the existence of an admirably organized and disciplined police-force, composed of natives, chosen irrespectively of religious creed, and yet acting in harmony—most usefully employed in the public service, the conservators of peace and order, and at the same time available and efficient whenever combined intelligence and activity are needed by the Government, so that through them "the most extensive inquiry can be conducted in Ireland with as much precision and exactness as a model operation on the most limited scale." This has been said of this force by one thoroughly acquainted with them, and we have pretty good proof of its correctness in the circumstance of the police of Ireland having been, as we are assured by the Commissioners, the executive machinery, as enumerators, in collecting the materials of the Census.

Marvellous indeed are the changes and the ameliorations in their totality in so short a period, and most of all considering to what they are principally owing. Let us hope that as the great causes of Ireland's disasters are now so well known, they will be avoided in future; and then, if only justice be done to the people, and they are just to themselves, humanly speaking, reckoning on the capabilities of the country, the career of its prosperity may be pronounced to be certain and almost boundless, and happy, twice happy, the future.

"O! fortunati nimium, sua si bona nôrint."

Review X.

Guy's Hospital Reports. Edited by Samuel Wilks, M.D. Lond., and Alfred Poland. Third Series. Vol. II.—*London*, 1856, pp. 428.

The second volume of the third series of the 'Guy's Hospital Reports' equally merits the praise which we bestowed upon the first. It contains twenty original communications and eight lithographic plates. The following is an analysis of its contents:

I. *Myeloid Tumour of the Scapula*, by Edward Cock; *with a Description of the Growth*, by Samuel Wilks, M.D.—This tumour, which weighed a pound, was removed from the spine of the scapula of a female aged twenty-seven. It consisted of a bony cyst, containing a white curdy substance. This presented the myeloid structure which has been described by Lebert, Paget, and Gray, and to which we have recently had occasion to allude.* There was nothing "malignant" in the history of the case.

II. *Third Septennial Report of Guy's Lying-in Charity. Also Report of the Lying-in Charity for Twenty-one Years.* Collated from the Records by S. J. C. Norman. Presented by J. C. W. Lever, M.D., and H. Oldham, M.D.—

* British and Foreign Medico-Chirurgical Review, vol. xix. p. 337.

These reports consist of a series of statistical tables, which, considering the large number of cases from which they are compiled, are of considerable interest and value. During the twenty-one years (October, 1833, to October, 1854), 22,498 women were attended by the pupils of Guy's Hospital in their confinements.

It is stated that 21,553 children were born alive, while 1128 were still-born; and of the former, 52·3 per cent. were male, of the latter 61·7 per cent.; thus showing an excess of nearly 9½ per cent. in the males still-born, as contrasted with the males born alive, and a corresponding deficiency of the females still-born. This disproportion between the number of male and female still-births, which is accounted for by the larger size of the head in the former case, and the consequent longer duration of labour, is even less than has often been observed.

Table VI. (p. 43), which shows the varieties of labour in 22,498 confinements, is worthy of attention. We find that perforation of the child's head was had recourse to in eighty-two cases, while Cæsarean section was performed twice. In one case of Cæsarean section, both mother and child died; in the other, both survived.

Seven cases of rupture of the uterus occurred during the twenty-one years, all fatal.

Twenty-seven cases of puerperal convulsions are reported. In twenty-one of these cases, the urine was examined, and, with one exception, found to be albuminous, the convulsions in the exceptional case depending on arachnitis. The coincidence of albuminuria and puerperal convulsions was, we believe, first pointed out by Dr. Simpson in 1841; and the recent researches of Frerichs and Braun leave little doubt, that all cases of true eclampsia result from uræmia in the course of acute Bright's disease.

The total number of deaths of mothers was 160, or 1 in 140.

In one case labour occurred (the uterus acting "powerfully") in a woman affected with perfect hemiplegia and incomplete paralysis of the other side. Such cases disprove the doctrine of Marshall Hall and others, that uterine action is dependent on spinal influence, and tend to show, as urged by Drs. Simpson and Lee, that it is rather of a reflex ganglionic nature. Dr. Simpson's view is also confirmed by his observation of contractility remaining in the uterus of the pig, twelve hours after removal from the body.

III. *Select cases.* By H. M. Hughes, M.D.—The first of these cases is one in which the urine assumed a brownish-black colour while the patient was taking creosote. There was no deposit on standing, and nothing revealed by microscopic examination. The colour was unaltered by heat, nitric acid, or liquor potassæ, even when boiled with it. Similar cases are stated to have been observed by Drs. McLeod, Marcet, and Elliotson; and Dr. H. Weber has recently ascertained that the inunction of tar produces occasionally the same result. Dr. Hughes confesses his inability to give any explanation of this curious and rare consequence of the use of creosote.

Dr. Hughes also records a case of intussusception, in which "the whole ascending colon, the cæcum, and an unascertained portion of the ileum, had passed into the transverse and descending colon and rectum;" and another of emphysema of the abdominal parietes, proceeding from perforation of the rectum by the careless introduction of an enema tube.

IV. *On Hernia; with an Analysis of* 126 *Fatal Cases.* By Thomas Bryant.—The author commences his observations by answering the questions, "What form of hernia is most common? what form most frequently requires operation? and which is most fatal?" This he does by appealing to the statistics of the London Truss Society. Of 84,478 cases of hernia, 88·8 per cent. were inguinal and 11·1 femoral. Of 281 cases requiring operation, the femoral exceeded the inguinal by 25·2 per cent.; and of 169 cures after operation, inguinal bore the proportion to femoral of 36·1 to 63·8.

In the analysis of 126 fatal cases, numerous tables are given, the chief results of which are contained in the following important "conclusions:"

"Inguinal hernia is more common than femoral, commences earlier in life, is less liable to strangulation on its first descent, and generally exists twenty years before it becomes so; requires operation less frequently, but is more fatal; is less frequently associated with gangrenous bowel, but more frequently with ulceration at the line of stricture.

"Femoral hernia is less common than inguinal, seldom appearing before fifty years of age; is more frequently strangulated on its first descent, but generally averages eleven years' existence; more frequently requires operation, but is less fatal; and is generally strangulated for a longer period before relieved. After operation, femoral hernia is more frequently followed by sinking than inguinal."

V. *On Concealed Accidental Uterine Hæmorrhage.* By Henry Oldham, M.D.—Dr. Oldham's paper contains a notice of four cases of accidental hæmorrhage, in which the blood was retained within the womb, little or none escaping externally. The rarity of the accident is shown by only three cases occurring out of 22,498 deliveries at Guy's Lying-in Charity. All of these three were fatal. The fourth case recorded—that of a lady—recovered.

VI. *Cases of Lardaceous Diseases, and some Allied Affections; with Remarks.* By S. Wilks, M.D.—Under the term "Lardaceous," the author has described those affections of the liver, spleen, and kidneys, which in this country more commonly are known under the designation of *Waxy.* Forty-five cases of the disease are recorded in the paper.

This affection has been, and is still, very generally confounded with Fatty Degeneration, and even Rokitansky has described them as mere varieties of the same disease. The points of distinction, as regards the liver, were described by Dr. W. T. Gairdner, of Edinburgh, in a valuable paper, published in the "Edinburgh Medical Journal" for May, 1854; the characters of waxy disease being great density, no increase of oily matter, but a substitution for the natural secreting tissue of a dense albuminous material: whereas, in fatty degeneration, the specific gravity is reduced, and a large quantity of oil is deposited in the cells of the normal tissues.

Dr. Wilks's description of the structural peculiarities of this lesion bears a very close resemblance to that which was laid before the Physiological Society of Edinburgh, by Drs. Gairdner and Sanders, in 1854,* and we are somewhat surprised to find no allusion whatever made to their valuable researches.

The clinical distinctions between waxy and fatty kidney have been lately made a subject of observation by Dr. Todd.†

Dr. Wilks has observed a curious connexion between waxy disease and disease of the bones. Thus, "out of thirty-six cases of lardaceous viscera, sixteen had necrosed bone, and eleven very evident disease of a syphilitic or other affection of the osseous system."

VII. *Report of the Post-mortem Examination of the Cases of Burn occurring during the last Year and a half, with reference to the Condition of the Duodenum.* By S. Wilks, M.D.—Dr. Wilks brings forward twelve cases of burn, proving fatal after the third day, and "in no case was there any disease discoverable in the duodenum." These observations are opposed to those of Mr. Curling, who, in the twenty-fifth volume of the 'Medico-Chirurgical Transactions,' recorded twelve fatal cases of burn, in all of which there was either inflammation or ulceration of the duodenum, as evidenced by inspection after death, and vomiting and purging of blood during life. The cause of this difference as to facts, it is endeavoured to explain, by the practice of giving children ardent spirits immediately after the receipt of burns being now obsolete.

* Monthly Journal of Medical Science, vol. i. pp. 186, 393. 1854.
† Clinical Lectures on Diseases of the Urinary Organs. 1857.

VIII. *Brief Report of the Post-mortem Examinations of the Cases of Fever which have died in the Hospital during the last two Years.* By S. Wilks, M.D. —As we have already had occasion to notice,* Dr. Wilks fully recognises the distinction between the two forms of fever, typhus and typhoid, in the cases occurring in Guy's Hospital. The only observation in the present paper to which we shall allude is this, in reference to typhoid fever :

"In three of the present cases, however, where the disease was not quite advanced to its height, some remarkable microscopical bodies were found. These consisted of rounded cells, containing numerous nuclei, which amounted in some to ten in number. They were found equally in the deposit of Peyer's patches and mesenteric glands."

These cells have been described and figured by Vogel, Wedl, and others, and the reason they have so seldom been recognised is, according to Wedl, that they cease to be distinguishable on the establishment of ulceration. It is still doubtful if they are characteristic of typhoid deposits.

IX. *Cases of Paraplegia.* By William Gull, M.D.—This paper contains the history of sixteen cases of paraplegia. In three cases the paraplegia depended on tumours pressing on the cord; in two of the three there were pain in the back, and involuntary muscular contractions of the paralyzed limbs. In one, both these symptoms were absent; in this case there was also considerable softening of the cord. In two, the sensibility was either increased or unaffected; in one, impaired.

In five of the cases, inflammation of the spinal membranes was the lesion, but in one of these cases the only paraplegic symptoms consisted in paralysis of the bladder and rectum. In four of the five cases, pain in the back was a prominent symptom; in one it was absent. In three out of four cases, there were involuntary spasmodic movements of the lower limbs; and in the fourth, though these were absent, there were "convulsive movements of the hands." In two cases, there were contraction and rigidity of the lower limbs, the heels in one being drawn up to the nates. In three cases, the sensibility was increased; in one, diminished: in one completely absent; but in the two last cases, there was also softening of the spinal cord. In one instance paralysis of the upper extremities preceded that of the lower for some months.

In four cases, softening of the cord appeared the principal lesion. In three of these, pain in the back was either absent, or not a prominent symptom; in one it was severe, and in this case only was there also inflammation of the membranes; in two, involuntary movements were either absent or not mentioned; in one, slight; and in one, severe: in this last, there was also inflammation of the membranes. In all four cases, there were more or less anasthæsia.

In the remaining four cases, the paraplegia was produced by effusion into the membranes, with slight softening, lead poisoning (?), tumours in the substance of the cord, and enlargement with induration of the cervical portion.

From the above cases it would appear, that pain in the back, and involuntary movements of the paralyzed limbs, are more common symptoms in affections of the membranes than in softening of the cord, and anasthæsia a less frequent one.

X. *On the Parasitical Vegetable Nature of Pityriasis Versicolor.* By W. Gull, M.D.—The author confirms the observations of Eichstedt on the dependence of pityriasis versicolor on the presence of a fungoid growth (*Microsporon furfur*), and gives a figure showing its appearance under the microscope.

XI. *Some Observations on the Abdominal Sympathetic Nerve, and on the Union of the Phrenic and Pneumogastric Nerves.*—By S. O. Habershon, M.D. —The most important of these observations is the description of a ganglion, called the "diaphragmatic," situated behind the lobus Spigelii of the liver, close to the

* British and Foreign Medico-Chirurgical Review, vol. xvii. p. 318.

vena cava, and formed by the union of branches from the phrenic and pneumogastric nerves and semilunar ganglion.

"The use of the diaphragmatic ganglion appears to be to bring the diaphragm into intimate relation in its action with the abdominal viscera, and to unite the digestive and respiratory and cardiac centres of the sympathetic nerve."

Various sympathetic phenomena are mentioned, to the production of which this ganglion may be subservient.

XII. *On Dysphagia, illustrated by some Cases of Disease of the Œsophagus and Pharynx.* By S. O. Habershon, M.D.—The principal causes of dysphagia are stated to be as follows:

"I. From the disease of the tonsils or palate.
"II. From inflammation of the cellular tissue of the pharynx or œsophagus.
"III. From disease of the laryngeal cartilages or epiglottis.
"IV. From functional or spasmodic stricture of the œsophagus or pharynx, as in hysteria, hydrophobia, &c.
"V. From paralysis of the muscles.
"VI. From acute inflammation of the mucous membrane.
"VII. From mechanical injury or poisons.
"VIII. From structural obstruction to the œsophagus, as—1. constrictions; 2. ulcerations; 3. cancerous disease; and 4. aneurismal or other tumours."

Cases illustrative of most of these forms are brought forward; and among these, are several in which a communication was established between the œsophagus and air-passages, either by simple or cancerous ulceration.

XIII. *On the use of Atropine in Iritis.* By John F. France.—Mr. France advocates strongly the use of belladonna, or atropine, throughout every stage of iritis.

XIV. *Ophthalmic Cases.* By John F. France.—These are two cases of artificial pupil brought to a successful termination.

XV. *On the Alkaline Emanations from Sewers and Cesspools.* By William Odling, M.B., F.C.S.—Dr. Odling has obtained evidence of the existence in the liquid contents of cesspools, of a base, approaching very closely in composition to that of ethylamine (C^4H^7N); but as yet, he is unable to say whether it "is or is not the toxic ingredient of sewer exhalations."

XVI. *On the Detection of Antimony for Medico-Legal Purposes.* By W. Odling, M.B., F.C.S.—Dr. Odling's valuable researches show, that Reinsch's test for the detection of antimony, in which the metal is deposited on a copper plate, is the most delicate which we possess. The trial of "Regina *v.* Palmer" was the first criminal case on record in which this process was employed for the separation of the poison. The details of the process are given, by which it appears that one part of tartar emetic in 500,000 of dilute hydrochloric acid will produce a "decided and characteristic deposit" on a square inch of copper plate. The precipitation of other metals under the same circumstances is also considered; and it is shown that several others, more especially bismuth and tin, may be confounded with antimony. Consequently, the separated metal must be subjected to additional tests; and as Dr. Odling observes, we require "some means for dissolving metallic antimony, having no solvent action upon metallic copper." None of the means previously resorted to effected a complete and easy isolation of the antimony, and hence Reinsch's test was seldom employed. The process, however, recommended by Dr. Odling leaves nothing to be desired.

XVII. *On the Gastric Juice as a Solvent of the Tissues of Living Animals.* By F. W. Pavy, M.D.—Dr Pavy gives the results of experiments of the following nature:—Through a fistulous opening which he had established into the stomach of a dog, he introduced, during digestion, the hind legs of a living frog, and the ear of a living rabbit. In both instances the parts introduced underwent digestion after two or three hours. Similar experiments, in the case of the frog, have been often performed on the Continent. From these experiments Dr. Pavy argues, that the capability of resisting its own digestive powers, possessed by the walls of the stomach during life, and which ceases with death, is not due, as John Hunter thought, to its being endowed with "the living principle," but to its epithelium and mucus being constantly reproduced as soon as digested.

XVIII. *On Poisoning by Strychnia, with Comments on the Medical Evidence given at the trial of William Palmer.* By ALFRED S. TAYLOR, M.D., F.R.S. —This paper, which extends over 134 pages, and which, with an Appendix, has also appeared in a separate form, constitutes a most complete refutation of the personal attacks which were made upon the author during Palmer's trial, both by the counsel for the defence and by a portion of the public press; and also contains much valuable information concerning the deadly drug by which Cook was poisoned. It would be out of place here to enter into the details of the circumstantial evidence, which alone was sufficient to convict the prisoner; but we shall confine our attention, for the most part, to a few medico-legal questions of great importance, and to the investigation of which Palmer's crime may be said to have first directed us; and first—

1. What are the differences between tetanus produced by strychnia and that resulting from natural causes?

It will be recollected that it was urged for the defence, that the tetanic convulsions of which Cook died, might have been due to idiopathic tetanus, or have been of a traumatic character caused by sore-throat. It became, therefore, a matter of the highest importance to ascertain, what are the differences in the physiological phenomena of tetanus produced by strychnia and that resulting from natural causes. Fortunately such differences exist in a marked degree, so that the secret murderer need no longer flatter himself that, by employing strychnia, he will escape detection. These differences are so important, that every practitioner should be acquainted with them; and we think we cannot do better than give the following extract in Dr. Taylor's own words:

"Tetanus, which implies a general spasm or cramp of all the muscles of the body, that are usually under the power of the will, may arise from—1. poison; 2. wounds (lacerations, severe bruises) (traumatic); 3. exposure to cold and wet (idiopathic.)

"As to *poisons*—arsenic, antimony, and other irritant poisons, may occasionally produce tetanic spasms of the muscles; but then there are always other symptoms which precede or follow, of a totally different kind.

"Strychnia is the only poison (with the exception of brucia) which produces tetanus in a pure and unmixed form.

"In Cook's case there was no wound or personal injury; hence it was not a case of what is called 'traumatic tetanus.'

"There was no exposure of the deceased to wet or cold.

"DIFFERENCES.

"*Idiopathic Tetanus from Exposure to Cold and Wet.*	"*Tetanus from Strychnia.*
"1. Symptoms have no connexion with any liquid or solid swallowed.	"1. Some solid or liquid taken within about two hours or less of commencement of symptoms.
"2. Symptoms commence slowly, and progress slowly; difficulty of swallowing; stiffness of jaws, of neck; after some time, the body, the legs, lastly the arms; hands not commonly affected.	"2. Symptoms commence suddenly with great violence. Nearly all the voluntary muscles of the body are simultaneously affected. Arms and hands spasmodically clenched at the same time as body and legs. Jaw not primarily affected, not always fixed.

	"(N.B.—Cook was able to swallow and speak within ten minutes of his death.)
"3. Opisthotonos, or body bent back in the form of a bow, resting on head and heels; does not come on until after many hours or days from the attack.	"3. Opisthotonos a very early symptom, in a few minutes commonly.
"4. Paroxysms, or fits of spasm, may be severe, and the person may die from exhaustion. Patient commonly recovers after some days or weeks.	"4. When symptoms are once clearly established, they progress to death or recovery. They occupy only minutes. In from ten minutes to two hours after commencement the person dies or recovers, according to the severity of the paroxysms and strength of his constitution.
"5. In idiopathic or traumatic tetanus there is no *intermission* in the symptoms, merely a remission of the paroxysms. The patient is always under the influence of the morbid cause, which remains until he dies or recovers.	"5. In tetanus from strychnia, if the dose should not be sufficient to prove fatal, the effects pass off; patient recovers; there is a complete intermission in the symptoms. "(N.B.—This was a remarkable feature in Cook's case.)

"*Post-mortem Appearances.*

"There is nothing of a peculiar character in these appearances, whether the tetanus depend on disease or on strychnia."

Dr. Taylor proceeds to consider each of the above differences individually, with reference to the case of Cook, and whether the symptoms in his case might have arisen from some natural disease. He brings forward all the cases of poisoning by strychnia which have been communicated to him, or which he has been able to find recorded, (26 in number, including those of the appendix,) and shows that the symptoms during life, and the contracted, empty condition of the heart found after death, were quite consistent with strychnia poisoning in the human subject. It was argued in the defence, that the length of time which elapsed before symptoms came on in Cook's case (1 or 1¼ hour), was inconsistent with strychnia poisoning. From experiments on sixty animals, Mr. Nunnely deposed on oath, that this interval varied "from two to thirty minutes," and was "more generally five or six." Dr. Taylor observes, that Mr. Nunnely's animals must have been singularly susceptible of the effects of strychnia; and he himself cites cases of animals in which the fatal tetanic symptoms did not supervene for five, eight, or eleven hours after the administration of strychnia; and he mentions one case of a man in whom the symptoms did not appear for two hours and three-quarters.

The next question to which we shall briefly allude, is,

2. Can a person die from strychnia and no trace of that poison be found by chemical analysis in the body? Every endeavour was made in the defence of Palmer to show that no man can die from poison, except poison be found in his body, and to throw mistrust upon symptoms and appearances as evidences of death from poison. Who under such circumstances would be safe? There are many vegetable poisons, such as the ordeal bean of Africa and laburnum seeds, for which chemistry as yet possesses no tests. There are poisons of such a nature, that

"While no chemical tests can reveal their presence in the body, their unlawful use may be surely and satisfactorily indicated by the suddenness, intensity, and peculiarity, as well as the fatal rapidity, of the symptoms which they produce."

Physiological and pathological phenomena, therefore, must not be rejected in cases of poisoning; and, as Dr. Taylor observes,

"Let it be remembered, that if the physician, as a pathologist or physiologist, may be deceived by symptoms, the chemist may be equally deceived by his tests. He may, and often has, pronouuced poison to be present when it was not, and he has overlooked it when it was present."

It was argued by Sergeant Shee, that if $\frac{1}{1000}$th of a grain of strychnia "had entered into the human frame at all, it could be, and must be, detected by tests which are unerring;" and Mr. Herapath avowed on oath, that if this poison had caused death, he could detect it up to the time that the body had become "completely decomposed;" in fact, when it was converted into a "dry powder!" Dr. Taylor proves, and proves most clearly, the utter groundlessness of such assertions. Cases are adduced (p. 356) of deaths from strychnia, in which that substance could not be detected after death. In one case, although two scruples had been taken, and the patient died in one hour and a-half, no strychnia could be found either in the stomach or blood. Another case is mentioned of a man who swallowed four grains of strychnia; one hour after, the stomach was emptied by the stomach-pump; the contents were examined by Dr. Stenhouse, of St. Bartholomew's Hospital, but "he did not detect any of the strychnia." Moreover, Dr. Taylor has had recourse to experiments on animals, but of six instances of animals poisoned by strychnia, and in which it was searched for after death, in two it could not be found in the contents of the stomach, and in none was it found in the blood or tissues. These facts speak for themselves.

Dr. Taylor also considers the reasons for the non-detection of strychnia in the bodies of persons who have died from its effects; under the heads of—1, the quantity taken; 2, the time which has elapsed after taking the strychnia, until the symptoms commence; and 3, the careful preservation of the stomach and its contents. In Cook's case, the stomach, before being sent to Dr. Taylor for examination, had been "cut open and turned inside out," and "there were no contents."

3. What becomes of the strychnia in a case of poisoning in which it cannot be detected by chemical analysis?

Strychnia is not easily destroyed by putrefaction. This Dr. Taylor admits. The advocates of Palmer maintained that it was indestructible as the diamond, and therefore ought to have been detected; while at the same time they endeavoured to persuade the jury, that the whole *body* had been searched for it, and not the *stomach* only. Now experiments have shown that strychnia may be absorbed from the stomach with great rapidity. Dr. Macadam has detected its presence in the urine of animals nine minutes after it had been swallowed. At the same time there is reason to believe that strychnia "partially undergoes some change in the blood." Thus, Dr. Harley was unable to detect it in the blood of a dog, poisoned by the injection of $\frac{1}{12}$th of a grain (of acetate) into its jugular vein. Here the poison must have undergone some change, for according to Mr. Herapath, there are tests which would have demonstrated the presence of $\frac{1}{50000}$th part of a grain. Moreover, Drs. Harley and Macadam have ascertained that animals may be fed on the flesh of others poisoned by strychnia, and exhibit no tetanic symptoms. Here again it seems probable that the strychnia, on entering the system, undergoes some change, so that it no longer possesses a poisonous action on animals.

We repeat, in conclusion, that Dr. Taylor has fully exonerated himself from the "imputations of rashness, ignorance, and prejudice," which were attributed to him, and has justified on every point the evidence which he gave at the trial of William Palmer.

XIX. *Analysis of the Water of the Great Geyser, Iceland.* By Alfred S. Taylor, M.D., F.R.S.—The peculiarity of this water consists in the large amount of silica which it contains (almost forty-eight grains in one imperial gallon). This is kept in a state of solution by carbonate of soda, and the high temperature of the water (190° F.).

XX. *Remarks on the Physiological Effects of Strychnia and the Woorali Poison.* By F. W. Pavy, M.D.—These two virulent poisons are produced by two plants, strychnos nux vomica, and strychnos toxifera, belonging to the same botanical genus. Dr. Pavy's experiments would show, that both produce death by arresting the respiration: strychnia, by inducing spasm of the respiratory muscles;

woorali poison, by inducing paralysis. He states, "under the influence of both, the heart remains perfectly free." These results, as regards strychnia, are opposed to those arrived at by Dr. Harley ('Lancet,' June 14th, 1856).

Review XI.

Traité de l'Angine Glanduleuse, et Observations sur l'Action des Eaux Bonnes dans cette Affection; précédés des Considerations sur les Diathèses. Par Noel Gueneau de Mussy, Médecin de l'Hôpital de la Pitié et de l'Ecole Normale Supérieure, Professeur Agrégé à la Faculté de Médecine de Paris. pp. 269. 1857.

Treatise on Angina Glandulosa, with Observations on the Action of the Waters of Eaux Bonnes in the Complaint; preceded by Remarks on Diathetic Affections. By Noel Gueneau de Mussy, Professor, &c.

The scrutinizing spirit of modern medicine which has brought to light so much of the morbid actions and products of different parts of the body, has of late years been very successful in making known the disorders of the laryngo-pharyngeal mucous membrane; which if not wholly unobserved by nosologists and the earlier writers, are at least unrecorded by them. Many important examples of disease in these parts have been carefully noted, and arranged in groups, according to the tissues affected, the diathesis, or the disease with which they are associated.

M. Chomel had for some years in his clinical lectures described certain affections of the throat, consisting chiefly of an enlargement of the glands of the laryngo-pharyngeal mucous membrane, common amongst speakers, readers, and singers, and associated, as he believed, with what he has termed a herpetic diathesis. But at the same time Dr. Green, of New York, as every one is aware, published his well-known work upon the diseases of the air-passages, including clergymen's sore-throat, &c. From that time the subject has received much attention in this country, and the topical medication recommended by Chomel and Green has had many advocates. Topical medication has extended to other diseases, such as hooping-cough; and other local agents besides those recommended by the above writers have been advocated in this country. Dr. de Mussy is, we believe, the first French physician who has devoted a separate treatise to the consideration of these maladies. His work discusses glandulous angina, but we have no doubt that under this title he has comprised examples of disease which would be more properly regarded as cases of chronic inflammation of the laryngo-pharyngeal mucous membrane; for the implication of the glandules, or, as Dr. Green less correctly regards them, of the follicles, is so slight, compared with the morbid condition of the membrane, in some cases, as by no means to justify the title. Membranous inflammation, with slight glandulous complication in its simple form, acknowledges the same causes, symptoms, and conduct under treatment. We believe that it is only in a moderate proportion of cases of chronic throat aliments, signalized by frequent "hemming," dryness and tickling, hoarseness and loss of voice, that the glands of the mucous membrane are conspicuously or pre-eminently affected.

The title of *angine glanduleuse*, or glandular angina, first employed by Chomel, and adopted in the present work, while it would restrict the principal part of the local disease to the glands, is otherwise not free from objection. The word angina signifies suffocation, and is derived from the Greek word ἄγχω, I strangle or hang, and by the Greek and Roman physicians it is applied to those diseases in which a tendency to suffocation is a marked feature. Now, it will not be asserted that in glandulous angina there is any material tendency that way. Glandular angina would be a very appropriate title—did we require a new one—for inflammation of the parotid and other salivary glands, and for enlargement of the lymphatic glands of the upper part of the neck, often dependent upon the absorption of poisonous secretions in the fauces, and which we have ourselves known to cause death by suffocation.

Chomel's doctrine of the almost invariable dependence of glandulous angina upon the herpetic diathesis, has been fully adopted by the author of the work before us. The introduction is devoted to the discussion of diatheses in general, and of the herpetic diathesis in particular. According to our author, most acute diseases are the results of accidental transient causes, while chronic maladies originate in an inherent disposition of the organism itself, or from the long-continued operation of external influences. The name of diathesis is given to those pathological conditions, to those morbid constitutional states which reveal themselves by multiplied, successive, or simultaneous manifestations.

The diatheses play an important part in the production of chronic diseases.

According to Dr. de Mussy, the diagnosis of diathetical affections, which do not present well-defined topical characters appreciable to the senses, is to be made out by reference to their duration. If diseases last longer than is usual with them, they are to be regarded as diathetical. With him unusual chronicity is almost always held to be pathognomonic of diathesis. This may be generally true, yet disease may be unusually prolonged by the operation of unknown external causes, or certain obstacles to cure in the body itself, altogether independent of diathesis.

Dr. de Mussy makes some judicious observations on the treatment of diathetical diseases. He says—

"When a person is under the influence of a diathetical affection, to devote exclusively to the local disease the whole of our therapeutic efforts, is in many cases fruitless, and sometimes a source of danger, if the manifestation which it is desired to repress does not compromise life, and if the diathetical action may develope itself in another organ, the functions of which cannot be disturbed without peril." (p. 16.)

He maintains that the physician should abstain from all topical medication when the disease is of long standing, when it entails little or no inconvenience, when there is reason to fear the substitution of graver disorders, and when the patient is old, has little elasticity, or few resources.

The author recognises two important causes in the production of diathetical manifestations. The first is debility of the organism; the second is a local irritation. In this matter he only reiterates the opinion of the medical world.

The author dwells upon the fact, well known to practical physicians, that affections of the mucous membranes frequently alternate with diseases of the skin.

"We often see herpetic affections, pulmonary catarrhs, and obstinate diarrhœa succeed and replace each other in so remarkable a manner, that it is difficult not to admit a connexion between these different manifestations." (p. 27.)

Again:

"When we see this balancing of morbid conditions, occupying different seats, when the internal malady which has replaced the external affection exceeds in its duration the ordinary limits of an accidental disease, we can admit, without carrying the results of inductive reasoning too far, that behind these different manifestations there is concealed or disguised, to speak in the picturesque language of ancient medicine, one morbific cause, one pathological condition of the organism—in a word, diathesis—the origin and substratum of these different disorders." (p. 27.)

The author enumerates the causes of the herpetic diathesis in the following paragraph:

"After hereditary taint, errors of regimen, residence in an impure atmosphere, an exciting diet, or the habitual use of certain unwholesome articles of food, exciting or prolonged emotions of the mind, long-continued watchings, neglect of hygienic rules suitable for the maintenance of the functions of the skin, direct chemical or mechanical irritations applied to this organ, excitement of the functions by heat of the atmosphere; all those conditions which can weaken the vital energy or disturb the harmony of the functions, such as puberty or the cessation of menstruation, pregnancy, fatigue of body and mind,—are the causes which appear to intervene the most frequently in the development of herpetic manifestations." (p. 33.)

According to Dr. de Mussy, herpetic manifestations once developed have a great tendency to persist and reproduce themselves; and if the tendency be strongly pronounced, it is not unfrequently observed that their disappearance, either spontaneous or procured by topical applications, is succeeded by affections of the internal organs. We see, he adds, glandulous angina following herpetic affections, which it seems to replace; "the skin is a blood instrument," he informs us, "a vast vascular network, an immense secreting or absorbing surface." (p. 38.) No one, we conceive, will be disposed to doubt the correctness of Dr. de Mussy's views respecting the causes of the so-called herpetic manifestations. His views on diathesis, and on the tendency which suppressed or repelled skin diseases have to induce disturbance of the internal organs, would be extremely useful had they not long since been inculcated. The truisms which he communicates—elegantly, we admit—would not be less appropriate, we are disposed to think, in the discussion of almost every other disease to which the human body is subject. We are inclined to admit no closer connexion between herpetic affections and the so-called glandular sore-throat, than exists between this affection and many other external and internal diseases. In support of this opinion we would simply state that, during an inquiry made recently at one of the hospitals in London, we have met with many examples of the disease in persons perfectly free from all disease of the skin, and that out of numerous examples, in only one or two was there the least manifestation of a herpetic character. On the other hand, we observed that persons suffering from pityriasis, eczema, and acne, to a striking extent, and therefore selected for examination, presented the uvula, the tonsils, and pharynx in a state of the most perfect health.

The herpetic eruptions may appear upon the soft parts in the interior of the mouth, we readily admit. We have occasionally seen vesicles on those parts. Within the last few days we have seen a very fine vesicle on the uvula of a great smoker of tobacco. But this indicates no peculiar attraction on the part of the herpetic diathesis for the interior of the mouth. How many diseases are found in that locality? do we not find syphilis, the exanthemata, and many skin affections there?

It was only yesterday we saw psoriasis well developed upon the tongue of an elderly lady, whose entire trunk and arms are covered with this squamous disease. Erysipelas is known to have its manifestation in the throat; and an enormous carbuncle on the nape of the neck, we have lately seen in combination with nearly all the symptoms which Dr. de Mussy would hold to mark the course of glandulous angina.

Following Dr. de Mussy, the characteristic of glandulous angina, as the title would perhaps indicate, is a morbid development of the glandules of the velum palati, uvula, of the pharynx, and larynx. The author informs us that the structures generally described as follicles, are in reality glands arranged in clusters, the only parts in which follicles are found being the posterior and lateral parts of the tongue. Into these follicles, according to our author, the excretory ducts of the glands open.

"Upon the posterior portion of the palatine vault, and upon the inferior aspect of the velum palati, the glandules in clusters form many continuous beds. On a level with the little depressions described by Albinus, and which, when they exist, are found in the vicinity of the palato-maxillary articulation, these glands, instead of being gathered together, separate and soon disappear. They show in great force in that part of the pharynx which is situated between the atlas and axis behind, the basilar process above, the superior face of the velum palati inferiorly, the posterior opening of the nasal fossæ in front, a region which has been described by M. Sappey under the name of posterior cavity of the nasal fossæ. They are found in great numbers at the superior part of the pharynx, and arrange themselves in groups around the mouths of the Eustachian tubes. They become more rare as we approach the œsophagus. . . . . Some of these glandules are found lodged in the little holes of the fibro-cartilage of the epiglottis. . . . . They are very numerous in the larynx, more particularly under the mucous membrane of the ventricles." (p. 14.)

We have recently examined these parts, and have seen no reason to doubt the accuracy of the descriptions of Dr. de Mussy.

Amongst the circumstances which favour the development of glandulous angina, Dr. de Mussy ranks sex. Men are more subject than women to this disease: sixty-six men suffered, while only twelve women were affected. This great preponderance of the male sex does not correspond with our experience. Of twenty-two patients examined only during the last ten days, we found that thirteen were females and only nine were males. The influence of profession has been dwelt upon by some writers; and though the point is noticed by Dr. de Mussy, we do not perceive that he has added any facts to our stock of knowledge. Dr. Green had remarked that clergymen were special sufferers, and M. Chomel has indicated that barristers, readers, and singers, are frequently affected.

Dr. de Mussy is of opinion that of all the occasional causes of glandulous angina, the practice of smoking tobacco is one of the most indisputable. It produces an irritation of the mucous membrane of the glandules. He believes that, in order to effect a cure, it is necessary to renounce the practice. The practice of snuff-taking he likewise believes to be injurious. One of his most remarkable cases occurred in an old man who took snuff to an immoderate extent. Mr. Solly no doubt can produce similar testimony. There can be no question whatever of the tendency of tobacco smoke, or of tobacco in the shape of snuff, to create irritation of the mucous membrane and of the glands situated beneath it. Yet we are strongly inclined to believe that the evils they actually produce are greatly overrated. A man suffering from material irritation of the lining membrane of the nasal fossæ and of the pharynx, is not likely to persist long in the immoderate use of tobacco. At all events, we see hundreds who daily smoke and snuff, and of these only a very small proportion suffer from glandulous or any other angina. We would say in passing, that while we deprecate the practice of smoking, we cannot admire that lax science which makes untenable assertions, even in furtherance of a good cause. We know many persons afflicted with phthisis—and comparatively few so situated are altogether free from some lesion of the laryngo-pharyngeal membrane—who not only smoke tobacco, but derive benefit from its use. Several of our patients smoke stramonium, and are soothed by it. Dr. de Mussy takes no notice of many acknowledged irritating agents. He is all but silent on the injury caused by excess in the use of spirituous liquors. The peculiar action of some medicines on the membrane under consideration is omitted. Croton oil and belladonna, and, in many persons at least, the iodide of potassium, exercise a most irritating influence; yet when treating of the causes of the disease, these agents are unnoticed. We have known a state of intense irritation produced in the laryngo-pharyngeal membrane by a strong enema of extract of belladonna, and the same state of things has occurred, together with severe coryza and swelling of the eyelids, from a moderate use of iodide of potassium. It would be interesting to know the result of the almost universal and hourly practice of chewing tobacco prevalent amongst British sailors. No one who has been in an English sea-port will accuse them of aphonia.

The disease which Dr. de Mussy describes he acknowledges to be essentially chronic, yet it sometimes assumes an acute character. In the acute form the progress is quicker, and it occurs in attacks marked by long intermissions. Febrile symptoms are associated with the local disorder, fits of coughing causing a sense of suffocation and oppression at the sternum, and tickling at the fauces, annoy the patient, who is in the frequent habit of making short and violent expulsive efforts from the larynx, producing a sound like that of the word "hem." This so-called English verb is of very frequent occurrence in the work before us, and is perhaps more familiar in the mouth of Dr. de Mussy than in that of any Englishman with whom we have met. By the way, he labours under a singular impression in respect to an allied word "hawking," which he characterizes as "picturesque."

Some examples of the acute form of the disease are given, but they contain nothing of material interest, while they occupy considerable space.

The author now proceeds to the consideration of the main subject—the symptoms and progress of glandulous angina. His descriptions are good, and we shall therefore give a few extracts.

"*Precursory phenomena.*—Glandulous angina, from its first appearance, seems sometimes to announce itself by frequent attacks of croup-like cough, coinciding with herpetic manifestations. Later, there is a disposition to transient hoarseness, which is repeated many times each year, chiefly in the variable seasons. This alteration of the voice, as I have already said, is often preceded by repeated coryzas, which leave in the posterior part of the nasal fossæ a habit of excessive secretion, of a thick yellow or greenish colour, which the patient is compelled to bring from the throat into the mouth." (p. 46.)

First Stage.—

"In general, the malady manifests itself after puberty. Sometimes its invasion is sudden; it occurs after immoderate exercise of the voice, or after sudden cooling. The patient experiences from time to time a sensation of embarrassment, of pricking, of tickling, of smarting in the throat, of closure of the larynx. . . . . Some patients complain at times of a little pain in swallowing. . . . . The expectoration is scanty; in the morning the patient from time to time, especially after efforts of 'hemming' or coughing, emits some viscous masses, sometimes opaque, more frequently transparent or slightly opaline, grey when the patient has inspired the smoke of fire-places or of artificial lights, strewn with white points very like diluted starch, as has been remarked by M. Chomel." (p. 47.)

We have found expectoration of this character to be composed of large mucous cells with dark granules.

Second Stage.—

"The voice is habitually harsh, rough, hoarse, often lower than its natural pitch. With some, aphonia becomes complete and permanent. The tickling in the larynx grows more continuous and more uncomfortable. With some patients, instead of a sensation of tickling, there is a snorting, a pricking, a painful sense of dryness, a burning, which seems sometimes to descend the entire length of the œsophagus, and to reach to the cardia. The patients are seized all at once with fits of cough of extreme violence, resembling the cough of pertussis, accompanied with a sense of anguish and suffocation; their features become livid, their eyes are bloodshot; then the sputa, which I have described, suddenly escape, and are projected to a distance of many yards, as if discharged from a shooting-tube, and the expulsion of which terminates the crisis."

Dr. de Mussy devotes a chapter to the consideration of the lesions and pathological conditions of the parts affected. The lesions are carefully described, but we do not perceive much that is new. The account of the pathological anatomy is given from the observation of the parts taken from only two patients, one of whom died of phthisis. Like Dr. Green, Dr. de Mussy has met with calcareous concretions in the tonsils. These sometimes are found, according to Dr. Cox of New York, sufficiently large to interpose a difficulty in excising the enlarged tonsil. Dr. Robin has proved by the aid of the microscope, that these concretions originate in small crystals found in the glandules. The uvula, Dr. de Mussy states, is elongated in a great many cases, and he refers this alteration to infiltration of the submucous tissue. He correctly describes the dilated bloodvessels, which exhibit their flexuous course upon its anterior aspect. He notices the fact, that the uvula, though double its usual length, may retain its full contractility, though he has generally observed that the muscular power of the part is much reduced. Low inflammatory action, and the presence of œdema, readily account for the loss of power.

The enlargement is not always due to œdema, it is referrible in many cases to actual hypertrophy. The author has seen much advantage arise from the amputation of this enlarged part, the proceeding having caused the vomitings and sense of suffocation to cease. There can be no doubt that this operation is calculated to be of great use when the uvula is so long as to lie upon the tongue, or to reach the glottis. We have lately examined the fauces of two patients, both afflicted with phthisis, in whom amputation of the uvula had been performed, and whose fauces had become perfectly healthy. The operation in one case had been performed by Dr. Simpson, of Edinburgh, and the other had been undergone by the advice of Dr. Stokes, of Dublin. The uvula frequently presents two kinds of elevation—one smaller, such as are found upon the vault of the palate; the other larger, the same as are presented by the large glandules in the pharynx. They often present at their summit a yellow colour, which Dr. de Mussy refers for the most part to the presence of pus. We have ourselves found plates of cholesterin and epithelial cells. The pharynx presents large elevations, sometimes the size of a lentil; these, according to Dr. de Mussy, are enlarged glandules. He has seldom seen ulceration of the pharynx, and we believe it is of much rarer occurrence than Dr. Green and others suppose. Neither has he found tubercular deposit in the pharynx, although, like many other observers, he has seen it in the larynx.

Dr. de Mussy finds the epiglottis frequently the seat of ulceration, the favourite points being its borders. Examined under the microscope, the tubes of the glandules were found larger than natural. The layer of epithelium lining their interior was a little thicker than ordinary. The characteristic of the morbid condition of the glands was one of simple hypertrophy. In some of the larger glandules were found little calculi, chiefly composed of carbonate of lime.

In Chapter X. we are informed that the most frequent complications of glandulous angina are coryza, tracheo-bronchitis, and tuberculous disease. The affection very often extends beyond the larynx, and invades the trachea and bronchi. Dr. de Mussy correctly remarks, that in a large proportion of phthisical patients the pharyngeal glandules are abnormally developed. It has been observed at the Hospital for Consumption and Diseases of the Chest at Brompton, that a large proportion of the patients suffer from aphonia more or less complete, and that the early supervention of this complication argues badly for the result. Of 116 patients, mostly phthisical, whom we examined in one day lately, 10 persons, or 8·6 per cent., had aphonia. The passage of acrid sputa over the surface of the larynx is doubtless a frequent cause of the laryngeal complication; but this is not always the cause, for we find it when there is little or no expectoration. It seems frequently to depend upon the deposition of tubercle in the mucous membrane, or the propagation of irritation along the bronchial tubes. Dr. de Mussy has seldom seen the œsophagus implicated, and he contests the view of Dr. Green, that when implicated the disease may become cancerous.

On the treatment of the disease, Dr. de Mussy is very expansive. When on the subject of hygienic rules, he enforces the importance of pure air and reparative regimen, and freedom as far as possible from moral emotions. The patient, he says, equally dreads a cold, humid atmosphere, and one that is very dry. Currents of air are to be shunned, and he should avoid walking contrary to the direction of the wind. The food and drink should not be exciting. The voice is not to be much exercised. These rules will meet with the approbation of the profession, but Dr. de Mussy has omitted all mention of clothing. Clothing sufficient to guard against external cold, or the undue lowering of the temperature of the body, is of the utmost importance, and it is essential to attend to this point by night as well as by day. We have known examples of disease of the larynx to depend upon the accidental sleeping without the usual night-cap, aye, even upon such a trifling reduction in clothing as is involved by replacing a larger shirt-collar by a smaller one.

While upon the subject of hygienic rules, we would suggest that the practice of having ventilators in our rooms and public conveyances, placed on a level with our ears and throats, is very favourable to the invasion of this and other allied diseases.

True to the doctrine that glandulous angina depends upon the herpetic diathesis, Dr. de Mussy, in treating the disease, relies chiefly upon the exhibition of sulphur; and here he closely follows his teacher, M. Chomel. The natural waters are the form most depended upon, and those of Eaux Bonnes, in the Pyrenees, are greatly preferred. He says:

> "For a long time the sulphurous waters have been employed in this disease, confounded under the name of bronchitis or laryngitis, in the class of those numerous catarrhal affections in which the sulphurous water-treatment is applied with so much success. For my own part (he says), I have often proved its efficacy. It appears to me to answer the various indications of the disease under discussion." (p. 129.)

The immediate results of the treatment are these:

> "It appears that the nervous action becomes more powerful, nutrition and assimilation are more active; all the functions are performed with more energy and harmony; the patient gives expression to these combined results by saying he has more life."

His appetite increases, his muscles acquire more vigour, he experiences a general excitement which is reflected upon the intellectual faculties. The functions of the skin are stimulated, in general the respiration is more easy and abundant. The Eaux Bonnes are administered internally, and applied externally in the form of baths. The duration of the treatment is divided into seasons of between twenty and thirty days. One season will frequently suffice, but in old standing cases two seasons are often necessary.

It has been long acknowledged by sagacious observers, that not a little of the benefit which the sick derive from a residence at watering-places is due to the change given to the mind, and to the operation of a pure atmosphere. The atmosphere and climate enjoyed by the frequenters of the Eaux Bonnes are of the most salubrious character. Dr. de Mussy says:

> "An incontestable fact in favour of Eaux Bonnes is the excellence of the atmospheric conditions. The air has that purity which we seek in the mountains, without being exposed to the violent agitations which are elsewhere experienced. At Eaux Bonnes the atmosphere is habitually calm; the girdle of mountains which surrounds this valley, and the slope on all sides, oppose an almost insurmountable barrier to the winds, or do not permit their arrival until they have lost all their impetuosity." (p. 142.)

The author has a few words, and only a few, to say on the use of other internal remedies. He has very little faith in mercurial preparations, and finds in Dr. Green a faithfulness to "the traditions of Anglo-Saxon medicines," because that physician combines the use of mercurial preparations with topical medication. He has had little experience in the use of the preparations of iodine. For our own part we have seldom seen iodine of much use in such diseases: applied locally it has produced great irritation; and even internally administered, as we before stated, it has caused irritative action in the neighbouring parts, such as the nose and the conjunctivæ. Some physicians have reported advantage from inhalation of iodine in pulmonary affections, associated with disease of the larynx. The consideration of internal remedies concludes with a few words on the employment of balsams and narcotics. The former are recommended after the more acute symptoms are appeased, and the latter, held to be necessary in the acute forms of the malady, are very unfit auxiliaries in the chronic state when the patient is of an irritable constitution. Of the latter class of remedies, the most important are opium, hyoscyamus, lactucarium, cherry laurel water. Belladona is contra-indicated.

Dr. de Mussy believes that the sulphurous treatment alone will in many cases succeed in the cure of the disease; but he acknowledges that examples of glandulous angina do occur which resist this treatment. He has then recourse to topical medication, which he holds to be very valuable.

The topical medication is discussed under three heads: dry insufflations, liquid applications, and gaseous inhalations. The dry powders which have been employed in France are sugar, sub-nitrate of bismuth, acetate of lead, alum, sulphates of zinc and copper, nitrate of silver, and calomel. Dr. de Mussy makes use of dry powders less frequently than of liquid applications. Of the latter agents, nitrate of silver is, as in this country, the most esteemed, and in applying it the common laryngeal probang is employed. This he claims as the invention of M. Trousseau. The author communicates little that is novel on this point, or on the use of moist inhalations. M. Trousseau has recommended the employment of cigars made of paper, saturated with a solution of arsenic. Dr. de Mussy is of opinion that the laryngeal probang very seldom penetrates the larynx, and shares the scepticism of some surgeons in this country. We would simply remark here that we have made the experiment upon the dead body, and after having, as we believed, easily introduced an instrument into the larynx, have cut down upon it, and found it there. Dr. de Mussy is not aware that the topical use of oils, glycerin, tannin, and other agents, has been proposed and adopted in this country. We happen to know that glycerin is employed with the most soothing, if not absolutely curative, results in some affections of the glottis and larynx. It is applied to the glottis by means of a large camel-hair pencil, or introduced into the larynx with the probang. The former method is easily accomplished, and scarcely ever fails to remove the sense of tickling and dryness, and to improve the voice from the moment it is practised. Though the author does not refer to the subject, we would mention that the glottis may be very advantageously brought into view with the aid of Avery's glottis speculum.

A few words on the use of vesicatories and croton-oil frictions, and recommendatory of the amputation of the uvula when elongated, close the treatment of the disease.

In conclusion, we would here express our conviction that the present work reflects credit on the author. The descriptions of the diseases and of the morbid appearances are excellent and truthful; the arrangement of the work is exact, and the language is ever remarkable for its elegance. The subject has been discussed in all its relations. The morbid appearances are well and faithfully depicted. The microscopical anatomy, as far as we know, is to be relied upon; it corresponds with our own observations; but we regret that the author's opportunities for research have been so limited. The connexion between glandulous angina and the herpetic diathesis we believe to be over-stated; and we know that the sulphurous treatment, at least in this country, is less essential than the author believes. There are grave omissions in the work—the necessity for regulating the liver and bowels is scarcely glanced at; the employment of quinine and the mineral acids is passed over in silence; and though the administration of alkalines, combined with vegetable tonics or the decoction of ulmus fulva, is so beneficial in some forms of skin disease he would designate herpetic, and in the lithic-acid diathesis, these important agents are only once referred to, and that incidentally in a note; cod-liver oil, dry cupping, and leeches, have not one word in their favour.

Dr. de Mussy is doubtless an accomplished gentleman and a painstaking physician, and we think he will yet improve his work. Let him add a little more pathological anatomy and microscopical observation, and withdraw some of the many not very interesting cases which so unnecessarily enlarge the volume. When he has made a further acquaintance with the statistics of the disease, he will find, or we are much mistaken, that it is less frequently associated with herpetism, less rarely a primary local disease, and more dependent upon tuberculosis or a scrofulous diathesis than his work would lead his readers to believe.

REVIEW XII.

1. *Chimie appliquée à la Physiologie et à la Thérapeutique.* Par M. le Docteur MIALHE, Pharmacien de l'Empéreur.—*Paris*, 1856. 8vo, pp. 703.
Chemistry applied to Physiology and Therapeutics. By Dr. MIALHE.
2. *A Treatise on Therapeutics and Pharmacology, or Materia Medica.* By

George B. Wood, M.D., President of the College of Physicians of Philadelphia, &c., &c. 2 vols. 8vo, pp. 1741.

The work of M. Mialhe, although under the new title of 'Chimie appliquée à la Physiologie et à la Thérapeutique,' is in reality the second editionof a treatise published by the same author in 1845, and named, 'Traité de l'Art de Formuler, ou Notions de Pharmacologie appliquée à la Médecine.' The present volume, however, is very considerably enlarged, the additions being made chiefly to that part which pertains to physiological chemistry. We shall endeavour to present to our readers a short account of the contents of the work, and dwell a little upon the points we deem either useful or possessed of such novelty or interest as to call for particular comment.

About fifteen pages are first devoted to general considerations, more especially as to the influence of the vital force upon those chemical laws to which inorganic bodies are subject. "Organized bodies," says our author, "either animal or vegetable, present, in the same way as inorganic substances, the physical phenomena of electricity, heat, light, weight, hygrometry, endosmose; and the chemical phenomena of affinity, attraction, of composition and decomposition. But whilst inorganic substances implicitly obey the general laws of nature, organized bodies exert a constant reaction against their destructive influence, in virtue of a constitution which is peculiar to them, and which, endowed with solids, liquids, tissues, organs, and systems, give rise to functions which in their aggregate constitute the incomprehensible phenomenon named life."

Life, then, is the continued and prolonged struggle of the laws of individual against those of universal nature; and the amount of vitality is proportioned to the degree of superiority of the first over the second. Without attempting to investigate the phenomena which accompany the higher functions of animal life—phenomena which as yet have baffled all scientific attempts, and remain an apparently impenetrable mystery—our author confines his researches to those immediately dependent on the material conditions of existence, or those of organic life only, and remarks, in the words of M. Dumas, "To others belong the care and privilege of developing the noble faculties of the human intelligence; our more humble task must confine itself to the field of the physical phenomena of life."

The first chapter is devoted to the phenomena of oxidation and nutrition, and is divided into many sections. When treating of the influence of oxygen in the transformation of the tissues, much stress is laid upon the power gained by the extended surfaces arising from the extreme porosity of the tissues, and some good examples brought forward in illustration; among others, an experiment by M. Dumas, showing that sulphuretted hydrogen is very easily oxidated and transformed into sulphuric acid, simply by the influence of moist clothes or sheets.

Another interesting experiment of M. Millon is quoted, showing the power that hydrocyanic or prussic acid possesses of arresting the oxidation of certain chemical substances. For example, when a few drops of this acid are added to a mixture of iodic and oxalic acids, the decomposition of the latter, which usualy rapidly ensues, is entirely arrested; and our author supposes that a similar explanation may account for the terrific and rapidly-poisonous effects which ensue when this acid is brought in contact with the living body. The application of the results obtained by M. Millon to the explanation of the toxicological effects of prussic acid, is by no means novel. We have for many years been accustomed to compare the two phenomena when explaining to our class the physiological action of this drug. In this section are given most of the phenomena of change which different substances undergo in passing through the animal economy; but these being met with in many other chemical and therapeutical works, need not detain us.

After devoting a few pages to the consideration of the ferments met with in the vegetable and animal kingdoms, in which we find little or nothing having any pretensions to novelty, the phenomena of the digestion and absorption of those alimentary substances which are indispensable to nutrition, are discussed, under the following heads:

1. Vegetable matters, or hydrocarbons; as sugar and starch.
2. Nitrogenized or albuminous substances; as albumen, casein, gelatin, and gluten.
3. Fatty bodies; as fats and oils.

The views of M. Mialhe upon the digestion and assimilation of the first class of these bodies are somewhat peculiar; and as they bear particularly upon the study of the pathology of certain forms of disease, and would, if adopted, greatly influence the treatment, we shall not scruple to devote some little time to their consideration. Our author, in the first place, states that crystallizable, or rather cane sugar, when injected into the veins of an animal, is thrown out in the urine without having experienced any change in its passage through the system; whereas if glucose or starch sugar is substituted for cane sugar, it is not eliminated by the kidneys, nor, after a short time, is it found in the blood;—facts which show that cane sugar is not immediately assimilable, but that it is necessary that a conversion into glucose should first ensue. This conversion takes place in the digestive organs, by means of the acids and ferments which it there meets with, and may be readily imitated out of the body by the use of weak acids or certain nitrogenized matters. Mialhe thence concludes, that glucose and the allied sugars are alone susceptible of being used in the secondary assimilation, and that cane sugar, if it enter the blood as such, acts only as a foreign substance, and is destined for elimination. When we speak of glucose and the allied sugars, those only are intended to be understood which are at once decomposed by caustic alkalies and alkaline earths into the coloured acids, and which possess the power of reducing copper to the state of the suboxide when the solutions of the protosalts of this metal—such as those known by the names of Barreswill's, Trommer's—are employed. This group includes not only glucose, but also the sugars from acid fruits, diabetic sugar, &c. Amylaceous matters are next shown to be converted into glucose in the digestive passages, and some pages are occupied in discussing the causes of this change, and the author endeavours to establish the correctness of his own discovery, published in 1845, namely, that this change is effected by aid of the saliva. Our space, however, will not permit us to enter into the merits of this question, as we are about to discuss more particularly another section of the chapter, relating to the destruction of sugar in the animal economy.

With regard to the source or origin of sugar in the system, M. Mialhe appears to disregard altogether the researches of Bernard, who has demonstrated that the liver is the chief sugar-producing organ in the body, and that it is efficient to generate all that is necessary, even from food entirely deficient in saccharine or amylaceous matters, and to agree with those who have endeavoured, unsuccessfully we believe, to show that the sugar found in the liver of an animal who has been fed for a long time on an exclusively meat diet, is derived from the traces of this principle said to be contained in flesh and eggs. The arguments in favour of Bernard's views, supported by experiments apparently incontrovertible, have been placed before our readers so fully in our January number, as not to need repetition here.

Whatever be the source of sugar, whether it be derived from the transformation of amylaceous matters by the action which takes place in the alimentary canal, or whether it be secreted by the liver, the question next arises, Why do we not meet with it in the different excretions, in the healthy condition of the economy? Why does it disappear so rapidly from the blood? Is it destroyed, to serve other uses in the system? Undoubtedly, in the healthy state at least, it is decomposed and converted into some other compound or compounds, and eliminated in these new forms by one or more of the excreting channels; it becomes, however, a question of great interest, as bearing directly both on the pathology and treatment of diabetes, to determine whether diseased states occasionally occur in which the change does not ensue; and again, whether such a condition exists in cases where the phenomena of glucosuria are exhibited.

We shall find that Mialhe is a strong supporter, perhaps the originator, of this view, and endeavours to support it by chemical analogies and some very few clinical facts: we must, however, here express our opinion of M. Mialhe, derived from the perusal of his work,—he is evidently but partially acquainted with physiology, and still less with clinical medicine, and he often gives very undue weight to certain relations which he thinks to exist between chemical changes occurring out of the body, and those which take place under the influence of the more complicated and less understood chemistry of vitality. But to return to our subject; it is well known that under the influence of alkalies in their free state or that of carbonates, glucose is decomposed by heat, and resolved into dark-coloured compounds, consisting of melassic, ulmic acid, &c.; but it is not so altered when alkaline phosphates, even those having an alkaline reaction, are made use of. Again, this form of sugar possesses deoxidizing powers, and is capable of decomposing certain metallic salts, especially those of copper, reducing them to the condition of suboxide, when an alkali is present; M. Mialhe, taking these chemical facts, endeavours to compare them with the phenomena which ensue in the interior of the organism: he states, for example, that some metallic salts, as the ferriocyanide, or red cyanide of potassium, injected into the veins, pass into the urine in the state of the ordinary yellow salt or the ferrocyanide; that in case of poisoning by the protosalts of copper, the metal is found in the tissues in the form of suboxide, and that corrosive sublimate is converted into calomel, and these changes are ascribed to the presence of glucose in the blood, and said to be aided by the administration of this substance; it is also shown that glucose is not able to absorb oxygen from the air, unless previously transformed into other compounds, and that this necessary change takes place from the presence of alkalies or their carbonates.

M. Mialhe asserts, that in the normal state there exist considerable amounts of alkaline carbonates in the blood, and therefore the glucose finds in that fluid all the conditions necessary for its oxygenation and transformation. The following he supposes to be the order in which the phenomena ensue: the sugar enters the blood, decomposes the alkaline carbonates, forms with the bases new salts, *glucosates*, setting free the carbonic acid; the salts thus formed, having but slight stability, are rapidly transformed into glucic, ulmic, and formic acids, or rather salts of these acids, which absorb oxygen, undergo a species of combustion, and give rise to water and carbonic acid as ultimate products. According to this view, the destruction of glucose is a phenomenon closely allied to combustion, and it is by means of the alkalies of the blood that this change is effected, and sugar is able to serve as a respiratory agent or element. From this point M. Mialhe passes at once to the consideration of diabetes, for if, says he, the glucose from any cause ceases to undergo the above-named changes, the blood must necessarily become charged with this principle, which, acting as a foreign body, is afterwards eliminated by different glands, more especially the kidneys, giving rise to the secretions of large quantities of saccharine urine, thus constituting the disease known by the names of glucosuria or diabetes.

After examining the various hypotheses which had been proposed to explain the nature of this obscure affection, our author was led to think that it was to be sought for by investigating the phenomena attending the destruction of the sugar in the system; and as the result of his search, came to the conclusion that the true cause is a *deficiency of alkali* in the blood; for, he remarks, the digestion of amylaceous matters takes place in the same way both in the diabetic subject and the healthy individual; in both there is the same transformation of starch into glucose under the influence of the saliva and pancreatic juice; but in the latter, the glucose is further altered and decomposed by the alkalies normally present in the blood, whilst in the diabetic patient there is an absence of this destruction of glucose from a want of alkalinity; this deficiency of alkali is ascribed to several causes, as the abuse of acid drinks, a too exclusively nitrogenized diet, and suppressed perspiration.

M. Mialhe certainly brings forward many arguments and illustrations which appear at first to be highly favourable to his views: for example, he instances that in the blood, when in a healthy state, glucose cannot exist or remain undecomposed, as it possesses alkalinity; whereas in the sap of vegetables, which is either neutral or acid, sugar is normally present; that as a plant watered with a slightly alkaline solution ceases to produce sugar, or rather destroys it as soon as it is formed, so in the animal economy, if by accidental or other circumstances the acid secretion of the skin becomes arrested, or if by the daily taking of acid substances, or substances easily convertible into acids, the blood loses its alkaline qualities, being saturated by the acids, it approaches in character to the condition of sap, and then the existence of sugar in the blood becomes possible, and the diabetic condition is induced. Our author states that the only important objection which has been opposed to his views, is the fact that the blood of diabetic patients is never either neutral or acid, but always preserves its alkaline reaction. He answers this by stating, that it is difficult to tell the amount of alkalinity of the blood, and again, that part of the alkaline reaction of this fluid is derived from the presence of alkaline phosphates which possess no power of decomposing glucose; he is therefore inclined to consider that the carbonates are deficient or absent, the phosphates remaining intact, thus preventing the fluid from exhibiting any but an alkaline reaction. M. Mialhe, however, allows that other circumstances besides the insufficiency of alkaline carbonates may prevent the combustion of sugar, and induce at least temporary glucosuria, and hence is inclined to agree with M. Reynoso, as to the influence of a deficient performance of the respiratory function.

In the treatment of diabetes, M. Mialhe strictly acts up to the indications dictated by his hypothesis, and proposes, above all, the use of alkaline remedies; under the influence of these agents, he states that the sugar in the urine quickly diminishes, and even disappears altogether. The alkalies prescribed are lime water, magnesia, Vichy water, and the bicarbonate of soda; at the same time he makes use of alkaline and vapour baths, flannel, friction, exercise, and sometimes sudorifics; he also orders a diet restricted at first as to the amylaceous principles, but these are gradually increased in amount according as the system is able properly to assimilate them; he reprobates the exclusive use of an animal diet, as generating an undue proportion of acids in the system. One case of a diabetic patient, who was treated on M. Mialhe's plan, is related, and as it exhibits a somewhat remarkable disappearance of sugar from the urine, we will present an abstract of it to our readers.

M. Garofolini, an Italian professor of music, had resided in Paris for several years; he formerly enjoyed excellent health, but latterly had been suffering from pain in the renal regions and from colic, causing frequent desire to pass urine and likewise some tenesmus. He occasionally took Vichy water, which rapidly removed the symptoms, and within a month his health *appeared* to be re-established. He remained well for two years; but after that time, during the intense heat of the summer, he was tormented with great thirst, to allay which he drank a very large quantity of acid drinks, and partook of acid fruits, but without relief to the thirst or the constant dryness of the mouth. The desire to pass water became more frequent, and the urine much larger in quantity, appearing even more in bulk than the liquids taken during the same period of time; there were also a feeling of general illness and great muscular debility, progressive emaciation, feebleness of vision, loss of virile power, and obstinate constipation; he then came under the cognizance of M. Mialhe. The urine was at once examined, and found to have a density of 1040; treated with potash it gave a dark-brown colouration, indicating the presence of a large amount of sugar. He was ordered to abstain entirely from acidulated drinks, and to take during the twenty-four hours 20 grammes (308 grains) of bicarbonate of soda, and 5 grammes (77 grains) of calcined magnesia; also two bottles and a half of Vichy water. The next day the urine had only a density of 1026 in place of 1040, and did not show a trace of sugar. Under the influence of the alkaline treatment, not only did the sugar not reappear in the urine, but the vision improved, and within two days became perfect. In four or five days the bowels ceased to be constipated, and some diarrhœa and bilious vomitings ensued, which recurred for a few hours; in each following day there was a marked improvement, the thirst became appeased, the urine less copious, the strength and virile power were restored, and after this time the patient was re-established in health, nor did the urine on several examinations give any trace of sugar. He was able to support all kinds of fatigue, and did not restrict himself to diet, but partook equally of animal and vegetable substances, avoiding only all acid drinks. M. Mialhe considers that in this patient the exciting cause was the excess of these acid drinks, and thinks that when it so happens the disease is more easy of cure than when it arises from suppressed perspiration.

The above case was published in 1849; but although many years had elapsed between that date and the publication of the work under consideration, no more recent case is announced, although the plan of treatment proposed is said to have furnished practical results of a most satisfactory character. Such is the theory of M. Mialhe as to the pathology of diabetes, and such the method he proposes for its alleviation or cure. Did the disease depend on the causes he assigns, the treatment would be most simple, and at the same time most complete; we know, however, that diabetes is one of the most intractable of maladies, and that the cures are very rare indeed; and this has been the case, not only before, but since the alkaline treatment has been made known and practised. This fact alone would argue that there is much that is faulty in the hypothesis. Let us now examine a little into the proposed explanation of the nature of glucosuria. We need hardly discuss the subject of the changes which amylaceous matters undergo in the alimentary canal—changes necessary in order that absorption may take place. It is, we believe, generally allowed that starch is changed into dextrine and glucose by the action of the saliva, but more especially the pancreatic fluid, and that it enters the portal vessels in the condition of the latter principle, starch-sugar; whether, however, it passes the liver as such, seems, from the experiments of M. Bernard, somewhat problematical. It appears, also, to be established beyond doubt, that the liver is a great sugar-forming organ, and that a very large amount is prepared by this viscus even when the food is entirely destitute of amylaceous principles, a quantity sufficient to supply the demands of the respiratory process. To this fact, however, M. Mialhe seems to shut his eyes, almost ignoring the truths elicited by M. Bernard, which certainly are among the most completely established of any that can be found in the domains of physiology. But M. Mialhe's theory does not depend on their admission, for it concerns more the destruction of sugar in the blood, whatever be the way it enters this fluid. The pathology of diabetes, however, is much involved in them, for the great question is this—Is there an abnormal formation of sugar in the system, or a loss of power of further decomposing this substance? We have seen that M. Mialhe assumes that the non-destruction of the sugar is the real cause of its appearance in the urine: is this correct? And again—If so, is it due to a want of alkaline substances in the blood? M. Mialhe brings forward no evidence to prove that the sugar is not decomposed in the blood in diabetic subjects; he tacitly assumes such to be the case, and immediately endeavours to explain why it so happens. There seems, however, to be no proof capable of being brought forward in support of this opinion; as there is no alteration in the respiratory function, no difference in the gases evolved, no diminution of the temperature of the patient,—all of which we should expect were the saccharine matter not burnt off by the respiratory process. And again, other matters, artificially introduced into the blood of these subjects—as salicine, the neutral potash or soda salts, with vegetable acids, as the citrates and tartrates—are decomposed, and the products of the oxidation found in the urine. And lastly, all the phenomena of diabetes may be equally explained by supposing that there is an augmented supply of sugar to the blood, produced, as shown by Bernard, by an exalted glycogenic function of the liver.

Next, as the explanation of the supposed deficiency of decomposing power in the blood for saccharine matter, we may remark, that M. Mialhe's stronghold is entirely without real support. For it seems questionable whether any neutral carbonates exist in the blood; and the bicarbonates of the alkalies do not possess the power of breaking up glucose; and no attempt has been made to show that the blood is less alkaline, or contains fewer carbonates, in diabetes than in health; and certainly, for the establishment of such an important point, it would not be difficult to procure a sufficiency of this fluid. And again, it is very questionable whether the explanation of M. Mialhe of the causes of the changes which sugar undergoes, is correct. That they take place is undoubted; but the reason may be very different from that assigned by our author. If the theory of M. Mialhe be right, we should be able to produce a glucosuria at will, simply by the administration of a sufficiency of acids. This, however, has not been shown to be the case. Again, as we approach the subject of the treatment of diabetes, we discover that the hypothesis is most faulty; for in very many cases to which alkalies have been administered in large quantities, more than sufficient to neutralize any abnormal acid present in the blood, not even a diminution of the sugar has been discovered; and the result of the alkaline treatment has not been such as to justify the conclusions arrived at. The case of M. Garofolini seems to have been an exceptional one; and we know that now and then the disease assumes a form which almost entitles it to be called intermittent diabetes; and this often occurs without any discoverable cause. Sometimes, however, the sugar suddenly disappears, from the supervention of some other affection. From these and many other considerations which we might adduce, we should be disposed to consider the above hypothesis of the proximate cause of diabetes erroneous, until further and much more convincing evidence be brought forward in its favour.

The remaining portion of this chapter, which is devoted to the digestion of albuminoid and oleaginous substances, presents no facts of sufficient novelty or interest to make it necessary that we pause to dwell upon them, more especially as we have devoted so much space to the discussion of the amylaceous principles.

The second chapter of the book is devoted to the subject of absorption, and to the consideration of medicinal agents which possess the property of rendering the blood either more plastic or more fluid; in this chapter, however, there is nothing which we can select with any advantage. Chapter III. contains researches upon the action of many important medicines, and the same order and plan is followed as in the work published by our author, and to which we have before alluded at the commencement of our article—viz., the 'Traité de l'Art de Formuler;' however, many facts are added, some of them possessing interest. Under the head of iodide of potassium, it is stated that iodine should never be prescribed in a free state, but as an alkaline iodide, and especially as iodide of potassium; and this latter salt should be neutral, and quite free from iodate of potassa. It appears, from the researches of M. Leroy of Brussels, that specimens of iodide of potassium which have given rise to much gastric pain and irritation, contained a very notable proportion of iodate of potassa, of which it was very easy to be assured by adding a little concentrated acetic acid to the solution of the salt, which has the power of liberating the iodine from any iodate of potassa, and thus colour the liquid. M. Mialhe states that such impure iodide of potassium is frequently to be met with at the druggists in France, and that many physicians in that country have seen the symptoms above enumerated produced by it. This last phenomenon he explains by supposing a portion of iodine to be set free by the action of the acids of the gastric fluid.

In this country such iodide has frequently been found in the market, and it arises from the method occasionally employed for its manufacture: when iodine, for example, and caustic potash are brought together, the iodine dissolves, and two salts are formed from the changes which ensue, six equivalents of iodine and six of caustic potash giving rise to five equivalents of iodine of potassium and one equivalent of iodate of potassa; the two salts crystallize together, and by the action of a high temperature, the iodate of potassa is resolved into oxygen and iodide of potassium, and thus six equivalents of the latter salt result; if, however, the last part of the process is not perfectly performed, a portion of the iodate remains undecomposed, and contaminates the salt. When, however, the process prescribed in the Pharmacopœia of 1836 is made use of, which is effected by the decomposition of the iodide of iron, there is no fear of any such impurity being present. In the present London Pharmacopœia, although no process for the preparation of the salt is given, yet a test is indicated which has for its end the discovery of any iodate, should it be present. Thus it is stated that when to a solution of the salt, starch and tartaric acid are added, no blue colour should be developed. Tartaric acid has no power of decomposing iodide of potassium and liberating iodine; but if the slightest trace of iodate of potassa be present, the iodic acid is set free, which immediately gives rise to the production of the intense blue colour by the formation of the iodide of amylin. Iodide of potassium containing iodate, colours the ointment yellow, and often itself becomes discoloured from free iodine being evolved. As yet we have not noticed the physiological symptoms described by MM. Leroy and Mialhe; whether their explanation is correct might easily be tested by administering small doses of the iodate itself, and comparing the effects with those induced by corresponding doses of free iodine; perhaps iodic acid itself would prove very irritating.

A very considerable space, more than sixty pages, is devoted to the consideration of the preparations of iron; and the investigations of M. Bouchardat, M. Quevenne, and others, as well as those of the author, are detailed; to attempt to illustrate these at all fully would occupy a space far exceeding our prescribed limits, and we shall, therefore, confine ourselves to giving the conclusions which are arrived at:—

1st. All the preparations of iron, which are either themselves soluble, or capable of becoming so under the influence of the acids of the gastric juice, and which are able to be decomposed by the alkaline substances contained in the blood, can be advantageously employed in the treatment of those affections which call for the use of iron.

2nd. All the preparations of iron, either soluble, or capable of becoming so under the influence of the acids of the gastric juice, but which are *not* able to be decomposed by the alkaline substances contained in the blood, cannot be advantageously employed in the like affections of the system.

3rd. Preparations of iron having the peroxide for their base, as well as those containing the protoxide, can be successfully employed in increasing the amount of blood-globules in anæmic conditions of the habit.

4th. The oxides of iron, which produce the physiological effects of the metal, have no need to be united either to carbonic acid or any organic acid, in order to become assimilable.

5th. The preparations of iron, with either the peroxide or protoxide for base, have the same final efficiency, but the latter, if little soluble, require to be administered for a much longer time; the chemical reason for this is evident.

6th. The insoluble iron preparations constitute a class of medicines having a real therapeutic value, although slow in action, possessing no activity except from the acids contained in the gastric fluids; and as this acidity is limited in quantity, and variable in different patients, the therapeutic activity of these drugs must be so likewise, and depend much upon individual peculiarity.

7th. The insoluble preparations do not produce their maximum intensity of therapeutic effect unless administered in divided doses.

8th. That among the insoluble ferruginous preparations of iron employed in medicine, the metal itself in a highly divided state, and the proto-carbonate, hold the first rank for activity; then the magnetic oxide; afterwards, among the least valuable, the red oxide of iron and Prussian blue.

9th. The soluble iron preparations are in general more active than those which are insoluble.

10th. All the soluble preparations of iron are not equally efficacious, many of them, from the acids with which they are combined, acting as astringents or styptics; as it is necessary to dilute these with a large quantity of water, their absorption is always imperfect—a circumstance which has induced some authors, especially M. Bouchardat, to erroneously look upon them as ineffectual in curing anæmia.

11th. The soluble salts of iron being capable of absorption without the aid of acids in the alimentary canal, those having the peroxide for their base have (contrary to what occurs with the corresponding insoluble compounds) as much or even more activity than the ferruginous preparations which are capable of being decomposed by the alkalies of the blood. It suffices to cast a glance upon their percentage composition, to know at once which is the most active. Being equally capable of absorption, that which contains most iron is the most powerful; the action of the ferruginous salt depending entirely on the contained iron, and not on the acid or other base which may be present.

12th. Among the soluble preparations of iron, those which at the same time possess least taste, are most rich in iron, and most completely absorbable, should be preferred; and no preparation of iron in these respects can be put on the same level with the tartrate of potash and the peroxide of iron—the ferri potassio-tartrates of our Pharmacopœia.

13th. The above salt, associated with the iodide of potassium, constitutes an ioduretted medicine more rational than any containing iodide of iron, and able to be substituted with the greatest advantage in the treatment of those affections which call for the united employment of iodine and iron.

Such are the conclusions (by no means novel) arrived at by M. Mialhe, and in the main we should be disposed to agree with him. Having paid considerable attention to the subject, and clinically investigated the action of the ferruginous preparations, we feel somewhat confident in giving expression to our opinion upon this point. We are perfectly sure of the correctness of the first conclusion, that all preparations of iron, with the exception of the few indicated in the second proposition, are effectual for the cure of anæmia. We have tried every variety—iron itself, the protoxide and proto-salts, the peroxide and per-salts, together with the numerous ferruginous compounds in which the exact condition of the iron is somewhat doubtful (we allude to the citrates, tartrates, &c., containing some other base)—and we have invariably found that by their use the blood-corpuscles, if deficient, are augmented in amount; although we are equally persuaded that some preparations are more powerful than others, or will produce the effects in a shorter time. The preparations which contain iron, but which probably produce no effect on the system from the presence of this metal, are the ferro-cyanide of potassium, the sulpho-cyanide of potassium and of iron. These are not decomposed in the blood, but rapidly pass through the kidneys, and hence cannot aid in the formation of the blood-globule. The third and fourth conclusions are specially dwelt upon, M. Bouchardat having asserted that the per-salts of iron acted only as astringents, and not as reconstituent or analeptic tonics; and that it was necessary to unite the protoxide with either carbonic acid or an organic acid, in order that it should be assimilated by the system. The assertions of M. Bouchardat are manifestly erroneous, every day's clinical experience proving that both the per-salts and the proto-salts, with a mineral acid, are capable of curing anæmic conditions of the habit. As to the fifth and sixth conclusions, we believe that experience has fully demonstrated that a considerable difference exists in the different preparations of iron in their capacities of blood-restorers. This often depends on the greater insolubility of some compared with others; sometimes on the small amount of iron contained in the doses of certain of the per-salts, the doses being regulated more by their astringency than the quantity of contained metal. This is the case, for example, with the tincture of the sesquichloride of our Pharmacopœia—the amount of iron in the ordinary dose of this preparation being very small indeed, and quite insufficient to cause a speedy effect. The condition of the stomach has doubtless a powerful influence, and many of our most powerful ferruginous preparations depend for their efficacy upon the solvent power of the gastric juice; hence the great importance of administering these drugs at the time of taking food. This applies especially to those which are administered in the form of powder or pill.

We should be willing to agree with the statements in the seventh and eighth propositions; for it stands to reason, that if a given amount of a very insoluble compound be taken at once, it will only be acted on by a small quantity of the dissolving agents of the intestinal canal, compared to that which would operate if the preparation were divided into many portions and given at intervals. This holds good not only with the little soluble preparations of iron, but likewise with those of other metals: for example, a few grains of calomel, if administered in one dose, will produce a purgative effect, and but little may be absorbed; if, however, the same quantity be much divided, and given at short intervals, the constitutional effects of the drug will probably become manifested. With regard to the order in which the so-called insoluble preparations are arranged—namely, the highly divided metal, the proto-carbonate, the magnetic oxide, then the red oxide or real per-oxide, and lastly, Prussian blue or ferro-cyanide of iron—our own experience would lead us to class them in about the same way. That the metal itself in a highly divided state, as *Fer réduit*, is a most active and valuable drug, we can assert with the greatest confidence, having used it in a great number of cases of anæmia with the most complete success: although insoluble in water, yet it appears to be rapidly acted upon by the gastric secretion, and is doubtless absorbed into the blood as a proto-salt; its rapidity of solution is evidenced by the generation and evolution of hydrogen, a circumstance which now and then militates against its employment. The proto-carbonate is also a valuable preparation, if properly preserved, as in the ferri carbonas cum saccharo of our Pharmacopœia, or in freshly prepared mistura ferri or pilula ferri composita.

The magnetic oxide, although a useful and cheap compound, is certainly far less soluble than the metal or proto-carbonate, and the solubility differs much with its method of preparation; when made by precipitation and carefully dried, it dissolves more readily than if prepared from iron scales, as is often the case: the peroxide, again, is efficient, but little is dissolved at a time, hence it may be given in very large doses without the production of any prominent symptoms. This preparation also is apt to differ much in quality; if made merely by the precipitation of the sulphate, as in the formation of the old carbonate of the Pharmacopœia, it is considerably more soluble than if afterwards heated, which is sometimes done to improve its colour. The peroxide of iron has, however, a great tendency to assume an insoluble condition. With regard to the Prussian blue, we have not had sufficient experience of its effects as a blood-restorer to enable us to give an opinion of its value: our impression is that it would prove very inert.

As to the ninth proposition, that the soluble iron preparations are in general more active than the insoluble, although in most cases true, yet we should be inclined to except the *Fer réduit* and the proto-carbonate: the former is certainly equal to any ferruginous compound, even the most soluble. In the remaining propositions we have little to notice, many of them being almost self-evident; however, we should be certainly inclined to differ from M. Mialhe with regard to the peculiar efficacy of the tartrate of potassa and iron. That it is a very efficacious salt we do notfor a moment hesitate to allow; but that it is much, if in any degree, superior to the ammonio-citrate of iron, ammonio-tartrate of iron, or many other similar salts which have or could be formed, we are inclined to doubt or even deny. Most of the important remedies are brought under consideration in a manner more or less similar to that in which the iron preparations are treated; much, however, contained in this part of the work has been known to the profession for some years before the publication of the 'Traité de Formuler,' or other writings of M. Mialhe.

The remaining chapters of M. Mialhe's work are more especially devoted to pharmacology, and many useful hints may be obtained from their perusal. In conclusion, we consider that the work we have passed under review contains much that is important both to the physician and pharmacologist, although it is by no means intended as a complete treatise on the various subjects. Frequently certain points only are touched upon, and these often but slightly. M. Mialhe certainly possesses much ingenuity, and has a happy method of explaining difficulties; but, as we have before observed, questions are very apt to be discussed in a partial manner, and there is also great evidence of a want of physiological knowledge and of clinical experience.

Dr. Wood of Philadelphia, a gentleman well known in this country by his work on the 'Principles of Medicine,' and, in conjunction with Dr. Bache, as the author of the 'United States Dispensatory,' has, within the last few months, issued a new work on therapeutics and materia medica, more particularly, however, devoted to the consideration of the action of medicines; and "while it aims to present whatever in therapeutics and pharmacology is directly and practically important to the physician, is intended also to be an exponent specially of what the author himself knows and believes on the subject of which it treats." Dr. Wood has had good opportunities of being well acquainted with the science upon which he now discusses, having for thirty years been a teacher of the subject at the Philadelphia College of Pharmacy and the University of Pennsylvania; and having also for twenty years held the office of Physician to the Pennsylvanian Hospital, which has given him facilities for testing the value of remedies greater than any amount of practice could afford. Dr. Wood's present work receives additional interest from the following statement, found at the end of the preface:

> "This is probably the last professional treatise of the author, as with its publication he will have exhausted what he has to communicate on those departments of medicine to which he has given a special attention; and advancing years warn him that the time is fast approaching when a failure of faculties, or the termination of life, will render labour in any new field impracticable. He asks for it only the same kind consideration which he has had occasion to acknowledge for his other works, and which has bound him to the profession by the strong ties of gratitude, in addition to those of duty and affection."

We trust that the author may long live, if not to produce another work, at least to enjoy his well-earned reputation as an ardent and industrious cultivator of medical science.

Of a work in two thick volumes, and containing 1741 pages, it would of course be impossible to present to our readers anything like a complete analysis; this, therefore, we shall not attempt, but by making selections from a few of the numerous subjects, give an idea of the mode in which the whole is executed.

The work is divided into two parts, the first affording an account of general therapeutics and pharmacology, and containing chapters on the operation of medicines, the effects of remedies, the mode in which they are applied, and the method of classification. The whole of this part occupies but ninety-one pages, and calls for no special remarks; the classification takes for its basis the physiological effects produced by the medicines, and is practical in character, attempting no useless refinement. The second part, including the chief portion of the treatise, is devoted to the consideration of the action of individual remedies, and of the groups in which they are arranged. The mode of preparation of the different drugs, the sensible and chemical properties, are but shortly touched upon, the reader being in many cases referred to the United States Dispensatory for more minute details, and therefore the chief space is given to the effects produced on the system, and the medical uses of the various preparations. Under the article Tea and Coffee, a much more lengthened account of the action of these dietetic substances is found than is usually the case in works on therapeutics; and as we are convinced from experience that the influence of these articles of food upon the system is much more potent than is generally supposed or allowed, we will give an extract from the work, showing the author's experience upon this subject, which in the main agrees perfectly with our own. When describing the effects of these substances, our author states—

"The first effect of a moderate quantity is usually a warming, cordial impression on the stomach, which is followed after a short time by an agreeable feeling of comfort, satisfaction, and an obvious exaltation of the imagination and intellectual faculties. The disposition to cheerful conversation, or to other exercise of the mental powers, is continued along with this increase in their vigour. Every one accustomed to witness social coffee or tea-drinking, must have noticed the increased vivacity, the more rapid interchange of thought, the general buzz which spreads through the company after partaking of the beverage. The student finds himself capacitated for a clearer understanding and more prompt appropriation of the subjects of his study, the writer for a more vigorous exercise of his mental powers, a quicker and happier arrangement of his thoughts or fancies, and a much greater facility of expression. In my own person I every day experience something of this effect from black tea. For hours after dinner—even a moderate and entirely temperate dinner—I am often unable to perform at all to my own satisfaction any intellectual task which may have devolved upon me. An immediate change is produced by the tea, and after the closing meal of the day, I find myself possessed of my intellectual capabilities, whatever they may be, to their full extent. Along with this nervous excitement there is a strong tendency to wakefulness produced, and under the influence of the beverage, if taken rather late in the evening, one's labour may often be prolonged far into the night, without any sense of fatigue or disposition to sleep. One or two strong cups of coffee at bedtime not unfrequently prevents sleep for the whole night, and persons who wish to watch prepare themselves often in this way. During all this time there is little acceleration of the pulse, and that which may be noticed is probably rather owing to the reaction of the excited nervous centres upon the heart, than the result of a direct influence upon the circulation. The state of exaltation subsides after many hours into a corresponding depression, and the self-indulgence is paid for the next day by a feeling of gastric uneasiness, languor, and general *malaise*, which gradually wears off or disappears under a repetition of the stimulant. It will be readily understood, therefore, that the habit of coffee-drinking is not on the whole healthful, unless carefully guarded as to extent, and counteracted by active physical exercise. I shall refer directly to the evils which are apt to result from the abuse of this luxury.

"When coffee is taken in excess, it causes a feeling of oppression or anxiety in the epigastrium, with over-excitement of the nervous system, indicated frequently by vertigo, headache, palpitation, muscular tremors, and other symptoms of irritation of the nervous centres. But, even in the largest quantities, it never produces, so far as I have ever witnessed, intoxication or stupor, or any other of those peculiar effects on the brain which characterize the cerebral stimulants or stimulating narcotics in full action.

"The habitual use of coffee in excess is very apt to occasion a train of very disagreeable and annoying symptoms, which can only be got rid of by abandoning the habit. The constantly repeated over-excitement, followed by the as constant depression of the nervous functions, gives rise at length to persistent irregularity, and the exhaustion of the excitability of the nervous centres by the strain to which they are subjected, ends in a deficiency of power and a consequent insubordination of all the functions placed under their regulating influence. These effects are especially displayed in persons of susceptible nervous temperament and those of sedentary habits. Some individuals appear to be almost insusceptible to influence of any kind from the ordinary use of coffee; and its effects, whether direct or indirect, may be greatly controlled by habits of steady and vigorous muscular exercise. Indigestion, habitual constipation, and torpor of the liver, are among the effects of its abuse, exhibited in the digestive function; nervous headache, sick headache, vertigo, various disorders of sight and hearing, neuralgic pains, and an infinite diversity of disordered sensation, palpitations, muscular tremors, hysterical symptoms in women, hypochondriacal in men, are some of the consequences of the same abuse in the nervous system. As the bloodvessels are little excited directly by the stimulant, the vascular system is apt to suffer less than the nervous, and it is unusual to encounter from the abuse of coffee any of those inflammations—as of the stomach, liver, brain, &c.—which are so apt to follow the use of the cerebral, or even the arterial stimulants in excess. Hence it happens that, unless the nervous disorder has been so long continued as to have at last brought about organic change, all that is necessary in order to escape from the evils is to abandon the use of coffee.

"As illustrative of the above statements, I will observe that, personally being of a somewhat nervous temperament, I am unable to use coffee steadily without much suffering; and the same peculiarity belongs to most of my immediate family. For years I was troubled with frequently-recurring nervous headache, which at times incapacitated me for the performance of any active duty. Scarcely a day passed without some uneasiness or deranged sensations in the head—such as roaring, buzzing, and singing in the ears, sounds as of pounding or bell-ringing in the distance, swimming or vertiginous feelings, muscæ volitantes, &c. &c.; and I never walked in the streets without the fear of a sudden attack of these symptoms, which, when they came, took away all mental energy. It occurred to me that a single cup of coffee which I took daily in the morning, and to which I had reduced myself from the necessity of escaping dyspeptic sufferings, which a more free use of it had occasioned, might be the cause of these distressing phenomena; I abandoned the habitual use of it, substituting black tea for coffee; and from two weeks after that time up to the present, a period of many years, I have been almost entirely free from the symptoms referred to."

Our personal experience bears out much of what we have just quoted as to the powerful influence of tea and coffee, often in cases where it is little suspected: we have seen intermittent action of the heart, faintness, vertigo, wakefulness, atonic dyspepsia, with flatulence and other symptoms, caused by the employment of these beverages, and their entire and instantaneous cessation when they were discontinued; and we are quite convinced that, however valuable they may prove in certain individuals, in others they are productive of much harm and discomfort, which not unfrequently pass undetected for years, being only discovered by their accidental omission for a time. Probably the symptoms are induced by the caffeine or theine, as the same phenomena are produced in susceptible individuals by either beverage, the difference being only in degree; and again, the same crystalline principle is contained in both. Cocoa or chocolate (possessing theobromine as its active principle) certainly does not produce the effects of tea or coffee to the same degree, and may generally be made use of with advantage by those who cannot employ the other beverages.

The following is the opinion of our author with regard to the use of tobacco, which, as it is derived from watching its effects in a country where the leaf is by no means sparingly employed, may be of interest, especially as the subject has recently attracted so much attention:

"The habitual use of tobacco by smoking, chewing, or snuffing, if indulged in moderately, is not generally productive of any obvious injury to the health; but in some individuals of nervous temperament, or great susceptibility of the nervous system, it cannot be employed even in small quantities without injury. In excess I have no doubt that it is often very injurious, greatly impairing the vigour of the nervous system, and of the health generally, and probably shortening life, if not directly, at least by rendering the system less able to resist noxious agents. The effects most frequently induced are *dyspepsia*, *defective nutrition*, *paleness and emaciation*, *general debility*, *and various nervous disorders*, of which the most frequent are *palpitations of the heart*, *hypochondriacal feelings*, and *neuralgic pains*, especially of the head and eyes. Very great habitual excess seems to be capable of directly inducing a condition similar to that induced by the omission of alcoholic drinks in the case of the drunkard; a condition prominently marked by muscular tremors, obstinate wakefulness, and hallucinations. The late Professor Chapman informed me that he had witnessed several cases of delirium resulting from tobacco, closely resembling delirium tremens, which ceased upon the omission of the drug. This fact very strongly illustrates the opposite effects of tobacco and alcohol; a condition being produced by the direct influence of the one, very analogous if not identical with that resulting from the omission of the other; even insanity has been ascribed to the abuse of tobacco. Snuffing appears to be less injurious to the general health than either smoking or chewing; but there can be no doubt that it is more or less hurtful in excess, and at all events it is apt to occasion diminished susceptibility of the sense of smell, and a disagreeable alteration of the voice."

The work before us differs in many respects from those devoted to the same subjects in our own country, in containing accounts of many drugs which are little, if at all, known amongst us. Many plants are employed in the United States not contained in our Pharmacopœia, and although the effects of perhaps the majority can be obtained by the use of such as we possess, still there seem to be some essentially different; we will select one of these, and quote Dr. Wood's remarks upon the action which it exerts upon the system: the plant we have chosen is the Veratrum viride, or the American Hellebore, called also the Swamp Hellebore, the rhizome of which is officinal in the United States Pharmacopœia.

"Locally applied, American hellebore is capable of producing irritation, rubefaction, and even vesication of the surface. Snuffed into the nostrils in the form of powder, even much diluted, it acts as an errhine and sternutatory. Its acrid impression on the mouth and fauces, when chewed, has been already mentioned. When swallowed, it is apt to cause uneasiness in the epigastrium, which, when the dose is sufficiently large, is followed by nausea and vomiting, the latter effect being often protracted, and attended with much retching, and sometimes with hiccough. Dr. Osgood noticed in his own case, that the vomiting was effected by a spasmodic contraction of the stomach itself, without participation of the diaphragm and abdominal muscles, and in another individual was preceded by a sensation as of a ball rising in the œsophagus, the result, no doubt, of a spasmodic contraction of that tube. The antecedent and attendant nausea does not seem to be severe, though the prostrating effects on the system, as will be more particularly noticed directly, are often very striking. The emesis is usually later in occurring from this than from other emetic medicines, three-quarters of an hour or more not unfrequently elapsing after its exhibition before this effect is experienced. A fact which, considering the drastic properties ascribed to veratrum album, was not anticipated, but which appears to have been confirmed by almost all who have reported their experience upon the action of the medicine, is that *it seldom, if ever, purges.* The remarks hitherto made have reference to its local operation; its effects on the system are even more striking. From doses insufficient to vomit, along with the epigastric uneasiness, or independent of it, there are sometimes feelings of chilliness and considerable diminution in the frequency and force of the pulse, with a sense of weakness in certain muscles, or want of due command of them, which are probably the results of a direct sedative influence upon the nervous centres. As a proof that it is not from the depressing influence of nausea that the reduction of the pulse takes place, Dr. Norwood states that he has reduced it as low as thirty-five in a minute, without the least nausea and vomiting. He also speaks of a feeling of numbness and tingling which he had experienced about the joints previously to vomiting, as well as during and after that process. We are told by Dr. Osgood that the farmers in New England, in order to protect their crops from birds, were in the habit of scattering in their fields grains of corn which had been soaked in an infusion of the root of the American hellebore. Soon after eating this grain, the birds became incapable of running or flying, so that they were readily caught; but if left undisturbed for a time, they recovered from the paralyzing effect and flew away.

"When the medicine is carried so far as to produce nausea and vomiting, its depressing effects on the circulation and nervous system are often very striking; the pulse falls from 75 or 80° down to 35 or 40°, and at the same time becomes small and feeble, and occasionally almost imperceptible. The surface is pale, and covered with a cold sweat, the patient at the same time experiencing a sense of chilliness, and sometimes of tingling or numbness: headache, vertigo, dimness of vision, with dilated pupils, faintness, a feeling as of stiffness of certain muscles, and a want of command over them, are other symptoms evincive of the sedative operation of the medicine. These signs of prostration are sometimes so great as to become alarming, although I have seen no account of fatal poisoning.

"All agree in the statement that the general depressing effects on the nervous system and circulation are attended with stimulation of the secretory functions. The salivary, pulmonary, biliary, and urinary secretions are increased, it is asserted, by doses insufficient to occasion nausea and vomiting; and during the existence of this condition, the same effect is produced upon the function of the skin."

From this account, it would seem that the American hellebore possesses powers unlike either the Veratrum album or the Colchicum autumnale; and we know that, although the two latter belong to the same botanical natural order, yet they differ much in their physiological action, both containing very potent yet different active principles. May we not therefore possess in the American plant a new therapeutic agent possessing powerful and valuable properties?

We can most heartily and sincerely recommend Dr. Wood's 'Treatise on Therapeutics and Pharmacology' to the notice of gentlemen engaged in the practice of medicine, feeling convinced that they cannot fail to derive much benefit from its perusal. The work has no great pretensions to originality, or to containing any very special or profound inquiry; but while it supplies the general information which we look for in a book of this kind, it also embraces the results of the careful observations of a highly-educated and talented physician during a long and successful professional career, which give the work a peculiar and valuable stamp.

PART SECOND.

Bibliographical Record.

ART. I.—*On the Diseases of Women; including those of Pregnancy and Childbed.* By FLEETWOOD CHURCHILL, M.D. Trin. Col. Dublin, M.R.I.A. Fourth Edition.—*Dublin*, 1857.

THE present edition of Dr. Fleetwood Churchill's well-known treatise fully supports the reputation of the original work, and contains, in a condensed and well-arranged form, an admirable *résumé* of the present state of professional opinion and practice in regard to the several diseases upon which it treats. As compared with the former edition, many alterations and additions have been made; some new chapters have been added, others have been either pruned down or expanded, and much new matter has been introduced in several parts of the work. In its present form, we have no hesitation in stating, that we regard it not only as an admirable text-book, but as one of the most comprehensive and systematic treatises on the diseases of females which has ever issued from the medical press of this country.

Of the new chapters introduced, we find one respectively upon Urethritis, Occlusion of the Vagina, Pelvic Abscess, Occlusion of the Os Uteri, and Ovarian Irritation in the Non-Puerperal Female; and upon Tetanus, Convulsions, Paralysis, and Arterial Obstruction in Puerperal Women. We have in a recent number given an epitome of what is known respecting the two latter diseases—viz., tetanus and arterial obstruction—and need not further allude to them here: but the subject of paralysis is one which has scarcely received adequate attention; and as it is very fully treated of by our author, we subjoin a brief summary of the chief facts and conclusions to which he has arrived respecting it.

The literature of our profession, we may premise, is singularly barren on the subject of this disease. Neither the works of the principal obstetric writers, nor those on the diseases of females, or the practice of medicine generally, contain more than the most meagre and cursory information respecting it. Our author has, however, collected thirty-five cases of the disease from various friends and authorities, and from them the following statistical facts and conclusions are deducible.

Of the 35 cases, there were 18 of complete, and 1 of partial hemiplegia; 4 of paraplegia, in 2 of which one leg only was affected; 6 of facial paralysis; 5 of amaurosis; and 3 of deafness.

Of the 35 cases, in 23 the attack occurred during pregnancy, and in 12 either during or after labour. Of the former, 13 were cases of hemiplegia; 1 of paraplegia, which had occurred previously; 4 of facial paralysis; 2 of amaurosis; and 3 of deafness. The seizure took place at variable periods of gestation, but more frequently in the later than the earlier months. Of 20 cases, 12 appear to have been cured before or by delivery, and in 8 the disease continued for a longer or shorter time afterwards. Of the 23 cases only 1 died, and in this it is evident that death was rather owing to disease of the brain of longer standing than the pregnancy, than to the paralysis, which increased during the process. In 3 cases only was the paralysis preceded by convulsions, and in the majority it does not appear that there were any premonitory symptoms, such as headache, or any other circumstance calculated to excite apprehension, before the paralysis supervened.

Of the 12 cases that occurred during or after labour, in 3 only did the paralysis take place during labour, and of these 2 were cases of convulsions. In all the others, it not merely succeeded labour, but, in the majority, after a considerable interval. Of these 12, 5 were cases of complete hemiplegia; in 1 only the arm was affected; 1 was a case of complete paraplegia; in 1 the right and in 1 the left leg was paralyzed; 2 were examples of amaurosis, 1 of facial paralysis; and in 3 only of the cases of hemiplegia the face participated in the attack. Other peculiarities might be added, but it is more important to observe that in the majority it took place without warning and without any obvious cause. The paralysis gradually subsided in most cases, but 3 proved fatal.

In speculating upon the nature and cause of the disease, our author dwells upon the fact, that in most of the cases the attack recurred without warning and without apparent cause. In only one case, for example, did it appear to depend upon any external influence—upon cold, exposure, violence, &c., or upon mental distress; and in few, if any, was there evidence of previous cerebral congestion or disease of any organ. Other causes are instanced, but their operation is assumed to be more than doubtful in the production of the disease; and, upon the whole, our author leans to the opinion that the kidneys play a more important part in the causation of these affections than has been suspected, and that the subject deserves more attention than it has received.

"For," he observes, "we find that in cases of convulsions terminating in paralysis, we may have albuminuria; in paralysis before delivery, without convulsions, we may have albuminuria; in paralysis occurring after delivery, we may have albuminuria; and further, that in the slightest cases, both the convulsions and paralysis diminish with the decrease of albuminous secretion. Whether, therefore, the paralysis be caused by the state of the kidneys, or the renal congestion and paralysis be both the result of some morbid matter in the blood circulating through the system, it is clear that a new element may be added to those which have usually been considered as giving rise to paralysis."

This view suggests to our author the necessity of directing our attention to the relief of the renal malady, and the restoration of the kidneys to such a state of efficiency as may enable them to remove the morbid constituents of the blood. We will not enlarge upon this part of the subject, but submit that the researches in question fully prove that a relationship exists between certain forms of paralysis and pregnancy on the one hand, and albuminuria on the other. Whether, however, this is fixed or casual, accidental or constant, is a matter which we are at present unable to decide, and which must therefore remain to be determined by further and more extended inquiries.

Of the additional chapters introduced into the sections on the diseases of the non-puerperant female, in this edition, the most original is probably that on ovarian irritation—a form of disease which our author believes to be very common, although little noticed in medical works. It is characterized by uneasiness or pain in one or both iliac or inguinal regions, but most frequently the left —probably from the propinquity of the left ovary to the rectum, and its exposure to any irritation thence arising. The pain may be a constant dull aching, or it may be acute, or occurring in paroxysms, and is greatly aggravated by standing and generally by walking. No tumour is perceptible in the seat of pain; but there is generally much tenderness, and in some cases great irritability, of bladder. Hysterial paroxysms are by no means unfrequently coincident.

We will not enlarge upon this subject beyond referring our readers to the author's opinions and practice in regard to it as embodied in the chapter referred to, and observing that we have long been cognisant of the existence of such a disease. We believe it is not at all unusual to meet with cases in which one or both ovaries are in a state of morbid sensibility—in which the ovarian region is exquisitely tender on pressure, in which there is no evidence of vascular or organic disease, and in which the symptoms subside under treatment addressed solely to the relief of an exalted or perverted state of the sensibility of the organ. We have regarded this lesion of the ovaries as analogous to that of the uterus known as the "irritable uterus," and have been led to consider both as being generally dependent upon various sources of irritation secondarily reflected from a distance upon the uterine and ovarian organs. We may remark that, in the practice of our author, the most successful treatment for the ovarian affection has consisted in the introduction into the upper portion of the vagina, by means of the speculum, of a pessary containing two grains of opium, half a drachm of white wax, and a drachm and a half of lard—the patient being directed to remain in bed during the remainder of the day.

There is one omission which we would advert to in the work—viz., the comparative absence of information upon the subject of syphilitic affections of the uterine organs. We believe that a chapter might have been advantageously introduced, embodying the various facts which have been collected on this subject by various writers; and we would recommend it to our author as worthy of his consideration in the preparation of another edition of his work. In Nos. 98 and 99 of the 'Association Medical Journal' (New Series), an elaborate paper will be found, by Dr. F. W. Mackenzie, embodying the results of a lengthened inquiry on this subject; and from the facts collected by him, he is led to conclude that the uterine organs suffer largely from, or participate in the effects of, syphilis upon the female constitution; and that such derangement is variously manifested by lesions of innervation, of menstruation, of mucous secretion, and of reproduction; whilst, in its progress, lesions of the mucous membrane and of the structure of the cervix are met with, the exact relations of which to syphilis are less obvious, and cannot so specifically be determined. Whether the views of this writer are correct to the extent he has contended for, is a question which further inquiries must determine; but the subject in itself is one of great social and scientific importance, and well worthy of the consideration of those who, like our author, are professedly engaged in sifting and determining the value of medical evidence.

With this exception, very little indeed that is practically useful in contemporaneous medical literature appears to have escaped the attention of our author, and we are glad to find that in his commentaries upon some of the more recent novelties introduced into uterine practice, his opinions are in harmony with our own. Thus the value and pathological importance of inflammation and ulceration of the cervix uteri are reduced to their legitimate limits; the mechanical means proposed for the cure of various flexions, versions, and obliquities of the uterus are accepted with becoming qualification; and the indiscriminate division of the cervix uteri for the relief of certain form of dysmenorrhœa is justly represented as a hazardous and doubtful proceeding.

ART. II.—*On the Prevention and Treatment of the Sheffield Grinders' Disease.* By J. C. HALL, M.D., Physician to the Sheffield Public Dispensary, &c. With six illustrations.—*London*, 1857.

AMONG the numerous arguments that may be adduced in favour of allowing our beards to grow instead of wasting our time every morning in scraping them off, not the least is, that it would render unnecessary the use of razors, and consequently, *pro tanto*, diminish the Sheffield grinders' disease. Dr. Hall brings good evidence to show the fatality among the men employed in grinding razors and other cutlery, owing to a form of chronic pneumonia produced by the inhalation of fine particles of steel given off in grinding. Much may evidently be done to diminish the danger inherent in the occupation by protective appliances: thus we find that the average age of grinders at death at the works of Messrs. Rogers is 42, while in the Suffolk Works it is 38½, owing to the difference in the arrangements at the respective manufactories. A characteristic feature in the expectoration, and in the lungs themselves, is the presence of large quantities of black matter, both disseminated through the organs and accumulated in globular spots over the surface. No steel appears to have been found in the lungs; but it would be interesting to know whether these lungs contain any unusual amount of iron, as it is not improbable that the steel inhaled might be altered by oxidation, and thus in part give rise to the black deposit. The greater part of this must be regarded as a secretion from the blood, especially that found in the bronchial glands, which are also in a melanotic condition.

We recommend this contribution to the history of industrial pathology to the attention of all who are interested in this important subject, whether on scientific or on philanthropic grounds.

Art. III.—*The Asylum Journal of Medical Science.* Published by authority of the Association of Medical Officers of Asylums and Hospitals for the Insane. Edited by John Charles Bucknill, M.D.—*London*, April, 1857.

The Association of Medical Officers of Asylums and Hospitals for the Insane is, it appears to us, admirably represented in the 'Asylum Journal,' of which we now have the twenty-first number before us. The general tendency of the periodical is one that maintains and fosters the vital connexion existing between the science of medicine at large, and its offspring, the science of mental disease. As long as derangements of the mind were regarded simply as a mysterious dispensation of Providence, offering no analogy with other forms of disease, it could not be expected that great advances would take place in our knowledge of their intimate nature. The fatalistic view was a barrier to all progress. A very able article, by Dr. Bucknill, in the April number of the 'Asylum Journal,' is devoted to the consideration of the relation of mental pathology to the physical agent of the mind. The path which the author pursues is, to our appreciation, one of legitimate induction; and we cannot but think that the physiological principles upon which he builds his superstructure are correct in the main. The essence of his views may be given in his own words:

"Mental health is dependent upon the due nutrition, stimulation, and repose of the brain; that is, upon the conditions of the exhaustion and reparation of its nerve substance being maintained in a regular and healthy state; and that mental disease results from the interruption or disturbance of these conditions."

This doctrine receives considerable development, and is very ably supported, so as to merit the special attention of all who are interested in the study of the human mind, in its healthy or diseased manifestations. We meet with Dr. Bucknill again in the same number, discussing the important question of asylum architecture and arrangements.

Dr. Wood, formerly the medical officer for Bethlem, brings forward strong arguments for the establishment of a State Asylum. The present condition of criminal lunatics appears to be one calling for speedy reform:

"Monstrous as it must appear to all who ever give a thought to the subject, the acquittal, *of whatever offence*, on the ground of insanity, leaves no alternative. The unfortunate offender may not now be dealt with but as the most atrocious villain who ever disgraced our nation. Of gentle, or it may be noble birth, it matters not that he may up to this moment have pursued a virtuous, honourable and useful career; the law recognises no distinction between such a one and the convicted felon who has become insane while undergoing his punishment."

The principle upon which the reform should be carried out is manifest. We trust that Dr. Wood will not be deterred by the difficulties that meet all innovators from pushing forward his propositions until they are realized.

Dr. Tuke supplies a paper on the various forms of mental disorder, and Dr. Boyd furnishes one on epilepsy; both deserving of careful perusal. Reviews and Retrospects complete the number.

ART. IV.—*The Functions and Disorders of the Reproductive Organs in Youth, in Adult Age, and in Advanced Life, considered in their Physiological, Social, and Physiological Relations.* By WILLIAM ACTON, late Surgeon to the Islington Dispensary, and formerly Externe to the Venereal Hospitals, Paris.—*London*, 1857. pp. 108.

WE doubt whether, among our human relations, there is one that exerts a greater influence upon most of us than that which draws its impulses from the sexual feelings. Indirectly, it governs the whole life of the female, from the time at which she handles her first doll to the time when she teaches her grandchild "pattycake, pattycake;"—the vices and the virtues of the sterner sex—less confessedly, perhaps, but no less really—result from the vagaries and dreams of boyhood, or the waywardness or resolution of adult age, that are prompted by the sexual instinct. No Draconian law can fetter the strongest impulses of our nature, and yet from the commencement of society, religion and social morality have enjoined the necessity of restraint; the highest rewards being the necessary lot of those who keep their body in subjection, while the train of evils, physical and moral, which inevitably pursue the Claudios of society, are almost identical with the miseries that surround us on every side; and yet, though so fertile a source of wretchedness,—though

"Our natures do pursue,
Like rats that ravin down our proper bane,
A thirsty evil,"—

what has been done, beyond passing certain enactments, which all but connive at the wide-spread taint, to check the social plague, and to spread a knowledge of the laws which bind together the physical and moral duties of man? The youth is left, in the most important question of life, to learn by sad experience—experience that often is synonymous with ruin—what he ought to have avoided; he is introduced by the very men who most should guard him and guide him, to the debaucheries of heathen mythology; and the silly prudery which ignores the sexual feelings of the adolescent, allows him to revel in the prurient tales of Lemprière. But who shall teach the guides and open their eyes? We have here one additional argument to the many that have already been urged, for the spread of a knowledge of physiological laws among the laity. Let the teachers of youth be able fully to appreciate the bearings of the questions at issue; let them know that the enemy must be met by an acquaintance with his wiles and pitfalls, and by self-control, and there is no doubt that, their eyes once opened, they will discover the proper means and occasions for giving the necessary information to those committed to their charge. With Mr. Acton, we would raise our protest against

"Allowing men of a larger growth to remain in their present profound ignorance of all appertaining to sexual matters, except such as they may gather from experience, or the equally vague and erroneous conversation so often heard in smoking-rooms, at supper parties, or that equivocal and unscientific information read with such avidity in newspapers—as disclosed in divorce cases and actions for *crim. con.*"

Sexual excesses are the monster evil of the present, no less than of former times; it is not, except in particular forms, a subject for legislation, because legislation cannot reach it; but it is essentially a subject for the clergyman and the schoolmaster to deal with. It is folly to ignore what every man who has been at a school must know to prevail. It is wisdom to avail ourselves of the holiest aspirations of the youth to enable him to shun evil, not from fear—though from fear, if need be—but from a just appreciation of the immutable laws which may be traced equally in Holy Writ and in natural theology. We think Mr. Acton has done good service to society by grappling manfully with sexual vice, and we trust that others, whose position as men of science and teachers enables them to speak with authority, will assist in combating and arresting the evils which it entails, and thus enable man to devote more enduring energies and more lofty aims to the advancement of his race, and to the service of his God."

With these few remarks we would specially introduce Mr. Acton's book, which forms a separate edition of a part of the third edition of his larger work 'On the Urinary and Generative Organs,' to the favourable notice of our readers. We would desire to see its subject made a matter of meditation by many out of the profession, and especially by the teachers of our young generation.

"The continent student will find reasons for continuing to live according to the dictates of virtue. The dissolute will be taught on positive and irrefragable grounds the value of self-control. The married man will find advice and guidance, and the bachelor, who is often placed in a trying social position, will glean consolation from observing that not only are his sexual sufferings appreciated and understood, but that rules are given him for their mitigation."

These words, which we quote from the preface, indicate truly the scope of the work. Although it contains some passages which we think might be advantageously omitted, we are of opinion that the spirit which pervades it is one that does credit equally to the head and to the heart of the author.

Art. V.—*Medical Examinations and Physicians' Requirements Considered.* By Thomas Mayo, M.D., F.R.S., President of the Royal College of Physicians. *London*, 1857. Pamphlet.

The character of examinations ought necessarily to vary with the objects for which they are instituted. As the introduction of competitive examinations into the Civil Service has roused public attention to the question, Dr. Mayo enters the lists to do battle in favour of the system hitherto pursued at the College of Physicians. Although we are not aware that the mode of examination followed by that ancient body has been impugned beyond the college doors, and do not therefore perhaps quite appreciate Dr. Mayo's motive for defending the system, we think with him, after considerable personal experience in the matter of examinations, that it is on the whole well adapted to determine the qualifications of candidates who desire to receive the stamp of the highest medical corporation of the country.

It would appear that the College of Physicians proposes, by its examinations, to ascertain whether a candidate for its license possesses a large practical acquaintance with disease, and a mind imbued not only with classical taste, but with that power of observing nature and appreciating vital and morbid phenomena in all their phases, which in itself is a proof of a thoroughly well-trained mind. If there is one thing wanting to render the examination of the College complete, it is that a further test should be applied of the practical tact and knowledge of the candidate, by allowing him to prove his aptitude at diagnosis and treatment at the bedside. Dr. Mayo, who is desirous to prevent the introduction of *competitive* examinations into the College, observes very justly, that

"Nothing can be more fatal to the evolution of *continuous* thought, than at the age at which ultimate habits are forming, to be involved in the preparation of four or five departments of severe thought against an examination. Neither time nor occasion to *master* them, but every inducement to adopt the perfunctory process, which will secure well-compacted answers to probable questions, the mind leaving each part of the subject as soon as this point is gained. Such, I believe, has been the state of things, and its result, in many cases of University honours. The candidate grasps his prize before the subjects of it have had time to settle into his mind, and the books which he had for that purpose lie cold on his table for the rest of his life."

The tendency of competitive examinations is to induce the candidate to pay special attention to subjects, which, however important in themselves, are more calculated to impart scientific distinction, than those qualities which are most required to fit the individual for the practice of his profession. Dr. Mayo's pamphlet in no way conveys that he underrates classical and mathematical attainments, and the study of the vast range of sciences auxiliary to medicine; he merely explains the grounds upon which he is opposed to the introduction into the College of Physicians of competitive examinations, and of examinations repeated at varying intervals.

The author concludes his observations with a graceful tribute to those who have laboured to reform the organization of the medical profession in Great Britain; and we hope with him that if, at this late period, some slight differences may appear to linger between the various corporations whose interests are more immediately involved,

> "The Government will exercise that most legitimate influence, by which the differences incident to every large body, made up of sections heretofore acting under separate banners, may be induced to yield to the general good."

ART. VI.—*New Remedies, with Formulæ for their Preparation and Administration.* By ROBERT DUNGLISON, M.D., Professor of the Institutes of Medicine, &c., in the Jefferson Medical College of Philadelphia. Seventh Edition, with numerous Additions.—*Philadelphia*, 1856. pp. 769.

As a work of reference upon all new remedies, this is one of the most complete with which we are acquainted. The quotations of authorities are extensive, minute, and carefully given; and we are satisfied that no medical man would regret following our advice to acquire it, as daily opportunities will occur in which he may both test its value, and increase his own knowledge, in searching for the practical information it affords.

ART. VII.—*The Hygienic Treatment of Pulmonary Consumption.* By BENJAMIN W. RICHARDSON, M.D., Licentiate of the Royal College of Physicians, Physician to the Royal Infirmary for Diseases of the Chest and to the Margaret-street Dispensary for Consumption, Lecturer on Pathology at the Grosvenor-street School of Medicine, and Corresponding Fellow of the Pathological Society of Montreal.—*London*, 1857. pp. 115.

WE hold the opinion that, whether tubercle consists in a fatty degeneration of epithelium, in the exudation of an unorganizable fibrin, or of an excessive production of epithelial *débris*, the efficient cause of the disease is to be found in the prolonged inhalation of a vitiated atmosphere. Other influences may be at work to impair the tone of the muscle and the plasticity of the blood, but none of them are able to produce tubercle, unless the food that is offered to the lungs is deprived of its due amount of oxygen, or adulterated by the addition of carbonic acid, sulphuretted hydrogen, ammonia, or similar noxious gases. If this principle be conceded, it follows, as a matter of course, that whether we have regard to the prevention or the cure of the tuberculous diathesis, the first element for the physician to attend to is the provision of a pure atmosphere. The patient must be constantly surrounded by air, which enables all the vital processes to be carried out vigorously. Without pure air, the depuration of the blood must be defective; and if the surface of the pulmonary mucous membrane, the extent of the human ventilator, has once been diminished by the deposit of any material that interferes with the function of respiration, the influence of atmospheric impurity will gain in noxious power in the ratio of a geometrical progression. Let all medical men ponder well upon the importance of pure air; let them unite to enforce the purification of the atmosphere outside our dwellings, and its free and unintermittent introduction into our rooms, and the misery and mortality resulting from tubercular diseases will be reduced in a manner which only those can estimate, even approximately, who are able fully to appreciate the physiological bearings of the process of respiration. But it will not suffice to wait till the oxygen is brought to us. We must go in search of it. We must encourage the metamorphosis of our tissues and the elimination of the waste products by exercise, while we supply healthy nutriment to the stomach, and provide against excessive waste and exhaustion, by proper clothing and due nightly rest. Dr. Richardson discusses all these elements in the physiological treatment of consumption in a healthful spirit; they are views which are, we believe, shared by many medical men, though they have not been, perhaps, so specifically brought to bear upon the treatment of pulmonary consumption as in the present instance. We shall gladly see the volume obtain a wide circulation in and out of the profession, because it is impossible to preach too widely and too frequently the superlative influence of pure air in the treatment and the prevention of disease generally, and of tubercular disease in particular.

The author divides his work into seven chapters. The first introduces the subject of the hygienic treatment of consumption; the second is devoted to the consideration of the supply of pure air as the first indication in its treatment, and the necessity of active exercise. Climate, dress, rest, form the subjects of the third chapter. In the fourth, the occupations and amusements of the consumptive patient are considered; while the importance of cleanliness, of abstinence from all kinds of sensual extravagances, the dangers of marriage on the part of consumptive females, are enforced in the fifth chapter, which concludes with remarks on diet, and the use of tobacco by phthisical patients. A few brief remarks on the medicinal treatment of the disease, in which Dr. Richardson well exposes the folly of seeking for a specific in the phthisis, occupy the sixth chapter. The seventh and concluding chapter is devoted entirely to a comparative examination of the various modes of artificial ventilation proposed, from the time of Dr. Hales downwards.

In taking leave of Dr. Richardson, we would thank him for the able addition which, in the book before us, he has made to the great edifice of sanitary science. It is here that our real strength lies. While we accept gratefully the beneficent agency of the contents of the Pharmacopœia, which Dr. Richardson, in our opinion, estimates at their true value, we know that the most powerful weapons that can be wielded against the inroads of disease and the devastations of premature death, are those which the patient too generally thinks least of, because they are at hand, and may be had without the intervention of a soothsayer or a prophet.

Art. VIII.—*The Structure, Functions, and Diseases of the Lungs.* By Thomas Williams, M.D., F.L.S., Physician to the Swansea Infirmary, Author of the Collegiate Triennial Prize Essay 'On the Structure and Functions of the Lungs,' and of the Article On the Organs of Respiration in the 'Encyclopædia of Anatomy and Physiology.' (Part I., Water and Air-lungs of Invertebrate Animals, and Aquatic Respiration.)—*London*, 1857. pp. 201.

The original of the present work, in the year 1842–3, procured for its author the high distinction of the triennial prize of the Royal College of Surgeons. That fact alone would make it imperative upon us to examine it with care, and place before our readers the results of the laborious researches of Dr. Thomas Williams. A cursory inspection shows the work to be one of no ordinary merit. For the present we must, however, content ourselves with announcing its appearance; we shall wait for the appearance of the second part, which will bring the physiological inquiries of the author, on the subject of the respiratory apparatus throughout the animal creation, to a close, and then seek to present to our readers the views and observations of Dr. Williams in a condensed form.

ART. IX.—*A Report upon some of the more Important Points connected with the Treatment of Syphilis.* By HOLMES COOTE, F.R.C.S., Assistant-Surgeon to St. Bartholomew's Hospital.—*London*, 1857. 8vo, pp. 141.

THE author enters upon an independent investigation of some of the phenomena of syphilis. We rejoice to see this fertile subject occupying the attention of hospital surgeons, and we trust that Mr. Coote will continue to pursue the inquiry he has commenced in that large field which is open to his labours.

In the introduction, Mr. Coote states that his experience does not confirm the doctrine that a primary sore giving rise to suppurating bubo, is not usually followed by secondary symptoms. In illustration of this he instances the cases of eleven male patients in the venereal wards of Bartholomew's suffering with secondary syphilis; in two there had been suppurating bubo. There were also nine females, of whom four had had suppurating bubo.

We are compelled to observe, that these statements prove nothing with regard to the real point at issue. Were the suppurating buboes *specific*, or were they ordinary inflammations of the lymphatic glands, such as we know are constantly recurring from slight local injuries of any kind, especially in weak constitutions? Nothing but the test of inoculation could have proved that. If the secretion from the inguinal swelling were inoculable, the affection was specific, and the probability is that the constitution would not subsequently suffer, the virus being (such is one theory) eliminated by the lymphatic system. If it were not inoculable, the affection was probably a simple inflammation, between which and the chancre there is no ground for believing any *specific* relation to have existed. That the latter was the true pathology of these cases, appears to us to be indicated by the author (although unintentionally) in the following remark:

"It must be remembered that women living on the streets are unable to rest at the commencement of their attacks of disease; and hence suppuration of the inguinal glands is by no means uncommon." (p. 11.)

The cause suggested explains the suppuration—a simple, not a specific one. Inability to rest would, in the same way, determine the existence of a suppurating gland after a scratch on the leg, or a sore corn. Without inoculation, the *specific* character of the inguinal swelling cannot be affirmed; and, wanting a knowledge of this important datum, no inference can be drawn respecting the nature of the process which has been set up there, whether it has been virulent or otherwise. Granting that these buboes were simply the results of local irritation, not of the specific poison (and this is certainly a probable view of the case), these cases entirely cease to controvert the doctrine which Mr. Coote disbelieves. A somewhat similar, but not identical doctrine, has been for some years past taught in Paris, and has in part been brought prominently forward in this country by Mr. Henry Lee, and as much of it as relates to this question—discussed as it is by Mr. Coote without reference to an observation of the primary sore—may be thus formuled—

A primary syphilitic sore, followed by three or four small indurated glands, without tendency to suppurate, is almost certainly followed by secondary symptoms.

A primary sore, followed by one acutely inflamed gland, which suppurates *and is inoculable*, is, on the contrary, rarely followed by infection of the system.

But then it is unphilosophical to regard this question of bubo separately from the character of the sore which gave rise to it. The first-named sore is the indurated chancre; the second is the non-indurated, non-infecting, but most common variety. Each has its own specific action upon the lymphatic glands, as described above: the first alone extends to the system also, and gives rise to secondary symptoms; the virulence of the second does not go beyond the suppurating gland. Either may give rise (from irritation, as of walking, or without it in certain constitutions) to a simple gland enlargement and abcess, but without specific virus in the pus. Hence a non-indurated chancre, followed by acute gland suppuration, the pus of which will produce a similar chancre, is never followed by secondary symptoms. Such is the doctrine referred to—a doctrine demonstrated and emphatically taught by Ricord, and which our own experience corroborates the truth of; but it is widely different from that which has been enunciated in its place by Mr. Coote, for purpose of disproof; viz., that—

"In those instances in which the irritation of the lymphatic glands is the greatest, and where, consequently, we have the best evidence that the morbid matter has entered them, there is very seldom any secondary syphilitic affection." (p. 10.)

But the author does, in the case of the men only, refer to the character of the sore, stating that "not one had the true indurated chancre. The primary sores were superficial ulcerations of the glans and prepuce, *leaving cicatrices*, and one case of primary phagedæna."

The italics are our own. The fact of the non-induration is affirmed, not on observation of the sore, but of its cicatrix. Nothing can be more doubtful than the inference. "But," the author proceeds, "in most cases the patients stated that the glands in the groin swelled up for a week or more, and then subsided." This is the very form of gland-enlargement, pointed out in the formula above, as characteristic of the indurated chancre, and is strong evidence, in absence of an actual observation of the primary sore, that such was its nature in the "most cases" adduced.

Passing to another topic, we learn that Mr. Coote adheres to the belief that gonorrhœa is the result of a specific poison. He quotes the following case as proving that "its existence retards, as it were, the action of the syphilitic virus." (p. 31.) A patient was admitted to the City Bridewell with gonorrhœa of fourteen days' standing. After three weeks' residence it left him, and very soon "a superficial non-indurated venereal sore" appeared on the skin of the under part of the penis. There could have been no fresh infection from the other sex. The source of this sore is then discussed by the author, who doubts that in this case intra-urethral chancre complicated the gonorrhœa, but that the sore on the penis was due to syphilitic virus, the incubation of which had been delayed by the first-named complaint, because

"There were no symptoms whatever of ulceration of the urethra; there was no excoriation at the orifice, nor induration along the canal, nor pain upon pressure, nor any sign by which such an occurrence could be suspected."

Surely we cannot forget that many infecting sores seen on the exposed part of the penis possess no characters by which it would be possible to determine their existence by any physical examination, supposing them to be seated within the urethra, and beyond the reach of vision. We cannot doubt that this was a case by no means rare, of gonorrhœa co-existing with intra-urethral chancre, of the ordinary non-indurated form, by which the skin of the penis was subsequently inoculated. That the two things do co-exist has been proved a hundred times by Ricord—a fact familiar to those who know his practice.*

In asserting the existence of but one syphilitic virus; that the character of the sore is determined greatly by the natural structure of the part in which it is situated; that the occurrence of phagedæna depends much on the constitution of the individual; and that the course of secondary symptoms is not to be predicated, Mr. Coote opens up topics, to enter upon the consideration of which would require very extended limits. Some of these positions are regarded by very high authorities as requiring modification, under the light which has, during the last few years, been thrown upon the subject of syphilis.

* See also his Lettres sur la Syphilis. Paris, 1851. The fourth to the eighth, inclusive.

Nothing new transpires respecting treatment. The plan commonly followed by the author consists in that invaluable resource, early cauterization and moderate mercurialization, when the chancre is indurated. To the value of the Turkish bath, "which may now be procured in London" (p. 109), for stimulating the cutaneous function, in the treatment of secondary symptoms, we are glad to have this opportunity of bearing a corroborative testimony. The successive applications of hot vapour, soap, thorough friction, and water douches, of which our own metropolitan form of that Oriental luxury consists, constitute, when made with proper precaution, a powerful means of elimination from that vast and important organ, the skin, the benefit of which we have had opportunities of witnessing in some cases, and desire to extend.

In conclusion, although obviously disagreeing with Mr. Coote on some fundamental doctrinal points in the pathology of syphilis, we regard this essay as a record of thoroughly practical labour, and most assuredly desire to thank him for the search after facts which it is evidently always his object to attain.

Art. X.—*Archiv für Ophthalmologie.* Erster Band, Erste Abtheilung, herausgegeben von Dr. A. von Graefe. Zweite Abtheilung, herausgegeben von Prof. F. Arlt, Prof. F. C. Donders, and Dr. A. von Graefe.—*Berlin*, 1854–5. pp. 358. Zweiter Band, 1855–6. pp. 346. Dritter Band, Erste Abtheilung, 1857.

The Archives of Ophthalmology. Vol. I., Part 1, edited by Dr. von Graefe. Part 2 edited by Prof. Arlt, Prof. Donders, and Dr. von Graefe. Vol. II. Vol. III., Part 1.

We owe an apology to our readers for having so long delayed noticing this new organ of ophthalmic science. It is in every way a worthy rival of the well-known 'Annales d'Oculistique,' which for so many years have been the medium of communication between Continental ophthalmologists and the profession at large. There is ample room for both publications to labour harmoniously in the same field, which they cultivate in a somewhat different manner. While in the 'Annales' the reader is kept *au courant* with the literature and ophthalmic news of the day, the 'Archiv,' instead of aiming at the character of a journal, rather resembles the 'Transactions' of our medical societies.

The volumes now before us contain several essays of sterling merit, and the chief editor, von Gräfe, has contributed a rich collection of miscellaneous cases from his public and private practice. Associated with him as contributors, we find the well-known names of Helmholtz, the inventor of the ophthalmoscope, von Ammon, Rass, Donders, Liebreich, Zehender, and others. Among many papers of interest in Vol. I., we may notice a careful essay by von Gräfe On the Action of the Ocular Muscles; and his miscellaneous notes of cases are all more or less interesting. He has been so fortunate as to detect, by the aid of the ophthalmoscope, the presence of entozoa—*cysticercus cellulosæ*—within the vitreous chamber. No fewer than nine cases of this singular affection have come under his observation; the entozoa being either fixed to the retina, or floating freely in the humours of the eye. We are not aware of such cases having been met with by any other observer, either in Germany or elsewhere. Among the essays in Vol. II. we may specially notice a very elaborate one, by Dr. Meissner, On the Movements of the Eyeball; a Contribution towards the Histology of the Choroid, by Dr. Wittich; and a further Series of Cases, by Dr. von Gräfe.

Only one-half of Vol. III. has yet appeared; but it well sustains the character of the work by the variety and interest of its contents. Dr. H. Müller contributes original researches On the Anatomy of the Ciliary Body and the Mechanism of Accommodation; the Histology of Capsular Cataract, &c.; Dr. Zehender, a paper On the Refracting Powers of the Media of the Eye; Dr. Donders continues his Pathological Contributions; and Dr. von Gräfe, in an article of more than two hundred pages, commences a review of the whole question of Strabismus, and the operations for its cure.

We heartily wish success to a work which so well sustains the high character of German ophthalmology; and we shall not fail, from time to time, to lay before our readers some of the more important facts recorded in its pages.

ART. XI.—*Notes on the Belgian Lunatic Asylum, including the Insane Colony of Gheel.* By JOHN WEBSTER, M.D., F.R.S., &c.—pp. 68.

To the philanthropist, we know no subject more gratifying than the treatment of the insane, and the management of lunatic asylums as at present conducted, compared with the past of no remote date. We are old enough to remember the time when chains were in use—when punishment was considered necessary, and coercion essentially so. We would fain hope that the progress which has been made in this most important branch of medical practice, and which is mainly due to the exertions of enlightened physicians, betokens not only an advance in the right line, but may be viewed also as some makeweight against the degrading influences of the various delusions which have of late prevailed, and are still prevailing, in society, under the names of mesmerism, table-turning, spirit-rapping, and the like; delusions which, one after the other, have started up, as if to show the weakness of human reason, and to check us in our too lofty aspirations regarding its efforts.

The lunatic asylums in Belgium, it would appear, from the account given of them by Dr. Webster, in his interesting 'Notes,' are an example of the progress we have alluded to, and are perhaps as good an example as could be adduced in any part of the *civilized* world; and we say civilized with emphasis, for where there is want of civilization, there we are sure no humanity is shown to the suffering insane. We may mention in proof that in Constantinople, a very few years ago, we had the pain to witness lunatics, chained like wild beasts, made a sight of, and allowed to be the sport of mischievous boys, and separated only from a managerie by an intervening wall.

In Belgium, this progress, this change of system from severe to mild, we learn, has only been effected within the last five or six years. In 1852, we are assured by a native writer, M. Guislain, in his lectures, 'Sur les Phrénopathies,' that "Lunatics in Belgium remain forgotten in sombre prisons;" that

"They resemble merchandize amongst speculators, who make them the objects of nefarious traffic, like animals from the farm-yard, fit only to be bought and sold as horses or swine."

Adding,

"Much talk has certainly taken place during the last thirty years: but so little has yet been accomplished, that our afflicted maniacs have only been turned round in a vicious circle of selfish and fatal administrative influence." (p. 11.)

We must refer to Dr. Webster's Notes on the several asylums for the particulars of the beneficial change, briefly remarking that it is connected with a radical change of the administrative system, organized as a public department, and directed by rules founded on just views of the malady; of the more important of these rules a summary is given by the author. The inspection of the asylums, whether public or private, at least three times yearly, by a different class of persons, and the sanction of the Government for the erection of new, or for alterations even in existing, asylums, are particularly worthy of attention.

Belgium is a country of all others, distinguished for its mixed breed of people; nowhere, not even in England, have races been more crossed; yet in Belgium it is worthy of remark, that the proportion of the insane is large—larger than in England, and indeed than in most countries of which we have trustworthy statistics. In a population of about 4,520,000, the number of recognised lunatics was recently 4907; in towns, in the proportion of one for every 470 residents; in the country, one for 1368 inhabitants. In regard to sex, too, it is worthy of remark, that the male preponderates, their number being 2630 to 2277 females.

The unusual prevalency of insanity in this country, seems to be owing to various causes, those on which the author lays most stress are poverty—poverty especially—and vice; the former conducing to it by a low and inadequate diet; the latter, through the intemperate use of ardent spirits and tobacco, and sensual excesses.

We trust that all who take an interest in the subject (and who are they who ought not to take an interest in it?) will read Dr. Webster's remarks in their details. The few remarks we have to offer must be limited chiefly to the "Insane Colony of Gheel," the oldest establishment for maniacs in Europe, which on many accounts is deserving of special attention, and most of all by our Government, and of all under it exercising authority in matters of lunacy.

The great peculiarity of this colony, of which the town of Gheel is the centre—situated in a country, a barren waste by nature, rendered fertile by industrial labour, and partly by that of the insane themselves—is, that the lunatics sent there become the inmates of private families. At the time of the author's visit, the number of the receiving families was 500, of which about 300 possessed cottages or farm-houses in the country, the rest residing in the town. The number of lunatics thus distributed was 774. Dr. Webster, speaking of their distribution, says:

"I visited numerous houses in the town, and a great many cottages scattered over the adjoining country, in which often one, although most frequently two, and occasionally three, insane persons resided."

Adding—

"That is the general system followed, with but very few exceptions,—seeing not more than five instances exist throughout the entire colony where beyond four patients are placed with the same family, but only then for special reasons, and after an express authorization from the Committee of Inspection is obtained." (p. 41.)

This unique colony is as singular in its origin as in its nature. Its origin goes back to a remote period, and to a tragic and atrocious act—the flight and beheading of an Irish princess—a saint and virgin, St. Dympna, in the sixth century, by a king, her father, a Pagan, and, something worse, amorous of his child, and enraged at her virtuous resistance, and to her change of religion and firm adherence to Christianity. The cruel acts perpetrated on her, according to the legend,

"Greatly frightened several lunatics said to be present, and, tradition reports, cured them immediately, through the strong impression this terrible spectacle produced on their excited feelings. Immediately the cry of 'A miracle! a miracle!' was raised by the wondering bystanders; and thus Dympna, saint and virgin, became ever after the patron of all mad persons. This faith having been spread abroad, lunatics were brought to Gheel to get cured through St. Dympna's intercession, and firmly established its reputation." (p. 32.)

Our readers may be interested to know that Gheel is now easy of access; that it may be reached from Brussels, by rail and a daily two-horse omnibus, in the short space of two hours and a half, and that in the town there are two inns, both good, affording very comfortable accommodations, and on surprisingly reasonable terms; three francs per diem—we record it as a matter of curiosity—covered the entire hotel charges of our author.

Amongst the more remarkable circumstances noticed by Dr. Webster, in describing this colony, is the part taken by children in the service and management of the insane, the security attending the freedom allowed the patients, and their few escapes, and the many and useful occupations in which they are employed. The particulars on these points, and on others given by the author, we have read with extreme interest; and we can recommend the perusal of them to others, confident that they must excite in them the same feeling; and more, that much in the system that distinguishes the colony of Gheel is worthy of the serious attention of our Government officials, and of being followed in our establishments for the insane.

We see that Dr. Webster raises his voice against palatial asylums constructed at an enormous amount of expense, burdening the rate-payers, without perhaps commensurately benefiting those unfortunates for whom they are designed; and the placing them, at a like extravagant cost, not on waste grounds, such as the locality of Gheel was, but on already reclaimed or fertile land of high marketable value, capable of little further improvement.

Art. XII.—*On the Diseases, Injuries, and Malformations of the Rectum and Anus.* By T. J. Ashton. Second Edition.—*London*, 1857. 8vo, pp. 390.

The rapidity with which the works upon diseases of the rectum, by Curling, Quain, and the present author, have passed into second editions, testifies at once to the prevalence of these affections, and to the insufficiency of the accounts given of them in the general treatises upon Surgery. Mr. Ashton's work must be regarded as the most complete one we possess upon the subject; and while he exhibits in it a considerable acquaintance with what has been written by others, the practical facts he contributes quite prevent the treatise being characterized as a mere compilation. We have heard it objected that it has been too much based upon the work of the late Dr. Bushe; but even were this true, which we do not think it is, it would, in our eyes, constitute rather a merit than a reproach. Dr. Bushe's treatise, though highly valued by those acquainted with its contents, never met with that general acceptance it deserved; and the wider diffusion of his views, by an intelligent critic and a good practical surgeon, would be very desirable.

Mr. Ashton's style is somewhat too diffuse, and his book would have gained by compression; but, even with this drawback, it well deserves the success it has met with.

Art. XIII.—*Practical Hints on the Management of the Sick-room.* By R. Hall Bakewell, M.D., Member and Licentiate in Midwifery of the Royal College of Surgeons of England, L.S.A.; formerly House Surgeon to the Middlesex Hospital, and to the Stafford County Infirmary; late of the Medical Staff in the Crimea; author of letters of 'Sanitary Reform in Rural Districts.'—*London*, 1857. pp. 47.

Dr. Bakewell informs us, that when a patient lies in bed "very little waste is going on in the system, and that that part of the food which is required for sustaining animal heat is almost wholly unnecessary—artificial heat being supplied." Does this mean that people who are confined to bed invariably use, or ought to use, hot water-bottles and stomach-warmers? Again, Dr. Bakewell, in speaking of "mustard plasters," says only they "should always be made with fresh mustard and cold water;" in a book intended specially for "lady readers with lily-white hands," we should have thought that some further information on the subject would have been necessary. We know from experience that such information would not have been thrown away. One more specimen of the instruction conveyed by Dr. Bakewell, and we have done: in speaking of pills, the observation "that it is advisable to take some fluid afterwards in order to assist in dissolving them in the stomach," is followed by the statement, "that it is better to take aperient pills immediately after a meal, as then they are digested with the food."

We think the above instances suffice to show that Dr. Bakewell has not yet attained a position which justifies his offering himself as an instructor to the public; and we would suggest for his consideration, that in order to become a successful popular writer, it is not enough to publish a mere *réchauffé* of scanty notes taken at school lectures.

Art. XIV.—*Summary of New Publications.*

During the past three months we have received, in addition to the works already spoken of in other parts of the Review, numerous productions of more or less value in medicine, surgery, and the allied sciences. Hygiene, we are happy to find, rejoices in an increasing body of powerful supporters. The Medical Officers of Health of the City of London, Hackney, Whitechapel, St. Pancras, St. Luke's, Chelsea, and Islington, have published valuable Reports; and with them we may mention Mr. Blyth's 'Minute of Information on Disinfection,' Mr. Gamgee's 'Letters on the Cattle Plague and Diseased Meat,' and Dr. Greenhow's 'Report on Murrain in Horned Cattle,' presented to Parliament.

In Medicine, we direct especial attention to the third edition of Dr. Budd's work 'On the Diseases of the Liver,' which has fairly established for itself a place among the classical medical literature of England. The pathology of fever is represented in Dr. Bartlett's work, 'On the History and Diagnosis of Fevers in the United States,' the original edition of which was reviewed at length by one of our predecessors.* Dr. Evans Reeves has published a volume 'On Diseases of the Stomach and Duodenum;' while cardiac pathology finds exponents in Dr. Cockle and Dr. Markham, the former having published a paper 'On the Second Sound of the Heart,' the latter having re-issued 'Contributions to Cardiac Pathology,' which first appeared in a cotemporary periodical, and the details of which will be found in the Quarterly Medical Report. Dr. Gull has republished in a separate form, the cases of paraplegia which have recently appeared in the 'Transactions of the Medico-Chirurgical Society,' and in the 'Guy's Hospital Reports.'

An elaborate memoir by Dr. Tholozan, 'On Metastasis,' has reached us from Paris. The author regards the cases commonly set down as metastases, as manifestations of the fundamental diathesis, and as independent of the other localizations of disease as these are of one another. An elaborate 'Report on the Recent Epidemics of Cholera,' has appeared, by the pen of Dr. Hirsch, of Danzig, already favourably known as an earnest inquirer. Climatology is represented by Mr. Edwin Lee, who has published a *brochure* on the Hyères, the Isle of Wight of France; by Mr. Smart and Dr. Aitken, who have respectively written on the climates of the Crimea and of Scutari.

We may here also mention a book on the theory and treatment of stammering, by Mr. Urling, which has much to recommend it, entitled 'Vocal Gymnastics.' Dr. Gairdner has thought it necessary to continue the controversy between medicine and homœopathy which he so ably commenced in the 'Edinburgh Essays.' We would earnestly recommend him, and all others who may by tempted to take up the cudgels, to act upon the principle enunciated in a passage of Dr. Gairdner's 'Few Words on Homœopathy,' and allow homœopathists "to remain, like the cuttle-fish, safe in their own ink." Even the cuttle-fish would choke if the sea did not wash away its ordure.

In Surgery, we have received a new work 'On Stricture,' by Mr. Henry Smith; the publication by Dr. Fell of the method adopted by him of treating cancer by the local application of caustics, has allayed all feverish excitement with regard to the subject, an excitement which the result scarcely appears to justify, either in a pathological or in a therapeutic point of view. Mr. Wharton Jones has brought out a 'Catechism of Ophthalmic and Aural Medicine;' a second edition of Mr. Hare's very useful Cases and Observations, illustrative of his mode of treating spinal deformities, and to which we drew attention in our April number, has already appeared. From abroad, Dr. Gay's 'Surgical Cases,' a reprint of a paper in which four cases of tumours of the extremities are discussed, have come to hand; with a French translation by M. Gosselin, of Mr. Curling's well-known work 'On the Testis,' with additions by the editor; an important work by M. Broca 'On Aneurism;' and another, deserving of attention, by Dr. Benedict, of Stockholm, 'On Hernia.'

* See British and Foreign Medico-Chirurgical Review, vol. xviii. p. 357.

The literature of Mental Pathology has received a ponderous contribution, in the shape of two large Blue-books, containing the 'Report of the Scottish Lunacy Commissioners,' which have already excited public attention by the abuses which they expose; Dr. Hood's 'Decennial Report on the State of Bethlehem,' also deserves mention.

An able treatise 'On the Wave Theory of Light,' by Dr. Lloyd, will, with many of the works previously mentioned, receive fuller attention at an early opportunity; we shall also have a word to add concerning Mr. Fenwick's popular treatise, 'On the Causes and Prevention of Disease,' and of an iatro-theological volume by Dr. Duncan, entitled 'God in Disease.'

Periodical literature boasts of numerous accessions. The most imposing is a quarterly medical review, published in Birmingham, under the title of 'The Midland Journal,' the first number of which contains several papers by well-known surgeons and physicians. The dentists have brought out the first number of a 'Quarterly Journal of Dental Science,' which represents those of the fraternity who hold it better to constitute themselves a distinct body than to be associated with the College of Surgeons. The Imperial Society of Constantinople is represented by a monthly paper, bearing the title of 'Gazette Médicale d'Orient,' the first number of which contains the commencement of an original article on miliary fever, by Dr. Tian, the *compte-rendu* of the Society during the first year of its existence; and a brief review of the cotemporary medical press. Our Dutch *confrères* have also issued a new periodical, under the title of 'Nederlandsch Tijdschrift over Geneeskunde,' under the authority of the Amsterdam Society of Medicine.

PART THIRD.

Original Communications.

Art. I.

Contribution to the Physiology of Saccharine Urine. On the Origin and Destruction of Sugar in the Animal Economy. By George Harley, M.D., F.C.S., of University College, London.

In the summer of 1853 I communicated to the Société de Biologie de Paris the discovery of a new method of producing diabetes artificially in animals, by means of stimulants introduced into the portal circulation. Since that time I have frequently repeated, and even extended, my experiments with similar satisfactory results.

It is generally admitted by physiologists that the various secreting organs in the animal body are stimulated to perform their different functions either by a direct or by a reflex nervous action; and it has been said by Professor Bernard that the normal production of sugar in the liver is dependent upon the latter kind; a stimulus being transmitted by the pneumogastric nerves to the brain, and reflected along the spinal cord and sympathetic nerve to the hepatic organ. M. Bernard was led to this conclusion by finding that immediately after section in the neck of *both* pneumogastric nerves, the liver not only ceased to secrete, but even the sugar contained in the organ at the time of the operation gradually disappeared. The division of only *one* of the pneumogastric nerves produced no visible effect upon the glucogenic function of the liver; and if a sufficient length of time for the re-union of the divided nerve, eight or ten days, were allowed to elapse, a section of the opposite pneumogastric might be made without causing any interruption of the saccharine secretion. He further observed that the application of galvanism to the upper ends of the divided nerves not only re-established the secretion of sugar, but, if the current were continued a sufficient length of time, augmented it beyond the normal amount,—so much so that animals thus operated upon not unfrequently became diabetic. On the other hand, the application of galvanism to the lower ends of the divided nerves was not found to be followed by any such result. These experiments clearly indicated that the nerve-force which excited the liver to secrete saccharine matter did not travel from the brain, through the pneumogastric nerves, to the hepatic organ; but rather that the stimulus proceeded along these nerves to the brain, and was from thence re-transmitted to the liver by some other nervous chain.

The data yielded by other experiments, which it is at present unnecessary to recapitulate, induced M. Bernard to adopt the opinion, that in a healthy animal the reflex action which incites the liver to secrete sugar originates in the stimulus given by the respired air to the pulmonary branches of the pneumogastric nerves. He believes, in fact, that at each act of inspiration the tender filaments of the pneumogastric nerve distributed in the lungs receive from the inhaled gases a stimulus, which is transported through the trunks of these nerves to the brain, and reflected from the nervous centre along the spinal cord and splanchnic nerves to the liver. The point of departure of the normal nerve force which calls into play the glucogenic function of the liver, may at the first glance appear a matter of little moment; but when we consider that the secretions of organs increase in proportion to the amount of stimulus applied to their nerves, and that an excess of secretion which not unfrequently constitutes disease, arises in many cases simply from an exaggeration of the normal stimulus, we shall at once acknowledge the importance of thoroughly understanding the physiological, before attempting to remedy the pathological, condition of any organ. When an answer has been given to the query, "Where is the sugar secreted?" the question next in importance to the physician is most assuredly, "By what means is the secretion excited?" A satisfactory answer to the latter question may not improbably furnish a guide to the successful treatment of a disease which has so long been regarded as ungovernable.

At present a great diversity of opinion seems to exist with regard to the cause of saccharine urine. Some authors speak of it as dependent upon a morbid condition of the liver, others as the result of disease in the nervous system, while a third class still adhere to the old opinion of its arising from disordered digestion. They appear altogether to ignore that one and the same symptom may spring from a multitude of causes, and that as saccharine urine is not of itself the disease, but only the most prominent symptom of a hidden complaint, it too may be the product of a variety of morbid actions quite distinct from each other, and consequently requiring diametrically opposite treatment. If, for example, the normal stimulus of the liver is exaggerated, an abnormal amount of sugar will be secreted; and if the quantity formed is greater than the amount requisite to supply the wants of the system, the excess which then acts towards the organism as a foreign body, will be eliminated with the urine, and the disease, diabetes mellitus, established. If, on the other hand, the stimulus, instead of being exaggerated, is abnormally feeble, a less amount of sugar will be produced by the liver than the wants of the system demand, and a disease which as yet we possess no means of recognising, will be the result. The presence of sugar in the urine does not, however, necessarily prove that the glucogenic function of the hepatic organ has been exaggerated. For even in cases where only the normal amount of saccharine matter has been formed, the sugar in the blood may be present in excess, in consequence of some diseased state of the system preventing its assimilation. In such cases, the sugar will gradually accumulate in the blood, until at last the excess circulating in the body will act as a foreign material, and as such be eliminated by the urine.

Diabetes mellitus may further originate, either in such a change in the structure of the parts which secrete the saccharine matter as will admit of their performing more than the ordinary amount of labour, or in some organic change in the nerves which call the function into action, causing them to over-stimulate the sugar-forming apparatus. There are yet other two causes of diabetes sufficiently important to be here noticed. The first may originate in a foreign stimulus, in addition to the normal one, directly applied to the liver; the second, in such an artificial irritation of the nerves, as will excite them to communicate an excessive stimulus to that organ. A good example of the latter cause is to be found in the experiment performed by Reynoso, who discovered that by making an animal breathe irritating or stimulating vapours, the reflex nervous action might be increased to an extent sufficient to produce an exaggerated secretion of sugar, and to render the animal so operated upon for a time diabetic. This fact has been adduced by Bernard as a strong proof of the correctness of his view with regard to the origin of the normal reflex action. Without wishing to question the fact that irritation of the respiratory organs in animals produces a flow of saccharine urine, it may be remarked that, in making similar experiments, I have not found it so easy to arrive at the same satisfactory results which Bernard seems to have obtained. For example, I caused a robust rabbit to inhale sulphuric ether during seventeen minutes. In twenty minutes afterwards, and again in one hour and a half, the urine was tested without the slightest trace of sugar being detected. To another adult healthy rabbit, in full digestion, I slowly administered chloroform until he became completely insensible,—indeed, much difficulty was experienced in restoring him. Two hours afterwards the urine was tested for sugar, with no better success than in the previous case. I compelled another rabbit to inspire ammoniacal vapours during five minutes, without being able to produce a flow of saccharine urine.

As the ill-success attending these experiments might arise from not giving a sufficiency of the respective stimulants, I administered to other rabbits a very much larger quantity; and in one case, the most satisfactory, I succeeded in detecting a small quantity of sugar in the urine. This result was not, however, attained until after I had rendered the animal five times completely insensible within twelve hours, by a mixture of chloroform and ether.* This success, although it confirms the observation of Reynoso, that the secretion of sugar may be augmented by an irritation applied to the pulmonary branches of the pneumogastric, even taken in connexion with the result of section of the cervical pneumogastric, does not appear to me to justify the conclusion of Bernard—that in the normal state, respiration is the excitor of the glucogenic function of the liver.

Every one knows that in exciting reflex action in the limb of a decapitated frog, it is perfectly immaterial to which branch of the sensory nerve the irritation is applied. There is thus no difficulty in accounting for the liver being excited to secrete sugar when an irritation is applied to the pulmonary branches of the pneumogastric; and we are not necessarily forced to believe that they are the branches which normally call into action the peculiar function of the organ referred to. Indeed, if such be in reality the case, how does it happen that while the respiration, and consequently the stimulus, continue at about the same rate during the entire day, the secretion, which is said to be the result of the stimulus, varies at different times? At one hour it is known to be exceedingly active; at another, a somewhat later one, almost dormant. Such a result has no parallel in any other organ of the body. A certain amount of stimulus, *cæteris paribus*, invariably calls forth a similar and definite amount of action; and upon what grounds are we warranted

* Cases have been reported of sugar appearing in the urine of patients after the administration of chloroform; but as this paper is a mere contribution to the physiology of saccharine urine, I refrain from entering fully into details.

in considering the function of the liver an exception to the general rule? I need scarcely detain the reader at present with further arguments against what appears to me an untenable hypothesis, as I believe the results of the subjoined experiments clearly indicate, that *if* the pneumogastric is the nerve which carries the stimulus to the brain, to be from thence transmitted by the spinal cord and splanchnic nerves to the liver, the point of departure of the stimulus is most probably in the liver itself, and that the cause of the reflex action may originate in the stimulating effect of the portal blood upon the hepatic branches of the pneumogastric nerve. If, for example, the stimulating effect of the blood of the portal vein be imitated as much as possible by injecting into that vessel substances such as alcohol, ether, chloroform, methylated spirit, or ammonia, the liver is excited to secrete an excess of sugar, and the animal operated upon is for a time rendered diabetic. The following experiment illustrates this fact very clearly:

I injected ten cubic centimètres of sulphuric ether, mixed with thirty cubic centimètres of water, into one of the branches of the portal vein* of a full-grown Newfoundland dog, half-an-hour after he had been fed. When he rose up after the operation he appeared intoxicated, and staggered a little as he moved about. This effect, however, soon disappeared, and in a few hours the animal looked as if nothing had been done to him. In two hours after the injection was made I passed a catheter into his bladder, but did not obtain sufficient urine to enable me to satisfy myself whether it contained sugar. Some hours afterwards, when I had obtained enough urine, I found that it readily reduced the copper in Barreswil's liquid, thus indicating the presence of saccharine matter. To assure myself that this effect was not due to the presence of any other substance, I boiled the urine in order to coagulate the albumen, of which it contained a little, then evaporated it almost to dryness, dissolved the residue in boiling alcohol, and filtered. The filtered liquid was next evaporated to drive off the alcohol, and an aqueous solution made. On testing the latter for sugar with the sulphate of copper solution, its presence was clearly indicated. Although by this method the existence of saccharine matter was rendered almost undeniable, I still wished to convince myself of its presence by some other means. The urine which the dog passed the next day was therefore fermented, and carbonic acid gas and a trace of alcohol were obtained, thus placing beyond a doubt the existence of sugar in the urine. In consequence of the dog breaking his chain and escaping, I am unable to state how long he remained diabetic; but he was certainly in that condition forty-eight hours after the injection of the sulphuric ether.

The following case, of which I shall speak very briefly, proves the presence of sugar in the urine until the third day after the operation: A very large dog (the largest I ever saw) was treated in the same way as the preceding one, but he appeared to suffer much more from the operation. His urine was so loaded with bile that I was forced to decolorize it before testing it for sugar with the tartrate of potash and copper, which however it readily reduced. I also fermented the urine, and was able to convince myself of the existence of saccharine matter in it until three days after the injection had been made.

In another case I injected nine cubic centimètres of ether, mixed with thirty cubic centimètres of water, into the portal vein of a small dog. He became insensible, and continued so during a few minutes. Twenty-four hours afterwards he was killed by section of the medulla oblongata, and in his urine the presence of sugar was detected, both by the fermentation and by the copper test.

The following case shows how ammonia has the same power as ether in causing the liver to secrete an abnormal amount of saccharine matter:

Into the portal vein of a good-sized dog, in full digestion, I injected 15 drops of liquor ammoniæ, diluted with forty cubic centimètres of water. In twenty-four hours afterwards, on the animal being killed, his bladder was found enormously dis-

* The experiment is very easily performed by using a sharp-pointed syringe, which can be pushed with facility through the coats of one of the large mesenteric veins.

tended with urine, which not only reduced the copper in the liquid of Barreswil, but fermented most rapidly.*

I have on several occasions repeated the experiment with ammonia, and have not yet met with a single unsuccessful case. Attempts with chloroform, on the other hand, are not invariably successful, as they sometimes result in the death of the animal, especially if the dose be considerable, as the following example proves:

Into the portal vein of a large sheep dog I injected a mixture of three grammes of chloroform, ten cubic centimètres of ether, and fourteen cubic centimètres of water. He died three hours after the operation. Notwithstanding this untoward circumstance, I found that the urine remaining in his bladder after death contained a certain amount of sugar. In operating with chloroform, it is best to use only a few drops, as then the animals seem to suffer but little inconvenience.

In another experiment I injected into the portal vein of a small dog ten cubic centimètres of a liquid composed of equal parts of alcohol and water. Two hours afterwards I examined the urine, and found that it contained sugar, but in small quantity. As I had great difficulty in obtaining the urine of this animal, I ceased making any further observations on him.

Into the portal vein of another dog, of the Skye-terrier breed, I injected ten cubic centimètres of the common methylated spirit, diluted with thirty cubic centimètres of water, six hours after he had eaten a full meal. For a few minutes after the operation he appeared to be intoxicated; but this effect soon disappeared, and on the following day he seemed perfectly well. When he was killed, his bladder was found distended with pale-coloured urine, which contained a considerable amount of sugar, as was seen by the quantity of copper it reduced, and the facility with which it fermented.†

It may be here mentioned that all the dogs so treated vomited after the operation, from the irritation, no doubt, of the pneumogastric nerves.

These experiments are selected from a number of others, which it is quite unnecessary to cite, as the results obtained were identical. From the total of my experiments upon this point, I conclude—firstly, that a flow of saccharine urine can be induced by means of stimulants introduced into the portal circulation, even in animals that have been fasting during twenty-four hours; and secondly, that the introduction of these stimulants sometimes produces albuminuria and an increased discharge of bile, as well as of saccharine urine.

The question now to be considered is, "In what manner do the stimulants act—is it directly, by exciting the tissue of the hepatic organ, or indirectly, through the nervous system?"

The assertion, that an organ like the liver can be excited to perform its function without the intervention of the nervous system, may appear to some as unwarranted. Indeed, were it not that we already know muscles to possess a contractile power altogether independent of nervous influence, I should not have dared to hazard such an opinion. Bernard, however, has clearly demonstrated, on frogs poisoned with wourali, that although the influence of the nervous system can be totally destroyed (as is seen by the muscle not contracting when the nerves are galvanized), galvanism, applied directly to the muscular fibre itself, excites immediate and violent contraction. I have frequently had occasion to repeat this experiment, and am well satisfied of the justness of Bernard's conclusions. In the case of the muscle we have, therefore, indubitable proof that the specific property does not exist in the nerves, but in the muscle itself. And I see no reason for doubting that the various internal organs of the animal body are constructed for the performance of a special and peculiar office, and possess within themselves their specific properties, altogether independently of nervous agency; and I am further of opinion, that when we shall be as able to separate the nerve agency from the

* This and the preceding experiment I had the honour of performing at the College of France, before a commission appointed by the Société de Biologie, and consisting of Profs. Bernard, Robin, and Verdeil.

† Some of the gentlemen who attended my class last summer may perhaps recollect this experiment. It was performed on the dog with an artificial gastric fistula.

internal organs, as we are to part it from the muscles, we shall be equally successful in calling their functions into action, by the direct application of electricity, or any other stimulus, to the tissue of the organs themselves. Professor Bernard seems to take a similar view of the subject, for, speaking of my experiments, he observes that the stimulants may have acted immediately on the tissue of the liver.*

There is certainly another, and apparently a more simple, mode of explaining the influence of the stimulants injected into the portal circulation upon the glucogenic function of the liver, and one which will, moreover, be more readily acceded to, because it does not oppose any of our old views regarding the specific properties of organs or of nerves. The stimulants may act by exciting the hepatic branches of the pneumogastric nerve to transmit an impression to the nervous centre, to be from thence reflected to the liver through the splanchnic nerves, and cause an increased secretion of saccharine matter; and if this be the correct explanation of their mode of action, the normal secretion of sugar is very probably caused by the stimulating effect of the nutritive materials in the portal blood. In those cases where the vena portæ is either accidentally or intentionally obliterated, the nutritive materials absorbed by the mesenteric veins will take a circuitous course towards the liver, and in that case the blood of the hepatic artery will excite the secretion of sugar. The following facts materially strengthen the view of the normal secretion of sugar being the result of a stimulus applied to the hepatic branches of the pneumogastric nerve.

During the time of digestion, the blood of the vena portæ must of necessity prove most stimulating, as it is then loaded with nutritive materials; and this happens to be exactly the period at which the greatest quantity of sugar is formed. On the other hand, the blood of the portal vein of a fasting animal contains very little nutritive material; it is therefore but feebly stimulant, and consequently during this period the secretion of sugar ought to be lessened. This, in fact, is exactly what occurs, for in a fasting animal the secretion of sugar has invariably been found to be at its minimum.

M. Bernard has pointed out that the liver of a dog nourished entirely on fat does not secrete more sugar than if the animal had received no food at all; and this is precisely what might be *à priori* expected if the above theory be correct. For in consequence of the fats, which are scarcely if at all stimulating, being absorbed by the lacteals, and entering the general circulation by the thoracic duct, without passing through the vena portæ and liver, the blood of the portal vein of a dog nourished exclusively on fat does not contain more nutritive material than that of a fasting one.† It cannot, therefore, be more stimulating in the one case than in the other, and consequently the production of sugar ought in both cases to be at the minimum. This agrees perfectly with the facts already cited.

These data show that the foregoing hypothesis of the reflex action, which normally produces the secretion of sugar, is not based on illogical grounds. There exists, indeed, but one argument against the theory—namely, that while section of the pneumogastric nerves in the neck at once arrests the secretion of sugar, division of the same nerves below the point at which they send branches to the lungs is not followed by a similar result. This, too, is the very fact upon which Bernard founds his theory of the reflex action originating in the lungs. And upon a cursory view of the subject, it might be considered equally valid as an objection to the former, and as a commendation to the latter hypothesis. If we examine the point of argument, however, we shall find that the mere fact of the disappearance of sugar from the liver after section of the cervical, and not after division of the thoracic pneumongastrics, is in reality of little value, since it can be readily accounted for on other grounds. The liver ceases to secrete sugar in all

* See Leçons de Physiologie Expérimentale, par M. C. Bernard, vol. i. pp. 345, 347.

† It may be asked, perhaps, "whether the blood of the hepatic artery would not in this case, as in that of obliteration of the portal vein, excite the saccharine secretion?" It is not very probable that it would do so: firstly, because the quantity of fats in the blood is very limited; and, secondly, on account of their feebly stimulating properties.

cases where animals are subjected to severe operations,* whether of the pneumogastric, or of any other nerve. Indeed, whenever a febrile state of the system is set up, the glucogenic function of the liver becomes immediately disturbed; section of the cervical pneumogastrics is moreover one of the severest operations to which an animal can be subjected; it is not, therefore, in the least degree surprising, that the saccharine secretion should be arrested. The same thing occurs after a variety of severe operations on different parts of the body, entirely unconnected with the nerves supplying the liver. Besides this, the slow asphyxia to which animals with divided cervical pneumogastrics are subjected, is sufficient of itself to account for the disappearance of sugar from the liver.†

On the other hand, since division of the pneumogastric nerves below the lungs in general entails neither the death of the animal, nor gives rise to any symptoms of asphyxia, it is not in the least surprising that the glucogenic functions of the liver should in that case suffer but a slight derangement. Another fact in favour of the statement, that the disappearance of the sugar from the liver after section of cervical pneumogastrics is simply dependent upon the severity of the operation, is to be found in the observation that an equal amount of injury done to the pneumogastric below the lungs is followed by a similar result. If, for example, the nerves are ligatured instead of divided, the animals frequently die, and in those cases no sugar is found in the liver. On examining the livers of two dogs, one of which died within sixteen, the other within twenty hours after the ligature of the pneumogastrics at their entrance into the abdomen, I found that the saccharine secretion had been arrested, just as happens when the pneumogastrics are divided in the neck, and probably from an identical cause, the severity of the operation.

Thus it is seen that there really exists no valid objection to the idea of the glucogenic function of the liver being excited by means of a reflex action, originating in the hepatic organ. On the other hand, there is a very important objection to the view of its originating in the lungs. For, as has already been observed, the air entering the lungs must present the same amount of stimulating action throughout the whole day; and yet the result of that supposed action is found to vary at different times.

If, then, we are unprepared to relinquish entirely the idea of the intervention of nerve agency, and to suppose that the portal blood excites the secretion of sugar by a direct stimulating action upon the tissue of the liver, we must, in absence of a better explanation of the fact, adopt the opinion that the glucogenic function in a healthy animal, under ordinary circumstances, is called forth by the stimulating action of the portal blood upon the hepatic branches of the pneumogastric nerves.

Since we now know that stimulants introduced into the portal circulation excite a flow of saccharine urine, we can easily understand how the excessive use of alcoholic drink may produce diabetes mellitus in individuals predisposed to the disease. The same fact explains to us how a disordered digestion is not unfrequently followed by saccharine urine. I may here relate a curious fact in illustration of the truth of the latter remark. About five years ago, at a time when I was much occupied in studying the physiology of diabetes, I regularly tested my urine twice a day, and on one occasion I found it to contain a small quantity of sugar. On the day in question I had partaken freely of asparagus salad; and thinking that this might perhaps be the cause of the presence of sugar, I determined to try the effects of a greater quantity. The following day, the sugar having entirely disappeared from the urine, I again partook of the same salad both in the morning and afternoon. In the evening, on testing the urine, I found very distinct indications of sugar. As the observation was to me one of great interest, I determined to make some experiments on the subject, in order to discover how many hours

* See some interesting remarks upon this point at p. 360 of Bernard's Lectures.

† In proof of this statement I need but quote a single sentence from Bernard's Lectures. He says, "Si encore on asphyxie un animal lentement, les angoisses de l'agonie font encore disparaître le sucre." (vol. i. p. 360.)

this state of saccharine urine would continue. During two days I ate large quantities of the asparagus salad, taking care to have it made as stimulating as possible with vinegar and pepper. The result was far beyond my expectations; for instead of the sugar disappearing from the urine in a few hours after I had ceased partaking of the diet in question, it continued to be secreted during several days, until I at last became very much alarmed, lest the disease had been permanently induced. On the evening of the fourth day the sugar had almost entirely disappeared; but on the fifth it returned in increased quantity—so much so, that a drop of urine falling on the boot left a distinct white spot. I could not account for the recurrence of the disease, as I had been particularly careful in my diet during the two previous days.

I have mentioned this experiment, because it appears to me that if a flow of saccharine urine can be induced in a healthy person, as I consider myself to be, by disordering the digestion and over-exciting the liver, it is very probable that a cause, insignificant in itself, but operating upon a predisposed constitution, might tend to produce the disease. Sugar in the urine has been found after eating cheese and other indigestible substances. It is worthy of remark, that Dr. Jessen, of Dorpat, has rendered horses diabetic by feeding them with hay damaged by moisture. M. Leconte has also found sugar in the urine of dogs after he had administered to them the nitrate of uranium. Several other substances have the same effect, and I have no doubt but that a great number more stimulants will be afterwards found to produce similar results. I cannot refrain from mentioning with what pleasure I perused a communication of M. Bernard's, entitled, On the Influence of Alcohol and Ether on the Secretions of the Digestive Canal, of the Pancreas, and of the Liver, read before the Société de Biologie.* M. Bernard, instead of putting the alcohol and ether, as I had done, directly into the portal vein, introduced them, by means of a long œsophagus tube, into the duodenum of dogs, and allowed them to be absorbed through the walls of the intestine into the portal circulation. The result, as might *à priori* have been anticipated, was identical with what I had previously obtained. M. Bernard, in fact, found that six cubic centimètres of alcohol mixed with an equal amount of water was sufficient to excite the liver to secrete a large quantity of sugar, even while the animal was fasting. With ether employed in a similar manner, he obtained no less successful results. It would be very interesting and important to ascertain if the simple introduction of alcohol into the stomach would produce the same effect. It is possible that in some cases it might fail to do so, on account of its being so acted upon by the gastric juice that it had lost its stimulating properties before it reached the portal circulation. The experiment is, however, one worth making, as in many works on diabetes, drunkards are said to be peculiarly liable to the disease.†

Having thus briefly considered some of the more important facts connected with the exciting cause of the glucogenic function of the liver, I shall conclude with a few remarks on the destruction of the sugar in the animal economy.

In the early part of last year, M. Chauveau communicated to the French Academy a very interesting memoir upon the destruction of sugar in the animal body. His experiments, which seem to have been most carefully executed, were made on the blood drawn from the arteries and corresponding veins of horses, donkeys, and dogs. And from the results which he obtained he concludes—firstly, that the sugar is not destroyed in any appreciable quantity during its passage through the lungs; and, secondly, that a certain amount of saccharine matter disappears in its passage through the capillaries of the general circulation. I have repeated M. Chauveau's experiments on the dog, and have made some others on the cat, and the results obtained are confirmatory of the conclusions arrived at by that gentleman.

The result of the following experiment upon a dog shows that the blood loses

* Gazette Médicale de Paris, Mai 10, 1856.

† Some interesting remarks upon the effects of diet are to be found in Dr. Garrod's Gulstonian Lectures. See British Medical Journal, April 18th, 1857.

very little of its sugar during its passage through the lungs. In order to obtain the blood from the right side of the heart, I followed the method adopted by Messrs. Bernard and Chauveau. The external jugular vein on the right side of the neck was separated for about an inch in extent from the neighbouring tissues, and a ligature placed on the vessel as high up as possible, to prevent the return of the blood from the head through this channel. An opening was then made in the vein immediately below the ligature, and a flexible catheter passed through it down into the right auricle. A portion of venous blood was now withdrawn from the heart by means of a syringe attached to the free end of the catheter. This blood of course contained the saccharine matter which had been poured out by the liver into the inferior vena cava. The blood from the left side of the heart was readily obtained by puncturing the carotic artery.

The manner in which I determined the amount of sugar in these portions of blood was the following: A quantity of distilled water, equal to four times that of the blood, was boiled in a capsule. To the water, when boiling, were added a couple of drops of acetic acid, and afterwards the blood was very gradually introduced. In order that the albumen might be thoroughly coagulated, a drop or two more of the acetic acid was added, care being, however, taken to avoid an excess. When the albumen was completely coagulated, which was known by its separating and floating in the then clear liquid, it was filtered. (I think this is preferable to Bernard's method of decolorizing the blood by means of sulphate of soda, and it is equally applicable when operating on the solid tissues—the liver for example.) The sugar in the clear filtered liquid was calculated by means of Fehling's standard solution of sulphate of copper. The blood from the right side of the heart was found to contain 0·100 per cent. of saccharine matter, while that from the left side of the heart contained 0·085 per cent. The small quantity of sugar in the blood is easily accounted for by the animals having fasted about fifteen hours previous to the withdrawal of the blood. This result shows that very little sugar had been transformed during its passage through the lungs.

In another experiment which I performed on a cat* in an exactly similar manner, I found that the blood of the right side of the heart contained 0·18 per cent. of sugar, and that from the left an exactly similar amount. In order to be certain that I had made no mistake in the determination of the sugar by the volumetric method, I carefully collected the reduced oxide of copper dried, and weighed it. The amount of precipitate from both bloods was identical, thus confirming the result obtained by the volumetric method. I may here mention that Chauveau on one occasion found more sugar in the blood after than before its passage through the lungs. I also obtained a similar result, but was fortunate enough, however, to find that it depended upon the position of the end of the catheter when withdrawing the blood from the right side of the heart. The result, therefore, is of no value, either for or against the theory of the pulmonary destruction of sugar.

Being still occupied with this subject, I refrain at present from entering more into detail, or quoting other experiments in confirmation of the conclusions drawn from the above observations.

It is almost superfluous to state that the results of these experiments fully confirm the conclusion arrived at by M. Chauveau, that the sugar is not destroyed in any appreciable quantity during its passage through the lungs. This indeed appears to me what might have been anticipated when the true nature of the pulmonary function is considered. For what are the lungs? They are not laboratories, like the stomach, but merely an aggregation of little thin sacs, whose function is purely physical; at least in as far as respiration is concerned. The only "vital" offices which they perform are simply those required for their own development and preservation. The mere absorption of oxygen and exhalation of carbonic acid gas would perhaps go on just in the same manner if a piece of goldbeater's skin occupied the place of the lungs. We now smile at Lavoisier's idea regarding the

* I had no means of knowing how long this animal had fasted. On killing him after I had obtained the blood, I found the stomach empty.

absorbed oxygen entering into immediate combination with the free carbon supposed to exist in the lungs for the formation of carbonic acid gas, and perhaps the next generation may with equal right ridicule the present idea of the combustion or fermentation of sugar in the lungs. Formerly it was believed, too, that the bile was burned in the lungs in order to keep up the animal heat; but this theory has been laid aside since the lungs were found to be the least warm of the internal organs. The blood takes but a second or two to pass through the lungs; so that if the saccharine matter is transformed in these organs, the process of transformation must be almost instantaneous. Granting that it is so, why, we ask, does the decomposition of the sugar take place in the lungs alone? We know that the presence of oxygen is not necessary for the transformation; and even if it were, abundance of that gas is to be found in the general circulation. It has been said that the presence of fibrin is necessary for the decomposition of sugar: if it is, are the lungs the only source of fibrin, or is the fibrin circulating in the capillaries of the pulmonary organs different from that in the capillaries of the rest of the body? Chemistry has as yet failed to detect any difference; and as there is plenty of oxygen as well as fibrin in the general circulation, I can see no reason why the transformation of saccharine matter should be entirely performed in one organ of the body. If Bernard is right in saying that the whole sugar is decomposed by the lungs, there must exist in these organs a something possessing the specific property alluded to. Are there any peculiar cells in the tissue of lung which secrete a substance capable of transforming the saccharine matter? I am well aware that Verdeil discovered an acid in the pulmonary tissue to which he gave the name of pneumic acid, and that Bernard imagined it possible that this substance might have the power of transforming the sugar in its passage through the lungs; but this was only a vague hypothesis, without any evidence to support it. Pneumic acid was not known to have the property of decomposing sugar.*

Again, it might be asked, Are there any peculiar cells in the walls of the pulmonary capillaries which have the magical power of transforming the sugar as it is rapidly carried past? Or does the blood, arriving at the lungs for the purpose of arterialization, leave the capillaries to become in any way incorporated with their tissue? No positive answer can be found to either question. Where, then, is a single proof of the destruction of saccharine matter occurring in the respiratory organs? except that Bernard found large quantities of sugar on the right, and very small quantities on the left side of the heart, and has been supported in this view by Dr. Pavy. Chauveau, on the other hand, has shown that carefully-executed experiments give an opposite result; in which opinion the conclusions drawn from my own investigations lead me entirely to coincide.

Having made these few remarks upon the probability or non-probability of the sugar being destroyed in the lungs, we now take a cursory glance at what may be called the opposite side of the question. Does the saccharine matter disappear in the capillaries of the general circulation? This question has been answered in the affirmative by M. Chauveau, who found that blood drawn from a vein contained less sugar than that taken from the corresponding artery. I may here very briefly quote one of my own experiments in confirmation of this statement. I took from the femoral artery of a middle-sized dog, four hours after he had been fed with animal food, one ounce of blood, and from the corresponding vein an exactly similar quantity. On analyzing these bloods, I found the arterial to contain 0·24 per cent., while the venous blood contained only 0·16 per cent. of saccharine matter. This fact seems to be supported by analogy, when the probable uses of sugar in the animal body are taken into consideration. We now know, for example, that all animals, from a very early period of their development in the uterus up to the time of their maturity and decay, have a sugar-generating apparatus in more or less active operation. We know that daily and hourly, so long as the liver remains in a normal condition, it is manufacturing saccharine matter from the ingesta, be they animal or vegetable. We further know that during digestion, when the

*** Dr. Cloetta, of Zurich, has found pneumic acid to be a product of decomposition, and composed of taurine &c.**

blood is loaded with nutritive materials, and when, consequently, the assimilative process is looked upon as being most active, the greatest amount of sugar is thrown into the general circulation. These and similar facts clearly indicate that sugar must play an important part in the nutritive process, and almost force us to believe that, like the other nutritive materials poured into the blood, it furnishes to the different tissues and organs some of the substances necessary for their development and repair. Not only am I disinclined to admit that sugar is supplied to the body for the purpose of keeping up the animal heat, but I am even loth to believe that any individual substance is taken into the system solely for that purpose. On the contrary, the production of animal heat may be regarded as a matter of secondary importance, and simply as the necessary result of the chemical changes which occur during the metamorphosis of the tissues.

If, then, the sugar formed by the liver goes to the support of the system, it is easy to understand how it should disappear from the general circulation during its transit through the minute capillaries of the different tissues. It will, in fact, leave the bloodvessels, to be transformed and incorporated in the animal fabric. On a former occasion I showed by direct experiment how some of the chemical changes by which the nutritive materials are prepared for assimilation constantly take place in the blood;* and it can scarcely be doubted that the saccharine matter takes part in these changes. In this way we are able to account for the disappearance of some sugar from the blood during its transit through the lungs, without being compelled to believe that its transformation depended on any peculiarity in these organs or in their contents. The theory of the disappearance of saccharine substances from the general circulation seems, as far as science has yet advanced, to be equally supported by reason and confirmed by fact; and although future research may cause physiologists to modify this opinion, we can never be blamed for having deduced it from the data which we at present possess.

Art. II.

On the Epithelium of the Air-vesicles of the Human Lung. By C. Radclyffe Hall, M.D., F.R.C.P.E., Physician to the Torquay Hospital for Consumption, &c.

In a series of papers on the pathology of pulmonary tubercle, published in former numbers of this Review,† I endeavoured to show cause for concluding that in such cases of chronic pulmonary consumption as do not originate with actual inflammation, fatty atrophy of the epithelium of the air-vesicles is a condition antecedent to the formation of tubercle. It would, of course, be fatal to my argument if it could be proved that in health there exists no such thing as epithelium at all upon the walls of the air-vesicles.

In the number of this Review for October, 1855, Mr. Rainey enters at some length into the "Critical Examination of the Evidence for and against the presence of Epithelium in the Air-cells of the Human Lung," and arrives at the conclusion which he had previously held, that there is no epithelium in the air-cells. From his acknowledged reputation as an accurate microscopist, any conclusion of Mr. Rainey on such a subject carries considerable weight; and those writers who dispute the existence of the epithelium referred to, do so mainly in deference to his authority.

Before proceeding further, I owe it to Mr. Rainey to apologize to him for an unintended misrepresentation of one of his statements. In my first paper it is

* April and Oct., 1855; and April, 1856.
† See a review of the Chemistry of Respiration in the last October number of this Journal.

stated—"Blood corpuscles, seen through the walls of the capillaries, were indicated by Mr. Rainey as having possibly been mistaken for epithelial cells." What I ought to have written was—"Nuclei seen in the walls of the capillaries," &c.

Mr. Rainey's arguments against the existence of the epithelium may be summed up under the four following heads: (1.) The negation of some good anatomists. (2.) The discrepancies in the descriptions given by those who affirm the existence of an epithelium in the air-vesicles. (3.) The real explanation of the appearances which the affirmers have mistaken for epithelium. (4.) The evidence of comparative anatomy.

The first head, involving merely a question of relative authority, needs no further comment than the remark, that the positive evidence of one trustworthy observer is usually allowed to overrule the negative evidence of many.

Under the second head, Kölliker is referred to as stating that the epithelium of the air-cells is difficult to demonstrate *in situ*, but as making no allusion to the want of distinctness or completeness of its individual cells, but, on the contrary, as actually giving their admeasurement; whilst I, on the other hand, am stated to make no mention of any difficulty in finding this epithelium in the lung, but to describe its individual cells as wanting that distinctness of outline and regularity of form which characterize other epithelia. From this diversity in our descriptions, Mr. Rainey infers that "there is every reason to conclude that the epithelium mentioned by Kölliker is not the same as that described by Dr. C. Radclyffe Hall." Surely this is little more than special pleading; particularly when Dr. Thomas Williams and myself are admitted by Mr. Rainey to have described the same thing, and in the main to agree in our representation with the delineation given by Van der Kolk.

Under the third head, the appearances which Mr. Rainey considers to have been erroneously supposed by myself, and others of higher authority, to represent pavement-epithelium, are referred to (*a.*) "imperfectly developed epithelial cells from the smallest bronchial tubes, which had been detached in the process of manipulating, and had got by accident into the air-cells. This is so common an occurrence, that such corpuscles are generally found in greater or less quantities in these cells; but they have not *the most distant resemblance to pavement-epithelium*, as seen in other parts of the body, *nor to the imaginary hyaline pavement-epithelium*, represented in Van der Kolk's plate, and the greater part of those described by Dr. R. Hall, which in most respects agree with the latter." If they do not resemble what we have described, why suppose we have seen one thing and described another? (*b.*) Nuclei belonging to the walls of the capillaries, both those capillaries which project and *those capillaries which are so blended with the membrane of the air-vesicles as not to present a distinct outline in the uninjected state.* The italics are mine. It must be difficult to decide that the nuclei really belong to the walls of such capillaries as cannot be seen in the uninjected state. Considering the extreme minuteness and delicacy of the air-vesicles, how is it possible to conclude that such nuclei do not belong to the wall of the air-cell rather than to the wall of a capillary which is invisible until the lung has been injected? (*c.*) Oval spaces bounded by meshes of capillaries. (*d.*) The sharp threads of elastic tissue, also, have "a part in producing the confused epithelium-like appearance in the air-cells."

In support of his views, Mr. Rainey depends chiefly upon his examination of *injected* lungs. I have never been able myself to find epithelium in the air-cells of an injected lung, for the obvious reason that I have never yet seen a specimen in which sufficiently high powers of the microscope could be employed for the purpose. In order to examine an object so fine as this epithelium is, the air-cell must be used as a translucent object, and must be very carefully prepared to be distinctive even in that condition.

Mr. Rainey speaks throughout as if one type and size only of pavement-epithelium were possible in the various structures of the body. We are not, however, tied down to any such supposition. If we find fine flattish nucleated plates lining an air-cell in a tesselated fashion, we have a right to designate them a pavement-epithelium, however greatly they may differ in size or regularity of shape from the typical pavement-epithelium elsewhere.

The arguments adduced by Mr. Rainey from comparative anatomy appear to be open to exception. For example, when Mr. Rainey states that in insects many of the tracheæ are so remarkably minute that "there can be *no room* for pavement-epithelium," there seems to be no absolute reason why an epithelium correspondingly minute might not exist. Moreover, if this be an argument against the presence in mammalia of an epithelium in the air-vesicles, it would be a still stronger analogical argument against the existence of the much larger *bronchial* epithelium, which no one disputes. But, as William Pitt is said to have remarked in reference to Butler's great work, "you may prove anything *by analogy!*"

Finding nothing in Mr. Rainey's observations to shake my conviction, founded on long-continued use of the microscope, upon the subject, I requested my friend Dr. Brittan, of Clifton, to read Mr. Rainey's paper carefully, and then to favour me by investigating the question afresh. I now publish the result of his examinations, merely adding one illustration of my own.

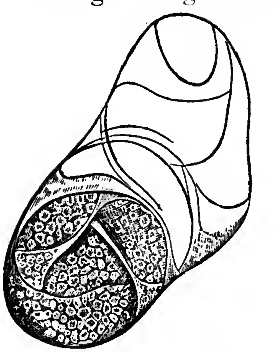


Fig. 32.* Appearances seen in air-vesicle of kitten's lung whilst yet warm and moist, previously inflated ¼ inch; this object became more indistinct in a few minutes, whilst under examination.

In examining the specimen from which the adjoining drawing was taken, the various possible sources of mistake pointed out by Mr. Rainey were severally borne in mind. I think the appearances, which I have here represented as faithfully as I was able—and Mr. Bagg, on his part, has done justice to the drawing—cannot be referred to any of them, or to anything else than the presence of a fine pavement-epithelium. The indistinctness which so soon came on, and to which Dr. Brittan afterwards alludes, was probably due merely to the drying of the specimen whilst under examination.

In assenting to my request, Dr. Brittan† stated that he had hitherto never made the point one of personal investigation, but that, so far as his impressions went, he was inclined to agree with Mr. Rainey. I proceed to quote from Dr. Brittan's subsequent communications, employing his own language, though not intended by him for publication *ipsissimis verbis:*

"1. *In a Toad.*—I did not find much ciliated epithelium; but I satisfied myself of the presence of an epithelium in the pouches, and I thought, over the ridges likewise. There was, however, nothing very clear—nothing one could have drawn."

"2. *A Frog.*—Ciliated epithelium very evident and plentiful; epithelium everywhere evident."

"3. *Sheep.*—There were in this specimen a profusion of small entozoa, and the bit of lung seemed much altered, as if from small effusions of fibrin, much like what you describe. I convinced myself of the presence of epithelium, however, after examining many portions."

"4. *Ox.*—A bit of fresh lung from the ox settled the point for me. When I had properly prepared the specimen, the epithelium was as plain to me as it could be; and I am satisfied none of the errors ascribed by Mr. Rainey applied here. The appearance of the cells is not analogous to that of the nuclei in the capillaries; they are to be seen all over the walls of the air-vesicles, sometimes in a continuous layer; whilst close by will be a portion of the wall without these cells, or with only a few scattered here and there still adherent. I cannot, therefore, be misled by the appearance of the fibres of the pulmonary tissue. It certainly was not

* The figures are numbered in continuation of those in former papers in this Review.

† Physician to the Bristol Royal Infirmary, and Lecturer on the Practice of Medicine at the Bristol Medical School; formerly Lecturer on Anatomy and Physiology.

bronchial epithelium, for I carefully removed a portion of the latter for comparison. I have no hesitation in saying that in this lung the vesicles were lined by epithelium. I send you a slight drawing of this specimen, in two aspects,—one focussed on the top, the other a little deeper."

1

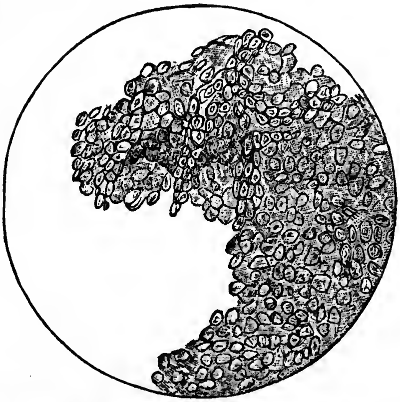
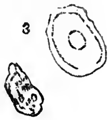


Fig. 83. From the lungs of an ox (Dr. B.). No. 1 is the same specimen as No. 2, but in different focus, showing prominently the edges of the air-vesicles.

2

Fig. 83, 2. From the lungs of an ox. No 2 is focussed down to show the sides or floor of air-vesicles.

3



No. 3. Cells separately drawn from No. 2: these were drawn as they appeared seen by the 1-12th.

"5. *Sheep.*—In this specimen of sheep's lung I found the epithelium as plain as plain could be. Two or three flakes of it lying at the edge of the specimen, afforded beautiful specimens of a single layer of tesselated epithelium."

"6. *Kitten.*—I examined the lung of a kitten quite fresh, and found the same, nor did I observe the change (of early becoming indistinct) to which you refer. In fact, the specimen shows almost as well now it is put up in spirit and water as ever it did."

"7. *Human Lung.*—This specimen was not sound, being taken from an emphysematous lung from a patient who died this morning. The epithelium seems, as described, much less firm and more 'hyaline' than in any of the other specimens which I have examined. On putting up a specimen, I find only scattered cells in great quantity all round in the fluid, sometimes six or eight or more together, and a few still adhering; but on the surface of the specimen they are plainly distinguishable. I have not the slightest doubt about them, nor had Etheridge, who happened to come in; but there was nothing evident enough to be worth drawing.

Of course, one would hardly expect the same condition in the lung of a patient thus diseased, as in that of an animal killed in health and vigour."

"8. *Human Lung.*—The specimens I examined from this were decisive to me. I compared them with a specimen of bronchial epithelium, and also with a bit of peritoneum with its epithelial coat still on, and I really cannot see how any one who has worked up the subject by making specimens for himself can entertain a doubt. Considering the scepticism expressed in my first letter to you, the certainty I now express is pretty strong, but so much the more trustworthy; and I am really obliged to you for having made me take up the subject and satisfy myself. I ought to have mentioned to you that I have remarked in the epithelium in question that the nucleus appears to be always evident, but the cell-wall less clearly marked, so that you often see contiguous nuclei without being able to trace the junction of the cell-walls; the cell-walls are best seen in the detached cells that float off at the edge of the specimen."

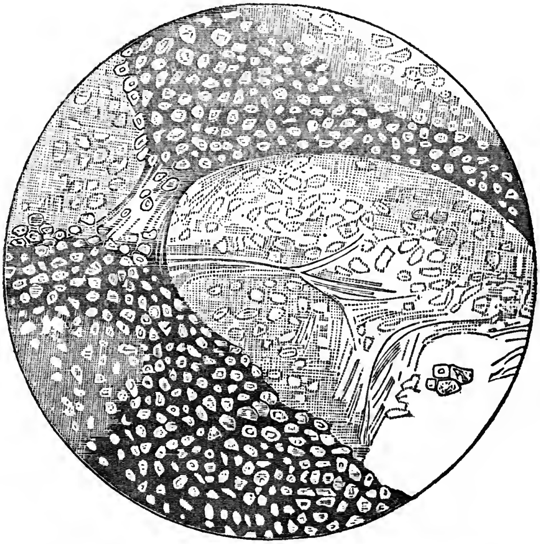


Fig. 34. Epithelium of air-vesicles in human lung. (Dr. B.)

a. Red globule, flat, and in profile. b. Bronchial epithelium.

"The lung of the bird I have not yet examined, for I could not obtain a bird fresh enough. However, having found the epithelium in human lung, I do not see what you want with the bird's lung."

Dr. Brittan brought the subject under the notice of the Bristol Microscopical Society—a society which includes all the leading medical men of Bristol and Clifton, and writes thus:

"As you know, it is impossible to show much to twenty men with six microscopes, especially when such minute matters are in question. I believe, however, that many are quite satisfied, and some I have promised to convince at leisure. I need not enter into further detail, but I repeat that I am assured of what I before stated—namely, that there is an epithelial lining to the air-vesicles."

To this unprejudiced testimony of Dr. Brittan, I can, of course, add nothing. I think it will be conceded that I could not have adopted a fairer mode of attempting to settle the point in discussion. The reader has my drawings and those of Dr. Brittan before him, and can judge for himself whether the appearances (assuming the representations to be tolerably faithful) are or are not characteristic of a minute tesselated epithelium.

ART. III.

The Mechanism of the Joints of the Sacrum. By CHARLES H. MOORE, Surgeon to the Middlesex Hospital, Lecturer on Surgical Anatomy.

THE mechanical relations of the sacrum can only be entirely understood by studying all its connexions with adjoining bones. The shape of its articulating surfaces exhibits the directions in which it can move, the ligaments show from what movements it is restrained; but the connexions of the bone are so complicated, and its stability depends on circumstances so unique, that confusion still appears to rest on the minds of some anatomists with regard to it. Dr. Matthews Duncan has recently thrown much light on the subject,* though his views appear not unmixed with error. Indeed it is impossible that the anatomy of this part of the body can be completely understood, so long as the precise form and direction of the articulating surfaces, and the use of the ilio-lumbar ligaments, are disregarded or mistaken, and so long as the assertion of Cruveilhier is accepted, that the sacro-sciatic ligaments are termed ligaments "rather on account of their fasciculated shape than from their use, which scarcely has reference to the union of the bones of the pelvis."†

The inclination of the sacrum backward beneath the spinal column, causes it to present its upper articulating surface to the body of the fifth lumbar vertebra on an inclined plane downward and forward. In the erect position of the body, therefore, that vertebra tends to slide upon the sacrum; and it does so slide until its articulating processes meet those of the latter bone. The sacrum then yields to the weight, and its promontory is depressed.

With regard to the ossa innominata, the sacrum is so placed that most of the surface by which it meets the last vertebra, lies in front of its lateral articulations with the ilia, and that two-fifths of the whole length of the bone project downward and backward, below and behind the sacro-iliac synchondroses.

Such being the position of the sacrum with regard to the other bones, it must be liable to two kinds of displacement. On the one hand, the innominate bones separating from one another, it might slip bodily down between them; and on the other hand, supposing it to be secured from falling between those bones, and yet articulated to them, it might yield so much to the weight of the vertebræ as to suffer an extreme degree of inclination, and thus become an insecure basis for sustaining the spinal column. Either, then, the ligaments of the sacrum, or the form of its lateral articular surfaces, must be adapted to resist those displacements.

The wavy auricular surfaces of most sacra, whilst unfitted for extensive motion, are yet adapted to permit a certain extent of the very displacements to which the position of the bone exposes it. Narrower from side to side at its pelvic than on its spinal surface, the sacrum is liable to drop into the pelvis. It could not, however, readily do so from the inequalities of its lateral articular surfaces. Again, in the direction of movement by which the sacrum would be too much inclined, there is a curved groove, fitted by a corresponding ridge on the ilium, and extending the whole length of the articular surface, first directly downward, and then obliquely backward and downward. By the anterior part of this groove, which is in the line of the centre of gravity, the promontory may descend verti-

* Edinburgh Monthly Journal, August and September, 1855.
† Cruveilhier's Descriptive Anatomy: Library of Medicine, vol. vii. p. 202.

cally; by the posterior part, the coccygeal end of the bone may be projected backward and slightly upward. The whole movement is one by which the inclination of the sacrum is increased, as if the bone turned upon a pivot passing transversely through it and both ilia. The imaginary pivot would be situated above and a little behind the articulation. The principal convex portions of the sacral surfaces are a slight and short elevation of the posterior edge behind the lower part of the curved groove, and a more considerable projection of the anterior angle. The former is not a constant ridge, but when it exists, would permit the descent of the sacrum. The anterior first favours, and then resists, the inclination and depression of the bone.

All sacra are not formed precisely as has been described. In some, the pivot actually exists within the area of the joint; in others, the concavity is on the ilium instead of the sacrum. But the movements of which they are all capable will be found to vary little from those of the most common and, as it might be called, the typical shape, which is that above described.

The security of the articulations, then, depends not on the bones, but on the ligaments; and these are so situated as to be only brought into use by the displacement of the sacrum already described. It is the liability to displacement which gives the bone its greatest stability.

From the tip of either lateral costiform process of the fifth lumbar vertebra, the ilio-lumbar ligament, exceedingly strong, and about an inch in length, passes outward, backward, and a little upward, to the crest of the ilium. Its attachment to the crest is in the vertical line above the sacro-iliac synchondrosis, and marks the anterior limit of that strong beam of bone of which the hinder part of the ilium is composed, and which, under the name of cotylo-sacral beam, reaches from the posterior tuberosities of the ilium to the acetabulum, and, beyond that, to the tuber ischii.

Scarcely any ligament exists on the anterior and inferior aspect of the synchondrosis, but behind and above it the back of the sacrum is bound to the overhanging projection of the ilia by short, numerous, and exceedingly strong vertical fibres (superior sacro-iliac).

From the posterior superior spine of the ilium, a flat and strong ligament descends vertically behind the lowest part of the synchondrosis, and is attached to the tubercles on the third and fourth vertebral segments of the sacrum (posterior sacro-iliac).

Lastly, the greater and less sacro-sciatic, originating in the ischium, and attached to the sacrum, cross from bone to bone, inward, backward, and upward, at a considerable distance below the level of the sacro-iliac joint. The shorter ligament, attached near the lower end of the sacrum, passes to the spine of the ischium almost horizontally. The use of these ligaments is as follows:

The sacrum is slung between the ilia by the short or superior sacro-iliac ligaments; but those ligaments are so placed as not to interfere with that movement of the bone, as it were upon a pivot, which its auricular surfaces are adapted to permit. The weight received on the sacrum from the spine is by these ligaments transferred to the posterior overhanging tuberosities of the ilium, from which it descends by the strong cotylo-sacral beams to the acetabula in walking, and in sitting to the tubera ischii. These ligaments answer to the pivot upon which the sacrum might be imagined to turn.

When the body of the fifth lumbar vertebra, by which the weight of the trunk reaches the sacrum, slides forward over or with the upper surface of that bone, the ilio-lumbar ligaments are tightened, and the ilia are pulled toward this vertebra and one another. But the ilia, in closing together, jam the sacrum between them, and fix it by lateral pressure with a security proportioned to the weight of the last vertebra and all it bears.

Whilst this movement is taking place at the upper part of the articulation, others may occur below. The coccygeal end of the sacrum may be tilted upward and backward and in the opposite direction to the depression of the promontory, or the whole sacrum may press downward and backward in the direction of its length. The latter displacement is stopped by the vertical or posterior sacro-iliac ligaments; but so far as it does occur, it necessarily tends to separate the ilia, and consequently to tighten the ilio-lumbar ligaments; these, as we have seen, draw the crests of the ilia together.

Still, as the ilio-lumbar ligaments secure the approximation of only the upper parts of the innominata, might not the depression of the wedge-like sacrum separate the innominata below? Provision is made against such a separation, and at the same time against too great an elevation, of the coccygeal end of the sacrum, by the sacro-sciatic ligaments. Far from being useless in the mechanism of the pelvis, as Cruveilhier avers, they are most important parts of that mechanism, concurring with the ilio-lumbar ligaments to jam the sacrum between the ossa innominata, and at the same time, in the reverse direction, restraining the tilting of the sacrum. The same weight which depresses the fifth lumbar vertebra and the promontory, and tightens the ilio-lumbar ligaments, also elevates the coccyx and tightens the sacro-sciatic ligaments. The greater the weight, therefore, the tighter will be the ilio-lumbar and sacro-sciatic ligaments, and consequently the more securely will the sacrum be fixed. Moreover, it will be seen that the sacro-sciatic ligaments, by the same mechanism, become important ligaments of the spine, since they support the column in its last curvature, and render the sacrum a firm basis upon which the vertebral bodies may be safely piled.

The foregoing explanations relate to the mechanism of the sacrum only as it is a part of the spinal column, and in its connexion with both the innominate bones at once. It remains to consider the mode in which the weight of the trunk is conveyed from the top of the sacrum to either femur singly. It might be the case that the pelvic circle of bones would act as a whole, and bear in all its parts the weight which arrives only on the sacrum. In that case the weight would be divided between the two innominata, and the whole mechanism already described would come into action equally when we stand on one leg or on both. But it is probable that only the hinder and stronger portion of the pelvis is really concerned in progression. A boy who had been crushed between a dray and a wall, walked into the surgery of the Middlesex Hospital scarcely lame; yet after death he was found to have had the pubes and ischial rami of both sides broken off from the back of the pelvis. A specimen is preserved in the museum of the same hospital, in which also the anterior part of the pelvis is broken off from the posterior, yet the man had walked with only a moderate limp. The weight in such cases must have descended from the vertebræ to either femur through the ilium of its own side alone. Dr. Matthews Duncan has satisfactorily shown that the weight in descending thus to one side does not pass through the sacro-iliac synchondrosis. It is conveyed by the superior sacro-iliac ligaments to the tuberosities of the ilium, which overhang the back of the sacrum, and then descends to the acetabulum through the thick beam of bone which composes the posterior part of the ilium.

A misapprehension as to the direction of this beam—the cotylo-sacral beam—of bone, has led Dr. Duncan into what I conceive to be an error with regard to the development of the pelvis, and the cause of a part of the deformity in Nägele's oblique pelvis. He describes this beam as curved outwards between its upper extremity, at which the superior sacro-iliac ligaments pull upon it, and its lower end, which rests on the head of the femur; and he attributes its curvature to the outward pressure of the sacrum between these two points—namely, at the sacro-iliac synchondrosis. But it is surely erroneous to say that bones depend upon pressure for their regular development. Undue pressure may deform them, but not that which they are made to resist: had they no power of growing into a definite shape, and maintaining it, they would be incapable of bearing the very forces to which they are ordinarily subject. Moreover, Dr. Duncan's explanation is misapplied. The cotylo-sacral beams are not curved at all. From their extremities at the tuberosities of the ilium to the acetabulum, although not vertical, they are straight. They have an appearance of curvature from their forming a part of the curved brim of the pelvis. But, in fact, they spread out below into the higher part of the broad acetabula, and acquire, as they spread, a concavity on their external as well as on their internal side, not a convexity, as would be the case if they were curved in the manner described by Dr. Duncan.

The converse of this principle is, with equal error, applied by the same author in the explanation of the deformity of the oblique pelvis described by Nägele. The sacro-iliac synchondrosis is obliterated in that form of pelvis, the innominatum is considerably bent between the anchylosed part and the acetabulum, whilst from the bent portion to the symphysis pubis the bone is not curved, as in the brim of the natural pelvis, but straight. Dr. Duncan attributes the want of curvature of the linea arcuata to the want of the outward pressure of the sacrum at the points of its attachment against the innominatum. It is indeed true that the sacrum no longer exerts an outward pressure against the cotylo-sacral beam, for as the joint is anchylosed, the ligaments and the posterior iliac tuberosities are useless; but it is not correct to describe the innominatum as straight. In that part which should be straight, according to the reasoning of Dr. Duncan—viz., the cotylo-sacral beam—the bone is in fact unnaturally curved, and the straight part is the anterior portion of the brim, which could not be affected by any lateral pressure of the sacrum. The theory of the effects of outward pressure therefore falls to the ground. First, its alleged effects cannot reasonably be expected, and next, the very opposite take place. For where the joint and the pressure exist, the cotylo-sacral beam is straight, and where neither exists, that beam is curved.

The deformity of the brim in Nägele's pelvis probably occurs in the following manner. The anchylosis renders useless all the articular ligaments and the posterior iliac tuberosities. The weight of the vertebræ, therefore, no longer passes from the sacrum to the femur by the superior sacro-iliac ligaments and the cotylo-sacral beam, but directly through the anchylosed joint; that is to say, from the upper to the lower extremity of a single and much-curved bone. Such a bone necessarily bends still more, and the increase of its curvature occurs about its middle, which is just external to the anchylosed joint. The only flattening which takes place is between the abrupt curvature of the cotylo-sacral beam and the pubes, and is due to the pressure of the bone itself against the top of the femur.

Art. IV.

On Secondary Deposits and Mortification from Disease of the Arteries. By Henry Lee, Surgeon to King's College Hospital, and to the Lock Hospital.

As in some of the lower animals it may be shown that different segments possess independent vitality, so in the higher may it be as clearly demonstrated that every part has a life of its own; and that this life is capable of being maintained for a time independent of the rest of the system with which it is naturally associated. A portion of blood, for instance, may be taken from an animal, and after remaining separate for a short time, may be again infused, so as to remain part of the living organism. Teeth, it is well known, have been transplanted in a similar way, as have also other parts of animal bodies. Such experiments clearly prove that although incapable of continued independent existence, each part may live, when separated from the rest, for a given time. Union implies reciprocal action; a dead part cannot unite with a living part. A tooth, therefore, which in its new relations shall adhere and grow, must have maintained its life when separated from its original bed; or a testicle removed from one animal, and placed in the peritoneal cavity of another, must in like manner, before it could have formed new adhesions and relations, have maintained for a time its own independent life. Mutual interchange of elements is essential to the continuance of existence. If a part cease either to receive, for any length of time, supplies from other parts, or to give off from the body, or back into the system, those elements which are redundant, it dies. The means by which this mutual interchange is effected in all the higher animals, is the circulation of the blood. The circulation, then, may be regarded as diffusing life through animal bodies; and the flowing blood may be looked upon as the life-sustaining stream of the various organs of which those bodies are composed. But as the bloodvessels are the means of conveying a life-diffusing energy to every part of the body, so may they be the means of carrying deadly influences. The stream which naturally is intended to support life, may itself contain the elements of death. The fatal influence may spread from the central and more vital organs to distant parts, or it may be communicated from any remote part to the centre, and thence again to the whole system.

The causes which induce separation, stagnation, and ultimate death of portions of the blood while in the living vessels, may therefore, as far as these affections present themselves to the surgeon, be divided into three classes—namely, 1st. Those which depend upon morbid deposit in the coats of the vessels, and subsequent discharge of that deposit into the current of the blood. 2nd. Those which originate in a division of all the coats of a vessel, either from injury or ulceration, and the admission of diseased secretion from surrounding parts. 3rd. Those which originate in some morbid action in the blood itself. In the first of these divisions may be included diseases affecting principally the arteries, and in which the *materies morbi* is carried from the centre of the circulation towards the circumference. In the second may be considered diseases of the veins, in which the morbid products are, on the contrary, carried, in the first instance, from the more distant parts of the body towards the more central and vital organs. The third division is alike common to arteries and veins, and to the structure of organs which intervene between their terminal branches.

Magendie,* from the experiments which he performed, was induced to believe that the fluids introduced into the arteries of animals returned quickly through the corresponding veins, and that this occurred even more rapidly in the living than in the dead body. The experiments of M. Gaspard, however, show that while some fluids pass readily from the arterial to the venous system, others do so with much difficulty or only after the lapse of a certain time; and it is probable that there may be others, again, which cannot do so at all without previously undergoing certain changes. Some clear fluids, such as solution of tartar emetic, of opium, and of nux vomica, when introduced into an artery, pass readily in the course of the circulation, and produce their full effect upon the constitution. In such cases, where the passage of the morbid matter is not opposed, little or no irritation is manifested in the organs through which they first pass. The first of the above-named poisons produces vomiting and purging, the second stupor, and the third tetanic rigidity, when injected into an artery, exactly in the same manner as if introduced into the stomach, or injected into a vein; but no great pain or loss of power is experienced in the limb upon which the experiment is made. On the other hand, M. Gaspard found that when introduced into an artery, the infusion of tobacco neither produced vomiting nor purging; the solution of acetate of lead did not act upon the intestines, and putrid fluids did not produce those copious alvine evacuations which commonly follow the introduction of the same fluids into the serous cavity, or into the veins. The injection of acetate of lead, however, produced the signs of local inflammation in those parts to which the artery was distributed; the muscles which it supplied became of the reddish-black colour, and the whole limb had the appearance of having been affected with gangrenous inflammation. The introduction of tartar emetic, in the same way, produced slight local symptoms, but was followed by diarrhœa and fatal vomiting. Experiments like these clearly indicate that some extraneous substances, when introduced into the blood, expend their action by producing a local, although perhaps most severe disease; while others which circulate more freely with the blood, leave no trace of irritation in the first system of vessels through which they

* Précis Elémentaire de Physiologie, tom. ii. p. 389.

pass, but expend their influence upon some part to which their action is peculiarly determined, or produce a constitutional affection.

Changes of various kinds are found after death in the coats of arteries. These consist chiefly of steatomatous or atheromatous deposit, and of cartilaginous or bony thickening. These changes consist essentially in an alteration of the nutrition of the part, and may give rise to inflammation, as a secondary effect, in the coats of the arteries.* Two principal forms of atheroma may be distinguished; one consists of a gelatinous deposit on the internal surface of the vessel, the other of yellow spots either on the surface of the internal membrane, or in its substance. The first is originally semi-transparent, grey, or reddish. It becomes subsequently opaque and white, and connected with the lining membrane. The structure of this deposit is generally amorphous and fibroid. After a time, these patches become drier, more opaque, and may assume a reticular appearance. The second form of atheromatous deposit is by far the most common. It consists of irregular patches deposited in the substance of the internal membrane or beneath it. These patches by degrees become thicker, and may extend to the middle coat, the circular fibres of which may become softened and more or less altered in structure. Subsequently these spots may become either hardened or softened, and these conditions may co-exist, at the same time that there is the gelatinous deposit on the surface of the lining membrane. The pultaceous softening of the atheromatous disease commences in a small spot, and raises the internal membrane in a pustular form. This appearance has often been mistaken for an abscess. Under the microscope this softened matter is found of a yellowish-white colour, containing molecular granules, vesicles, or granules of fat, crystals of cholesterine, calcareous granules, and *débris* of the elements of the middle coat. The internal membrane, instead of being raised in a pustular form, is often eroded and worn away. A crack is thus first formed, then an erosion, and finally an ulcer of greater or less depth. The atheromatous ulcers present, besides the elements above mentioned, little coagula of blood deposited on their surface. Of these the only traces that often remain are the brown or blackish stains which they leave when removed by the current of the blood.

The edges of the atheromatous ulcer are flat, uneven, thin, detached, occasionally undermined by the blood. The base is uneven, of varying depth, extending sometimes to the outer coat, which then becomes thickened. This thickening is accompanied by a certain degree of congestion.

When the morbid product is firmly adherent to the coats of the vessel, a deposit of fibrin from the blood may take place, which will then temporarily unite the torn edges of the divided lining membrane, and cover the ulcerated surface. But if the atheromatous disease in the artery has undergone any great degree of softening at the time the internal coat gives way, then its more fluid parts become necessarily mingled with the blood, and carried to a greater or less distance in the course of the circulation. The same general effects may then be produced, whether these morbid deposits be poured into the circulation as now described, whether similar morbid matters be generated in the blood itself, or whether they be brought from some distant part of the vascular system. There is this important local difference, however, that when any morbid matter is generated in an artery, it will necessarily expend its first and principal influence upon the parts to which that artery is distributed. The remainder of the system may or may not then be directly affected.

Disease of the arteries has very generally been considered as a cause of mortification, and the diseased conditions have by most authors been attributed to inflammation of the arterial coats. Earthy concretions, for instance, in the arteries of the lower extremities, since first noticed by Cowper and Naish, have been regarded as the cause of the closure of the arterial canals, and of the consequent mortification of the extremities. But, as is observed by Mr. Hodgson, "our know-

* The description of the morbid changes which the coats of arteries undergo is taken from Lebert's Anatomie Pathologique, pp, 512–13.

ledge of the power of the collateral circulation in every part of the body will not allow us to admit the obliteration of the trunks as a sufficient cause of mortification from a deficient supply of blood." Some other cause of this kind of mortification had then to be sought, and a sufficient one was thought by some writers to have been found in the theory, that if the large arteries were diseased, the small ones would be so also; while others have supposed that the want of elasticity and organic power would interfere with the due supply of blood, in a degree sufficient to account for the effects observed in gangrene. Both these suppositions are entirely devoid of proof; for although in cases of dry gangrene the arteries are often found to contain bony deposits, and the smaller arteries are also sometimes found diseased, yet are these deposits not found in those situations in which the gangrene actually takes place,—for instance, in the pulpy vascular extremities of the toes, in the lips, cheeks, or lobes of the ears. In the arteries of the size here found is there never any bony deposit detected. That gangrene is not necessarily connected with disease of the arteries, is proved by the fact that it often occurs when no such disease is suspected. And, on the other hand, M. Cruveilhier has shown* that an artery which supplies a limb may be obstructed for a very considerable distance without any mortification following. If, however, an irritating injection be thrown into such an artery, mortification may readily be produced. It is remarkable that secondary mortification seldom attacks those structures which have a scanty supply of arterial blood, but that it almost invariably shows itself first in those parts which have the largest supply of bloodvessels. The vascular cheeks and lips in children, the vascular extremities of the toes in those in advanced age, are parts very frequently affected. If a simple deficient supply of blood were the cause of mortification, in such cases we should expect to find it first developing itself in the tendons and ligaments, and we should anticipate that the cellular tissue would perish always before the skin. The reverse of all this in secondary forms of mortification ordinarily occurs. The parts in which mortification in general shows itself have in truth a wonderful power of sustaining their life with a very small supply of arterial blood, and for a short time even when separated from the body. This fact is amply illustrated by the way in which portions of skin may be actually severed from the fingers and reunited, or by the length of time that the flap of skin taken from the forehead will maintain its life when converted into an artificial nose. In a case recently under the author's care, a flap of skin was taken from the side of the chest and placed across the neck in the centre of the cicatrix of a burn. It appeared to unite favourably in its new position, when the patient suddenly got into very bad health. The old cicatrix ulcerated, and left the portion of skin newly introduced attached by a small base, peninsulated. This portion of skin, however, remained unaffected by the ulceration, and ultimately again re-united to the skin of the old cicatrix. In such cases, the arterial impulse through the transplanted portions of skin must be very slight; and in the face of such well-known facts, the want of arterial impulse can hardly be seriously maintained as a sufficient cause in itself for mortification. When a portion of skin is frozen, it turns white; and when great heat is suddenly applied, it does the same. Strong nitric acid placed on the surface of the skin or of the mucous membrane, will leave the centre of the slough which results of a dirty white colour. In these instances, it is reasonable to imagine that there may be an actual want of blood in the affected part. In some cases of spontaneous gangrene, the same may in all probability happen. But in these latter cases, which are comparatively very few in number, some unusual obstruction to the transmission of the blood through the arteries has generally been manifest; and it may fairly be presumed, from the want of pulsation in the arteries, and from the coldness oi the affected part, and other symptoms, that in these cases of mortification a deficient supply of arterial blood was a principal cause of the mortification.

In the vast majority of instances the parts affected with gangrene afford the strongest contrast to such appearances. The mortified part is commonly of a

* Anatomie Pathologique, vol. ii. p. 301.

deep livid colour, evidently surcharged with blood, which it has not the power to propel. This is equally true, whether there be a mechanical cause of obstruction to the return of the blood, or whether no such apparent cause exist. In either case, the deep livid congestion precedes mortification—a condition quite compatible with obstructed circulation in the affected part, but utterly inconsistent with a deficient supply of blood. But, it will be asked, do not the cases in which a ligature has been applied to an artery in cases of aneurism, show that obstruction to the flow of blood through a large artery is often followed by mortification in the limb which it supplies? To this it must be replied, that it is a remarkable fact that mortification, in such cases, has only occurred when the ligature has been placed between the aneurism and the heart, and where, consequently, any softened fibrin or other morbid products which the aneurismal sac may have contained, have had an opportunity of becoming mixed with the blood of the limb upon the distal side of the ligature, and of producing their effects upon the nutrition of the parts there situated. Upon this subject we need only observe at present, that in thirty-three cases of aneurism collected by Paul Broca,* where the ligature was placed on the distal side of the sac, no instance of mortification occurred.

Inflammation of the Arteries has, by nearly all the ablest writers, past and present, been assigned as one of the causes of mortification; and the way in which this result is produced is by them ascribed very generally to the blocking up of the arterial tubes by inflammatory effusion from their lining membrane. Tiedemann, Gendrin, Hasse, Hodgson, Travers, and many other celebrated names, may be quoted as holding the doctrine of the obliteration of arteries by inflammatory effusion of lymph upon their lining membrane. "If an irritating injection," says M. Gendrin, " be thrown into an artery included between two ligatures, the inflammation which follows is characterized by the formation of a plastic layer, which ultimately fills up the cavity of the vessel."† This opinion was currently received, and the prevalent notion for many years was, as expressed in Mr. Travers' most valuable treatise 'On Inflammation,' that these fibrinous layers were " secreted by the capillary vessels under inflammation."

Having reason to doubt the correctness of this theory, the author entered upon some researches which led him to the conclusion, that while the lining membrane of the arteries remains entire, no inflammatory effusion of lymph can take place upon its free surface. The following experiments will illustrate this point: A dog was placed under the influence of chloroform, and the carotid artery exposed low down in the neck. A ligature was then placed upon it, and the vessel was opened on the distal side of the ligature. Several small portions of cotton wadding (as large as could be introduced into the vessel) were then saturated with a solution of sulphate of zinc, and propelled, by means of a probe, along the artery towards its distal extremity. A portion of the artery was thus left obstructed, on the one hand by the ligature, and on the other by the plugs of cotton wadding. Little blood would, it was thought, under these circumstances, find its way into the vessel. The experiment was performed on September 4th, and the animal was killed and the parts examined on the 8th. The internal and middle coats had been divided at the points where the ligature had been applied. The outer cellular coat and the surrounding parts were here much inflamed, and a large abscess had formed in the immediate neighbourhood. At the part of the vessel in which the cotton wadding had lodged, the coats of the vessel had also given way, and an abscess had formed in the surrounding tissues. The intermediate portion of the artery, over which the saturated cotton wadding had passed, had become considerably retracted, so as to occupy perhaps not more than half its natural length. The lining membrane in this portion of the artery was entire, and of its natural colour. On its surface was a very small coagulum of blood, and at another point a very slight thickening or elevation of the lining membrane. With these exceptions, the inner surface of the vessel presented its

* Des Anévrysmes et de leur Traitement. 1856.
† Histoire Anatomique des Inflammations. Paris, 1826.

natural appearance. The contact of the cotton wadding soaked in the solution of sulphate of zinc had therefore not produced any appearance of inflammation, or of effusion, in those parts over which it had passed, but where it had not been allowed to lodge.

In like manner the left jugular vein of a donkey was exposed in two parts of its course, and two ligatures placed upon it at something less than four inches interval. After the blood had been removed, the cavity of the vein was filled with cotton wadding. The wounds in the vein and in the skin were then carefully closed with sutures. The animal was killed at the expiration of forty-four hours. The lining membrane of the vein in the space included between the two ligatures was of rather a deeper colour than natural, but in no point did it present any trace of effused lymph. The outer coat of the vein was thickened and inflamed, and suppuration had commenced in its outer cellular connexions.

The conclusions arrived at from the author's experiments and observations agree with those published by Virchow—viz., that chemical and mechanical irritants, when applied to bloodvessels, produce their effects only, as far as any inflammatory exudation is concerned, on the external and middle coats of arteries, or the outer coats of veins. That the epithelial and fibrous longitudinal coats of bloodvessels may become detached, and when once detached, a plastic layer from the outer coats may be poured into their cavities.

In thirteen experiments performed by Virchow, in which the lining membrane of arteries was irritated by various means, in none was there any plastic exudation from the surface of the lining membrane. Mortification of the lining membrane may be produced, but it necessitates the coagulation of the blood in the vessel to the extent of the lesion, and may be subsequently followed by effusion from the outer coats into the vascular canal.

Although effusion of lymph, as the result of inflammation, can be with such difficulty produced in the cavities of arteries, yet is there hardly any artery or vein in the human body that has not been found narrowed, closed, and impervious to blood. Professor Tiedemann, in his work on 'The Closure of Arteries in Disease,' has collected together a great number of instances, and these he refers to four heads—viz., 1st. To inflammation of the inner smooth coat of arteries; 2nd. To growth and morbid excrescences of the inner coat; 3rd. To deposits of solid or earthy concretions, or purulent matter between the coats; and 4th. To clots of blood, which, like plugs, close the sides of arteries. The first and second of these divisions we believe to be referrible to deposits from the blood. The lining membrane of an artery being a non-vascular structure, cannot be said to inflame, and so long as it maintains its integrity, lymph cannot be effused upon its surface. That which has been supposed, therefore, to be lymph effused as a result of inflammation, must in reality be fibrin deposited from the blood. Upon a microscopic examination of the white fibrinous plugs found in arteries, they may be found to consist of a delicately fibrillated material resembling that which constitutes ordinary fibrinous coagula, and in the meshes of this may be often seen an abundance of roundish corpuscles, unacted upon by acetic acid in the same manner as pus, but not unlike pus globules in general appearance. The fibrinous concretions also contain ordinarily numerous refractive globules, which, from their solubility in ether, are evidently fatty in their nature. These concretions may be very slightly, or not at all, adherent to the vessels in which they are found, or they may adhere with more or less firmness to the sides of the vessel, and sometimes they may become so immediately connected with the artery as to be with difficulty distinguished from it. In such instances they may resemble an excrescence from its lining membrane. A thin, smooth, polished membrane often forms on the surface of these fibrinous masses, which is continuous with the lining of the vessel, and gives the appearance of the inner coat being continued over them. These white fibrinous deposits are found in almost every degree of consistence, and they may extend to any distance along the arterial canals. When they are detained in one of the larger arteries, they commonly adhere to one side of the vessel; but they are often carried along with the current of the circulation, and lodge in the substance of the organs to which the arteries are distributed. In some rare instances the fibrinous deposit may be traced continuously from the artery to its remotest ramifications. Thus, in an experiment performed by the author, in which some viscid pus was injected into the jugular vein, fibrinous cords were found to extend from the right side of the heart to the minutest ramifications of the pulmonary artery. In a case, mentioned by Sir B. Brodie,

"In which there was mortification of the right foot, the muscular structure of the heart was soft, thin, flaccid, and easily torn; one coronary artery was impervious, and the right iliac artery, for the extent of three inches, was impervious also, in consequence of its being completely filled by a mass of firmly coagulated blood. In another case, in which there had been mortification of the right foot, the muscular structure of the heart was pale and flaccid; one coronary artery was contracted and impervious; the cavities were dilated; a mass of dense coagulum, resembling that found in the sac of an aneurism, occupied the appendix of the left auricle, and there was a similar coagulum obstructing the popliteal artery and vein of the right side, and extending some way down the branches of those vessels in the leg."

Now, Mr. Gulliver has shown that the deposit which constitutes the most ordinary form of disease of the arteries is a fatty degeneration or deposit. This deposit, when it occurs in arteries, becomes softened down, causes a rupture of the thin, brittle, internal coat, and either becomes covered with fibrin, or is discharged into the blood. If covered with fibrin, this may remain firm, or it may itself become softened down, and find its way into the circulation. In old aneurismal sacs such portions of fibrin may be found softened, and containing globules of various sizes, some not unlike pus, but unacted upon in the same way by acetic acid.

"Case I.—On the 14th of December last, a girl seven years of age was examined, after death, at St. George's Hospital. She had long suffered from anomalous symptoms, which were not referrible to any one origin in particular. On inspecting the heart, two aneurismal dilatations were found communicating with the left ventricle by very narrow openings. The aneurismal dilatations contained fibrin which had undergone the process of softening in various degrees, and it was evident that at each contraction of the heart some of the contents of these dilatations would be impelled into the general circulation. The kidney and the spleen both contained fibrinous deposits. Around these the structure of the organs was softened, and the deposit itself, after being kept for a day, became softened in its centre, and dissolved. The arteries at the base of the brain contained some small isolated masses of white fibrin."

"Case II.—A woman, aged twenty-seven, died of endocarditis (so called) on the 10th of January, 1856. The mitral valve was covered with brittle masses, which, under the microscope, were seen to consist of dense, amorphous, highly granular, yellowish-looking lumps. Portions of fibrinous concretion (emboli) were found in the fine branches of the coronary artery of the heart. These could be recognised by the naked eye, and had produced an acute yellow softening of the muscular structure. Numerous hæmorrhagic knots were found in the spleen, in which the endocardiac emboli could with great constancy be traced into the penicilli. These emboli were also found to fill some of the little arteries of the kidneys."*

"Case III.—J. F. L., a boy, aged six, apparently in previous good health, suddenly fell down, without any complaint, on the 23d of July, 1855; his limbs were found quite relaxed, and after a short time he said it was so dark that he could not see: when taken home he spoke little, and incoherently, but his extremities were then stiff and cold. In the evening he revived, became warm, and slept. During the night he complained of pain in the left foot and great toe; the latter was found to be black at the tip. Towards morning he complained of pain in the thigh and leg of the same side. On the 24th, a cord, of the thickness of the finger, was felt from Poupart's ligament downwards, in the direction of the vessels, about one inch and a half long. This cord was tender on pressure, and the skin over it was somewhat red coloured. A black gangrenous vesicle as large as a bean, in a half-moon shape, presented itself under the nail. The toe and that next to it were swollen, and there was considerable œdema up the instep. Motion in the left leg was attended with pain. Over the aorta a doubling was heard of both sounds of the heart; these sounds were propagated into the vessels of the neck. There was here also a strong venous murmur.—July 25th. The upper part of the thigh was œdematous.—28th. The œdema of the instep had disappeared.—August 1st. The œdema of the groin was gone, but large vibices occupied its situation on the thigh.—8th.

* Virchow's Archiv, Band ix. p. 307.

The sore on the great toe was rapidly healing.—24th. The patient was dismissed. The sore on the toe was nearly healed. The reduplication of the sounds of the heart continued."*

"CASE IV.—James Hunter, aged forty-four, was admitted into King's College Hospital, under the care of Dr. Todd, on the 1st of June, 1854. He had been ill for seven or eight months, and had, as post-mortem examination subsequently proved, some tubercles in the lungs, and disease of the kidneys. On the 26th of May, he had been seized for the first time with extreme dyspnœa. This symptom was so distressing that it precluded the possibility of his sleeping even for a few minutes. When admitted into the hospital he had had no sleep for four days and nights, and complained only of this, and a sense of anguish about the præcordial region. He sat up at night and laid his head upon the table, that being the only position in which, as he said, he could get any ease. On the evening of the 4th of June, it was noticed that the left foot was cold and livid. On the 5th, the pulsations could be felt in the left femoral artery, but not in the corresponding dorsal artery of the foot. He died on the 8th, thirteen days after the first attack of difficulty of breathing. The body was examined fourteen hours after death. The left foot and lower half of the left leg were of a livid blue colour; upon cutting open the left ventricle of the heart, a quantity of thick, discoloured fluid flowed out. Besides this fluid, the left ventricle contained many clots of semi-solid fibrin in various stages of softening. The left common femoral artery, just above the origin of the profunda, was found to contain a dirty-white fibrinous clot, which quite filled up the canal of the vessel, but which was not in the least adherent to its inner coat. Below this the vessel contained a very little red fluid blood, and appeared quite healthy. The popliteal artery contained a mottled coagulum an inch and a half in length, firmly adherent to the inner surface of the vessel, and completely blocking up its canal. Below this again the vessel was contracted. Its lining membrane was very red, and had attached to it shreds of fibrin. The popliteal vein at this point was obstructed by a clot, and the surrounding tissues were infiltrated and condensed. The coagulum was so firmly adherent to the lining membrane of the popliteal artery, that a stream of water allowed to fall upon it from a common jug, at the distance of from eight to twelve inches, did not in the least detach it. The preparation is preserved in the Museum of King's College."

If, then, we find, as in the foregoing cases, that fibrin deposited in the heart may undergo a process of softening, and may then be conveyed in the course of the circulation to arteries of the smallest as well as of the largest diameter, and may there lodge, producing softening of the surrounding tissues, or even mortification, what must we suppose to be the result of similar changes when they originate in the arteries themselves?

Fibrinous deposits not unfrequently form in diseased arteries, and may here, as elsewhere, undergo the process of softening. The product of this softening then, together with the *débris* of the internal coat of the artery, and the softened atheromatous deposit, are carried along the course of the circulation, until arrested in the smaller tubes, or in the actual substance of organs. Wherever they stop, other changes occur. In some cases the fibrin, still retaining some consistency, and adhering in its new bed, may become absorbed, and cause a puckering and contraction of surrounding parts. Nearly all arteries that have thus been obstructed have been found contracted after a certain time. This is so generally the case, that Professor Tiedemann, in describing this disease, has assumed for his title, *Arctation, and Closure of the Arteries.* In general, however, post-mortem examinations reveal that softening, accompanied by cell-development, has taken place in portions of fibrin that have been stopped in their course. When this occurs in a blood-vessel, it produces inflammation of its outer coats and neighbouring tissues; when in the structure of organs, it is accompanied by softening of the surrounding parts. If we find, then, as the result of disease of the arteries, that morbid materials find their way into the blood, and produce a separation of fibro-albuminous deposits which in their ulterior changes are liable to poison the different organs to which they are conveyed in the acute forms now noticed, we are led farther to inquire whether there are any chronic forms of the same affection. In cases of long-standing disease of the arteries, the products of morbid deposits between their coats, which have undergone the process of softening, must constantly pass into the circulation, as must also any portions of liquefied fibrin which have temporarily

* Professor Gustav von Dirben.

adhered to those parts where the lining membrane has given way. If the quantity of morbid deposit or of liquefied fibrin be small, it is probably disposed of without any great inconvenience, but when larger, it would appear that the contaminated blood has a tendency to lodge in the substance of the first organ to which it is conveyed. In parts where the circulation is vigorous, the impediment may probably be readily overcome, but in those parts in which the circulation is more languid (although perhaps they may contain a large quantity of blood), there we find the injurious effects produced. Now, these are exactly the conditions in which senile gangrene ordinarily occurs. A diseased artery gradually but constantly pours the product of fatty degeneration into the blood which is conveyed to the most distant and dependent part of the circulation. The excessive pain coincides with that which experiment proves to be the result of the injection of arteries with fluids which do not readily pass into the veins. The skin, which contains the largest quantity of the morbid material, perishes first; and in succession the cellular membrane, bone, tendon, and ligament. If this be the true pathology of dry gangrene, it explains at once how futile amputation is likely to be while the original source of disease remains in the form of fatty degeneration of the artery supplying the limb. It explains also why opium and tonics are found to agree so much better than the antiphlogistic plan formerly recommended by Dupuytren. Finally, it explains how, when the morbid material which produces the gangrene ceases to be supplied, the patient may recover, as in case No. III.

The following case, which lately came under the care of my friend Mr. Bowman, at King's College Hospital, shows the tendency there is to the formation of fibrinous deposits in those situations where the inner coat of the arteries has been removed by disease:

"Case V.—Philip Shaw, aged fifty-six, a porter, was admitted into Fisk Ward on the 4th of February, 1857, with gangrene of the left foot. On the 31st of January, after having been exposed to cold during the day, he felt in the evening some numbness and stiffness in the left foot. He subsequently experienced considerable pain, and the skin of the toes became of a dark bluish colour. When admitted into the hospital, his countenance was pinched and congested, as if from habitual intemperance. The pulse intermitted at every seven or eight beats, and there was a distinct bellows sound at the apex of the heart. In the beginning of March, the whole of the metatarsal bones had become exposed by the separation of the sloughs, and on the 23rd of the same month he died. The body was examined thirty-four hours after death. A distinct arcus senilis presented itself in each eye. On opening the chest, the lungs were found healthy, and everywhere crepitant. The cavities of the heart were empty; there was some thickening of the mitral valve; the arch of the aorta presented numerous atheromatous patches. In the abdomen the liver, spleen, kidneys, and intestines were found perfectly healthy. The aorta here presented similar deposits to those already noticed. Near the origin of the inferior mesenteric were some fibrinous flakes adhering to the posterior part of the vessel, and connected with the coagulum which extended some three or four inches down the vessel. The common and external iliac arteries on both sides were much diseased, the atheromatous deposit here having undergone various degrees both of hardening and softening. The femoral artery of the left side was almost obstructed in its lower part by white fibrinous coagula. Between this deposit and the wall of the artery a channel appeared to have been formed, through which the blood had passed. In the popliteal space the vein and the artery had become firmly adherent, and both were obstructed. The left posterior tibial artery was almost closed by fibrinous coagula. In the anterior tibial artery no disease was discovered. The axillary, brachial, radial, and ulnar arteries on both sides were apparently healthy. The arteries at the base of the brain showed some distinct patches of atheromatous deposit. A preparation of the arteries containing the fibrinous deposits is preserved in the museum of King's College."

The practical idea which suggests itself from the foregoing observations and cases is, that a diseased or partially obstructed artery may be more dangerous to a patient's welfare than one which is completely closed. The blood in the case of obliteration of the main trunk would probably be conveyed to the extremity in diminished quantity, but flowing through collateral and comparatively undiseased channels, it would be more free from the admixture of any morbid matter which it might receive in its passage through the limb.

A greater danger may, therefore, arise to a limb from the principal artery being partially or temporarily obstructed, than from its complete and permanent obliteration. This point is illustrated by the following case, taken from Dr. Oldham's notes, and for which I am indebted to Mr. Birkett, of Guy's Hospital:

"CASE VI.—A very tall, healthy, muscular and robust Scotch peasant, thirty years of age, was admitted into Guy's Hospital, on the 15th of August, for a popliteal aneurism. On the 17th of August a 'temporary ligature' was applied to the femoral artery. The ligature was removed at the expiration of seventy-two hours. For the next four days everything appeared satisfactory, when, on the morning of the 25th of August, a small dusky spot was observed by Mr. Birkett on the dorsum of the foot. This spot increased, and it was evident that mortification had commenced. During the course of the day, some hæmorrhage took place from the situation of the temporary ligature. This again recurred on the following day, August 26th, when amputation of the thigh was performed."

Whether the partial and temporary obstruction to arteries by pressure in cases of aneurism is liable to be followed by any similar accidents to those attending upon the temporary ligature, experience has yet to decide. Three instances have lately come under the author's notice in which mortification of the leg followed the treatment of a popliteal aneurism by pressure. In two of these cases the femoral artery was at length tied, and before the mortification had apparently commenced.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

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I. *On Indian Febrifuges.* By Assistant-Surgeon W. R. CORNISH. (Indian Annals of Medical Science, October, 1856.)

AMONG the indigenous febrifuge plants of India, the margosa or neem tree has long enjoyed a considerable reputation. This tree belongs to the natural order of *Meliaceæ*, and to the genus *Azadirachta.* The important part of the margosa tree, considered as a febrifuge, is the bark, which varies in thickness from a quarter of an inch to an inch, according to the size of the tree. On making a section of the bark, the outer layer is found to be of a bright purple colour, while the inner is almost white; these separate readily from each other, the inner being the thicker of the two. If a small portion of the latter be chewed in the recent state, it has at first a sweetish taste, followed quickly by a powerful and lasting bitter. The author of the paper remarks upon the curious fact, that although the margosa bark has long been recommended as a febrifuge, yet it has hitherto been very rarely used by European practitioners in India; and he points out the necessity of searching for some cheap and efficient substitute for quinine at the present day, when the cinchona forests of South America are gradually becoming extinct. Mr. Cornish's experience of the efficacy of the margosa bark extended over a period of six months, during which time nearly all the fever patients which came under his care while he was doing duty with a native troop of Horse Artillery at Secunderabad were treated with margosa decoction. He employed an emetic in all the cases as a preliminary measure, and afterwards prescribed a decoction of the margosa bark, prepared by boiling the dried bark in water for fifteen to twenty minutes, and straining it while hot through calico. The dose was an ounce and a half to three ounces, given repeatedly before the accession of the paroxysm. The physiological action of the bark can only be described negatively. A large dose of the decoction has no immediate action upon any of the animal functions, and continued doses are borne without any inconvenience to the system. It does not appear to be a very active remedy, and it rarely cuts short a paroxysm of fever. It does not produce any disagreeable effect upon the internal economy, and under its use the tongue becomes clean, the appetite generally improves, the febrile paroxysms become milder, and are soon worn out. The neem bark is unquestionably a tonic, but it is difficult to determine whether it can be regarded as an *anti-periodic.* The author thinks that the properties of the bark depend on the presence of a bitter alkaloid principle, to which he applies the term *margosine.* This bitter principle is found in the greatest quantity in the inner bark, while the outer bark contains an astringent principle closely allied to the variety of tannin found in catechu. In addition to these principles, the bark yields an essential oil, a bitter resin, gum, starch, and sugar in considerable quantities. The results of his experience are such as to convince Mr. Cornish that the margosa is quite as effective in the treatment of intermittent fevers as cinchona and arsenic, and he found that the percentage of failures was even less under the margosa treatment.

Under the impression that the action of tonics and astringents is identical, Mr. Cornish has been induced to examine the effect of the latter class of medicines in the treatment of fevers, and the results have been somewhat favourable. He comes to the conclusion,—1. That vegetable astringents may be substituted for quinine in the treatment of simple quotidian and tertian intermittent fevers. 2. That in the former, vegetable astringents will fail in from five to ten per cent. of the cases treated. 3. That in the latter, quinine has little or no advantage in breaking the febrile paroxysms or curing the patient. 4. That the double tertian intermittents do not readily yield to vegetable astringents, and in this type of fever quinine is superior. And 5. That vegetable astringents have failed in a smaller proportion of cases of all forms of fever, than the febrifuges, cinchona and arsenic. The vegetable astringents which have been tested in hospital practice have been galls, catechu, and dibi divi, or Cæsalpinia coriaria. The seed-pods of the latter plant contain a large percentage of astringent matter, and Mr. Cornish therefore employed them in the treatment of fever. He gave the dibi divi powder to nearly one hundred patients suffering from intermittent fever in its various forms, and with considerable success. The dose of the powder-pods commonly used was from forty to sixty grains three times a day. Constipation of the bowels was a very uncommon symptom, and in two cases the dibi divi even seemed to cause diarrhœa. Cases of fever, complicated with anæmia and splenic enlargements, appeared to do best under this treatment.

The author remarks incidentally, that the amorphous quinine supplied to the Indian hospitals does not appear to possess any great advantage over some of the common native febrifuges, but that the sulphate of quinine is undoubtedly the best febrifuge in existence. He is accumulating evidence to show that the amorphous quinine is not of equal value as a febrifuge with the crystalline variety.

II. *On Chloride of Gold and Sodium, employed as a Solvent in the Treatment of certain Tumours.* By Dr. Rouault. (L'Union Médicale, Feb. 21st, 1857.)

The author of this communication relates some cases observed by Dr. Debreque and himself, in which it was found that the preparations of gold possess a special elective action in the treatment of glandular tumors. In chronic adenitis in general, and particularly in cervical adenitis, the solvent properties of the preparation alluded to appeared even more energetic and certain than those of iodine. One of the circumstances favourable to its employment is the presence of several tumours, separated or united in the form of a chaplet, or of ganglionic knots. The author remarked that its efficacy was less evident when there existed only a single ganglion, the resolution of which then only takes place with extreme slowness, and often not at all. Gold is also useful in benignant tumours of the breast, such as simple engorgement, hypertrophy, and sub-inflammatory tumours; and it also appeared to Dr. Rouault to be undoubtedly efficacious in certain tumours which were evidently of a malignant nature. The chloride of gold and sodium was the preparation generally employed, being combined with starch and gum arabic, and made into pills. With one of these pills friction was made every evening on the tongue, the gums, and the inside of the cheeks. The friction should be employed for some minutes, and the patient ought not to spit, so as to swallow any remains of the matter which is rubbed in. This plan is to be followed for at least six weeks. Several cases are related in which this plan appears to have been attended with success.

IV. *On Pepsin, and its Chemical and Physiological Properties.* (Bulletin de Thérapeutique, January, 1857.)

In an analysis of a paper read lately by M. Boudault to the Société de Pharmacie of Paris, the author, after discussing the general properties of pepsin, proceeds to make the following remarks upon that substance employed as a medicine: Its administration presents some rather considerable difficulties, in consequence of its liability to alteration when the vessel which contains it has been open. Besides this, its origin, its viscosity, and its disagreeable smell were so many motives for disliking it on the part of the patient. It was necessary, then, to find a method of transforming it without injuring its medical action. It was to be feared, in associating it with an inert substance, that the latter would experience a kind of digestion, or would act upon the pepsin as a ferment. It was necessary, besides, that this substance should be sufficiently hygrometric to absorb the humidity of the pepsin, and not to attract, in addition, the humidity of the air. Sugar was one of the substances with which it appeared most easy to associate pepsin; but at the end of some days the cane-sugar is transformed, under its influence, into glucose, and afterwards into lactic acid, for here the pepsin acts as a true ferment. Starch dried at 100 degrees (Cent.) has given to M. Boudault the most favourable results. Starch, which has the property of not injuring the digestion, forms with pepsin a pulverulent matter, the odour of which is very weak, and the taste partly disguised. This powder is preserved very well in well-stopped bottles, and time does not modify in any way its physiological properties. Under this form, pepsin may be mixed with a number of medicinal substances which do not at all modify its therapeutic action: thus, with hydrochlorate of morphia, to relieve violent pains of the stomach; with strychnine, to stimulate the peristaltic movements of this organ; with nitrate of bismuth, lactate of iron, carbonate of iron, iodide of iron, and other similar preparations. It is very efficacious in dyspepsia, and in all cases of difficult digestion which generally follow the convalescence from serious or chronic diseases; and it has been found a powerful digestive agent in cases of consumption caused by insufficient food. Pepsin is administered in the first spoonful of soup, or even before meals, wrapped up in a wafer; and precaution must be taken not to eat immediately afterwards food which is at a higher temperature than 45 deg., for then the digestive properties of pepsin would be destroyed. It is employed in the acid or in the neutral state. In the acid state it takes the place of the gastric juice, when the latter is not formed in sufficient quantity in certain morbid affections; in the neutral state—that is to say, feebly acidulated—in cases where the stomach contains too great a quantity of acid. It may be shown that chemical or artificial pepsin may very well take the place of the gastric juice, and may be considered among one of our most heroic remedies.

III. *On the Preparation and Therapeutical Employment of Subcarbonate of Bismuth.* (Bulletin de Thérapeutique, February 15th, 1857.)

The following is the mode of preparation of the subcarbonate of bismuth described by M. Hannon, Professor at the University of Brussels. The bismuth is first purified by melting this metal in powder with ten times its weight of powdered nitre. After cooling, the metal is again powdered and mixed with ten times its weight of nitre, and after a second fusion the bismuth may be considered as entirely free from the arseniurets and sulphurets which it almost always contains. Then three parts of nitric acid are put into a retort, and one part of pure bismuth is added. When the reaction is complete, about a third of the liquid is evaporated, then the solution is poured drop by drop into a solution of carbonate of soda, and a white precipitate is obtained, which is subcarbonate of bismuth. The precipitate, after having been washed five or six times with distilled water, is thrown upon a filter, and washed again to remove the last traces of carbonate of soda. It should be preserved in well-stopped bottles. The physiological properties of the salts of bismuth are very little known, for the simple reason that the subnitrate is the only salt which has been employed in medicine. The operation even of this salt is not well understood, as its insolubility offers an obstacle to the observation of the physiological phenomena which might have been observed in the other salts of bismuth, such as the citrate, the tartrate, or the carbonate. It is also the insolubility of the subnitrate which renders it inefficient in the greater part of the cases in which it is indicated; and it also occasionally produces a very inconvenient sensation of weight at the stomach. The subcarbonate is soluble in the gastric juice, its action is rapid, it produces no sensation of weight at the stomach, it rarely constipates, colours the stools less than the subnitrate, and may be employed for a long time without oppressing the stomach. The action of the subcarbonate appears to be sedative during the first days of its employment, and subsequently to excite all the phenomena which result from the action of the tonics.

As to its therapeutical action, it may be noted that all cases of gastralgia consecutive upon phlegmasia of the digestive passages, cases in which the tongue is red and pointed, and cases in which the digestion is laborious and accompanied with putrid or acid eructations, or in which there is a tendency to diarrhœa or spasmodic vomiting, demand the employment of the subcarbonate of bismuth. This salt is also required in the vomiting of children, whether caused by dentition or succeeding to frequent fits of indigestion, and in the diarrhœa of weak children, especially when occurring at the time of weaning. One great advantage possessed by the subcarbonate of bismuth is, that it neutralizes the acids in excess which are found in the stomach. The subnitrate, as is well known, fails always in this respect. In all the cases where the subcarbonate has been taken, the pain in the digestive passages is first found to disappear; then the eructations cease, together with the vomiting or diarrhœa; the digestion becomes less and less laborious, the tongue gradually receives its normal form and colour; and if the use of the subcarbonate is continued, the appetite increases from day to day, the yellow tint of the countenance disappears, and the face becomes coloured at the same time as it ceases to be shrivelled.

The subcarbonate of bismuth is perfectly insipid, and excites no repugnance. It is given before meals. Adults take it in a little water, and children in honey. It may also be made into lozenges. The dose for adults is from one to three grammes, taken three times a day in increasing doses.

V. *On the Curative Properties of Sulphureous, Ferruginous, and Alkaline Springs.* (L'Union Médicale, April 4th, 1857.)

In a late discussion at the Société d'Hydrologie Médicale of Paris, M. Cahen discussed the question, whether sulphureous, ferruginous, and alkaline springs possess any other curative properties besides those possessed by sulphur, iron, and bicarbonate of soda; and he comes to the conclusion that the benefit arising from the use of such waters is of a strictly physiological and chemical nature, and is due to the presence of the mineral which is held in solution. It is true that there are accessory circumstances which are not to be neglected in considering the effects of mineral waters in the restoration of health,—such as the journey—the change of air, of diet, and of habits—the influence of amusement, and even of hope; but these are not in themselves, except in special cases, sufficient for effecting a cure. In explaining the influence of mineral waters, however, an exclusively chemical view of their character is to be deprecated; for the human system in contact with such waters cannot be regarded in the same light as a chemical experiment made in the laboratory, where the conditions of the experiment are fixed and constant. Thus the Vichy waters, acting upon the mucous membrane of the stomach affected with pyrosis, attended with hypersecretion of alkaline matters, are not to be regarded as an alkaline fluid saturating an excess of alkali; for physiology has shown that in contact with a small quantity of alkaline water, the mucous membrane of the stomach secretes abundantly *acid gastric juice*, and it is this acid gastric juice which removes the inconveniences of an abnormal secretion. The water acts only mediately in this case, by the reaction which it has excited. It has also been said, that while mineral alkaline substances introduced in excess into the economy produce an alkaline cachexia, yet gouty persons drink every day and for a long time enormous quantities of Vichy water without the slightest inconvenience. Now this happens *because they are gouty*, and because there exists in them an acid diathesis which opposes the influence of alkaline drinks. M. Cahen concludes his observations by remarking that, in his opinion, there is nothing latent or mysterious in the action of these waters, and that they act, on the one hand, in a physiological manner, and, on the other hand, by virtue of the mineral substances which they contain. In the course of the discussion which followed the remarks of M. Cahen, M. Durand-Fardel denied the accusation sometimes brought against the Vichy waters, that they had often induced an alkaline cachexia; and he stated that he had himself lived at Vichy ten years, and had seen a great number of persons take the mineral waters in excess, and suffer inconvenience from so doing, but had never observed anything approaching to what has been described as alkaline cachexia. M. Cahen, on the other hand, although admitting that gouty persons are with difficulty rendered cachectic by the use of the Vichy waters, contended that this cachexia did really exist. He himself had employed immediately the waters and the baths of Vichy, and he fell into a distinctly asthenic state; his blood, drawn from a vein, presented a defibrinated appearance. He also stated that the inhabitants of Vichy are of squalid appearance, which circumstance may be attributed to their habit of using the waters in their daily occupations.

VI. *Balneological Sketches.* By Prof. LÖSCHNER, of Prague. (Vierteljahrsschrift für die Praktische Heilkunde, 1857.)

Prof. Löschner, after some remarks upon the gases introduced into the lungs by the breath during bathing, describes the operation of certain baths in the cure of disease. He treats first of the operation of the Marienbad springs in the diseases of children. It should be premised that the diet of all the patients was regulated upon a uniform scale. The Kreuzbrunnen and Ferdinandsbrunnen of Marienbad are found efficacious in scrofulous affections of the glands, of the skin, of the bones, with and without the appearance of reaction; glandular inflammation in different parts of the body, formation of abscess, caries, ulceration of the cornea, eczema, herpes, psoriasis, zoster. The operation of these waters is shown by constant increase of the functions of the intestines and kidneys, with appearance sometimes of the formation of sulphuret of iron and development of sulphuretted hydrogen, together with remarkable secretion of bile, sometimes of uric and oxalic acids, particles of fat, shreds of mucus, diminished excretion of phosphoric acid, and afterwards decrease of the weight of the body, but nevertheless increased vital activity in combination with powerful changes in the whole process of nutrition. The latter is especially shown in the vivacity of the children, which at first is diminished, but is subsequently increased; in their better and purer colour; in the disappearance of glandular tumours (unless when they are infiltrated with tuberculous masses); in the diminished swelling of the bones; in the drying-up of chronic exanthemata; in the discontinuance of inflammatory symptoms in the eye and ear. The activity of the heart and arteries was augmented, the tympanitic condition of the abdomen subsided, the mental operations became active and lively. The mode of operation of the Marienbad springs, in accordance with their chemical peculiarities, may be stated to consist in bringing about a more active metamorphosis of tissues, acceleration of the digestion, normal conversion of the nourishing material into the organic juices, and more powerful nutrition by means of the increase and improvement of the constituents of the blood. Dr. Löschner then describes the use of the iodine water at Halle and Fracchia's sea-baths in children's cases. The diet should first be regulated by allowing a copious supply of meat and a limited quantity of vegetables. The subjects most appropriate for treatment in these baths are those suffering from torpid scrofulous affections, and rhachitic patients with a high degree of swelling of the epiphyses; the former in the most intense form of abdominal, cutaneous, and glandular scrofula; the latter being cases of long duration, and already beginning to exhibit ossification of the swellings of the epiphyses. The author knows no mineral water containing iodine which exhibits its operation so powerfully and so quickly upon the organism as that now described; and this effect he attributes to the absorption of iodine into the system. Baths with the iodine water of Halle and the artificial sea-baths soon produce, when used continuously and in a concentrated form, powerful symptoms of reaction, and the appearances of iodism, with tumultuous and reducing metamorphosis of tissues; while baths of moderate temperature, of brief duration, employed every second day, may be continued for weeks, and even months, without producing such a tumultuous operation, and accomplish in a tranquil manner the changes of the tissues. It is remarkable and surprising, under such circumstances, to observe the disappearance of scrofulous tumours, of chronic catarrhs of the nose, throat, and genital mucous membrane depending upon a scrofulous origin, such as scrofulous ozæna and utero-vaginal catarrh; the subsidence of swellings of the epiphyses in rhachitic patients, with striking improvement of the aspect after a moderate previous excitement of the function of the skin, and the separation of abnormal quantities of mucus, with salts of uric and oxalic acid, through the respiratory and urinary organs. Dr. Löschner found the iodine waters of Halle very useful, when employed internally, and when inhaled by the nostrils, in a case of long continued ozæna in a young woman approaching puberty, in whom for many years a great number of remedies had been employed in vain; also in glandular swellings of the abdomen; in chronic utero-vaginal catarrh, in which artificial sea-baths and the internal use of the iodine water have effected a complete cure; and in chronic exanthemata of scrofulous children, in which this water is a most powerful remedy. Latterly, Dr. Löschner has made some experiments with the iodine water of Halle in the syphilis of children, using at the same time the artificial sea-baths, if exanthemata were present at the same time. Four cases only of this kind of treatment have been observed; but they appear to the author to justify him in the belief that the operation of the water is also beneficial in these maladies.

VII. *On the Anæsthetic Action of Carbonic Oxide.* By Dr. OZANAM. (Archives Générales de Médicine, Feb. 1857.)

Dr. Ozanam considers that the results hitherto obtained by the use of anæsthetic agents concur to demonstrate the truth of the law, that the whole series of carbonized bodies, volatile or gaseous, are endowed with anæsthetic power, and that they possess this power in proportion to the carbon which they contain. He has therefore undertaken some experiments with carbonic oxide, carbonic acid, and cyanogen, and the results observed with the first named gas are detailed in the present memoir.

The carbonic oxide is obtained by the action of sulphuric acid, aided by heat, upon oxalic acid; when the latter is resolved into carbonic acid and carbonic oxide, and the carbonic acid is removed by lime-water, while the carbonic oxide remains in the gaseous state. The experiments and observations were thirty in number, of which twenty-five were on rabbits and five on man. The author divides the phenomena produced by the inhalation of carbonic oxide into four periods—viz., 1st. The prodromic period; 2nd. The period of excitement; 3d. The period of anæsthesia; and 4th. Death or restoration of sensibility.

A pipe connected with a bladder filled with carbonic oxide was introduced into the mouth of a well-fed rabbit, whose nostrils were closed; an assistant pressed upon the bladder, and the animal, forced to breathe by the mouth, inhaled the gas mixed with atmospheric air. During the first five or six inspirations the animal made no effort—it was motionless and astonished, as if under the impression of a danger which it suspected but did not know, and of which it did not yet feel the violent effects. But at the end of fifteen to thirty seconds, this preliminary period was succeeded by a stage of excitement: the animal leaped and made efforts to escape; then these voluntary movements were succeeded by very strong convulsions, contractions, throwing the head backwards, trembling, &c. During this period, the circulation was accelerated at first from fifteen to twenty pulsations under the influence of the convulsive agitation; then it returned to its normal rate, which it soon quitted and became slower. The respiration, on the contrary, offered from the beginning a marked tendency to become slower. To the convulsive state suddenly succeeded the period of collapse or stupor; all movement ceased, the body fell back like an inert mass, the head hung down, the eye was widely open, the pupil dilated, the sight almost abolished, the four members were paralyzed, the urine passed involuntarily, the pulsations of the heart became more slow, the respiration also was less frequent. If the inhalations are prolonged, the respiratory act is weakened still more; it occurs only about once in five or ten seconds, by a general and jerking effort, resembling hiccough; but prolonged to this degree, the anæsthesia becomes dangerous, and it should be narrowly watched, for the inspiratory nerves are almost paralyzed, and the animal approaches the state of apparent death.

The experiments made and recorded by Dr. Ozanam prove beyond a doubt the anæsthetic action of the gas, and he regards it as more energetic in its action than chloroform, but less prolonged in its operation; its effects are rapid, violent, and transient, so that an animal may pass in a few minutes from the state of apparent death to a normal condition. On the other hand, it possesses certain advantages over chloroform, in the absence of a strong, or penetrating, or caustic smell—a circumstance which renders the gas easily respirable by every person; while ether, chloroform, and the carburets of hydrogen, have all a penetrating smell, which renders them offensive to many persons, and they are caustic when applied to the skin. It is also easily measured, owing to its permanently gaseous condition, which is not the case with chloroform and ether: their volatility varies under the slightest influence, as the summer, the heat of a room, or the vicinity of a stove, will cause a patient to absorb double the quantity of the vapours which would have been breathed if the circumstances had been different. The anæsthetic operation of carbonic oxide terminates in recovery from insensibility, or in death. When the inhalations are discontinued, the animal is abandoned to itself. During one to three minutes, the anæsthesia remains absolute, and the animal might be considered dead, if ausculation did not still reveal the weakened sounds of the heart, and some rare inspiratory efforts. The ordinary life soon recommences, respiration is re-established, and the heart progressively resumes its normal rate, and sometimes slightly exceeds it. But occasionally the passage from stupor or apparent death to real death is sudden, unexpected, and similar in this respect to sudden death by chloroform: the heart and respiration, already very slow in their actions, cease at once and for ever.

In case of poisoning by carbonic oxide, the antidote most likely to prove serviceable is ammonia; and Dr. Ozanam details two cases of rabbits poisoned by carbonic oxide, which were restored by the application of the vapour of ammonia.

VIII. *On the Use of Amylene as an Anæsthetic Agent.* By M. LUTON. (Archives Générales de Médicine, February, 1857.)

After describing the properties of amylene, as recorded by Dr. Snow in some papers recently published in this country, M. Luton details the results of two experiments made in Paris upon young children with this anæsthetic. In both cases there was sudden and remarkable lachrymation, as in breathing the vapours of ammonia; and there was evident repugnance at first to the inhalation; but this soon passed away, and anæsthesia was induced. Both children rapidly recovered after the amylene was withdrawn. Admitting that the results observed in two cases only are insufficient to justify the expression of any decided opinion upon the qualities of amylene as an anæsthetic, M. Luton thinks that he may draw the following conclusions: The advantages resulting from its employment are, that its action is rapidly manifested and rapidly dissipated, owing to its great volatility; that the insensibility is sufficient, although the sleep is less deep than that induced by chloroform; and that there is less uneasiness to the patient during the course of the operation. The inconveniences of amylene are to be found in the necessity of employing a great quantity of it during the operation, and its disagreeable odour, which is so strong as to be offensive to the persons engaged in the operation, and, of course, is still more so to the patient.

IX. *Of the Employment of Electricity in the Suppression of the Lacteal Secretion.* (L'Union Médicale, January 3rd, 1857.)

M. Becquerel, in a late communication to the Société Médicale des Hôpitaux de Paris, has made some remarks upon the influence of electricity in restoring the secretion of milk. His attention was called to the subject by a case related to him by M. Aubert, who had employed electricity in the case of a young woman whose milk had been suppressed in consequence of a double pneumonia. The electricity was applied to the breasts by means of moist excitors, and after four applications, each lasting twenty minutes, the lacteal secretion was completely restored. M. Becquerel was at first incredulous as to the reality of the result; but the following case, which fell under his observation, removed his doubts:

A young woman, aged twenty-seven, well formed, although of a nervous temperament, had suckled a young infant for six months, but, on the occasion of some intense and often-repeated mental emotions, the lacteal secretion diminished considerably; the right breast retained a little milk, but the left was almost completely dried up. M. Becquerel applied the electrical current at first to the left breast, placing the moist excitors, made of sponge, successively in the different points of the circumference of the breast, so that the currents might traverse the organ in all directions. Three applications were made, each lasting a quarter of an hour. The patient suffered very little, and indeed experienced little more than a feeling of inconvenience. From the time of the first application, the rush of milk supervened almost immediately after the application of the electrical currents. After the third application, the secretion was full and entire; the child had taken the breast, and the milk was abundant in the left breast, and sufficient in the right to obviate the necessity of applying the electricity on that side.

X. *On a Case of Diabetes treated by the Use of Rennet.* By Dr. IVERSEN. (Archiv des Vereins für Gemeinschaftliche Arbeiten, 1856.)

Dr. Iversen relates the case of a patient, in the lower class of life, who had well-marked diabetes, who was treated with rennet, and the details of whose case were carefully recorded day by day. As all the usual plans of treatment had been unsuccessful before the patient's admission into the hospital under Dr. Iversen's care, he made an experiment of the rennet treatment. In order to obtain as accurate a result as possible, it was determined, in the beginning of the treatment, not to alter the diet of the patient, except to recommend the greatest possible abstinence from drinking. By the table prepared by Dr. Iversen, the treatment seems to have been successful in diminishing the quantity of sugar in the urine; but from some circumstances which are not explained, the patient was seized suddenly during the progress of the case with fainting, followed by spasms, ending in death. No post-mortem examination was permitted, and the case is therefore imperfect. Notwithstanding the unfortunate result, Dr. Iversen considers that the constant diminution of the urine, both in its actual quantity and in its saccharine ingredient, was very remarkable. He shows that in the first four days, during which the patient took no medicine, the average quantity of urine voided, amounted to 10·108 cubic centimètres. In the following period of seven days, during which she took the rennet, the quantity of urine reached only 7·927 cubic centimètres, with a quantity of sugar amounting to 324 grammes. In the next five days, during which she took the rennet in combination with phosphate of soda, the average daily quantity of urine sank to 6·988 centimètres, with 250· 317 grammes of sugar. The patient herself attributed to the rennet the power of allaying in some measure the burning thirst which she experienced.

XI. *On the Use of Sulphate of Atropia in Diseases of the Eye.* By Dr. Friedrich Mosler. (Archiv des Vereins für Gemeinschaftliche Arbeiten, 1856.)

As the result of practical investigations upon the use of sulphate of atropia in ophthalmic medicine, Dr. Mosler arrives at the following conclusions:—1. That the sulphate of atropia is preferable to the pure alkaloid for therapeutic purposes. In a state of purity the sulphate, employed with the necessary precautions, even in large doses (such as five grammes to an ounce of distilled water), produced no unfavourable effects upon the eye. In using it, care must be taken of the absorption of the tears running from the eye and mixing with the solution, and the absorption of the solution itself is to be guarded against. 2. In ophthalmoscopic investigations, atropia has rendered especial services in many cases; in order to diminish as much as possible the inconvenience felt by the patient in its use, attention must be paid to the investigations of Donders, upon the more or less enduring operation of the different strong solutions. The employment of atrophia is not *à priori* to be recommended in every ophthalmoscopic investigation. 3. In inflammatory states of the eye, especially those characterized by violent pain, intolerance of light, and abundant lachrymation, as particularly in injuries of the eye, with or without affection of the iris, we have been acquainted with atropia as an essentially soothing agent, as by its operation on the sensitive nerves of the eye it possesses the power of removing rapidly the state of excessive irritation. As a decided remedial agent, it appears moreover to act by its operation upon the motor nerves in the eye, inasmuch as, according to the explanations of Dr. Von Gräfe, it paralyzes the muscles which are found in and about the eye, and which in such cases exercise an excessive pressure upon the internal structures of the eye, and in consequence of the return of the blood being impeded, give rise to accumulation of blood in those structures. It is thus explained why abscesses of the cornea under its use are less perforating and more easily healed, and why hypopyon is more rapidly absorbed. 4. Astringent eye-waters, especially the stronger cauterizing fluids, are better borne, and are attended with more rapid success, when the excessively heightened sensibility of the eye, which exists in the cases where this remedy is applicable, has been previously deadened by atropia. 5. Cauterization of the eye, employed only once daily with all necessary precautions, is better borne in many cases than the more frequent instillation of eye-waters, which every time appear to induce a new and well-marked irritation.

XII. *On the Employment of Iodide of Calomel (Chlorure Mercureux) as a Local Application in Uterine Engorgement.* By Dr. F. Rochard. (L'Union Médicale, January 6th, 1857.)

Dr. Rochard having applied the iodide of calomel in certain hypertrophic and sub-inflammatory affections of the neck of the uterus, has arrived at the following conclusions in favour of this kind of medication.

When a pledget of charpie, covered with a pommade of iodide of calomel, is applied to the neck of the engorged uterus, ulceration being absent, the women in general experience no particular sensation, but sometimes they feel towards the conclusion of the application a slight sensation of heat in the hypogastric region; when ulceration exists the sensation of heat is manifested very early, and is habitually followed by pains which may be rather severe. As soon as the dressing is removed, the sensation or even the pains are immediately relieved, and the neck of the uterus when examined appears more voluminous than before. If it is not ulcerated, there is formed upon all the surface of the mucous membrane touched by the pommade a thin exudation of a greyish-white colour, and of a consistence rather less than that of boiled albumen, which, when examined by the microscope, exhibits neither pus, nor epithelium, nor fibres, but only a granular, transparent, and apparently amorphous mass.

When the neck is ulcerated, the same exudation is formed, but it does not remain adherent to the mucous membrane, and is removed with the dressing; in this case it contains some remnants of deformed epithelium. Besides this exudation, the charpie which has served for the dressing is always moistened with a serous liquid, sometimes sufficiently abundant to flow outwards and to form greyish spots upon the patient's linen. In the days succeeding the dressing, the exuded coagulum is detached by degrees, the volume of the os uteri diminishes, and becomes less than it was before the topical application; if there was any induration, which is generally the case, this induration is much less from the day succeeding the dressing. At the end of eight, ten, or twelve days, if the amelioration has made no progress, the dressing is renewed, and gives rise to the same phenomena, although in a less marked degree; and after two, three, four, or five applications made at the same intervals, the os is usually restored to its normal volume, and the ulcerations are cicatrized. The patients, who feel themselves *less heavy* on the first application, are relieved from all painful sensation, particularly those who had no ulceration. The latter recover only after a longer period; the others can generally walk with ease after the second application, even when walking was previously impossible. The mode of applying the pommade is by preparing a pledget of charpie of suitable thickness, and of rather larger dimensions than the volume of the os uteri. The centre alone is covered with a light layer of the pommade, so that the edges which remain dry defend the vaginal mucous membrane from the contact of the application, which might cause inflammation.

XIII. *On the Treatment of Scrofulous Affections by the Iodide of Potassium.* (L'Union Médicale, February 17th, 1857.)

Dr. Vincent Duval adopts the following plan in the administration of iodide of potassium in infantile scrofula. In children from one to three years of age, he prescribes the iodide of potassium in solution in distilled water, in the dose of ten to fifteen centigrammes a day during the first week, and of twenty to thirty in the three succeeding weeks. At the end of this time he discontinues the use of the drug for a week, and during this interval he purges the patients with castor oil, or preferably with calomel. Then he recommences the use of the iodide as before. At the end of two months, if the digestive passages are in an unfavourable condition, he orders one or two grammes a day of bicarbonate of soda, dissolved in sugared water or the infusion of hop. After a fortnight or a month of the use of the bicarbonate, he returns, if necessary, to the employment of iodide or bromide of potassium for one or two months. In children of more advanced years, the dose must be augmented in proportion; but even in adults, Dr. Duval seldom gives more than one gramme in a day. He often adds to the iodide of potassium the sulphate or the citrate of iron, more frequently the latter. When the patients are thin and weak, cod-liver oil agrees very well, not only as an iodized medicine, but also as a fatty body; it renders the blood more plastic and more fibrinous, the respiration more active, and the absorption of oxygen more abundant. Given at the same time with the iodide of potassium, this latter medicine does not cause emaciation in the patients. If citrate of iron is added, independently of the iodide of potassium, its action is still further augmented. [Combinations, ready prepared, of cod-liver oil with iodine, iron, and other alteratives and tonics, have been long employed in British practice.—REPORTER.]

XIV. *On a New Principle of the Colchicum Autumnale.* (L'Union Médicale, January 10th, 1857.)

M. Oberlin has just communicated to the Académie des Sciences at Paris some observations on the Colchicum autumnale, from which he has extracted a neutral crystalline principle which he calls *colchiciene*, and which differs from *colchicine*, a complex and uncrystallizable product. The properties of colchicéine are to crystallize very easily in pearly laminæ, and to be almost completely insoluble in water, but to communicate to this fluid a slight bitterness, which increases sensibly when it is boiled. At this temperature a notable part of the product is dissolved, but is deposited immediately after cooling. The solvents of colchicéine are alcohol, ether, methylated spirit, and chloroform, which contract, when mixed with it, a very intense and persistent bitterness. The alcoholic solution of colchicéine is coloured by the addition of bichloride of platinum, but no precipitate is formed. Pure concentrated nitric acid dissolves colchicéine, and becomes coloured of a very intense yellow tint, passing into a violet colour, then to a deep red and a clear red, and finally returning to its primitive yellow colour. Concentrated sulphuric acid forms with it a solution of a very intense yellow colour, which is preserved even when it is diluted with water, and brownish flocculi are formed in it. Hydrochloric acid dissolves it with a clear yellow colour. The acetic acid also dissolves it, but without change of colour. Colchicéine is soluble in ammonia, and crystallizes by evaporation in the air; and it dissolves in caustic potash. It is unalterable in the air; it has no effect upon turmeric paper or litmus paper; exposed to heat, it first softens and afterwards fuses at 155° (Cent.?) The elementary composition of colchicéine is C 62, 83+H 6, 60+N4, 19+O26, 38=100, 00.

XV. *On the Treatment of Strangulated Hernia by the Internal use of Belladonna.* (L'Union Médicale, January 27th, 1857.)

Dr. de Larne, of Bergerac, relates the following case, in which the employment of belladonna combined with the taxis succeeded in effecting the reduction of a strangulated hernia. An old woman, aged seventy, had suffered for about seven years from a crural hernia of the right side, which, however, did not generally give rise to inconvenience or pain. On rising from bed on the 17th of September, 1856, she experienced the symptoms of strangulation of the hernia; the belly was stretched, tympanic, and painful to the touch; there was repeated vomiting, most frequently stercoraceous; total absence of evacuation by the anus; small, weak pulse, without marked frequency; skin dry, moderately hot; respiration anxious; slight thirst; dislike of food: the tumour was resistent, of a violet hue, as large as a turkey's egg. All attempts at manual reduction having failed, the patient was ordered rest, low diet, a suitable position, cold water for drink, and a belladonna mixture. The latter preparation was composed of the watery extract of belladonna, 20 centigrammes; of syrup of orange flowers, 30 grammes; and of distilled water, 60 grammes. It was given in the dose of a teaspoonful every quarter of an hour, and was continued for nearly four days, when the obstacle having been sufficiently removed by the use of the belladonna, the hernia yielded readily to the taxis. The author of the communication observes, that during the administration of this drug a notable and real amelioration of the tumour was observed, although the strangulation continued; and that, notwithstanding the dose (which amounted altogether to one gramme and a half of the extract of belladonna), only a few alternations of delirium or sleep were induced, without any other toxical symptoms of importance.

XVI. *On the Therapeutical Applications of Glycerine. Supplemental Notes.* By Dr. W. Lauder Lindsay. (Edinburgh Medical Journal, April, 1857.)

In continuation of the remarks previously made by Dr. Lindsay on the therapeutical applications of glycerine, he has collected together a large amount of evidence in favour of this substance as a remedial agent in various affections, employed both internally and externally. As an internal medicine, it would appear to possess properties very similar to those of cod-liver oil, but it has the recommendation of being more pleasant to take; and in certain cases where the cod-liver oil was not tolerated, glycerine has been used as a substitute with the best results. It should be mentioned that the effects of glycerine vary considerably in proportion to the kind of article supplied, and that the specimens obtained from various sources are by no means of equal purity and value. As an external application, glycerine may be considered as a palliative, if not a specific, in a variety of skin diseases. The evidence adduced from the practice of several surgeons proves that it is a very valuable application in scalled head, in combination with hyposulphite of soda; in itch, combined with sulphur; in inveterate psoriasis; in pityriasis, lepra, lichen, eczema, impetigo, prurigo; in certain forms of lupus, and of strumous and syphilitic eruptions. As a solvent or excipient, or vehicle for pharmaceutical preparations, glycerine is now much employed; and Messrs. Price & Co., of London have prepared a series of medicinal compounds, in which glycerine is the solvent basis. It is much to be regretted that the price of glycerine is still so high as to preclude its general use among the lower classes.

HALF-YEARLY REPORT ON PHYSIOLOGY.

By Hermann Weber, M.D,

Licentiate of the Royal College of Physicians, Physician to the German Hospital.

I. On Food and Digestion.

1. Vernois and Becquerel: *Analysis of the Milk of the principal types of the Cow, the Goat, the Sheep, and the Buffalo.* (L'Union Médicale, t. xi. No. 26, 1857.)
2. Berthe: *On the Assimilation of different Oleaginous Substances.* (L'Union Médicale, t. x. No. 62, 1856.)
3. Corvisart: *On a function of the Pancreas which is little known: Digestion of Azotized Substances.* (L'Union Médicale, t. xi. No. 50, 1857.)
4. Colin: *On the Digestion and Absorption of Fats, without the Influence of the Pancreatic Juice.* (L'Union Médicale, t. xi. No. 50, 1857.)
5. Donders: *On the Absorption of Fat in the Intestinal Canal.* (Moleschott's Untersuchungen zur Naturlehre, vol. ii. p. 102, 1857.)
6. Kölliker: *Remarks on the Absorption of Fat in the Intestinal Canal; on the Existence of a Physiological Fat-Liver in Young Animals; and on the Functions of the Spleen.* (Verhandl. der Würtzburg. Gesellschaft, vol. vii. p. 174, 1856; and Schmidt's Jahrb., vol. xciii. p. 20, 1857.)
7. Von Wittich: *Contribution to the Doctrine of the Absorption of Fat.* (Virchow's Archiv, vol. xi. p. 37, 1857.)
8. Moleschott: *New Proof of the Entrance of Solid Particles into the Conical Cells of the Intestinal Mucous Membrane.* (Moleschott's Untersuchungen zur Naturlehre, vol. ii. p. 119, 1857.
9. Hollander: *Contribution to the Researches regarding the Entrance of Small Solid Bodies, from the Intestinal Canal into the Blood.* (Virchow's Archiv, vol. xi. p. 100, 1857. Extract from an Inaugural Dissertation.)

Vernois and Becquerel offer the results of the analysis of the milk of sixteen different breeds of cows, five of goats, one sheep, and one female buffalo. They infer from their observations that the composition of the milk varies considerably with the country in which it is examined; the country where an analysis has been made ought therefore always to be stated. 1. While the cows of Paris and its neighbourhood give 36 to 37 parts of butter to 1000, those of the Tyrol, Switzerland, Holland, and those of the Angus-race yield 70 to 98 parts. 2. There exists an antagonism between the richness of the milk in butter and albumen, and the richness in casein and sugar; a difference which is so marked, that it allows a division into butter-cows and cheese-cows (*vaches á beurre et vaches á fromage*). 3. An analogous difference the authors found in the milk of women and of sheep. Here, too, the greatest degree of variation is found with regard to the per-centage of butter. 4. One cannot, in an absolute manner, declare one kind of milk superior to another; but that if one breed may be preferable in a certain case on account of the larger quantity of butter, that of another in a second case on account of the greater richness in casein, that of a third be chosen for its proportion of sugar, &c. Thus, the Angus-breed yields most butter, the Norman-race most casein. 5. The quantity of food appears to be of considerable influence on the proportion of the different constituents; a large quantity seems to cause an increase of sugar and casein, a moderate quantity to induce an increase of butter and albumen. 6. The authors draw especial attention to the large per-centage of albumen (13 to 1000) in milk of goats, as also to the richness of the female buffalo, in solid substances in general, and in albumen (13) and butter (80) in particular. Finally, Vernois and Becquerel express the opinion, that the milk of nurses of different climates may offer similar differences as that of different races of cows, according to the differences in soil, in food, &c., corresponding probably to the varieties in character and customs of different nations.

The fact that some fatty substances, when eaten, are almost entirely excreted by the alvine dejections, while the amount of fat in the fæces does not become increased by the moderate consumption of other fats, led Berthé to the examination of the quantity of fat excreted with the fæces under the influence of various oleaginous matters administered to the same healthy subject in doses of from thirty to sixty grammes per diem. These experiments led the author to the inference, that there is a point of saturation of the body for most of the fatty matters, from whence almost the whole amount of fat ingested passes unassimilated through the intestinal canal. This point is arrived at after about twelve days with olive and almond oil, and almost all vegetable oils; after about a month with butter, whale oil, and English purified cod-liver oil (*huiles de baleine et de foie de morue Anglaise, décolorées ou lavées*); while the use of the pure brown cod-liver oil (*huile de foie de morue brune et pure*) did not lead to an increase of fat in the fœces, even when its use had been continued for more than a month. Berthé therefore proposes a division of the fatty substances into three groups:—1. Substances of difficult assimilation (olive, almond oil, &c.); 2. Assimilable substances (butter, whale oil, English cod-liver oil, and probably all animal fats); 3. Very assimilable substances (brown and pure cod-liver oil).

Corvisart confirms the observation of Purkinje and Pappenheim regarding the existence of a substance in the pancreas (pancreatine) endowed with the virtue of dissolving azotized constituents of food. The author contends that the pancreatic juice exercises its influence only on that part of the nitrogenous substances which has escaped the action of the gastric juice, producing a kind of albuminose similar to that resulting from the influence of the gastric juice. The reaction of the surrounding fluid, whether alkaline, neutral, or acid, is of no importance regarding the performance of this function of the pancreatic juice. Corvisart further states that the active principles of the gastric and pancreatic juice (pepsine and pancreatine) counteract each other when mixed together; that in the normal condition this is prevented, *a*, by the pylorus; *b*, by the gastric digestion itself, through which the pepsin is consumed; *c*, by the admixture of the bile, which destroys the power of the pepsin.

Colin's experiments regarding the absorption of fats are made on cows, in which he considers the formation of pancreatic fistula, and the opening of the pancreatic duct, as easy operations. Comparing the contents of the thoracic duct obtained from cows in which a pancreatic fistula has been established, with that of others where the pancreatic juice was not removed, the author arrived at the inference, that after the elimination of the pancreatic juice, the fat is digested and absorbed in the same manner as in the normal condition. It will be remembered that the experiments of Lenz, Herbst, and others led to the same result.

Donders, too, found that absorption of fat takes place without the influence of the pancreatic juice; that, however, this process is assisted by the emulsifying power of the pancreatic fluid. Donders further refers, respecting the absorption of fat, to the results of Von Wistinghausen's experiments, that the passage of fat through animal membranes is much facilitated by their previous impregnation with bile. Microscopic observation leads the same author to consider the presence of canaliculi in the thickened walls of the epithelial cells of the intestines, as described by Funke and Kölliker, as highly probable.

Kölliker proved, by injection of oil into the rectum of a young cat, the possibility of the absorption of fat through the epithelial cells of the colon. He further constantly found fat in the epithelial cells of the stomach of sucking animals, but no white chyliferous vessels. He corroborates Brück's observation, that fat is absorbed by the follicles of Peyer's glands, but does not offer decided proof in favour of the absorption of fat by bloodvessels.

Hollander repeated at Dorpat, under the superintendence of Bidder, some of the experiments of Marfels and Moleschott. The authors injected in many instances the defibrinated blood of oxen, calves, and sheep, through an elastic tube, into the stomach of frogs; they continued doing this in the same animals for several days running, once or twice daily, but they never succeeded in finding any of the injected globules in the blood of the frogs. The essay shows that the experiments were performed with much care, but the negative result does not offer a valid objection to the permeability of the epithelial cells of the small intestines, as it appears more than probable that the blood-globules had undergone a considerable alteration under the influence of the gastric juice, before they reached the cavity of the jejunum and ilium.

Donders, likewise, in a great number of experiments, never succeeded in obtaining a proof for the passage of solid molecules through the epithelial cells of the intestinal tube.

Moleschott, on the other side, has repeated many of his former experiments, in order to find under which circumstances the entrance of solid particles into the cells takes place. Although he again obtained many positive results, principally when he had employed recently-precipitated particles of Berlin blue, yet he has hitherto not been able to ascertain which are the most favourable circumstances, or why the absorption takes place in one case, and not in several others.

Von Wittich contributes an observation of great importance regarding the question at issue. A rabbit killed (by bleeding) six hours after it had been bitten in the back by a dog, and thus deprived of the use of his posterior limbs, exhibited the chyliferous vessels, originating from the lower half of the ilium, filled with an entirely red fluid. This redness was shown to be caused merely by the admixture of the red blood-globules in a large proportion, not by that of colouring matter. The corresponding part of the intestinal tube contained mucus mixed with blood, after the removal of which the mucous membrane manifested the appearance of fine red dots, which, by means of a lens, were recognised as villi filled with blood. Von Wittich does not hesitate to explain this state of things by adopting the view, that the blood-globules pass as such through the epithelial cells and the parenchyma of the villi into the chyliferous vessels; he is of a similar opinion regarding the entrance of fat and other minutely-divided solid substances into the absorbent vessels. After various unsuccessful attempts, the author succeeded also, by means of the experiment, in proving the entrance of blood-globules into the chyliferous vessels of the cæcum, five hours after he had injected blood into that portion of the intestinal canal. Von Wittich agrees with Brücke and Moleschott, in opposition to Hyrtl and others, in attributing to the contraction of the muscular coats of the intestinal tubes much influence on the absorption of substances contained within its cavity.

II. Blood; Respiration; Circulation.

1. Zimmermann: *On Fibrin, and the Cause of its Coagulation.* (Moleschott's Untersuchungen zur Naturlehre, vol. i. p. 133, 1856.)
2. Harley: *On the Chemical Changes of the Blood during Respiration.* (Virchow's Archiv, Band xi. p. 107, 1857.)
3. Bernard: *On the Elimination of Sulphuretted Hydrogen through the Surface of the Lungs.* (Archiv. Génér. de Méd., Fevr. 1857.)
4. Hoppe: *On the Influence of Carbonic Oxide on Hæmato-globulin.* Preliminary communication. (Virchow's Archiv, Band xi. p. 288, 1857.)
5. Hoppe: *On the Influence exercised by Change of the Pressure of the Air on the Blood.* (Müller's Archiv, p. 63, 1857.)
6. Valentin: *Contribution to the Knowledge of the Hybernation of Marmots.* (Moleschott's Untersuchungen zur Naturlehre, Band i. p. 206, 1856.)
7. Aberle: *On the Measurement of the Diameter of Arteries in Living Man.* (Dissert. Inaugur., Tübingen, 1856.)
8. Wagner R.: *On the Observation of the Circulation of the Blood and the Locomotion of the Chyle in Warm-blooded Animals.* (Göttinger Gesels. der Wissenschaft., No. 13, 1856; and Schmidt's Jahrb., Band xciii. p. 18, 1857.)
9. Kunde: *Physiological Experiments on Apparent Death.* (Müller's Archiv, Jahrgang, 1857, p. 280.)

Regarding the nature and origin of fibrin, Zimmermann repeats that he considers it as an excrementitious substance, exhibiting a certain stage in the metamorphosis of proteinaceous bodies, not any longer fit to serve in the nutrition of the organism. A small quantity of fibrin is regarded as a necessary constituent of the blood, but "the healthier the subject, the smaller the quantity of fibrin." By further oxidation, fibrin is, in the normal state, transformed into other excrementitious circumstances. "Sometimes, however," the author says, "the formation of fibrin takes place in so tumultuous and rapid a manner, that the transformation into excrementitious substances cannot take place; whence arises exudation of the accumulated fibrin, a process through which the blood is, for the time, purified of this substance. Later, when the cause for this abnormal crisis has ceased, the fibrin may again be absorbed and otherwise excreted. Such is the case in pleuritis, pneumonia, &c." (p. 181.)

The coagulation is caused, according to Zimmermann, by the commencement of decomposition or putrescence; it is accelerated by the addition of substances in the state of transposition of elements, as pus or ichorous fluid from gangrenous wounds (Nasse), in the whole by all influences favouring putrescence; it is retarded, on the contrary, by such influences as retard or prevent decomposition. The putrescence causing the coagulation of fibrin does not take place in the fibrin itself, but in other constituents of the blood, and principally in the red blood-globules. The chemical constitution of the fibrin passing into the solid state is regarded as remaining unchanged, with the exception of transposition of its atoms, effected by the contact with a substance in the state of decomposition, analogous to the transformation of casein by the action of rennet.

Harley describes some valuable experiments which he performed at Heidelberg in the laboratory of Professor Bunsen, and with the assistance of that distinguished chemist. After having repeatedly shaken a certain quantity of blood with renewed portions of atmospheric air, until it was saturated with oxygen, and had given off as much carbonic acid as possible, he placed the blood thus treated with an equal volume of atmospheric air, in an hermetically-closed vessel, shook it frequently, and examined the air after it had been for a varying space of time in contact with the blood. In this manner the author found the air, after twenty-four hours' contact with fresh blood of oxen, to have lost 10·54 per cent. oxygen, and gained 5·05 per cent. carbonic acid. In another experiment with fresh arterial calves' blood, the minus of oxygen in the air employed was 9·63 per cent., the surplus of carbonic acid, 5·96 per cent.

In both experiments, we meet with a greater loss of oxygen than is accounted for by the surplus of carbonic acid: the author inclines to the view that this remnant of oxygen is spent in the formation of water. Amongst Harley's experiments respecting the share which the various constituents of the blood exercise on the atmospheric air, we observe that those performed with *fibrin* lead to a result similar to that described by Scherer,* i.e., that fibrin has the power of absorbing a considerable quantity of oxygen, and of giving off carbon or carbonic acid. *Albumen*, too, was found to be possessed of the same property; but the quantity of oxygen absorbed, and that of carbonic acid excreted, are not so great as is the case in the experiment with fibrin. The comparative experiments with the *coagulum* and the *serum* of blood, manifest that the absorption of oxygen and the excretion of carbonic acid are larger in the former than in the latter case. *Hæmatin* was observed to exercise on the surrounding air the same influence as that ascribed by Scherer to the urohæmatin—namely, to deprive the air of a large amount of oxygen, and to enrich it with carbonic acid—a virtue which Harley attributes to the colouring substances in general, as well in the vegetable as in the animal economy.

Although the author's researches on the subject are not yet brought to an end, yet the experiments before us make it probable (in opposition to the view formerly maintained by Magnus) that a part of the oxygen admitted during respiration enters at once into a chemical combination with the various constituents of the blood.

The circumstance that *sulphuretted hydrogen* can be ingested in considerable quantity into the digestive canal without producing symptoms of poisoning, while the admixture even of so small a proportion as one part to 800 parts of air is sufficient, when inhaled, to kill a middle-sized dog, led Bernard to search, by means of experiments, for the cause of this remarkable difference. The injection of sulphuretted hydrogen gas into the jugular vein was rapidly followed by several deep inspirations and expirations, through which a large quantity of sulphuretted hydrogen was eliminated (proved by testing with acetate of lead): this process of elimination being completed within a few seconds. Similar was the effect when a concentrated solution of the gas was injected into the jugular vein. The ingestion of such a solution into the stomach was likewise followed by exhalation of the gas; but the interval between the ingestion and the exhalation was considerably greater, and the process of elimination lasted longer. Injection of the solution into the rectum led to an analogous result, sixty-five seconds having elapsed before the first traces of the sulphuretted hydrogen were discovered in the expired air.

The author alludes to the value of such experiments for the determination of the celerity of absorption, circulation, &c. Thus the injection of the solution into the jugular vein led to dark spots on the test paper after three seconds; that into the crural vein only at the end of six or seven seconds; showing the greater space of time required for the transmission of the blood through a greater distance. The question, whether the whole amount of the sulphuretted hydrogen injected is inhaled through the lungs, Bernard is inclined to deny, as the injection of a small quantity into the arterial system of a dog did not cause the appearance of any gas in the expired air. This, however, took place when another injection was made soon after the first.

The observation made by Dr. Wolff, of Waldenburg, in Silesia, that the blood of labourers who had perished in coal-mines, and that of rabbits killed by means of carbonic oxide, are bright red, induced Hoppe to examine the change caused in the defibrinated blood of oxen by the admixture of carbonic oxide. As well the arterial as also the venous blood of oxen, when shaken with this gas becomes bright red—the redness differing, however, from that of the normal arterial blood by being possessed of a violet hue. Neither the action of carbonic acid, nor that of atmospheric air, nor that of commencing decomposition, effect a change in the colour produced by the carbonic oxide. From these facts, the author infers that the gas is not only absorbed by the blood, but enters into a chemical combination with the hæmato-globulin, by which circumstance the blood-globules lose their virtue of being the bearers of oxygen.

Hoppe exposed various animals to a considerably *diminished atmospheric pressure*, by means of the air-pump. The symptoms exhibited by different classes of animals were very different. Frogs and a blind-worm bore the reduction of pressure to 30 millimetres mercury, and below this, without dying; swelling up of the whole body, expulsion of gas through mouth and anus, and syncope, were the principal symptoms. Rats and young cats became suddenly convulsed at a pressure of 50 millimetres; the convulsions were soon followed by syncope. Admission of air in this state restored them to apparent health, but these animals died at the reduction of the pressure to 40 millimetres. In guinea-pigs, sudden diminution of pressure to 80 millimetres caused convulsions and syncope. Two swallows became convulsed when the pressure was not lower than 130 millimetres, and died at the reduction to 125 millimetres. All the animals killed by low pressure exhibited small bubbles of air in the large vessels and

* Scherer, in Liebig's Annalen, vol. xl.

in the right ventricle, while no air was found in the bloodvessels of frogs examined in the state of syncope. The author directs attention to the facts, that birds die before the pressure is reduced to the boiling point of the blood, that mammalia die at a pressure slightly exceeding the boiling point, while amphibia survive the reduction to the boiling point. Hoppe ascribes the symptoms caused by low pressure, not to the want of oxygen in the blood, but to the development of gas within the vessels: the sudden death is considered as the effect of the blocking-up of the pulmonary capillaries by this gas. The point at which the development of gas takes place, appears to depend on the preceding degree of pressure on the temperature of the animal, and the greater or smaller power possessed by the blood of absorbing gases—which power is probably dependent on the proportion of blood-globules.

Regarding the influence of *increased pressure*, the author offers only a few experiments. An increase of 150 millimetres (i. e., to about 908 millimetres) was borne by a pregnant rat without any symptoms of uneasiness. *A priori*, it is to be supposed that the blood must show an increased power of absorbing gases, augmented heat, &c. Sudden diminution of the previously-increased pressure of air probably leads to development of gas within the vessels, and the pathological phenomena just mentioned. Hoppe is inclined to attribute cases of sudden death without anatomical lesion, observed in coal-mines, to this cause.

If we compare with these results Valentin's observation on marmots during the state of hybernation, we find that this author, too, witnessed symptoms of great uneasiness when the *reduction of the pressure* was carried to below 10 millimetres, which was equal to about $\frac{1}{71}$ of the pressure of the surrounding air; but; after some time, the animals became again quiet, and continued to sleep. On further reduction to $\frac{1}{91}$ of the external pressure, the symptoms exhibited by the animal, after having been for more than two hours under this influence, were such, that though the sleep was not interrupted, yet Valentin considered it necessary to admit air in order to save the animal. In another experiment, when the air was extenuated to 4·1 millimetre—i. e., to $\frac{1}{175}$ of the external pressure—a stream of blood rushed from the animal's nostrils, but by the admission of air of the usual density the animal recovered quickly. The appearance of a greater quantity of moisture at the nostrils was a regular phenomenon produced by the more rapid extenuation of the surrounding air. Valentin particularly points out the difference in the effect of a rapid and gradual diminution of pressure, the latter being borne much better than the former. The same author has also frequently subjected sleeping marmots to *increased pressure*. When the air is pumped in slowly, the pressure could be increased to that of three atmospheres (2160 millimetres) without awaking the animals, or producing any striking symptoms; rapid pumping in of air caused the animals to awake for a short time; sudden emission of the condensed air had the same effect, and effected in all animals a profuse discharge of mucous fluid from the nostrils, and in one case hæmorrhage from the same parts. The record of the other contents of Valentin's valuable paper on the hybernation of marmots we must defer to the next Report.

Aberle measured with Vierordt's instrument, and under the superintendence of that physiologist, the diameter of the radial artery, on several persons, and at different periods of the day. He found that the diameter of the artery is larger in the afternoon (after dinner) than in the forenoon. The average diameter, in different persons, varied between 2·09 millimetres (short stature) and 3·18 millimetres (tall people); in the forenoon we find the figures, 1·74—2·92 millimetres; in the afternoon, 2·45—3·44 millimetres.

Wagner recommended, as the best object for the observation of phenomena connected with the circulation, the mesenteric vessels of young cats or rabbits under the influence of ether, as offering a much more distinct view of the capillary circulation than other objects generally used. The author constantly saw in his examinations the following three different formative elements: *a*, red globules, principally in the more rapid central part of the stream; *b*, colourless granular globules, of much slower movement, in the peripheric part of the stream, where they sometimes considerably accumulate, through the diminished power of the heart; *c*, small, sometimes aggregated, strongly refractive globules, much like fat-granules. The observation relating to the colourless globules shows, that the estimation of their proportion in a drop of blood, abstracted from a certain part, may lead to erroneous conclusions, as their quantity may vary, in the same vessel at different times, with the variation of pressure, &c. The turgid chyliferous vessels of the mesentery exhibit only very small molecules, here and there larger fat-globules, and always a few red blood-discs, the progressive movement of which could be distinctly traced. This movement was not continuous, but periodic. The contraction of the villi and of the intestinal canal, and other movements of the animal, appeared to exercise influence on this motion; the acts of respiration did not seem to promote it. Sometimes, in the course of the observation, the chyliferous vessels became almost filled with blood-globules, which the author is inclined to explain by the supposition of the rupture of small bloodvessels within the villi.

Kunde adopts Bichat's view regarding the three principal organs from which death may originate, substituting, however, the medulla oblongata for the brain. Apparent death may be artificially produced from every one of these organs. The author's experiments relate, however, principally to apparent death from the heart. The method adopted by him consists simply in compression of the atria of the heart between the fingers, which can easily be effected in young cats, dogs, rabbits, and in frogs, without any lesion of the thorax. If the heart of a young cat is compressed, the respiratory movements continue for a time, the diaphragm contracts, and the animal cries; soon the mucous membrane of the mouth and nose becomes blue, and then completely pale; after this, the respiratory movements cease, the pupils become dilated, the revolutionary and reflex movements cease. If the compression is at this period discontinued no sounds of the heart are perceived; but soon the first sound reappears, and then the second, and the heart resumes its functions as usual. After this, a respiratory movement is observed preceding all the other motions of the muscles of the trunk or limbs; later, the mucous membranes regain their colour; finally, the animal rises, moves first in an unsteady manner, but by degrees recovers completely. In the frog, the author witnessed the cessation of the reflex movements at first in the posterior extremities; the lymph-hearts cease last; the animal appears without life; the capillaries contain only a small quantity of blood; the veins are gorged. Further experiments, performed on frogs under the influence of strychnia, show that the tetanic convulsions become suspended as soon as the heart is compressed, and reappear after the discontinuance of the compression. The author concludes from this fact, that paralysis of the nerves may be produced by a mere change in the tension of the vessels. He is further inclined to confirm Bichat's proposition, that an important influence is exercised on the brain by the motion incessantly imparted to it through the contractions of the heart. In favour of the latter inference, Kunde adduces Heidenhain's* discovery, that a tetanic state of a nerve may be caused by a continuous repetition of mechanical concussion; and points to the constantly-repeated shocks applied to the nervous centres by the passage of the blood-globules through the capillaries.

In the course of these experiments, Kunde was enabled to confirm in many points Kussmaul's observation regarding the influence of compression of the bloodvessels and the heart on the state of the iris. Contraction of the pupil was the first phenomenon, which was soon followed by rotatory movements of the bulb, anæmic state of the vessels of the iris, dilatation of the pupil, slight convulsions, and exophthalmus, with complete dilatation of the pupil. Suspension of the compression is followed only after fifteen to twenty seconds by gradual contraction of the pupils. Dilatation, however, was the only constant phenomenon, and this is the regular consequence of diminished pressure from the heart, whatever may be the cause of the latter.

III. Ductless Glands.

1. Kölliker: *On the function of the spleen.* (Loc. cit., sub. i.)
2. Iaschkowitz: *Contribution to the Experimental Pathology of the Spleen.* (Virchow's Archiv, Band xi. p. 235, 1857.)

Kölliker's researches confirm the view, which is being more and more generally adopted, that the colourless blood-globules are formed in the spleen; and partly in that organ itself, parly in the liver, partly in the blood, are transformed into red globules.

Iaschkowitz studied the effect produced by section of the plexus lienalis on the structure of the spleen. Nine experiments performed on dogs, by section either of the whole plexus lienalis, or only of the superior or inferior half of it, show that this section causes in the corresponding part of the splenic tissue an altered state—viz., congestion of blood, softening, tension of the capsule, effusion of coagulating blood in large quantity through incisions into the capsule. We see, therefore that the result of these experiments is analogous to that of section of the sympathetic nerve of the neck; and the author draws the inference, that mere nervous disturbance, without pathological change in the composition of the blood, may cause an alteration of the tissue of the spleen. Further experiments must elucidate the influence exercised by such alterations of the spleen on the mixture of the blood.

The same author observed, during these experiments, contraction of the spleen in the direction of the longitudinal axis, as well under the influence of galvanic irritation as under that of the atmospheric air.

IV. Secretion; Excretion; Metamorphosis of Matter.

1. Bernard: *On the Influence of Alcohol and Ether on the Secretion of the Intestinal Tract, the Pancreas, and Liver.* (Gaz. de Paris, No. 19, 1856: and Schmidt's Jahrb., vol. xciii. p. 24, 1857.)

* Heidenhain: Physiologische Studien. Berlin, 1856.

2. Dornbluth: *Observations on the Mechanism of the Secretion of Urine.* (Zeitsch für Rat. Med., vol. viii. p. 174, 1856; and Schmidt's Jahrb., vol. xciii., p. 275, 1857.)
3. Beigel: *Researches on the Quantity of Urine, Urea, &c.* (Wien, 1856; and Schmidt's Jahrb., vol. xcii. p. 5, 1856.)
4. V. Franque: *Contribution to the Knowledge of the Excretion of Urine in Man.* (Dissert. Inaugur., Würzburg, 1855.)
5. Neubauer: *On the Decomposition of Uric Acid in the Animal Body.* (Annal. der Chem. und Pharm., vol xcix. p. 206, 1856; and Schmidt's Jahrb., vol. xciv., p. 7, 1857.)
6. Cloetta: *On the Presence of Inosit, Uric Acid, &c., in the Animal Organism.* (Annal. der Chem. et Pharm., vol. xcix. p. 289, 1856; and Schmidt's Jahrb., vol. xciv. p. 9, 1857.)
7. Delore: *On the Formation of Sugar in the Liver.* (Gaz. Méd. de Lyon, No. 2, 1856; and Canstatt's Jahrsber. der Physiologie, p. 160, 1857.)
8. Chauveau: *New Researches on the Question regarding the Formation of Sugar.* (Compt. Rend., May, 1856; and Canstatt, l. c. p. 162.)
9. Hensen: *On the Formation of Sugar in the Liver.* (Vernhandl. der Würzburger Gesellsch., vol. vii. p. 219, 1856.)
10. Hensen: *On the Formation of Sugar in the Liver.* (Virchow's Archiv, vol. xi., p. 395, 1857.)
11. Berard: *On the Place of the Production of Sugar in the Organism.* (L' Union Médicale, tome xi. No. 61, 1857.)

Bernard injected between five and six cubic centimetres (*i.e.*, rather more than $\frac{3}{10}$ of a cubic inch) of alcohol, diluted with an equal quantity of water, into the stomach of a dog, and found a few minutes later, when the animal was killed, the stomach filled with fluid exhibiting the characters of gastric juice, and a considerable quantity of the secretions of the pancreas and the intestinal glands in the cavity of the digestive canal.

In order to learn the influence of alcohol on the glycogenic function of the liver, the author chose two dogs that were as much as possible in the same condition: after having deprived them of food for an equal space of time, he killed one of them immediately, the other after repeated injections of alchohol into the stomach. The liver of the former contained only a small, that of the latter a large, quantity of that insoluble substance which is afterwards transformed into sugar. The action of ether was found similar to that of alcohol, only more powerful.

The results of Valentin's experiments—that the quantity of albumen passing over from a solution of that substance, through a membrane into water, according to the laws of endosmosis and exosmosis, is increased or decreased by the greater or smaller degree of pressure acting on the solution—leads Dornblüth to the inference, that the absence of albumen in the urine indicates a low pressure acting on the secreting vessels of the kidneys. The circumstances leading to albuminuria are such as cause retardation in the return of the venous blood; and, through this, increased pressure on the secreting vessels—as contraction of the renal veins, tumours of the liver pressing on the vena cava, valvular diseases of the heart, &c. The pressure of the blood in the Malpighian bodies, the author argues, cannot be great, as the diameter of the vasa efferentia bears to that of the collected capillaries the proportion of 10 to 18, which must be connected with diminution in celerity and pressure. The impediment caused by the return of the blood from the capillaries into the vasa efferentia is rendered smaller by the acuteness of the angles under which the transit takes place, by the communication of the vasa efferentia with wide meshes of capillaries, as also by the diminution of the quantity of blood, in consequence of the abundant secretion in the Malpighian bodies. This reasoning is borne out by the result of Ludwig's experiments, showing the pressure in the real veins to be equal to that in the jugular veins. The prominent points in the mechanism of the secretion are, according to the author's view, that in the Malpighian bodies, under a low pressure, a diluted transudation takes place, carrying with itself the easily diffusible substances that are not retained by combination with proteinaceous constituents; the amount of solids in the transudation depends on the diffusive faculty and the relative quantity of the substances present in the serum of the blood. In the urinary tubuli an endosmotic interchange takes place between the transudation of the Malpighian bodies and the blood circulating in the capillaries round the tubuli; one of the principal results of this interchange being a transition of water from the fluid in the tubuli into the blood. The quantity of urine depends principally on the process in the Malpighian bodies; the larger the amount secreted by these, the more accelerated will be the stream in the tubuli, the less the time for absorption of water by the blood.

Beigel gives the result of his observations on ten healthy male and six healthy female individuals; the age of the former was between twenty and thirty years, height 169 to 176 centimetres, weight 74 to 7[illegible] kilogrammes; age of women, nineteen to thirty years, height 165 to 170 centimetres, weight 63 to 67 kilogrammes; diet of both sexes, mixed. Beigel adds remarks on the influence of very liberal and low diet, of exercise, and of several medicinal agents. Of similar nature are Von Franque's observations made on his own person, being in the twenty-second year, 173·8 centimetres high, weighing 62·64 kilogrammes. Both authors agree in corroborating the experience of other physiologists:—1st. That an increased ingestion of azotized food leads rapidly to increased egestion of urea through the urine. 2nd. That diminished ingestion of azotized food does not lead to a corresponding diminution in the excretion of urea; that continued abstinence from nitrogenous food is followed only after some time by a decrease of the normal quantity of the urea. 3d. That the excretion of urea is much augmented by bodily exercise.

Beigel's examinations lead him, in addition, to the assertion, that Bischoff's inference regarding the coincidence of a high specific gravity of the urine and a large proportion of urea, is correct with reference to healthy male individuals, but not equally so to women, whose urine was found to contain in the average less urea than that of men, in spite of the high specific gravity. Another proposition arrived at by Beigel, as the result of his experiments, is, that increased metamorphosis of matter need not be connected with increased temperature, nor diminished metamorphosis, through insufficient ingestion of food, with decreased temperature, provided the abstinence be not continued too long.

Neubauer, after having ascertained the composition of the urine of rabbits living on their usual food, and especially the absence of uric acid, added between 2 and 3 grammes of uric acid to their daily allowance of victuals; the principal alteration thereby produced in the urine was an increase of urea from 1·3 grammes to 2, to 2·5, and even 4·2 grammes, which increase disappeared almost immediately when the uric acid was left off. Other experiments of a similar nature led to an analogous result. Neubauer infers from this, that uric acid is decomposed within the body into urea and carbonic acid. When a larger quantity of uric acid was given, a small portion of it was excreted as uric acid, and perhaps also in the shape of oxalic acid.

Cloëtta's memoir forms a contribution to our knowledge of the metamorphosis of matter, and particularly with respect to the questions, whether certain products of decomposition are peculiar to certain organs and tissues, and which are the products met with in all, or almost all, the organs. Referring to the essay itself for the method of examination, we mention only—1. That the *lungs* of oxen yielded uric acid, inosite, taurin (which Cloëtta considers to be the substance mistaken by Verdeil for pulmonic acid), and leucin, but no glycin and tyrosin; 2. The *kidneys* of oxen contained, according to the first analysis, a large amount of inosite, cystin, and a small proportion of either xanthin or hypoxanthin, the quantity of the latter having been too minute to allow a distinction between these two bodies. A second analysis of the kidney of an ox manifested the same indefinite body; no cystin, but a large quantity of taurin; a circumstance which suggests the hypothesis, that these two bodies (viz., cystin and taurin) may sometimes take each other's place; 3 Neither in the *urine* of cows, nor in the normal urine of man, was inosite found; it was discovered, however in that of a woman suffering from Bright's disease; 4. In the spleen the author proved the presence of inosite (in a similar quantity as in the lungs, of uric acid, hypoxanthin, leucin, and two other substances, the nature of which was not ascertained. Scherer's lienin is considered by Cloëtta as identical with inosite; 5. The *liver* of oxen contained as well uric acid as inosite; 6. Only a single examination of the blood of the jugular veins was performed, which did not show the presence of either of the two last-named substances.

Délore defends the correctness of Bernard's inferences regarding the formation of sugar in the liver, against the views propounded by Figuier.* He further corroborates the fact observed by Bernard,† that formation of sugar takes place in the liver, even after this organ has been most carefully washed out. He shows by experiment that the transformation of the glycogenic substance into sugar is not influenced either by electricity or by an atmosphere of pure oxygen, but that it is arrested by an atmosphere of hydrogen. In one of Délore's experiments, the formation of sugar lasted six days.

Chauveau communicated to the Académie des Sciences a series of experiments, performed as well on herbivorous animals provided with their usual food, as also on dogs fed exclusively on meat. He found sugar in the larger vessels even after several days' (one to six) abstinence from food; the arterial blood of the same animal contained the same proportion of sugar from whatever vessel it was taken; the veins of the various parts of the body, too, with the exception of the vena hepatica and the lower part of the vena cava, and of the vena portæ during the digestion of food, rich in sugar or starch, exhibited no remarkable difference regarding the per-centage of sugar in their blood. The following are the inferences arrived at by the author: 1. There does not exist any essential diversity between herbivorous and carnivorous animals regarding the sugar contained in their nutritive fluids, but the quantity of sugar is

* See British and Foreign Medico-Chirurgical Review, No. 33, p. ~~262~~ Jan. 1856.
† Ibid., No. 35, p. 232. 1856.

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rather larger in the former than in the latter; 2. The sugar contained in the blood of the right heart is not destroyed in its passage through the lungs, but is transmitted unchanged into the left heart, and from thence into the aorta; 3. A certain amount of the sugar of the arterial blood disappears during the circulation through the capillaries, but part of this returns through the lymphatics to the right heart; 4. The large quantity of sugar in the blood of the hepatic vein, contrasted with its absence in that of the portal vein of animals deprived of food, or exclusively fed on meat, is a certain proof in favour of the formation of sugar within the liver.

Hensen's memoirs contain the results of his researches, made at Würzburg, in Scherer's laboratory. By the former of the two, the author does not only corroborate Bernard's discovery, already mentioned, regarding the presence of a glycogenic substance within the tissue of the liver, but he throws further light on the nature of this substance by the observation, that saliva and pancreatic extract materially accelerate the transformation into sugar. The experiments made, with the view to examine whether, perhaps, the ferment contained in the pancreatic juice is absorbed by the portal vein, and thus carried into the liver, do not allow of any decided conclusion.

In the second memoir, Hensen claims the merit of having isolated, independently of Bernard, the glycogenic substance of the liver. It appears certain that he has exhibited this substance before the Naturwissenschaftliche Gesellschaft, at Würzburg, in December, 1856, and again in Virchow's and Hoppe's Pathological Institution on the 1st of April, 1857; but the great French physiologist has the advantage in his favour of having first published an account of the nature of this substance, and of the manner in which it is to be obtained. As we have not yet received Bernard's publication, contained in the 'Gazette Medicale' of March 28th, we will defer our communication, as well on Bernard's as on Hensen's paper, to the next Report.

Bérard endeavours to disprove the correctness of Bernard's view, contained in the following words: "Le foie de l'homme, à l'exception de tous les autres tissus du corps, renferme de la matière sucrée." Bérard found sugar in the chyle of a bull fed exclusively on meat; he found this not only in the fluid of the thoracic duct, but also in the contents of a large chyliferous vessel situated on the mesenteric artery, thus showing that the sugar of the fluid examined was not derived by communication with the lymphatics of the liver. Bérard infers from this observation, that the sugar is not *exclusively* formed in the liver, and proposes, for further investigation, the questions,—whether not independently of the liver, sugar is constantly being formed in all parts of the body, and conveyed through the lymphatics to the centre of the circulation; and whether there does not exist, besides this constant production of sugar, another one of an intermittent, but much more active, nature under the influence of digestion.

V. Nervous System.

1. Chauveau: *New Experimental Investigations on the Properties of the Spinal Marrow.* (L'Union Méd., Nos. 61, 62, 66; 1857.)
2. Kölliker: *On the Vitality of the Nerve-Fibres of Frogs.* (Verhandl. d. Würzb. Gesellschaft., vol. vii. p. 145, 1856; and Schmidt's Jahrb., vol. xciii. p. 145, 1857.)
3. Flourens: *On the Sensibility of the Dura Mater, the Ligaments, and the Periosteum.* (L'Union Méd., tome xi. No. 53, 1857.)
4. Samuel: *On the Extirpation of the Plexus Cœliacus.* (Wien. Med. Wochenschrift, No. 30, 1856; and Schmidt, vol. xciii. p. 146, 1857.)

Chauveau, the distinguished professor at the Veterinary College at Lyons, whose observations on the movements and sounds of the heart we have related in the last Report on Physiology, has recently communicated to the Académie des Sciences the results of his experiments, performed on more than a hundred horses, asses, and mules, as also on many dogs and rabbits, regarding the nature of the spinal marrow. Although we have not yet before us the conclusion of the author's lectures, we will give the principal inferences as far as we are acquainted with them. 1. The *grey substance* of the spinal marrow is the conducting organ of the reflex phenomena. The posterior, as well as the antero-lateral white columns, may be dissected without the loss of the reflex function; but as soon as the grey substance of any part of the spinal marrow is thoroughly destroyed, no reflex action is observed to transgress that point, either from above downwards, or in the opposite direction. Thus, for instance, the grey substance having been destroyed in the middle of the dorsal portion of the spinal marrow, pricking of the anterior limbs would cause reflex action in those limbs themselves, but none in the posterior limbs; and pricking of the posterior limbs would never cause any reflex motion in the front part of the body—i.e., in that part which is provided by the portion of the spinal marrow above the section of the grey substance. 2. The *grey substance* of the spinal marrow has nothing to do with the transmission of peripheric impressions to the encephalon. Thus this substance may be completely destroyed in the cervical portion, and yet the animal retain its sensibilisy undisturbed. 3. The *white columns* of the spinal marrow are the means of conveying the sensitive impressions to the brain. 4. The *posterior* of these columns do not form the principal organs for the transmission of sensitive impressions.

It will be seen from this preliminary report, that Chauveau is in opposition to Brown-Séquard in a most important point—namely, in denying that the grey substance conducts the sensitive impressions. Chauveau explains this discrepancy between Brown-Séquard's results and his own, by the supposition that Brown-Séquard has interpreted reflex motions as signs of pain. At the same time it will be observed, that our author is in accordance with the just named physiologist on a point of not less importance—viz., in asserting that our views regarding the posterior columns, as fulfilling the function of transmitting sensitive impressions to the brain, are entirely erroneous.

Kölliker gives the results of his experiments on the influence of various solutions and fluids on the irritability of the nerves of frogs. 1. In water, and in diluted solutions of the salts of alkalies, as also in those of various indifferent organic substances, as sugar, urea, and albumen, the nerves swell and lose their irritability within from one to three hours. 2. In solutions of these substances, of a certain degree of concentration, the nerves do not swell, and retain their irritability for a long time. 3. In still more concentrated solutions they shrink and die off more or less rapidly. 4. The degree of concentration keeping up the irritability longest varies in different salts. Thus culinary salt, in a solution of one-half per cent., preserves the irritability for twenty-five hours; in a solution of nine per cent. for an hour. 5. The application of some salts, in solutions of a certain strength, causes convulsions, and even tetanus (culinary salt, in solutions of 4—5 per cent., convulsions; of 20—30 per cent., tetanus). 6. Nerves that have lost their irritability in water or weak solutions, regain it in stronger solutions. 7. Nerves deprived of irritability by stronger solutions regain it by the application of water and weak solutions. 8. Nerves allowed to become dry (a process accompanied by active contractions of the respective muscles), may, by means of water, be restored to irritability after having completely lost it. 9. The author concludes that the neurine is not possessed of high physiological importance, as even after its coagulation the irritability of the nerve-fibres may continue. He considers himself, therefore, entitled to the inference, that the axis-cylinder is the only conducting part of the nerve-fibre. He adduces, however, no decided proof that the neurine has been really coagulated in those fibres which retained their irritability.

While Haller, after numerous experiments, considered the *dura mater* as perfectly insensible, Flourens found it, in the normal state, likewise so, with the exception of dogs, in whom he found it sometimes to be possessed of sensibility; but in the state of irritation and inflammation, the latter author describes the dura mater as constantly highly sensitive, while the layer of the brain immediately underneath it remained completely insensible. In the same manner, Flourens infers from his experiments that the ligaments, the tendons, and the periosteum, are altogether insensible when in their normal state, but that they become very sensitive as soon as irritation or inflammation is set up in them. From these observations, Flourens is inclined to attribute to the dura mater, the tendons, ligaments, and periosteum, during the state of health, a *latent sensibility*, and to reject the view of the existence of any completely insensible organ in the animal body.

Samuel found in his experiments, performed on rabbits, dogs, and cats: 1st. That the hyperæmia of the intestinal mucous membrane, produced by the extirpation of the plexus cœliacus, is so great that it exceeds all pathological hyperæmias hitherto known, even that from cholera. By comparing this result with those of other experiments, the author considers himself entitled to assert, that this hyperæmia cannot be attributed to the peritonitis caused by the operation, the less so as it does not extend to the lower parts of the intestines. 2nd. That the secretion of the mucous membrane becomes increased by the extirpation of the plexus, but not to the same degree as this is the case in violent diarrhœa, and much less so than in cholera. The complication with peritonitis, the section of the ramifications of the pneumogastric nerves entering into the formation of the plexus, and the hyperæmia of the liver, may be considered as influences lessening the secretion.

The results of Iaschkowitz's experiments on the influence of section of the plexus lienalis on the structure of the spleen, are related under III. of the present Report.

VI. Sexual Phenomena.

Delafond: *On certain Physiological Phenomena connected with Parturition and Lactation in Bitches that have not been Fecundated when in Heat.* (L'Union Méd., tome xi. No. 61, 1857.)

Delafond directs the attention of the Académie de Médecine to several phenomena deserving further examination. He corroborates the observation made already by the great Harvey on doe-rabbits, and by Buffon on Bitches, that the breasts of animals which have not been fecundated when in heat sometimes become turgescent, and secrete milk at the time when the parturition would take place if the animals had become pregnant. The author observed the commencement of the turgescence in bitches already two or three weeks before the term of the pregnancy would have been elapsed. In addition to this, the author witnessed, at the period when the parturition would have occurred, enlargement and swelling of the vulva, and increased viscous secretion of the mucous membrane of the vagina; he even saw the animal in a restless state arranging a resting-place, as if for an expected process of whelping; a few days later he discovered the symptoms of milk-fever, and the bitch in this state allowed a puppy placed underneath her to suck her breast, she bestowed on it the same signs of affection as if it had been her own, and the young animal was evidently thriving by the nourishment it thus received. Leblanc, Roche, and Moreau, who took part in the discussion on the subject, related observations of their own of a similar nature, and Roche mentioned that Dubois had met with analogous phenomena in women.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.

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I. Affections of the Nervous System.

The Influence of Cerebral Disease upon Diabetes Mellitus. (L'Union Médicale, March 14th, 1857.)

In the Academy of Sciences, March 2nd, 1857, M. Leudet reports four cases which had fallen under his observation, and which tended to show that lesion of the brain may be productive of glycosuria.

Case 1.—A female, aged thirty-two, in the sixth month of pregnancy, suddenly lost the sight of the left eye, without any other symptom of paralysis. Headache and vomiting supervened at the same time. Seven months and a half later there were sudden symptoms of coma, which lasted for a day, leaving paralysis of the third and fifth pairs of nerves of the left side, facial left-sided anæsthesia, great thirst, and general symptoms of diabetes. By the aid of Barreswill's test, the presence of sugar was proved in the urine. Under the use of iodide of potassium the paralysis diminished, and the diabetes disappeared. A temporary relapse in regard to the cerebral symptoms occurred five months later, when no sugar was discovered in the urine.

Case 2.—A female, aged fifty-three, was suddenly seized with right-sided hemiplegia, depending upon cerebral lesion, and accompanied by epileptiform attacks; the power of motion was restored, particularly on the affected side; two years after these apoplectic seizures, diabetes manifested itself; there was sugar in the urine; a year later, albuminuria and a state of general cachexia set in.

Case. 3.—A female, aged eighty, was suddenly seized with hemiplegia of the left side; at the end of eighteen months she suffered from intense thirst, sugar was found in the urine by Moore's and Barreswill's tests; humid gangrene of the right foot and death followed.

Case 4.—A female, aged thirty-nine, was seized at the sixth month of utero-gestation with paraplegia and convulsions. These symptoms gradually disappeared; vertigo remained. Six years later there were frequent hæmorrhages, dyspepsia, and finally diabetes mellitus. Variola supervened, which proved fatal.

We think these cases fairly open to criticism, inasmuch as, with exception of the first case, the period intervening between the occurrence of cerebral symptoms and of the appearance of sugar in the urine, was so long as to justify a doubt as to the casual relation of the former. In the first case, the fact of sugar being discovered at the time of the first apoplectic seizure confirms the observation of M. Blot of its presence in the urine of all females under those circumstances, while its absence at the time of the relapse tends to prove that there was no relation between the glycosuria and the cerebral symptoms in the first instance. We have ourselves shown that in epilepsy the presence of glycosuria is at least an event of very rare occurrence, since in fourteen cases of epilepsy in which we have examined the urine for sugar, we have failed to discover it.

Clinical Studies and Observations on Cerebral Rheumatism. By Dr. Adolphe Gubler, Physician to the Beaujon Hospital, &c. (Archives Générales de Médecine, March, 1857.

It is well known that in by far the majority of cases of rheumatic fever accompanied by cerebral symptoms, delirium, mania, stupor, coma, or convulsions which prove fatal, no post-mortem lesion is found to indicate local disease within the cranium. Dr. Watson,* among others, goes fully into this question, and details some interesting cases, showing how this class of symptoms may be due solely to the sympathetic irritation of the encephalon. Dr. Gubler re-opens the discussion; but although he states that "the reality of cerebral rheumatism must be considered as settled," we only find one case in which he is able to demonstrate the existence of meningitis. It occurred in an Englishwoman, aged thirty-two, who had had twelve children, and had before her admission into Beaujon (July 2nd, 1856) been subjected to great bodily fatigue and mental anxiety. The immediate cause of the acute rheumatic affection had been a long walk, during which she had felt repeated shiverings when taking rest, a week previously. All the joints were swollen and painful; the patient was unable to make the least movement. The affected parts were red, and inflamed lymphatics could be traced along the skin. There was slight jaundice and enlarged liver; an impetiginoid eruption was seen on the legs. The aortic and mitral valves each presented a systolic souffle; pulse 130, full and strong. No albumen in the urine. She was treated with quinine, one gramme (gr. xv.) during the day, in three doses, to which a small quantity of opium was added on the 3rd July. On the 4th, the first symptoms of cerebral excitement occurred. There was sub-delirium; speech short and anxious; the face flushed; the eyes brilliant; the pupils contracted; pulse 136. The souffle very loud and harsh; the joints the same. A venæsection, a blister to one leg, an "antispasmodic potion without opium," were ordered. Diarrhœa and delirium occurred during the day. The blood was much cupped and buffed; the serum yellow, coloured green on the addition of nitric acid. Twenty leeches were placed behind the ear. During the ensuing night the delirium and agitation were extreme. There were no irritable convulsions and no vomiting. The patient expired at six a.m.

We must refer the reader to the original for the interesting details of the examination of the joints and the external parts affected, which are given with much minuteness. In the chest, satisfactory evidence was found of endocarditis affecting especially the valvular tissues on the left side of the breast. Within the cranium, on the anterior part of the convex surface of the brain, a vivid redness was found, which was not removed by washing; the arachnoid and the pia mater at this point were strongly injected. The subjacent grey matter was softened, of a rose tint; under a stream of water it presented a velvety appearance. The lateral ventricles contained a reddish sanguinolent serum; the ventricular parietes were not softened. The choroid plexuses were infiltrated, and presented a large number of minute transparent vesicles. The liver was found enormously enlarged, "of the colour of beeswax, of firm consistency; when cut, the fibrous tissue was found hypertrophied. The microscope showed the parenchyma to consist of hepatic cells distended and deformed by oil-globules; there was also free fat." The spleen was also enlarged to nearly double its normal size.

Professor Gubler terms the above morbid condition of the liver, "wax liver des Anglais;" and informs us at the same time that he has only once seen it among his compatriots. Our readers will probably agree with us that the above description in no way tallies with the microscopic features which we regard as constituting waxy liver. Dr. Gubler reports two other cases of articular rheumatism, in which cephalic symptoms were manifested; but as one recovered, and the other, although fatal, exhibited no trace of cerebral lesion, they only tend to confirm the prevailing opinion relative to the purely symptomatic character of the cephalic symptoms accompanying rheumatic fever.

* Lectures, vol. ii. p. 275,

On Partial Paralysis of the Extremities induced by the continued Use of Snuff containing Lead. By Dr. Moritz Meyer, of Berlin. (Virchow's Archiv, Band xi. Heft 3.)

Four interesting cases observed by the author are given in detail, in which the history and the symptoms justify the diagnosis of lead poisoning. The features characterizing the cases were—1. A more or less advanced paralysis of the extensors of the forearm; 2. A projection of the metacarpal bones; 3. A yellowish sallow complexion. In three of the cases, repeated attacks of colic had preceded the appearance of the paralysis; in one, it was absent. In three cases, the extensores digitorum communes, and in one, the deltoid muscles, had suffered chiefly. The presence of lead in the tobacco used by each patient was proved chemically. A large number of other kinds of snuff besides those used by the above-named patients were analysed, and all that had been packed in lead were found to be more or less impregnated with lead or oxide of lead, the amount of impregnation varying from 0·78 to 1·78 per cent.

II. Affections of the Thoracic Viscera.

Memoranda of a New Method of Measuring the Thorax. By Dr. Woillez. (Archives Générales, p. 583, May, 1857.)

The author has presented to the Academy of Medicine a new instrument for measuring the thorax, which he terms a cyrtometre, with which he avers that he can at once determine the modifications of certain diameters, and of the circular outline of the thorax. He is of opinion that the method of mensuration hitherto pursued is erroneous, because based upon two false principles; the one being the supposition that the healthy side presents an uniform capacity, while the diseased side alone is regarded as susceptible of modification; the other being the opinion that mensuration is a means of diagnosis in the strict sense of the word. The instrument consists of joints of whalebone of two centimetres each, moveable in such a way that when applied to any surface the whole may take and retain the curve of that part. The outline of the curve of the thorax thus obtained being transferred to paper, the comparison of curves, taken at different periods of the malady, aids to determine the successive changes in the affected part. Without diagrams, it would be useless to go more into detail.

On Redness of the Cheeks as a Symptom of Pneumonia. By Adolphe Gubler. (L'Union Médicale, No. 23, April 28, and May 2, 1857.)

Dr. Gubler takes up the old doctrine that the redness of a cheek in a case of pneumonia indicates the side on which the disease lies. Modern authors have paid little attention to the subject, but Dr. Gubler has satisfied himself, by extensive observation in the Salpêtrières, that the general law is true. The author has guarded against the fallacy which might result from the patient lying on the cheek presenting the greater redness, and has measured the relative temperature of the two sides of the face with the thermometer. Numerous cases are detailed, and the following is the summary of his observations: 1. The redness of the cheeks which commonly coincides with pulmonary inflammation, is not, as is commonly thought, a fortuitous circumstance, but a functional disturbance bearing a definite relation to the disturbance of the respiratory passages. 2. This redness is not necessarily proportioned to the extent and degree of the anatomical lesion, but bears a relation to the intensity and progress of the inflammatory action. 3. A sensible, and sometimes considerable elevation of temperature (from 0·50° to 5·40° Cent., or nearly 1° to 10° F.) accompanies the hyperæmia, and gives it the character of active congestion. 4. The congested cheek corresponds to the lung which is the seat of phlegmasia, or the one which is most affected. 5. The flushed cheek is seen, not only in pneumonia, but also in the majority of other pulmonary inflammations—in those which accompany tuberculization, as in typhoid pneumonia, and even in capillary bronchitis. It appears to be most marked in pneumonia of the apices—a circumstance already pointed out by Bouillaud.* 6. The production of other morbid conditions may be promoted by the habitual hyperæmia of the face; thus a spot of erysipelas has been seen developed on the cheek of the affected side. 7. The redness of the cheeks in acute diseases of the lung may be explained by the stimulation of their nervous plexuses extending to the brain, and reflected upon the respiratory nerves of the face. 8. The phenomenon may be

* Nosographie Médicale, tom. xi. p. 484.

regarded as a manifest example of sympathy established between two distant regions by the agency of the nervous system.

On Extravasation of Blood in the Tissue of the Valves of the Heart. By Prof. H. LUSCHKA, in Tübingen. (Archiv für Pathol. Anatomie, &c., Band xi. Heft 2.)

Prof. Luschka, in 1852,* discovered the existence of bloodvessels between the layers of the endocardium, or the valves. His researches led him to conclude, that "the exudations or fibrinous vegetations observed on the surface of the valves in endocarditis are dependent on an hyperæmic condition of these vessels, accompanied by exudation of lymph from them." He now considers the formation of small extravasations which occur in the tissue of the valves in consequence of the rupture of these vessels. The Professor has but rarely met with such ecchymoses in the heart of adults; he found one near the free edge of the anterior curtain of the tricuspid in a man aged eighteen, and another in the same subject in the anterior flap of the aortic valves. A well-defined ecchymosis occurred in the left flap of the aortic valves near its insertion, and an extravasation was observed on one of the larger tendons of the mitral in a female who died of pneumonia. Dr. Reuss is quoted as having seen an ecchymosis within a fold of the pulmonary valve. Dr. Luschka has much more frequently met with hæmorrhage in the tissues of the valves of new-born infants. The extravasations almost always occur in the vicinity of the free margin of the mitral and tricuspid valves. They are chiefly visible on the inner layer, and generally cause the surface to project somewhat. They are generally circular, and look like red granules scattered through the tissue, varying in size from that of a poppy seed to that of a millet seed. They are rarely solitary, but more frequently there are from three to six. Their colour is yellowish red, or blackish red, or even absolutely black. When quite recent, we may detect in them blood-corpuscles and *débris* of tissue. In those that offer a reddish-yellow hue, amber-coloured molecules of pigment, decomposing blood-corpuscles, and oil-globules are never absent; and in the black spot, which is probably of older date, doubtless granular black pigment is found in considerable quantity.

These extravasations appear to be of very frequent occurrence in the new-born infant. They were met with 41 times in 165 post-mortems—viz.,

13	times	in the	tricuspid valve.
8	"	"	mitral valve.
17	"	"	tricuspid and mitral valves.
2	"	"	tricuspid, mitral, and pulmonary valves.
1	"	"	mitral and pulmonary valves.

Of the 165 children, 128 were born alive; 37 were stillborn. Of the former, 31, of the latter, 10, exhibited these ecchymoses.

On the Relations of Bright's Disease and Cardiac Affections. By H. BAMBERGER, Professor of Medicine in Würzburg. (Archiv für Pathol. Anat. und Physiol., Band xi. Heft 1.)

The well-known frequency† of the complication of Bright's disease and valvular disease has been differently accounted for by different authors. An essential question regarding the etiology of the two classes of affections is, which of them precedes the other? Prof. Bamberger states that, with the exception of a small number of cases in which, during the course of Bright's disease, endocarditis took place, he has never seen a case in which the renal affection was the first; whereas he has observed many in which morbus Brightii was developed during the existence of valvular disease. He therefore concludes that valvular disease is a frequent cause of Bright's disease, but admits that the latter, under these circumstances, frequently does not pass beyond the first stages.

The author next considers the relation of hypertrophy of the heart, unaccompanied by valvular affections, to Bright's disease. The ratio appears to be above 20 per cent. of the former; Dr. Bright himself estimated the frequency of cardiac hypertrophy at 23 per cent. Dr. Bamberger is of opinion that the mechanical explanation ordinarily offered, according to which the hypertrophy is produced by the physical influence of the derangement in the circulating fluid, is untenable. He admits that hypertrophy of the heart, in the majority of in-

* Archiv für Path. Anat., p. 182, 1852; and British and Foreign Medico-Chirurgical Review, July, 1853.

† Willigk (Prager Vierteljahrsschrift, Band xxxviii. p. 44) shows that in 209 cases of Bright's disease, valvular disease was present in 31, or about 15 per cent. See also Chambers, Decennium Pathologicum, who establishes a much higher ratio.

stances, is secondary to the renal affection; but he shows that it is found with large kidneys and contracted kidneys, and argues that a very different effect upon the momentum of the aortic current must be produced by each of these forms of renal disease. The Professor remarks, that if the obliteration of some renal capillaries could exert so palpable an influence upon the heart as that observed to accompany many cases of contracted and granular kidneys, the obliteration or application of a ligature to any larger artery ought to produce the same result. He points out that granular liver is analogous to granular kidney, and yet is not productive of cardiac hypertrophy.

In order to arrive at a solution of the question as to the efficient cause of the cardiac hypertrophy in these cases, he carefully tabulates and analyses 48 cases of Bright's disease observed by himself during life, in which post-mortem examinations were made. In these 48 cases there were 25 in which there were marked alterations in the heart; in 15 there was either recent, or traces of former, pericarditis; in 10, fatty degeneration; in 4, degeneration of the aorta; in 3, the remains of endocarditis. Hypertrophy and dilatation of one or more cardiac divisions were met with 19 times. In 28 cases there was serious disease in the lungs; 11 times tubercle, 10 times pneumonia, pleurisy 9 times, emphysema 3 times; the spleen was enlarged in 24 cases; the liver was cirrhosed 3 times, in a state of adipose or bacony enlargement 16 times.

It is manifest that serious derangement occurs in most of the vital organs as a complication of Bright's disease. The author concludes that the hypertrophy of the heart is therefore not explicable on purely physical grounds; but that it must be regarded as a purely "vital phenomenon," belonging to the same category as so many other derangements of nutrition which are developed in the course of Bright's disease.

Contribution to the Pathology of Heart Disease. By W. O. Markham, M.D. (British Medical Journal, April 4th, 1857.)

An interesting case is detailed by the author, of a child aged four years, who during life had presented "a rough, loud, systolic bruit, which was audible all along the base of the heart, and in the whole of the left subclavicular region; it was indistinctly heard below the nipple, and was scarcely audible at the heart's apex; its point of greatest intensity was to the left of the upper part of the sternum; it was not audible up the right edge of the sternum, along the course of the aorta." There were slight traces of cyanosis before death, when febrile symptoms, with chronic twitchings of the arms, strong beatings of the heart, drowsiness, and other indications of cerebral disturbance, supervened. Although at one time pulmonary tubercle was suspected, the auscultatory evidence of its presence was unsatisfactory. All the symptoms before death indicated an acute affection of the heart. This organ, however, presented "neither externally nor internally the slightest trace of inflammation, nor was there, as far as the eye could judge, any deviation from their normal condition observable in any of the valves, or of the orifices of the organ. There was neither constriction of the orifices, nor of the roots of the great vessels, nor any defect in the valvular apparatus. In all respects the heart appeared healthy and normal, excepting one, and this was in an open condition of the foramen ovale. The foramen ovale, though largely open, so as to permit the point of a finger to pass from the right into the left auricle, was partially closed on the left side of the septum by a peculiar adjustment of the membranous valve;" the membrane being attached above and below, so as to present two narrow semilunar slits, one on either side of the valve. Both lungs were studded with miliary tubercles. It is stated that, the heart having been submitted at the Pathological Society to some of our most distinguished cardiac pathologists, who were unable to detect any other lesion than the open foramen ovale, it is a fair inference that the bruit was due to the latter condition.

The case is an important contribution to our knowledge of heart disease, and to the auscultation of the organ, inasmuch as it meets the chief objection to the production of a bruit by an open foramen ovale, that hitherto where a murmur has been found in connexion with this lesion, there has also been a constriction of the pulmonary orifices.

A Peculiar Cavernous Degeneration of the Muscular Tissue of the Heart. By Dr. C. Skrzeczka. (Archiv für Pathol. Anat. und Physiol., Band xi. Heft 2.)

A strong-built man, aged twenty-one, who had always enjoyed good health, was pursued by a boy of twelve, seized, and thrown down. At the same moment he breathed hard once or twice, and expired. The heart was found of average size. The parietal and visceral layer of the pericardium entirely adherent, chalky deposits intervening between them in the form of hard laminæ. A section of the left ventricle, at the left margin of the heart, presented the appearance of the section of a fine sponge. Small cavities, varying in size from a pin's head to a small bean, lay closely aggregated together, yellowish-brown muscular tissue intervening. The larger ones lay externally, the largest immediately beneath the pericardium. Some of the latter were subdivided into several compartments by fine membranous or thready expansions, stretched across from one side to the other. When the heart was examined by Dr. Skrzeczka, it had lain in spirit some time. The cavities were found full of spirit, excepting one, which lay immediately under the pericardium, and contained some coagulated blood. The cavities had no lining membrane, but appeared to be mere lacunæ in the muscular texture. The whole left ventricle exhibited the same degeneration, as did also the septum. The right ventricle was partly affected in the same way, and the papillary muscles of the left ventricle showed traces of the same condition. The valves were all normal; the arteries showed no atheroma, and the coronary arteries were normal. The muscular tissues throughout exhibited fatty degeneration in an advanced degree.

The author has in vain sought for an analogous case in the records of medicine. He considers that the cavities cannot be regarded as cysts, as the remains of apoplectic foci, or as the residues of abscesses. The only explanation of the condition which he offers as at all compatible with the history and post-mortem appearances of the case, is that the cavities were the result of absorption of parts which had previously undergone complete fatty degeneration.

III. Affections of the Blood and disordered Secretions.

The Constitution of the Blood in Syphilis. (L'Union Médicale, May 16, 1857.)

In the eighth lecture on chancre recently delivered by M. Ricord, we find the following interesting contribution to hæmatology, in the shape of a series of analyses of the blood of syphilitic patients, by M. Grassi, *pharmacien-en-chef* of the Hôtel Deiu:

I. *Blood of Patients affected with Simple Chancres.*

	1st patient.	2nd patient.	3rd patient.	4th patient.	5th patient.
Water	762·4	762·4	768·0	763·8	750·0
Fibrin	2·9	2·9	3·0	2·6	3·9
Albumen	94·3	94·3	88·5	95·5	112·5
Corpuscles	140·4	140·4	140·5	138·1	133·6
	1000·0	1000·0	1000·0	1000·0	1000·0

	6th patient.	7th patient.	8th patient.	9th patient.
Water	755·2	758·5	749·1	760·9
Fibrin	4·0	3·6	3·0	3·0
Albumen	113·7	84·3	109·0	97·0
Corpuscles	127·1	153·6	138·0	139·1
	1000·0	1000·0	1000·0	1000·0

These analyses show that in simple chancre the blood presents no material deviation from its physiological condition. The following cases seem to prove that in indurated chancre and secondary syphilis, there is uniformly a diminution of blood-corpuscles, and an increase in the amount of albumen, but no perceptible variation in the quantity of fibrin:

II. *Blood of Patients affected with Indurated Chancres.*

	1. Indurated chancre.		2. Indurated chancre.		
	First bleeding.	Second bleeding, after a month's treatment by iodide of potassium.	First bleeding.	Second bleeding, after a week's treatment by iodide of potassium.	Third bleeding, after a month's treatment by iodide of potassium.
Water	796·6	774·2	797·0	794·6	784·0
Fibrin	3·0	3·3	3·0	3·5	3·5
Albumen	104·5	113·5	106·0	95·2	84·0
Corpuscles	95·9	109·0	94·0	106·7	128·5
	1000·0	1000·0	1000·0	1000·0	1000·0

3. Indurated chancre.

	First bleeding.	Second bleeding, after 20 days' treatment by iodide of potassium.
Water	797·3	768·6
Fibrin	2·4	2·4
Albumen	123·9	87·0
Corpuscles	76·4	142·0
	1000·0	1000·0

4. Indurated chancre and roseola.

	First bleeding.	Second bleeding, after 25 days' treatment by iodide of mercury.
Water	769·7	765·0
Fibrin	2·6	3·5
Albumen	102·6	106·0
Corpuscles	125·1	125·5
	1000·0	1000·0

5. Indurated chancre, syphilides.

	First bleeding.	Second bleeding, after 8 days' treatment by iodide of mercury.
Water	769·5	784·4
Fibrin	3·1	3·6
Albumen	102·6	89·7
Corpuscles	124·8	122·3
	1000·0	1000·0

6. Indurated chancre.

	First bleeding.	Second bleeding, after 19 days' treatment by iodide of potassium.	Third bleeding, after 28 days' treatment by iodide of potassium.
Water	789·5	768·7	796·9
Fibrin	4·7	3·8	3·5
Albumen	115·4	121·0	68·0
Corpuscles	90·4	106·5	131·6
	1000·0	1000·0	1000·0

In the following three cases the reduction of the quantity of corpuscles is remarkably great:

	7. Indurated chancre and roseola. First bleeding.	7. Second bleeding, after 12 days' treatment.	8. Indurated chancre.	9. Indurated chancre, with syphilitic spots.
Water	830·7	759·5	815·1	821·2
Fibrin	2·4	2·5	3·2	3·0
Albumen	108·0	110·5	126·7	127·5
Corpuscles	58·0	127·5	55·0	48·3
	1000·0	1000·0	1000·0	1000·0

In all the cases examined the outbreak of syphilis was recent; the above results must not therefore be regarded as representing the state of the blood in the later stages of the disease.

On Spanæmia, Chlorosis, and Analogous Conditions, as the predominant Characteristic of the present Age. By Dr. Pollitzer, Director of the first Chilorms Hospital in Vienna. (Zeitschr. der K. K. Gesellsch. der Aerzte, February, 1857.)

Dr. Pollitzer takes a very gloomy view of the condition of the human race at the present time, and considers it to be an established fact that the physical deterioration in Europe is profound, "a sad memorial of civilization." He admits the general diminution of mortality in all civilized countries, but affirms this to be a fallacious test, as there is not a corresponding increase in the health and vigour of the race, or in the number and character of the diseases. The reduction of the mortality the author attributes to the increase of hospitals and similar charitable institutions,—to quarantine, vaccination, and numerous sanitary regulations. The boundaries of health and disease, he observes, are daily becoming less marked, and he considers it characteristic of modern pathology, to affirm that there are numerous conditions which are undoubted deviations from the healthy standard, though it is impossible to delineate or give definite portraits of them, because they make their appearance during a state of "relative health." The physician has no name for the disease, but the patient maintains that, not feeling in health, he has no alternative but to call himself ill. This anomalous condition Dr. Pollitzer accounts for by the spanæmia and chlorosis, which he regards as the feature peculiar to our times—the soil in which the feebleness and deterioration of our race take root. After developing his views more in detail, the author proceeds to show how these conditions are fostered by modern civilization. A constant stretch of the mental powers—a restless excitement of the passions—a perpetual struggle for advancement—the fresh wants of every day, science and the arts themselves being subservient even to the luxury and demoralization of the times—the destruction of all moral harmony and peace—are advanced by Dr. Pollitzer as the evils of modern civilization. And these evils react especially upon the younger generation; and the demands made upon the youth of eighteen or twenty of the present would formerly have been considered a sufficient tax for the strength of a man of upwards of five and twenty. He inveighs especially against the polymathy (if we may coin the word) of children, among whom the spanæmia and chlorosis of the age especially flourish.

Having for seventeen years devoted himself to the study of children's diseases, he has arrived at the conclusion that the features which characterize our age have their source in the treatment of childhood, and that the deterioration of the race at large takes its origin in that of childhood.

The facts upon which Dr. Pollitzer bases his remarks are, that anæmia and chlorosis occur alone, or associated with rickets, hypertrophy of the lymphatic glands, and of the spleen and liver, to an incredible extent, even from the first month of life. Of 1000 children that were treated in the children's hospital, on an average 700—800, or from 70—80 per cent., were thus affected. He also observed that the anomalies of the blood and constitution, which are so widely diffused, invariably appear where the nutrition of the child has been imperfectly effected. The stomach and intestinal tract are the parts that first suffer, hence it is in these organs that we discover the prevailing morbid conditions of childhood; and while they materially influence the mortality of children they equally affect the state of their future health when they survive childhood.

An Investigation of the Urine in Remittent and Intermittent Fevers, proving the Hyperphosphatic state of the Blood; also the eliminating Properties of Quinine. By H. M. Stuart, M.D., of Beaufort, South Carolina. (Charleston Medical Journal and Review, May, 1857.)

On a Physiological Action of the Disulphate of Quina. By H. Ranke, M.D. (Medical Times and Gazette, May 30th, 1857.)

The observations and experiments of Drs. Ranke and Stuart appear to directly contradict one another, inasmuch as the former shows that, in health, at least, the effect of the disulphate of quina is to diminish materially the quantity of uric acid excreted by the kidneys; while the latter demonstrates, that in intermittent and remittent fevers the administration of quina induces an increased excretion of uric acid, urate of soda, biliary matter and mucus, and triple phosphates. Dr. Stuart's essay has received the College Premium at the commencement of the Medical College of South Carolina, and therefore comes before us with a special claim upon our attention. We will examine his paper first, premising that his determination of the salts and other deposits of the urine was made with the urinometer and the microscope; the different elements were not isolated, nor was the balance employed, as in the experiments by Dr. Ranke.

Four cases of remittent, three of intermittent, fever were examined by Dr. Stuart. The accounts are very scanty, but in all the urinary salts appear to have been increased during the administration of the quinine. We select the two best cases as illustrations:

"Roper Hospital, Bed No. 6.—Remittent. June 21st, admitted. Urine examined before medicine had been given. Sp. gr. 1·037; colour high; slight sediment, consisting of urate of soda, uric acid, and biliary matter. Quinine (grs. x. and v.) given, seven hours after which urine was examined. Heavy deposit of triple phosphate, uric acid in small crystals, colouring matter, and a little mucus.—June 22nd: Sp. gr. 1·040; high colour; more uric acid than on 21st; triple phosphates in same proportion; other things as on 21st.—June 24th: No fever; sp. gr. 1·055; colour very dark, and very heavy sediments of phosphate of lime and uric acid. Quinine (grs. x. and v.) given.—June 25th: Sp. gr. 1·040; colour high; ammoniaco-magnesian phosphates in very large crystals, but not so abundant; other things the same. Quinine (grs. x. and v.) given.—June 26th: Sp. gr. 1·050; colour high; triple phosphates in large quantities; uric acid, &c., in same proportion. The sp. gr. of this patient's urine before leaving the hospital had fallen to 1·030, and it began to appear natural.

"Roper Hospital, Bed No. 14.—Intermittent. June 28th, admitted. Before quinine was given, nothing unusual under the microscope. Sp. gr. 1·040; sediment scarcely perceptible, consisting of a little uric acid; colour rather dark. Quinine given (grs. x. and v.)—June 29th: Sp. gr. 1·050; very heavy sediment of urate of soda, and the triple phosphates; uric acid; colour very high. Quinine given (grs. x.) during day. No fever.—June 30th: Sp. gr. 1·056; colour high; sediment even greater than on the 29th; the triple phosphates increased both in size and quantity; uric acid also in large quantity. Repeated quinine. This patient's urine quickly fell to the normal standard, and he recovered speedily."

Dr. Ranke's experiments were made upon himself and two medical volunteers. The urine was tested only for uric acid, which was determined by mixing 100 cubic centimetres of urine with concentrated hydrochloric acid, and allowing it to stand for forty-eight hours. The uric acid was carefully collected on a filter, well washed, and weighed.

Dr. Ranke found that on a mixed diet his average secretion of uric acid (deduced from twenty analyses) is 0·629 grammes in twenty-four hours. In the first experiment he took twenty grs. of disulphate of quina in twenty-four hours, and during the next forty-eight hours he passed 0·542 grammes, or 0·271 grammes for twenty-four hours. The second experiment gave a similar result, the quantity of uric acid excreted during forty-eight hours, after fifteen grs. of quina had been taken, being equal to 0·790, or 0·395 in twenty-four hours. On the third day after quina had been taken, Dr. Ranke again excreted about his normal average—viz., 0·621 grammes; and on the two following days, 0·543 and 0·656 grammes respectively. He now took quinine for a third time, and the quantity of uric acid again fell to 0·438 grammes on the first, and 0·392 grammes on the second day.

Dr. S. passed 0·544 and 0·543 grammes of uric acid respectively on two days before the experiment. He then took two ten-grain doses of quina, and on that day excreted 0·376 grammes of uric acid; the next day he took grs. v. of quina, and the quantity of uric acid fell to 0·317 grammes; on the three following days he passed respectively 0·483, 0·450, and 0·654 grammes.

Dr. M., on four days before taking quina, passed 0·662, 0·774, 0·585, and again 0·585 grammes; he then took ten grs. of quina, and on that day excreted 0·358, on the next 0·387, and on the third 0·670 grammes of uric acid; it then remained stationary.

Dr. Ranke, in two of the experiments, determined the other constituents of the urine, and found the solids in general, and the urea, not materially affected by the quina, but the phosphoric acid appeared to be augmented.

On the Abnormal Presence of Urea in the Pancreatic Juice in Man. By Dr. F. Hoppe. (Archiv für Pathol. Anat. und Physiol., Band xi. Heft 1.)

In a man who died with icterus in the Charité, at Berlin, the gall-bladder and the larger bile-ducts in the liver were found distended with bile, and the pancreatic duct was cylindrically dilated, and many of its branches in the gland converted into ampullæ of the size of hazel-nuts. A dense cicatricial tissue which surrounded the orifices of both ducts in the duodenum was the cause of the arrest of the two secretions. The pancreatic fluid was collected without the admixture of the smallest quantity of blood, and analysed with the following result:

	Grammes.	Per cent.
Urea	0·007	0·12
Fatty matter	0·001	0·02
Alcoholic extract	0·049	0·87
Watery extract	0·030	0·53
Insoluble residue	0·028	0·49
Inorganic salts	0·032	0·57
Total solid residue	0·147	2·60
Water	5·508	97·40
	5·655	100.00

On evaporation of the ethereal extract, excepting a trace of fat and a few microscopic globules of leucin, nothing but crystals of urea were observed.

The author remarks that the case proves that we need not fear the excessive liability of the urea to become decomposed in the blood and other fluids of the body, as so much urea was discovered, and not even a trace of ammoniacal salts.

IV. Sundries.

On the Existence of Herpes in Domestic Animals, and its Communication to Man. By Dr. Von Baerensprung. (Annalen des Charité Krankenhauses, Achter Jahrgang, Heft 1.)

The author quotes numerous writers who have directed attention to the occurrence in animals of cutaneous eruptions similar to those found in man. Alibert has remarked the occurrence of herpes circinnatus in horses; Dr. Fehr has observed a peculiar herpetic eruption in Switzerland communicated from cattle to human beings; similar observations are quoted from Hering's 'Repertorium der Thier Heilkunde,' Band i. 1840; from Gurlt and Hertwig's 'Magazin für die Gesammte Thierheilkunde,' Band vii. 1841; Letenneur's 'Réflexions sur l'Herpès Tonsurant,' 1852; and other works. From his previous investigations into the nature of herpes in man, Dr. Von Bärensprung assumed that the eruption in question was characterized in animals as well, by the formation of a confervoid growth. In them it resembles the herpes tonsurans of man; circular, well-defined spots form, upon which the hairs are partly broken off, partly fallen out, and invested with a white asbestine scurf; the subjacent surface is red, and covered with papulæ. These spots occur in all parts of the body, but especially in those which the animal is unable to reach with its tongue. Each hair is enveloped at its base with a thin whitish sheath, a prolongation of the sheath of the root of the hair, which commonly ceases at the point at which the hair issues from the cutis. This occurrence is due to a cryptogamic vegetation, which glues the sheath to the hair; this consists in sporules and filaments; the former are circular or angular, and without granular contents; the latter are elongated, branched, and jointed. The characters are the same as those found in herpes as contradistinguished from tinea and chloasmi; but the cryptogamæ are found not so much, as in man, in the hairs themselves, as between the hair and the sheath. Dr. Von Bärensprung rubbed some scales containing much of the confervoid growth on his left forearm. For some days no effect was produced, but after a time considerable itching reminded him of the experiment, and to his surprise he found a well-formed spot of herpes circinnatus of the size of a sixpence. This gradually increased, and in three weeks attained to the size of a crown-piece. In the fourth week the first spot began to heal, but others formed in the vicinity, and the author now arrested them by the application of white precipitate ointment.

QUARTERLY REPORT ON SURGERY.

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

I. *On Foreign Bodies introduced into the Bladder.* By M. Denucé. (Moniteur des Hôpitaux, 1856. Nos. 126, 7, and 8.)

In this paper, M. Denucé, of Bordeaux, relates a case that occurred to himself, in the person of a woman who had introduced the handle of a stiletto into the bladder. After several days of severe suffering, she came to the hospital, and as the urethra was found to be in a very dilated state, the extraction of the foreign body was easily accomplished, by means of a polypus forceps passed along the index finger.

M. Denucé has collected the particulars of 391 published cases, and the enumeration he gives of the bodies in question is both curious and useful. In 78 they were portions of catheters or lithrotrity instruments—viz., 15 metallic catheters, 9 gum elastic catheters, 7 gutta percha catheters (a large proportion, considering the short time these dangerous instruments have been in use), 28 catheters (their nature not being specified), 16 bougies, and three branches of *brise-pierres.* Then we have 82 needles, pins, or tags, 1 stiletto, 1 crotchet needle, 6 bone or ivory needles, 6 ear-picks, 3 ivory whistles, 1 ivory spindle, 1 ivory stiletto handle, 15 leaden balls, 3 small keys, and 8 instances of metallic fragments of various kinds. In 12 bones or splinters of bone, in 10 pebbles or china, 6 penholders, 15 needle-cases, 10 pieces of tobacco pipe, 4 portions of glass tubes. In 21 instances fragments of wood, as 3 pencils, 1 piece of a match, 1 ramrod, 1 mustard spoon-handle, &c. In 34 there were fragments of plants, as ears of corn, stalks, &c.; in 26 fruits or kernels; in 4 tents of charpie, 1 strip of linen, 1 skein of cotton, 3 *débris* of cotton or wool, 2 pieces of cord, 4 portions of wax candle, 3 pens, 1 piece of whalebone, 2 leather boot-laces, 1 piece of tendon, 2 *débris* of fæcal matter, 1 pessary, 1 shell, 14 instances of various fœtal *débris*, 6 locks of hair, 2 of larvæ of insects, and in 1 pills.

If we abstract from this curious list the bodies which have obtained accidental entrance into the bladder, whether from clumsy surgical manœuvres, or communications established through the walls of the bladder, either externally (as in the case of balls and wounds), or with the rectum, vagina, or ovary, there will still remain 258 cases in which no legitimate explanations can be given of the presence of these bodies. Those assigned by the patient are usually as singular as are the bodies themselves; some being said to find their way there while attempting self-catheterism, others from the patient having fallen on them, while others again are stated to have been swallowed. The true and principal cause of their introduction, when not accidental, is to be sought in the vagaries of an abandoned depravity. Of these 256 cases, 119 are stated as having been males, and 96 females, while in 41 instances the sex is not indicated. In 14 instances they occurred in children from the age of a few weeks to fifteen years.

After a foreign body introduced into the urethra has become propelled into the bladder, in a few days it begins to be covered with incrustations. At the end of some weeks, these have attained a considerable thickness; while at the end of some months, true calculi may be constituted. The form of the body, however, exerts considerable influence upon the mode of deposit. In rounded or short bodies the incrustation becomes general, while in those which are elongated, it takes place especially towards the middle. Thus, in most cases in which calculi have been formed on needles, the ends of these are found projecting beyond the deposit; and it is such calculi that especially give rise to cystitis and other dangerous accidents.

Among the 391 cases collected by M. Denucé, in 21 death took place independently of any operation. In 13 of these the affection was recognised at the autopsy, but it is not stated with precision whether the vesical affection was the cause of death; in 2 death resulted from the *débris* of a fœtus passing into the bladder; and in 6 it was the direct result of the introduction of the foreign body. These last were examples in which the points of pins or needles became imbedded in the walls of the bladder, giving rise to intense and fatal cystitis. Cases in which art does not intervene do not, however, always terminate thus, the efforts of nature in some sufficing to liberate the economy from their presence. Thus they may be carried out by the urine, especially when the bodies are small and women are the subjects. In 31 instances in which the age is specified, this occurred in 14 men and 17 women. Occasionally, certain bodies, such as broken needles, escape by penetration of tissue, and this is stated to have occurred in four cases.

As in the great majority of instances nature will not be able to secure the discharge of these bodies, the surgeon, in order to prevent the occurrence of dangerous accidents, must interfere, endeavouring first to secure their removal by extraction, and if unsuccessful, resorting to lithotomy. In the cases here collected, lithotomy has been had recourse to 125 times—viz., in males, perineal lithotomy, 87 times; recto-vesical, 2; hypogastric, 2; and in females, urethro-vaginal lithotomy in 22, and hypogastric in 12. Unfortunately, in only 61 of these cases has the ultimate result been stated. In 39 occurring in males, perineal lithotomy was performed in 36 (31 recoveries and 5 deaths), hypogastric in 2 (both recoveries), and recto-vesical in 1 (fatal). In the 22 cases occurring in females, there were 15 urethral or vaginal operations (13 recoveries and 2 deaths), and 7 hypogastric (5 deaths and 2 recoveries). On the whole number therefore, 48 recoveries and 13 deaths. The hypogastric operation has thus proved very fatal, 5 deaths taking place to 4 recoveries; and in the latter, in two instances the operation consisted simply in enlarging an aperture already existing.

The performance of the operation may be rendered difficult by the nature of the bodies to be removed. Thus, their softness may be such as to render their recognition and removal difficult; the length of others may lead to their being seized in the middle, and brought across the aperture; while, if they have remained long in the bladder, the size and irregularity of the incrustations present additional obstacles. The precision of measurement attainable by lithotrity instruments, however, now comes to our aid. As to the particular operation indicated, it may be stated in a general way that the perineal operation in men, and the urethral in women, suffice for small bodies, or for such as are thin and elongated; those which are of larger size require the bilateral or vaginal operation; while the hypogastric, seeing its unfavourable results, should be reserved for quite exceptional cases. It must be remembered, however, that in women the urethral operation is almost always followed by incontinence of urine, and the vaginal by vesico-vaginal fistula.

Extraction of the foreign bodies has been performed in 112 of these cases, death following in three. In reference to the influence which the introduction of lithotrity has exerted upon the improvement of extracting instruments, M. Denucé makes the following calculation:—Of the whole 239 cases in which interference has taken place, 127 occurred prior to, 122 subsequent to, 1830, when lithotrity may be considered to have generally established itself as an operation. In the early series, lithotomy was practised 100, times, and extraction 27 times only; while in the latter series, it was resorted to but 21 times, and extraction 101 times. Thus, formerly, lithotomy, with its mortality of 15 per 100, was the rule; while now, extraction, with 3 deaths in 112 cases, is the rule, and lithotomy the exception. The progress that has been made is still more apparent when we consider the case of the male urethra, with its long, narrow, sinuous canal. Of 20 cases of extraction noted prior to 1830, 16 occurred in women, and but 4 in men; while of 73 since that epoch, in which the sex is indicated, 46 occurred in males, and 27 in females.

II. *On the Treatment of Hydrocele in Children.* By M. Richard. (Gazette des Hôpitaux, 1857, No. 41.)

M. Richard, while attending for M. Guersant at the Hôpital des Enfans, met with no less than twelve cases of this affection in the course of one month; and although accident may have led to this accumulation, he yet believes that it is of more common occurrence than is usually supposed.

The hydrocele of children is commonly termed congenital, and as in the great majority of cases the vaginalis communicates with the peritoneum, *congenital persistence* seems to be one of the conditions of the disease. Not that all these serious collections can be reduced by the hand, for it is more common to find them irreducible; but nearly all of them, if watched sufficiently long, are found appearing and disappearing, increasing and diminishing, from time to time. Of these 12 cases, 2 only were purely funicular, the 10 others invading the scrotum and cord. The cysts of the cord, which often simulate a third testis, are in children and adolescents developed in the funicular portion of the persistent vaginalis; while in the adults we observe cysts of the epididymis, containing a turbid fluid and spermatozoa.

Experience has shown that in the treatment of these hydroceles, the persistence of the communication with the peritoneum is not of the importance that might have been expected. M. Richard's cases are treated in the following manner:—1. The liquid is evacuated to the last drop by means of a short exploratory trocar, of very small calibre. 2. An assistant exerts compression upon the lower part of the belly and the track of the inguinal canal. 3. From six to seven grammes of alcohol (40° of Beaumé's areometer) is then thrown in. 4. The canula is suddenly withdrawn so as to leave the fluid in the sac; and after continuing compression over the inguinal canal for a minute, the operation is concluded. The consequences are very simple. The tumour increases a little towards evening, becomes a little painful next day, after when all pain entirely ceases. From the tenth to the fifteenth day the tumour entirely disappears, and the child is cured. If he is of an age to admit of it, he is allowed to walk or play about after the operation. Sometimes at the instant of withdrawing the canula, owing to the strong contraction of the cremaster and dartos, a little of the injected fluid is expelled, the little patient being at the same time seized with erection, or even expelling his fæces. But this is of no consequence. Sometimes, however, a few drops of the fluid enter between the skin and the vaginalis. This is followed by redness of the skin, and the formation of a small abscess, which bursts of its own accord without interfering with the progress of the cure. The *smallest possible trocar* must be employed in this delicate operation, although without practice such an instrument is more difficult to use. We must render the hydrocele very tense with the left hand, isolating it as much as possible, and, holding the trocar in the right hand, apply the pulp of the thumb and the index finger very near the pointed extremity of the instrument. In place of thrusting this in, as in the adult, we must insinuate its point as if using a bistoury. When the cure has been obtained, a good precaution consists in wearing an inguinal bandage for three or four months; for a principal advantage of the operation for congenital hydrocele is derived from the protection it affords against the production of hernia.

III. *On Diphtheritic Ophthalmia.* By MM. Warlomont and Testelin. (Annales d'Oculistique, vol. xxxvi. pp. 228–243.)

This article constitutes one of the additional chapters its authors have contributed to their translation of the last edition of Mackenzie's treatise 'On Diseases of the Eye.' A form of pseudo-membranous ophthalmia was, they say, indicated by Bovisson, of Montpellier, in 1847; and M. Chassaignac has alluded to the pseudo-membranes which occur in the ophthalmia of new-born infants; but the present affection has been particularly described by Gräfe, of Berlin, the disease indeed, thus far, seeming peculiar to Germany.

Symptoms.—In an eye in its normal state, but more frequently in one that has already suffered from inflammation, great irritation is suddenly set up, accompanied by much tumefaction of the conjunctiva, an inconsiderable amount of chemosis taking place. A network of large vessels ramifies over the conjunctiva, and the membrane itself, of a yellowish colour, is marbled over by a great number of minute red points. If an incision be made into the chemosis, fluid does not flow out, the submucous tissue being infiltrated with gelatiniform fibrin. The upper eyelid is remarkably tense, and so much pain does its eversion produce, that Gräfe resorts first to chloroform. When the conjunctiva is thus exposed, it presents a polished yellowish colour, which is due to a fibrinous exudation that penetrates into its substance, and leads to arrest in the circulation. The exudation can only be detached from the membrane with difficulty. It may be well seen by causing the patient to look downwards, and by everting the superior eyelid sufficiently to see the oculo-palpebral fold. The lower eyelid is also rigid, but little moveable, and very red. Besides the symptoms mentioned, there are two others—a great and continuous increase of temperature, and an abundant discharge of a dirty grey fluid, carrying with it yellowish flocculi. This condition, which constitutes the *first* stage of the affection, continues for a longer or shorter period; and the diphtheritic process may still be going on after the original swelling of the eyelid has diminished.

After awhile the rigidity of the eyelids disappears, and the conjunctiva assumes a spongy appearance, abundant fibrinous masses becoming detached from its surface. In parts it may retain its habitual colour, while in other places the exposure of the vessels gives rise to copious bleedings. The portions deprived of the mucous membrane swell more and more, and assume an appearance very like that seen in chronic blennorrhœa. There are also numerous nodosities formed upon portions of the conjunctiva, which, resistant at first, soften with the progress of the affection. The chemosis of the ocular conjunctiva now loses its hardness and yellow colour, and a dense vascular network is developed, so that the diagnosis of the affection at this period is very difficult.

The third stage is characterized by retraction of the eyelid, proportionate to the amount and depth of the original fibrous infiltration. In some patients, day after day false membranes are removed three-quarters of a line in thickness, and having a hole in the centre corresponding to the circumference of the cornea. These are the pathognomonic signs of the affection; but to them may be added various degrees of opacity or ulceration of the cornea.

Nature and Causes.—As to the nature of the affection, it is derived from an inherent disposition of the mucous membrane to take on the diphtheritic action. It is a general disease, occurring more frequently in unhealthy than in healthy individuals, and internal affections frequently prevail during its progress. Thus, among 40 children, the subjects of it, M. Gräfe found death result in 3 from croup, and in other instances from pneumonia or hydrocephalus. It also frequently coincides with diphtheritic inflammation of the skin or apertures of the mucous membranes. Eight of these 40 children were the subjects of congenital syphilis. When one eye is affected the other often suffers, whatever precautions may be taken to prevent inoculation. Epidemic influences are its principal cause, for after months have elapsed without a case occurring, great numbers may be suddenly met with. New-born infants are not especially predisposed to it, although in the ophthalmia they suffer from, a fibrinous exudation, giving a certain amount of rigidity to the eyelid, may be observed. The affection is indubitably contagious. As already-existing inflammation, especially when traumatic, predisposes to it, operations must be abstained from when the affection prevails epidemically.

Diagnosis and Treatment.—The only affection it can well be confounded with is gonorrhœal ophthalmia, and the author exhibits the distinctive signs at some length. With respect to treatment, copious depletion, by means of leeches applied near the angle of the eye, but especially to the root of the nose, is recommended by M. Gräfe. Ice-cold affusions are also to be frequently applied, and the eye is to be kept scrupulously clean, for which purpose milk is one of the best appliances. In certain forms of the disease caustics may be required. M. Gräfe strongly recommends the energetic employment of mercury, this being the only internal medicine of any value. The regimen must be strict, and but little fluid should be taken. In several cases the second eye has been preserved from an attack by keeping it closed.

IV. *On Tracheotomy in Croup.* By M. André. (Bullétin de Thérapeutique, tome lii. p. 471.)

The medical officers of the Hôpital des Enfans have long advocated an early performance of the operation of tracheotomy in croup; and M. André, one of the *internes* of that institution, has just published an account of the operations performed during 1856. The following table exhibits the results according to age:—

Age.	Total.	Deaths.		Recoveries.	
		Boys.	Girls.	Boys.	Girls.
15 months to 2 years	6	2	4	—	—
2 to 3 years	9	4	3	2	—
3 to 4 "	13	5	4	4	—
4 to 5 "	11	6	3	1	1
5 to 6 "	6	3	1	1	1
6 to 6½ "	3	1	1	—	1
7 years	2	—	1	—	1
8 "	2	—	1	1	—
9 "	1	—	—	1	—
9½ "	1	—	—	—	1
	54	21	18	10	5

Thus it will be seen that a considerable proportion of the cases were successful, and that this has been so in proportion as the children have been advanced in age. In all the children of less than two years of age, the operation proved fatal; and the others who succumbed, with two exceptions, scarcely exceeded that age. In the two older children (seven and eight years of age) who died, there were other causes of death independently of the operation. The explanation of this circumstance M. André supposes to exist in the fact that children of four years of age, who recover more frequently than younger children, offer greater resistance to both the accidents of the operation itself, such as hæmorrhage and traumatic fever, to the diphtheritis, and the complicating affections, such as capillary bronchitis and pneumonia. They are also more docile, and allow more readily of the repeated examinations of the wound and canula that are necessary; while suitable diet, so essential, and so difficult of management in very young children that have been operated upon, is more easily regulated. It is probably also due to the greater power of resistance possessed by boys, that the proportion of their recoveries exceeds that of the girls. Another circumstance to be mentioned is the deplorable facility with which children who have not already had the measles or scarlatina contract these affections upon admission into the hospital; and although, usually, eruptive fevers are uncommon prior to the fifth year, scarlatina attacked no less than ten of these little patients, of whom a third part died. M. André agrees in the justice of the opinion long held by the officers of the hospital, that the ulterior success of tracheotomy is much interfered with by the earlier employment of debilitating remedies, such as venæsection, leeches, blisters, &c.

V. *A Case of Strangulated Hernia Obturatoria.* By Dr. Lorinser. (Wien Wochenschrift, 1857. No. 3.)

As far as the author is aware, there is but one case on record (by Mr. Obrě, in the "Medico-Chirurgical Transactions," vol. xxxiv.) in which this form of hernia has been recognised during life and relieved by operation; and even in that instance, the nature of the hernia was not suspected prior to the commencement of the operation. He now relates a case which occurred to himself, the nature of which was detected, and an operation performed with success.

On July 21st, 1856, a feeble, spare woman, aged sixty-five, while reaching a heavy object from on high, felt as if something burst in the groin. She was seized with pain in the abdomen and vomiting, and was brought to the hospital, and as there was no appearance of a hernia, she was at first supposed to be suffering from simple peritonitis. To the other symptoms were, however, added obstinate constipation, and after a while, fæcal vomiting. The author first saw her on August 1st—i.e., eleven days after the accident—when she exhibited general symptoms of the most unfavourable description. On examination, the inguinal and femoral canals were found quite free, but the triangular space formed by the adductor longus, Poupart's ligament, and the femoral vessels, was observed on the left side to be level with the surface, instead of depressed as on the right side. When the fingers were passed upwards towards the foramen ovale, a soft swelling, about the size of an egg, and sensible to pressure, was perceived behind the pectineus, stretching from the foramen ovale to the outer border of the adductor. It was placed posteriorly, and somewhat internally to the pectineus, and was yielding rather than tense, the colour and temperature of the skin covering it being normal. Upon percussion, the tumour imparted a deep, full, tympanitic tone. From the vagina, a somewhat tense, very sensitive tumour could be felt at the posterior edge of the foramen ovale. All movements of the thigh caused pain. The diagnosis was much facilitated by the sparseness of the woman, and by the tympanitic tone elicited by percussion over the whole circumference of the tumour.

From the duration of the strangulation, and from the full, deep tympanitic sound, it was concluded that the intestine had become perforated and gas effused into the sac. An operation was, however, resorted to, and the pectineus being brought into view, it was slit up, somewhat obliquely, upon a grooved director, as far as the border of the adductor. Immediately behind it was found cellular tissue filled with exudation, and then the discoloured and softened sac. On opening this, a stinking fluid, partly watery, partly purulent, containing particles of fæcal matters, flowed out. The finger was now passed into a cavity which was bounded upwards by the obturator ligament, and in a cleft at the upper part of this ligament, and in part adhering to it, lay a relaxed and collapsed portion of intestine, about the size of a walnut. Behind the intestine, at the lower angle of the cleft in the ligament, the pulsations of the obturator artery were supposed to be felt. On account of the great depth of the parts, no ocular examination of the intestine could be made. As the intestine was ruptured, and sufficient egress of the contents was secured, further division of the fibrous cleft was abstained from, in order not to endanger the separation of the recent adhesions, and consequent fæcal effusion into the abdomen. The chief care was employed to secure a free discharge of the fæcal matters, preventing them lodging in the surrounding cellular tissue. To this end the cavity of the sac was well syringed out, linen rags being then applied, and the whole covered with cold applications. A clyster of tepid water was ordered every two hours, and the diet was low.

As soon as a certain amount of fæcal matters had been discharged through the wound, all symptoms of strangulation ceased. For the first few days, the discharges of fæces were pretty frequent, rendering the repeated cleansing out of the wound requisite. The clysters were soon employed but twice a day, a small quantity of fæces being discharged per anum. As these discharges continued, and contained matters of which the patient had partaken since the operation, there seemed every probability that only one side of the intestinal noose had been strangulated, the uninjured portion keeping up the communication between the upper and lower portions of the gut. The wound gradually cleansed and diminished in size, while the woman's strength and appetite increased until the end of August, when she became the subject of bronchitis. This delayed her progress, but by November the fæcal fistula, which had long been inconsiderable, had quite closed, and she only remained longer in the hospital on account of the chest affection.

VI. *On Bleeding from the Ear as a Consequence of Injury done to the Chin.* By M. Morvan. (Archives Générales, cinquième série, tome viii. pp. 653–654.)

Bleeding from the ear as a consequence of *contre-coup* has been accepted by surgeons as an almost certain indication of fracture of the base of the cranium. M. Morvan has, however, met with two cases in which injury to the chin gave rise to this phenomenon. The subject of the first of these was a robust lad, five years of age, who had, five or six hours before the author's arrival, fallen on his face on the pavement from a height of several feet. Immediately after the fall, a large flow of blood took place from the right ear, this being continued, when M. Morvan saw him, only in occasional drops, in which condition it lasted for three days longer. No fracture or dislocation of the jaw could be detected, and the membrana tympani was not ruptured. The child suffered much from pain in front of the right ear, from attempts at deglutition, and from any movement of the jaw. In the second case, a very strong man, aged forty-seven, received a kick from a horse on the chin, which almost deprived him of consciousness, and gave rise to an abundant jet of blood from the right ear. No fracture or dislocation could be found, but deglutition was excessively difficult. Prompt depletion dissipated the cerebral symptoms, and all went on well, a considerable amount of deafness remaining in the right ear. The membrana tympani was uninjured.

On searching, the author has been able to find only three analogous cases, and these only meagrely detailed, making thus, with his own, five cases. In three of these the blow on the chin resulted from a fall, and in two was produced by a kick from a horse. In three, the bleeding took place from one ear, and in two from both ears. In the author's cases the force acted obliquely, and the bleeding occurred on the opposite side to that of the point of contact. When bleeding has occurred from both ears, the blow has been central. In one only of the five cases did fracture of the jaw occur. In order to produce bleeding by this form of *contre-coup*, it is probably necessary that the shock should be entirely transmitted to the articulation of the jaw, while when fracture takes place, its force is usually exhausted in the production of the lesion of the bone. In the three cases in which the point has been noted, the difficulty of deglutition and mastication has been excessive at first, and has continued for a long period; and M. Morvan suggests that the lesion which gives rise to this symptom, as well as the bleeding from the ear, is a fracture across the glenoid cavity, which explains the occurrence of the abundant hæmorrhage, the membrana tympani remaining entire. Some experiments he has made in the dead body, by inflicting blows upon the chin, have failed to produce this form of fracture, but have induced fracture of the base. Thus no doubt can exist that this description of *contre-coup* may also produce fracture of the base, with bleeding from the ear, and rupture of the membrana tympani; but when we meet with such bleeding as a consequence of violence done to the chin, and without rupture of the membrane, the hæmorrhage may be regarded as a far less dangerous symptom.

VII. *On Secondary Syphilitic Affections of the Lachrymal Passages.* By M. Lagneau, Jun. (Archives Générales, cinquième série, tome ix. pp. 536–555.)

M. Lagneau, after taking a review of the scattered observations which have been made upon this subject by various authors, and narrating the particulars of four cases that have come under his own notice, arrives at the following conclusions :

1. The syphilitic nature of certain affections of the lachrymal passages seems sufficiently proved. 2. There is usually more or less complete obliteration of one of the lachrymal points, which is generally caused by an osseous lesion (periostitis, exostosis, caries, or necrosis), having its seat in the os unguis and the ascending apophysis of the maxillary bone—sometimes in the angular apophysis of the frontal bone. Sometimes the closure seems to depend upon swelling of the inflamed mucous membrane, arising from chronic blephoritis, and at others upon a lesion analogous to and accompanying naso-palatine syphilitic affections. 3. The symptoms which enable us to distinguish syphilitic from other affections of these parts, are the existence of a hard, resisting tumour,—the chancrous appearance of the cutaneous surface of the fistula lachrymalis, when this is present,—the co-existence of syphilitic affections of the mucous membrane, or bones of the palate and nasal fossæ,—the presence of syphilitic eruptions of the face, and the co-existence of supra-orbital cephalalgia and exostoses,—together with the history of the patient, or the discovery, on inspection, of the marks of syphilis on other parts of the body. 4. The progress of the affection is usually indolent, although sometimes there is a certain amount of erysipelatous inflammation. 5. The prognosis of syphilitic disease of the lachrymal passages and of adjoining parts (ancholops, ægilops) is less serious than when the same affections are not induced by a specific cause, for, when it is employed in time, they are usually curable by appropriate treatment. 6. When the obliteration is due to only a lesion of the soft, in place of the osseous parts, we may sometimes, as a palliative treatment, re-establish the channel for the tears by means of catheterism. 7. Most of the cases recorded have been successfully treated by mercury; but the author thinks, with M. Tavignot, that the iodide of potassium may be usefully employed when the bony parts are affected.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)

LETTSOMIAN LECTURER ON MIDWIFERY, ETC., ETC.

I. Physiology and Pathology of the Unimpregnated State.

1. *On Amyloid Degeneration of the Female Sexual System.* By Virchow. (Monatsschr. f. Geburtsk. April, 1857.)
2. *Case of Artificial Enucleation of a Large Fibroid Tumour of the Uterus.* By T. F. Grimsdale, Esq. (Liverpool Med.-Chir. Journal. January, 1857.)
3. *Clinical Researches on Peri-Uterine Phlegmons.* By G. Bernutz and E. Gonfiil. (Archives Gén. de Méd. March, 1857.)
4. *Case of Peri-Uterine Hæmatocele Cured.* By Dr. Marc Picifio. (Gaz. Méd. d'Orient. May, 1857.
5. *On Blistering the Cervix Uteri.* By Dr. Johns. (Dublin Quart. Journ. May, 1857.)
6. *On the Treatment of Ovarian Dropsy by Iodine Injections.* The discussions in the French Academy of Medicine. (Union Médicale; Gazette des Hôpitaux. 1856 and 1857.—M. Dolbeau: Gazette Hebdomadair. Oct., 1856.—M. Philipart: Gazette des Hôp. January, 1857.—M. Pignaut: Moniteur des Hôp. January, 1857.)

1. Virchow has related to the Berlin Obstetrical Society a case exhibiting a form of disease of the female generative organs not hitherto described, which he calls "Amyloid Degeneration." It was that of a woman who had sunk under amyloid degeneration of different organs, and who exhibited the entire sexual system affected by this peculiar process, which of all diseases is that which has the widest extension throughout the organism. The degeneration extended over the ovaries, tubes, and uterus, the last being in consequence enlarged, transparent, clear, and remarkably anæmic. The deposition of the amyloid mass had here particularly followed the organic muscular fibres, so that scarcely one of these did not present the characteristic iodic reaction. The disease had in this woman, as commonly happens, run its course concurrently with the symptoms of Bright's disease.

2. Mr. Grimsdale's case of artificial enucleation of a large fibroid tumour of the uterus is a valuable illustration of the pathology and therapeutics of this affection. The tumour occupied the posterior wall of the uterus and cervix: an incision was made in the posterior cervix, forming a sort of artificial os for the tumour; through this the tumour was gradually pro-

jected by the contraction of the womb, ergot of rye being repeatedly given, and on the fifteenth day it was wholly severed by breaking down adhesions with the finger and the scissors. A good deal of irritative fever accompanied the extending process. No hæmorrhage attended the final separation. Mr. Grimsdale contends that the operation for enucleation is occasionally not only justifiable, but desirable.

3. Drs. Bernutz and Gonfil have contributed a valuable clinical contribution to the history of peri-uterine inflammation, an affection which, under different names, has lately attracted particular attention. They first relate in detail two cases of peri-uterine inflammation, in which swellings, taken to be symptomatic of pelvic cellulites, had been recognised during life, but which, terminating fatally through intercurrent diseases, were found on dissection to present evidence of peritoneal inflammation only.

Case I.—Absence of previous uterine affection, blennorrhagia occupying the urethra, vagina, and uterus; the twelfth day after the commencement of this blennorrhagia, general illness and acute pains in the inferior part of the belly. On the twentieth day, when admitted into the l'Ourcine Hospital, very acute pain was manifest in the hypogastric region, especially on the left, and the touch revealed a tumefaction surrounding three-fourths of the uterine neck. An acute pleurisy supervened, which destroyed the patient. Peritoneal adhesions were found uniting on the median line the bladder and the anteflexed uterus, and fastening the uterus to the ligmoid flexure and rectum; on the right, adhesions united the sigmoid flexure, which was much "elbowed," to the broad ligament; on the left, adhesions united the parietal peritoneum to the broad ligament, the sigmoid flexure, and rectum. Between these organs, that is, placed behind the broad ligament, in front of the ligmoid flexure, was an intra-peritoneal collection of pus in contact with the left ovary, the proper tissue of which was healthy, as was that of the opposite side. The cellular tissue which surrounded the uterus and broad ligaments was healthy.

Drs. Bernutz and Gonfil are of opinion that the blennorrhagic inflammation extended along the Fallopian tubes, leading to an ovaritis, in a manner analogous to the extension of blennorrhagic inflammation along the epididymis to the testicle in the male. They remark that the dissection which revealed the healthy state of the cellular tissue of the uterus and appendages compelled them to renounce the conclusion they had arrived at during life, that the cellular tissue was the seat of the peri-uterine induration. This was in reality formed by the peritoneal adhesions binding the pelvic organs together in an irregular manner.

Case II.—Regular menstruation; pregnancy; labour and puerperal stage normal; vaginitis cured without having caused any morbid disorder of the uterus; syphilis. During the treatment of this disease, the catamenia, heretofore regular, were suppressed after two days. Shortly afterwards, development of a retro-uterine phlegmon; malignant small-pox, rapidly fatal. The autopsy revealed uterine deviation; peritoneal adhesions between the posterior surface of the uterus and rectum; the peri-uterine cellular tissue healthy; inflammation of the Fallopian tubes. Drs. Bernutz and Gonfil make a similar commentary upon this case. They remark that in them and in another (cited in a previous Midwifery Report), peritoneal adhesions binding together loops of intestine, may so closely simulate a tumour containing a liquid as to deceive even skilful exploration.

The next case exhibits a peri-uterine phlegmon, originating in a more usual way—namely, in labour.

Case III.—Hysteria; dysmenorrhœa; cessation of hysterical complications when the patient became pregnant; laborious delivery, followed in a few days by metro-peritonitis, which persisted in a latent form for five months. At this period the catamenia returned, and at the same time the acute symptoms. She was admitted into the wards of M. Valleix, when the existence of a purulent collection, which soon emptied itself by the rectum, was recognised. Scarcely discharged from the hospital, the symptoms returned. Admitted under M. Nélaton, who recognised peri-uterine phlegmon. Relieved, she returned to the care of M. Valleix, who after some time applied his uterine pessary, in order to cure the pains experienced by the patient, and which he attributed to the existing retroversion. The instrument was abandoned, on account of the pain and renewal of hysterical attacks it produced. Concurrently with these hysterical attacks, pelvic pains, and existence of peri-uterine tumours, for which the patient successively entered the wards of MM. Gendrin, Monat, Briquet. The symptoms abated concurrently with the spontaneous cessation of menstruation and the development of pulmonary phthisis. For this last disease she came under the care of M. Bernutz, and died five years after the labour to which she ascribed all her sufferings. The autopsy revealed peritoneal adhesions binding together all the pelvic organs, and these to the pelvic peritoneum. The cellular tissue of uterus and broad ligaments was healthy. The authors infer that the terms "pelvic cellulitis," "peri-uterine phlegmon," imply an erroneous pathology, and ought to be replaced by peri-uterine peritonitis. It is a return to the old pathology. They cite Andral's authority in support of their interpretation of the last case.

4. The diversity of opinion yet entertained as to the pathology, diagnosis, and treatment of the various forms of pelvic sanguineous effusions, renders it desirable to collect cases throwing light upon this affection. That of Dr. Picifico is interesting. A Turkish woman, aged fifteen, menstruated from thirteen normally; had been married nine months. Three months after marriage, she began to experience bearing-down pains, and sexual intercourse became too painful to be continued. When examined, a large ovoid tumour, with limits well marked, resisting, elastic, painful on pressure, was felt extending obliquely from below upwards from the left iliac fossa to the level of the umbilicus; excessive pains in groins, especially in the left, and in the loins. Examination by vagina reveals a large tumour bulging into the vagina and filling it; the neck of the uterus—very high—was thrown forwards, and to the right. The temperature of the vagina was increased, its sensibility also; fluctuation was very clear. The tumour also pressed upon the rectum; no febrile movement or shivering. An exploratory puncture was made at the most fluctuating point of the vaginal tumour. A dark red liquid escaped in small quantity. A discharge of similar fluid continued for some days. Fifteen days after puncture, the tumour had diminished considerably, but the patient was much exhausted. Frictions with iodine ointment, and steel pills were prescribed. Three weeks after puncture, a severe hæmorrhage suddenly took place: the tumour had opened in the vagina, and discharged a considerable quantity of blood. This ceased on the following day. Great febrile irritation, with shiverings and depression, followed, and fluctuation increased. A second exploratory puncture was made by the vagina, and then ichorous and very fœtid pus escaped. The opening was enlarged by a bistoury, and the sac washed out by injections. Instant relief followed. Under quinine and good diet, the patient recovered favourably, the tumour completely disappearing.

Dr. Picifico remarks that there appeared two distinct stages in this case: the first, *hæmorrhagic*, the second, *phlegmasic*. Was the hæmorrhagic effusion caused by the ovarian irritation produced by early marriage?

5. Dr. Johns recommends the practice of blistering the cervix uteri in those cases especially in which, after active inflammation has been subdued, neuralgic pains persist. The agent he employs is the following: a strong solution of cantharides in chloroform, in which some gutta-percha is afterwards dissolved. This is applied by a camel-hair pencil. It is said to cause very little pain, either during or after application. Small vesications appear at the time, and a watery discharge sets in within half an hour. This discharge starches the linen, and is in other respects similar to that from blisters externally formed; it lasts commonly for three days, when to it succeeds one of a slightly purulent nature, but not productive of pain. At this stage the epithelium is thickened and raised, and comes off in patches, like bits of chewed paper. Dr. Johns has never seen strangury follow. He relates several cases illustrating the beneficial effects of the remedy.

6. The question how far and in what cases iodine injections can be trusted to as a cure of ovarian cysts, is one of great interest in itself, and it has lately assumed a greater relative importance since the operation of ovariotomy has been generally condemned. The question is mainly to be solved by experiment. The collection of authentic cases in which this method has been tried is, therefore, a task of immediate practical utility. In the recent discussion upon this subject in the French Academy of Medicine, M. Caseaux brought forward from Boinet, Monod, Nélaton, Demarquay, Giraldés, Maisonneuve, Simpson and others, 117 cases of ovarian cysts in which iodine injection had been used. In some of these it was previously determined to close the wound immediately after the operation; in others, to incite suppuration by leaving a canula in communication with the sac. It appeared that the iodine injection had acted most favourably in all; for in all the 117 cases, not one exhibited a serious mishap. There were 62 cases of unilocular serous cysts, of which 48 were cured; 11 were operated upon without result; in only 4 did death follow. In the 11 cases some improvement was observed.

The experiments with the retained canula were, for the most part, unfortunate.

To this summary we will add some cases recently recorded:

1. M. Dolbeau: A woman aged thirty-nine, had borne two children. Symptoms of ovarian disease had commenced seven years back. Two years back the tumour had been apparently spontaneously healed by the bursting and escape of a quantity of fluid through the vagina, but in six months the tumour had resumed its former size. In December, 1855, the sac was punctured, and four litres of albuminous fluid drawn off. The canula was left in the sac. Purulent discharge came after a time. On the eighth day the purulent matter was fœtid, and an iodine injection was thrown in; but the patient got worse, and sank under profuse diarrhœa on the 19th. Autopsy showed the cyst much contracted; the walls were fibrous, of various

thickness, and in three points occupied by fibrous tumours. The inner surface was lined by yellowish, strong, resisting membrane, pieces of which were found in the fluid and plainly consisted of a combination of the iodine with the organic substance. It contained small vessels, and was easily separated from the wall of the sac.

2. M. Philipart's case. A woman aged twenty-three, had menstruated regularly from the age of sixteen. Increase of size of abdomen first observed in 1852. On September 25th the first puncture was made, and twenty-five litres of albuminous water withdrawn. This operation was repeated thirty-five times, down to October 18th, 1855. The condition of the patient had become worse. Iodine injection was then determined upon. Thirty grammes of tincture, 4 grammes of iodide of potassium, and 250 grammes of water were injected; no pain. Irritation at first; but in a few days improvement. On October 26th, the injection was repeated; considerable reaction ensued. On January 3rd, 1856, the third puncture; eight days after this her condition was quite satisfactory. The collection, however, returned, and down to March 6th, three more punctures were made, and the canula left in the wound. The patient is said to have been destroyed by an unskilful washing out of the cyst by a nurse, which caused a fatal peritonitis.

3. M. Pignaut's case. A woman aged fifty, had recognised a simple cyst for fourteen years, which had latterly become so large as to be unbearable. After puncture, iodine injection, no reaction followed. Cure was complete in fourteen days.

Several other cases have been reported in the British journals, which want of space forbids us from citing. Several cases are known to be under observation. It is hoped that within a reasonable time a sufficient body of experience to determine the application and value of this method will be amassed.

II. Pathology of the Fœtus.

Case of Double Fœtus. By Professor Meigs. (American Journal of Medical Science, January, 1856.)

Dr. Meigs's case of monstrosity is remarkable. It is an example of more or less perfect fusion of two embryos. The two heads present the least perfect fusion. The genital organs are female. The right eye of the right and the left eye of the left fœtus are perfect; while the right eye of the left, and the left eye of the right fœtus are fused together into one single eyeball, covered by a compound palpebra with three canthi, the fused inner canthus being in the middle of the lower eyelid. All traces of the outer canthi of this compound eye are lost. In the fusion of the heads, the left and right ears of the right and left fœtuses are lost, with the exception of a small tubercle seen in the middle of the faces. The left fœtus has a double hare-lip, and the right one a common hare-lip. The calvaria are deficient in both children, so that they are anencephalous. There was also failure of development of the spinous processes and bridges of the cervical and dorsal vertebræ, giving rise to spina bifida of both the dorsal and cervical ranges, while the lower lumbar and sacral vertebræ are perfectly well developed. This circumstance, Dr. Meigs observes, is interesting, showing as it does, that the simplicity apparent in the two well-formed arms and legs, and the single trunk, is nevertheless a real duality of individuals. The dark dermoid excrescence that covers the top of the head is too imperfect to contain any hair-follicles. There was but one navel and umbilical cord. Dr. Meigs however concludes, from physiological reasoning, that the liver was double. The specimen was not dissected, in order not to diminish its value as a museum object.

III. Pregnancy and Labour.

1. *Account of a Case in which Impalement of the Uterus occurred in the Production of Criminal Abortion.* By F. Broughton, Esq. (Transactions of the Medical and Physiological Society of Bombay, 1855, 1856.)
2. *Case of Ruptured Uterus.* By M. Reynolds, M.D. (Charleston Medical Journal, January, 1857).
3. *Rupture of the Uterus. Gastrotomy successfully performed.* By John H. Bayne, M.D. (The American Journal of Med. Science, Jan., 1857.)
4. *Report of Seven Cases of Transfusion of Blood, with a Description of the Instrument invented by the Author.* By Alfred Higginson, Esq. (Liverpool Med. Chir. Journ., Jan., 1857.)

1. Nursingua was fifty years old. Her husband having died two years, she formed an inti-

macy with a goldsmith, and pregnancy was the unexpected and undesired result. To obviate discovery and evade the punishment inflicted in this country upon both parties when convicted of adultery, she was as usual tempted to submit to measures for the induction of abortion. A five months' fœtus was expelled. Slight hæmorrhage but considerable pain followed. The pain increased and she was sent into hospital. She died in a few hours.—*Autopsy:* The abdomen was immensely distended with serum. Bands of coagulate lymph passed in every direction, of recent formation. The peritoneum fiery-red, the whole mass glued together. Upon lifting up the bowels some difficulty was experienced in exposing the uterus, owing to the presence of a rod, which passed through the fundus and projected three inches into the bowels, which were lacerated, and hanging upon its extremity. The other and broken end occasioned the abrasion of the internal membrane. The uterus was firm, and tolerably contracted, and would appear to have been more passive under such fearful injury than the intestines. It is remarkable that the patient was fifty years of age, and that she survived this injury ten days.

2. Dr. Reynolds' case of ruptured uterus is referred to because it illustrates the conservative behaviour of the uterus in certain cases of this injury. A negress, in labour with twins, suffered rupture of the uterus during the expulsion of the first child. The second escaped into the abdomen. She died shortly afterwards. On autopsy, the fœtus and appendages were discovered in the abdomen. The uterus was so firmly contracted as to efface all visible mark of laceration until it was distended by passing the hand into the cavity. The seat of the laceration was the cervix; it ran somewhat obliquely, and measured in the contracted uterus three inches. A little coagulated blood was found in the cavity, and very little effusion of blood into the peritoneal cavity. There had been no hæmorrhage during labour. There was slight inflammation of the peritoneum. The viscera were all in a state of integrity. [It may be assumed that this poor woman died of shock, since the strong contraction of the womb prevented all hæmorrhage. The Reporter has witnessed a case of recovery after rupture of the womb, in which powerful contraction of the womb was the conservative agent. The rupture occurred during the delivery of triplets. The children and three separate placentæ were expelled *per vaginam.* A large loop of intestine was *felt and seen* protruding between the thighs. This was spontaneously retracted, and the woman completely recovered. This tendency of the uterus to contract being an essential element in the process of recovery, should not be frustrated by attempts to drag the child back through the rent, in order to deliver *per vias naturales.* If the child has been cast into the abdomen, gastrotomy seems to be indicated by nature.—R. B.]

3. Dr. Bayne's case of rupture of the uterus occurred in a woman aged twenty-five, of robust constitution, in her fourth labour. When seen she complained of having experienced some hours previously an excruciating pain in the epigastrium, accompanied with a peculiar tearing sensation. There was then an entire cessation of pain and of all expulsive uterine efforts. Pulse 130. Difficulty of respiration, and prostration. Coma, and tendency to collapse. The head of the fœtus, felt at first presenting, seemed to be rapidly receding; and in a very short time the entire contents of the uterus had escaped into the abdomen. Child then felt very distinctly through the parietes of the abdomen. An extensive laceration had taken place in the anterior portion of the fundus. Gastrotomy was immediately performed, without anæsthetic agents; as soon as the abdominal cavity was opened, there was a sudden gush and escape of at least one quart of sero-sanguineous fluid. No hæmorrhage. A very large fœtus was removed. Womb at the rupture thin, and the laceration jagged and irregular. Peritonitis set in on third day. A dark grumous, offensive, purulent discharge continued *per vaginam* for several weeks. She recovered. Dr. Bayne observes that the antero-posterior diameter was less than the standard, and the capacity of the pelvis less than usual. Her labours had always been protracted. He does not refer to the condition of the womb as regards contraction.

4. The cases of transfusion reported by Mr. Higginson are of extreme interest. We cannot extract the description of the author's instrument. It is ingeniously contrived to keep the blood used for transfusing warm, and to prevent the injection of air into the vein.

The following is a summary of the cases:

Case I.—*Extreme prostration from protracted suckling of twins.*—The exhaustion was extreme; the patient fainted when raised from her pillow; it was considered she would die during the night. Twelve ounces were injected from a healthy female servant; a state of quietude succeeded; pulse improved; she seemed sleeping; but in a few minutes a rather severe rigor came on. This did not last; reaction occurred; the patient sang a hymn in a loud voice. She steadily recovered.

Case II.—*Hæmorrhage after birth of child, on expulsion of placenta.*—Complete prostration from sudden loss. The lady's sister supplied the blood, and between ten and twelve ounces were easily injected. The benefit was immediate and striking, and no bad symptoms retarded her recovery.

Case III.—*Hæmorrhage from Placenta Prævia. Fœtus retained.*—Sudden and exhausting hæmorrhage. The placenta had been removed; the child's head occupied the os uteri, and hæmorrhage was over. The skin was of a livid hue, as in the asphyxiated stage of cholera. A female servant gave the blood, and six or eight ounces were injected, when a sudden jactitation jerked the pipe from the arm; coagulation impeded the operation. She died within half an hour, undelivered.

Case IV.—*Hæmorrhage from adherent placenta. Uterus empty; hæmorrhage ceased.*—Twelve ounces were injected. The patient lived seven days, but gradually sank. Uterus found internally purulent and offensive. No disease of the veins, either in the uterine region, or in the arm where the puncture had been made.

Case V.—*Partial Placenta Prævia. Hæmorrhage before delivery.*—Partial separation; great hæmorrhage. Woman appeared sinking, neither placenta nor fœtus expelled; hæmorrhage had however ceased. The blood injected was "dark and sluggish," and produced scarcely any effect, not more than five or six ounces having entered. Warm water with a little common salt was then injected to twelve ounces, slightly improving the circulation. Delivery was then speedily effected, but life was extinct before this was fully accomplished. [The Reporter would observe, that although the delivery was here, according to received rule, forced, yet this operation was precisely calculated to extinguish the patient. Why was it held necessary? The hæmorrhage had ceased. But it might return? This fear is not altogether justified by the true physiology of placenta prævia, nor by clinical experience. The natural hæmostatic stage had been reached; the labour had truly become a natural labour.]

Case VI.—*Mania: refusal to take food; exhaustion; approaching collapse.*—Pulse had disappeared from the radial arteries. A good supply of rather dark blood was obtained from a female. Pulsation returned at intervals, the breathing improved, and the expression of the countenance became much better. Twenty ounces or more were injected. The following day she appeared much better, but sinking came on, and she died in forty hours. The heart contained dark fluid blood.

Case VII.—*Placenta prævia (forced); delivery, and subsequent draining; transfusion and rally of the patient; return of flooding; death.*

MEDICAL INTELLIGENCE.

Vaccine Stations at Hospitals.

The 'Annual Report of the National Vaccine Board,' issued in the month of March of the present year, concludes with a suggestion which we think cannot too soon be adopted; and as it would be difficult to discover any reasonable objection, we trust that by giving it further publicity we may secure its speedy realization. The Board are of opinion that it would be "very advisable that all the metropolitan hospitals should institute the plan of having one of their house-surgeons specially appointed to the duty of vaccination." It is obvious that, by the introduction of this feature, students would have an opportunity of becoming properly acquainted with the course of the vaccine disease, no less than with the proper mode of performing the operation. At present the young practitioner acquires this knowledge in a fortuitous way, and at the commencement of his practice feels less confidence in this matter than in more serious proceedings. And yet it is important that every practitioner should be well informed in regard to the theory and practice of vaccination. Moreover, if this subject is properly carried out at hospitals, we shall acquire a further field of study, which will not fail to yield fruit. Thus the question of the influence of vaccination in the production of other diseases, which has a strong hold upon the public mind, has never been properly investigated, and, with others of a similar bearing, would be more readily solved if subjected to a general and careful scrutiny.

We may take this opportunity of reassuring the profession of the real value of vaccination. The Report gives us statistics which amply confirm the protective power of the vaccine lymph, while, *per contra*, unmodified small-pox is shown still to offer all its former terrors. We quote the following remarks from the document, in evidence:—"Mr. Marson, surgeon to the Small-Pox Hospital, states to the Vaccine Board, that in five years, from 1852 to 1856, 2253 patients have been admitted into the hospital with small-pox after vaccination. Of these, 355 had each four or more vaccine cicatrices. Three of these patients have died, one from small-pox, and two from superadded disease, wholly independent of small-pox; so that it may be fairly said, deducting two cases, that of 353 patients having four or more cicatrices, only one died. This number, added to the cases already published, makes 620 cases followed by three deaths, or rather less than half per cent.

"For sixteen years, in the published accounts, the unvaccinated cases die at the rate of 35 per cent.; and the patients badly vaccinated, having only one indifferent cicatrix, or none at all, but believing themselves vaccinated, died at the rate of 15 per cent. This statement shows, 1. The great loss of life from natural small-pox; 2. The great protective power of vaccination.

"The Board therefore feel fully justified in unhesitatingly reiterating, that vaccination properly performed is all but a complete protection."

Middle-Class Education.

Fact is indeed stranger than fiction. What many of us have yearned for, but what none could have expected to see realized, is come to pass: Oxford and Cambridge have spontaneously, and with all the gracefulness of ancient lineage, offered to become the patrons and promoters of the education of the middle classes of this great country. The silent working of the spirit of intellectual development, the germination of the seeds of mental growth, scattered with no sparing hand by the best of their generation through the land, have been observed and acknowledged by those whom we all would wish to regard as the watchers over the mind of the nation, no less than the tutors of the few. Oxford has taken the lead in an undertaking which the promoters themselves may scarcely compass in its promise of rich and never-failing fruit, which will develop the intellectual energies of the people of England, and fertilize the fields of mental culture now unproductive from mere want of seed, while it will redound to the honour of the Universities, and secure to them a power which will find no rival in ancient or modern story.

We should be loth to withhold our sympathies from any great movement affecting the highest weal of our compatriots; but the proposal to which we advert has a special bearing upon the medical profession, to whose attention we therefore warmly commend it. The Rev. F. Temple—to whom be all honour as the immediate originator of the scheme in question!—proposes that Oxford and Cambridge should undertake the task of guiding and testing the instruction given in the schools of the middle classes. Their education, he observes, in a letter addressed to the Master of Pembroke College, Oxford, suffers at present from the want of any definite aim to guide the work of the school-masters, and from the want of any trustworthy test to distinguish between good and bad. Mr. Temple dwells upon the unsatisfactory results generally obtained in the private schools of the middle classes, where the masters rarely sufficiently understand their duties, or know the precise object they are to aim at; and where they thoroughly understand their duties, they have no means of convincing the parents of their pupils that they are doing so.

Mr. Temple proposes no complicated scheme in order to place middle-class education on a proper basis. He asks that "the University should confer some such title as Associate in Arts on every person who passed an examination before examiners appointed either by the Hebdomadal Council, or by a delegacy, as might be thought best." This examination should pretty nearly follow the precedent set by the present final schools. "It should be held annually in Oxford. But if the gentry or local authorities of any place asked for an examination to be held in their neighbourhood, and would undertake to bear the expense of the necessary arrangements, an examiner should be sent down to them."

That the country will be glad to avail itself of a test such as the one proposed, can scarcely be doubted. There may be sluggards and dullards who would rather lag behind, or avoid entering into a new and an unknown course; but the *vis à tergo* will be too powerful for them, and the current once having set in, will sweep along with it all recusants.

The desire of the public for an independent standard has been manifested on various occasions. One of the best known schemes of examination of an analogous character is that commenced already by the Society of Arts; and the best evidence of the value in which it is held, is afforded by the number of persons who subject themselves to the ordeal, and the interest taken by their friends in the success or failure of the denizens of distant towns and counties, as recorded in the daily papers. But important and demonstrative as these individual instances of spontaneous action may be, they will fail of the universal influence that must be acquired, unless a general organization is secured. It is this that is held out by the proposed scheme of Mr. Temple.

The importance of the certificate to be obtained will be a sufficient security that the examinees will not be wanting. The security that the examiners will do their duty, will lie in their independent position,—independent as far as petty and local or nepotic influences are concerned, dependent only upon the controlling influence of enlightened public opinion.

The medical profession cannot, we are assured, but hail the prospects of a high standard of testing preliminary education—the education that must precede professional studies, if those studies are to bring fruit commensurate with their importance. Our medical corporations have striven nobly to secure this preliminary education in all their candidates, but it is manifestly not the proper sphere of a licensing body to do more than to ascertain the fact that the candidates have gone through a suitable curriculum. It will therefore necessarily be their interest to promote such a scheme as the one proposed, by requiring all their candidates not possessed of a University degree, to pass the examination before the Oxford or Cambridge Board. We would suggest that they at once put themselves in communication with the Board, as soon as constituted, so as to arrange about the character of the examination which they would wish to regard as a minimum qualification.

With these few remarks, and with a special request to our readers to study a pamphlet by Mr. Acland,* on 'Middle-Class Education,' in which these questions are amply discussed, though without reference to individual classes, we introduce the subject to our readers, fervently hoping that they may, as far as in them lies, give their full aid in realizing a scheme fraught with immeasurable benefit to our children and children's children.

Since the preceding observations on Middle-Class Education have been in type, we have received the following Report, which we have much pleasure in introducing to our readers:

"*Report of the Committee on Middle-Class Examinations.*—(Dr. Williams, Vice-Chancellor; Dr. Cotton, Provost of Worcester; Dr. Jeune, Master of Pembroke; Dr. Scott, Master of Balliol; Dr. Cradock, Principal of Brasenose; Professor Macbride, Principal of Magdalen Hall; Professor Daubeny; Professor Pusey; Mr. Michell; Mr. Gordon; Mr. Mansell.) Your Committee,—having taken into consideration, in connexion with the subject referred to them, two letters addressed by the Rev. F. Temple to the Master of Pembroke, a Pamphlet on Middle-Class Education published by T. D. Acland, Esq., and numerous memorials to the Hebdomadal Council (which are appended to this Report),—and having had the advantage of a conference with the Rev. F. Temple and the Rev. H. W. Bellairs, two of her Majesty's Inspectors of Schools who attended by permission of their superiors, and with Mr. Acland, —report as follows:

"1. It appears a duty that the University should answer to the call made upon it, and endeavour to extend its beneficial influence to the education of classes now for the most part beyond its reach.

"2. For the instruction of the children of the poor, Parliament makes large provision. The Universities exercise a great influence, directly or indirectly, on the training of a considerable portion of the young in the upper classes.

"3. As to those, however, whose parents occupy an intermediate position, nothing is done, perhaps nothing could be done, beneficially, by the State; and there is little connexion between them and the Universities.

"4. But it is desirable that the efforts of good teachers in the schools frequented by this class of pupils should be guided, encouraged, and pointed out to public approbation; and, on the other hand, that parents should be put on their guard against incapacity and false pretences.

"5. It is desirable that the industry and talent of the boys should be stimulated by the prospect of attaining distinction of a higher character than can be gained in a small school; that promising youths should be pointed out to employers; and that all the talent of the country should be directed into the course in which it can be most effectively employed.

"6. A well-digested and well-administered system of voluntary periodical examinations and distinctions is calculated to effect these objects.

"7. Administered by the Universities, whose motives are above suspicion, and whose command of men of ability is very great, such a system would, it may be hoped, obtain the confidence of the country, and would produce all the fruits which may be fairly expected from it.

* Middle-Class Education. Scheme of the West of England Examination and Prizes for June, 1857. With Introductory Remarks addressed to Members of the Universities. By T. D. Acland, Esq. London: Ridgway. 1857. Price 1s.

"8. It appears to your Committee, that there should be two Examinations annually, one for boys under the age of fifteen, the other for boys under the age of eighteen; the former to secure soundness in the elementary training, without which more advanced education cannot be satisfactorily carried on; the latter to prove that the candidates are well fitted for the situations in life into which young men usually enter about that age, or for continuing their studies with advantage.

"9. The examination would probably be holden in the Long Vacation.

"10. Oxford itself would offer many advantages for the purpose; but it might be desirable that the examination should be conducted simultaneously in other considerable places, by an examiner or examiners deputed for the purpose. The papers should be everywhere the same.

"11. There should be an examination in the rudiments of religion, suited to the character of the University and to the age of the candidates; but not in cases where objections are signified by parents or guardians.

"12. The distinctions given at the first examination (beyond the certificate of the examiners) need not be numerous or great. At the second there should be a distribution of the candidates into classes according to merit; and some certificate, attested by the Vice-Chancellor, given to all who pass; showing their position in the award of the examiners, and conferring some title marking the connexion of the possessor with the University of Oxford.

"13. It is difficult to find a title of this kind wholly free from objection. Perhaps that of Associate in Arts (A.A.) of the University of Oxford, suggested by Mr. Temple, would be as good as any.

"14. It is impossible to foresee the number of candidates who may be expected annually to come forward; and therefore it appears inexpedient to propose any definite arrangements. Nor does it seem advisable to fix by legislation the system of examination in any detail, or the precise classification of the successful candidates.

"15. Your Committee is of opinion, that the best course for the University to pursue, is to appoint a delegacy of persons entitled to its confidence, with power to nominate such a number of examiners as may be from time to time required, to prescribe the subjects of examination, to frame a system of honours, to fix upon the times and places of examination, and to determine the salaries of the examiners and other officers who may be required. The experience of the delegates would probably enable the University at no distant period to frame a more definite statute.

"16. The delegacy should consist of the Vice-Chancellor, the Proctors, and eighteen other members; six chosen by the Hebdomadal Council from its own body; six by Congregation; and six added by the Vice-Chancellor and Proctors. The first delegacy to last for three years.

"17. The delegacy should be bound to make an annual report to the University.

"18. The system ought, in the opinion of your Committee, to be self-supporting; but should not be permitted to yield any profit. There is reason to believe that a fee of 5*s*. from every candidate for the first examination, and of 1*l*. from every candidate for the second examination, would be sufficient. The delegates ought, however, to be entrusted with the power of varying these sums.

"19. The Committee has reason to believe that the proposed scheme of examination has been favourably received by distinguished persons in the University of Cambridge. Should both Universities arrive at the conclusion that it is their duty to adopt it, the country will be well served by their harmonious, but independent, action. Your Committee would not in such a case propose any geographical division of labour, or any system of combined examination. The choice should be left to the convenience or inclination of the candidates or their parents.

"20. Your Committee recommend that a statute, in accordance with these suggestions, should be presented to Congregation and Convocation."*

Appended to this report are numerous memorials from all parts of the kingdom in favour of the scheme, not the least important of which is one signed by a large number of lecturers attached to the London schools of medicine.

*** On June 19th, 1857, the Statute on Middle-Class Education was submitted to Convocation, in two classes, each of which was carried.**

BOOKS RECEIVED FOR REVIEW.

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First Report of the Home and School for Invalid and Imbecile Children, Gayfield Square. Edinburgh, 1857.

The Report of the Committee of Visitors and Medical Superintendent of the Devon County Lunatic Asylum. Exeter, 1857.

Surgical Cases communicated to the Boston Society for Medical Improvement, Oct. 27th, 1856. By G. H. Gay, M.D. Boston, 1856. pp. 24. (Reprint.)

The History, Diagnosis, and Treatment of the Fevers of the United States. By Elisha Bartlett, M.D. Fourth Edition, revised by A. Clark, M.D., &c. Philadelphia, 1856. pp. 610.

On the Function of the Thyroid Body. By O. Martin, M.D., R.N. Proceedings of the Royal Society, Jan. 8, 1857.

Des Anévrysmes et de leur Traitement. Par Paul Broca, Chirurgien des Hôpitaux, &c. Paris, 1856. pp. 931.

The Structures, Functions, and Diseases of the Lungs. By Thomas Williams, M.D. Part I. Water and Air Lungs of Invertebrate Animals, and Aquatic Respiration. London, 1857. pp. 201.

The Functions and Disorders of the Reproductive Organs in Youth, in Adult Age, and Advanced Life. By William Acton, late Surgeon to the Islington Dispensary, &c. London, 1857.

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Die Iod und Schwefelhaltigen doppelt kohlensauren Natronquellen zu Krankenheit bei Tölz in Oberbayern. Von Dr. Gustav Höfler. Freiburg, 1856. pp. 28.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1857.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *J. F. Heyfelder über Resectionen und Amputationen.* Mit vier Steindrucktafeln.—*Breslau und Bonn*, 1855. pp. 269.
On Resections and Amputations. With four Lithographic Plates. By Dr. J. F. Heyfelder, Professor of Medicine at Erlangen, &c.
2. *Mittheilungen von Resectionen am Fuss.* Von Dr. Robert, zu Coblentz. (Vierteljahrsschrift für die Praktische Heilkunde.) xii. Jahrgang. Dritter Band.—*Prag.* 1855.
Contributions on the Subject of Resections at the Foot. By Dr. Robert, of Coblentz.
3. *De Articuli Humeri et Cubiti Resectione, Inauguralis Chirurgica Dissertatio.* Auctore Panagiostes Georgius Kyriakos.—*Berolini*, 1854. pp. 22.
Dissertation on Resection of the Shoulder and Elbow-joints. By P. G. Kyriakos.
4. *De Articuli Cubiti Resectione, Dissertatio Inauguralis Chirurgica.* Auctore Albertus Tobold.—*Berolini*, 1855. pp. 24.
Dissertation on Resections of the Elbow-joint. By Albert Tobold.
5. *On Excision of the Knee-joint.* By R. G. D. Butcher, Esq., M.R.I.A., Surgeon to Mercers' Hospital, &c. &c. ('Dublin Quarterly Journal of Medical Science,' May, 1855.)
6. *On Excisions of Joints.* By the same Author. ('Dublin Journal,' November, 1855.)
7. *Second Memoir on Excision of the Knee-joint.* By the same Author. ('Dublin Journal,' February, 1857.)
8. *Cases of Operation upon Diseased Joints.* By W. A. Green, M.D., Bengal Medical Service. ('Indian Annals of Medical Science,' April, 1855.)
9. *Gun-shot Fractures*, by Dr. Stromeyer, and *Resection in Gun-shot Injuries*, by Dr. Esmarch. (Slightly abridged.) Translated by S. F. Statham. *With some Remarks on Tonic Treatment*, by the Translator.—*London*, 1856. pp. 210.

10. *An Essay on the Excision of Diseased Joints.* By Mr. BLACKBURN. ('Guy's Hospital Reports,' first series, vol. i. 1836.)
11. *On Excision.* By Surgeon THORNTON, 9th Regiment. 'Reports of Crimean Medical and Surgical Society' ('Medical Times and Gazette,' Sept. 13th, 1856, and Sept. 20th, 1856.
12. *On Excision of the Hip-joint.* By Mr. HANCOCK, Surgeon to Charing-Cross Hospital, &c. ('Lancet,' April 18th and 25th, 1857.)

THE subject of resection of the articular ends of bones having been so prominently brought before the profession during the last few years, and having been so ably discussed and strongly advocated in the case of individual joints, by many eminent surgeons, it will be our endeavour in the following pages to collect and analyse the evidence in favour of or against its employment, in the larger articulations, and by comparing the results of the operation, to deduce such rules of practice as may be gathered from what has been already published on the subject, and chiefly from the works enumerated in the foregoing list.

The practice of resection originated in this country, and was first performed by Mr. Filkin of Northwich, who operated on the knee-joint on August 23rd, 1762. This case, though perfectly successful, was not published until years afterwards—indeed, not until after the death of the operator, and thus its benefits were confined to the individual on whom it was practised, and furnished no suggestions or data for the guidance of the surgeon who in reality established the principle and practice of resection. In 1768, Mr. Charles White* of Manchester, removed the head of the humerus for acute destructive disease of the shoulder-joint; four months afterwards, this patient, to the surprise of his surgeon, had regained to a great extent the movements of his shoulder-joint. About the same period, Mr. Wainman† (see p. 224) removed the lower articular end of the humerus, in a case of compound dislocation, with success. These two latter recoveries, and a remarkable case of recovery after traumatic necrosis of the joint-ends at the elbow, with sloughing of the capsule of the joint, encouraged Mr. Park to put in practice the excision of the knee-joint—an operation he had long before conceived, and had since made a constant subject of reflection. On July 2nd, 1781, Mr. Park performed the first resection of the knee-joint with perfect success. The patient, a sailor, returned to his ordinary duties of a seaman, and continued to follow his occupation, until his death, which occurred eight years afterwards. The same surgeon again operated on a less favourable case, the patient never recovering from the effects of the operation.

It is to Mr. Park that we really owe the enunciation of this mode of treatment; he it was who not only first published the account of resection of the knee-joint, but in his memoir on the subject, suggested the applicability of the same operation to other articulations. The truth of these speculations was soon put to the proof by M. Moreau, who excised the knee-joint once and the ankle twice‡—in every case with a favourable result, so far as the operation was concerned.

Notwithstanding the encouraging success that attended these efforts of conservative surgery, resection met with but little general encouragement, and though on the Continent Sabatier,§ Roux,‖ Fricke, Textor, Müller, and others, made use of the operation for different joints, with variable results, yet in this country, at the beginning of the present century, it had fallen into disuse, and especially in the treatment of the diseases of the knee-joint.

In 1829, Mr. Syme, following the example of Sir P. Crampton, who seven years before had resected the knee-joint in two cases, revived the performance of this operation on the shoulder and elbow-joints, and soon established its practice by his successful operations on these articulations, while by the less satisfactory result he obtained in one of two cases of resection of the knee, he consigned this latter operation to unmerited oblivion. Twenty years afterwards, Mr. Fergusson was

* Case of Surgery, vol. i. † Mr. Park's Letters to Mr. Pratt. ‡ Obs. Prat. relat. à la Résect. des Artic. Paris, 1803.
§ Mém. de l'Institut, tom. v. 1805. ‖ De la Résection. Paris, 1812.

bold enough to put the operation again in practice. His example was quickly followed by Mr. Jones, of Jersey, and others, since when, the operation has been frequently and successfully practised.

The general result to be aimed at in all resections is the restoration of a useful limb to the patient. The very different functions of the upper and lower extremities of the body have led to the practice of endeavouring, in operating on the former, to preserve a yielding bond of union in place of the excised joint; while, in excising the joints of the lower extremity, we endeavour, by all means in our power, to promote bony ankylosis between the divided ends of bone. To secure ultimate motion, it is neither necessary to remove all the articular extremities of a joint, nor even to destroy the cartilaginous surfaces of the bones; our practice in this respect is guided by the condition of the parts themselves. While, on the other hand, to obtain osseous union, it is absolutely essential that the opposed cartilaginous surfaces of the joint be removed, and that fresh-cut surfaces of bone should be brought and maintained in apposition.

There are fifteen recorded cases of examination of joints that have been subjected to resection, and where repair had either fully taken place, or was in progress. They are recorded by Syme,* Textor (quoted by Heyfelder), Heyfelder,† Roux, Thore, Dr. Green,‡ Reid, and Hutchinson.§ Seven were elbow-joint excisions, six were shoulder-joints, and the remaining two were cases where the knee-joint had been resected. No cases are here included but those where repair to some extent had taken place. The above-mentioned examinations were made at periods varying from three months to nineteen years after operation. From these it appears that in the upper extremity, with a single exception, no false joint was formed, nor anything comparable with a synovial capsule, but rather that the ends of the bones were connected by a tough fibrous tissue, which in most cases, and under appropriate treatment, permitted of no inconsiderable amount of flexion and extension. This tissue is not only a bond of union between the bones, but prevents their ends from coming into contact, and is itself the seat of the movements of the new articulation. The exceptional case is related by M. Roux, where, after resection of the elbow-joint, the ends of the bone were found smooth and rounded off. In but one case was any reproduction of the excised ends of bone discovered. The two examinations of resected knee-joints were made at intervals of three months and fourteen years after operation respectively. In the former, a considerable formation of callus had taken place about the divided ends of bone, while in the latter the femur and tibia had become one, by the direct growth of bone from one to the other. In all cases, both of the upper and lower extremity, the divided muscles and tendons were found matted together in the cicatrix of the operation.

When a surgeon, after perhaps a long and anxious struggle to save a patient's limb, has at last decided on the necessity for amputation, and has performed the operation, he rests, as it were, from his labours, and for the future merely bestows that amount of care and attention which constitutes all *he* can do to promote the recovery of his patient; the ultimate result, life or death, depends in most cases upon the vital powers of the patient, while the difference between a skilful and careless management of the wound will but result in the formation of a good or bad stump, as the case may be. But in resection it is far otherwise; in both cases errors in constitutional treatment may bring on a fatal issue; and while, after amputation, want of skill will produce the effect above mentioned, the same fault after excision will endanger, in the case of the upper extremity, the loss of the limb; in that of the lower limb, the patient's life.

As in all efforts of conservative surgery, the greatest patience and skill are required; in these cases the surgeon's real responsibilities commence on the completion of the operation, nor do they cease until the patient has fairly become convalescent. Nothing but a personal superintendence of the local treatment, and a

* Syme on Resection.
† Ueber Resectionen.
‡ Indian Annals of Medicine. April, 1855.
§ Med. Times and Gazette. 1857.

ready resort to such expedients as the necessities of the case may require, and the watchful care and ingenuity of the surgeon suggest; nothing short of this will be even likely to secure a favourable result.

From what is generally known of the cause of the failure of union in fractured bones, as well as from our personal examinations of ununited fractures, we venture to assert that "false joint" is a comparatively rare occurrence, and can only result where no attempt at the reparative process has taken place; whereas, by far the most frequent result of a maltreated fracture is the formation of a fibrous bond of union between the ends of bone, such as may be found in any fracture three weeks or so after its occurrence, or after a well-treated resection of the elbow-joint. This arrest of the ossifying process, though sometimes the result of constitutional defect, is too often brought about by the inefficiency of the apparatus used to keep the parts at rest.

The foregoing considerations may serve to account for the absolute necessity, in the treatment of resections of the lower limb, for maintaining complete rest, to promote osseous union; while, after excisions of the shoulder and elbow-joints, it is as essential to endeavour, by passive motion and other means, to arrest, as far as possible, the osseous transformation of the new tissue between the divided ends of bone.

We propose to consider the resections of the larger articulations only, and first, those of the upper extremity, the shoulder, elbow, and wrist, and then to discuss the merits of the same operation on the three large joints of the lower limb.

The Shoulder-joint.—Resections of the shoulder, unlike those of the knee, generally necessitate the removal of but one of the articular extremities entering into the joint; it is very seldom that more than the head of the humerus need be removed, either for disease or injury, and this from the very nature of the affections of the joint, which while they seriously damage or even destroy the head of the humerus, rarely attack the glenoid cavity of the scapula. Again, the exposed position of the humerus, which renders this bone so peculiarly liable to injury from gun-shot wounds, forms a protection for the scapula. Fortunately, the full benefit of resection may be secured by the removal of one articular extremity, as it is not our object to obtain bony ankylosis.

The operation of excision was first applied to the shoulder-joint by Mr. Charles White* of Manchester, who, in 1768, removed the head of the humerus from a boy, aged fourteen, for acute necrosis, with destruction of the joint. He adopted a longitudinal incision, extending from the acromion half-way down the upper arm. Through this opening the head of the bone, which was denuded and dead, was thrust, and removed by a saw; the arm was then confined to the side by a bandage; the case went on well, though during the convalescence a large portion of dead bone separated from the sawn end of the humerus. At the end of four months the patient had recovered the *perfect* use of his arm, and upon examining the parts, Mr. White informs us that, apparently, the head and shaft of the bone had been completely reproduced; there was no deformity, and the extremity was only an inch shorter than the opposite one. Mr. Bent† and Mr. Orred‡ of Chester, soon followed Mr. White's example, and before long other surgeons, both in England and on the Continent, put it in practice. M. Moreau§ was the first to remove the whole articulation; this he did with success in 1786. For some time after this the operation met with little general encouragement, though practised from time to time by various surgeons with variable success, and though used by Larrey occasionally, its practice had well nigh died out when Mr. Syme, in 1829, took it up, and by the fortunate issue of his cases soon established it as one of the most encouraging undertakings in conservative surgery.

This operation is applicable to compound dislocations, to cases where a bullet may have lodged in the head of the bone, and to all wounds of the shoulder-joint complicated with crushing or fracture of one or both bones entering into the

* Cases of Surgery in Phil. Transact., vol. lix.

† Philosophical Transactions, vol. lxvi.

‡ Phil. Transact., vol. lxix.

§ Obs. Prat. relat. à la Résect. des Artic. Paris, 1803.

articulation, unless, of course, the severity of the injury, by division of the great vessels, or by extensive laceration of the soft parts, necessitates ex-articulation of the limb. Nor need the extension of the injury to the shaft of the humerus deter the surgeon from attempting the operation. During the Schleswig-Holstein war, in more than one case, as much as four or five inches in length were removed from the shaft of the bone, and that with the most complete success.

In disease, either one or both articular extremities of the joint may be removed for caries, or indeed for any other incurable affection of the articulation, which renders it not merely useless as a joint, but by its presence either destroys the utility of the whole extremity, or seriously affects the general health of the patient. Lastly, this operation may be substituted for ex-articulation, in cases where tumours affecting the head of the humerus do not by their extension to the shaft necessitate the removal of the entire limb. We apprehend that resection of the joint is by no means justifiable for mere ankylosis, provided that this is the only inconvenience the patient suffers from. The movements of the fore-arm and the rotation of the scapula compensate so considerably for this, that we cannot see the necessity for the operation. The operation is counter-indicated where, together with compound fracture, there is any excessive destruction of the soft parts, or injury to the great vessels or nerves. Neither should cases of necrosis or caries be submitted to operation, unless the disease be confined to the articular extremity of the bone, or at any rate be within reach of removal.

The objects of this operation are to restore to the patient a strong, painless, and if possible, a moveable articulation; and these results are best secured by a careful selection of suitable cases, a well-planned operation, and appropriate after-treatment.

The object of any mode of operation must be the sufficient exposure of the joint, with the least possible injury to the parts about. It appears, from the history of former operations on this articulation, that though Park, White, and others had adopted a single longitudinal incision for the removal of the head of the humerus, yet previous to its performance by Professor Langenbeck, the long tendon of the biceps had always been divided. He it was who first practised an operation which had for its object the preservation of this tendon. Langenbeck's method consists in an incision commencing at the acromion, and extending downwards on the anterior aspect of the joint for three or four inches; this should fall just over the bicipital groove, which is then opened, and the tendon drawn inwards; the muscles inserted into the tuberosities are now divided, and the head of the bone thrust out of the wound and removed by an ordinary saw. This operation is sufficiently easy of execution when the head of the bone retains its connexion with shaft, and the soft parts are not tense or swollen. On the other hand, it is difficult by this incision to remove the disconnected head of the humerus, and especially when the integuments are swollen and œdematous. To remedy this, and to provide a more dependent aperture for the escape of the secretions from the wound, Stromeyer made use of a semicircular incision, commencing at the posterior edge of the acromion, and extending downwards and outwards for three inches, having its concavity forwards; the joint is thus freely opened from above and behind, the tendon of the biceps can be preserved, and a free and dependent aperture is left for pus. Stromeyer states that patients recover from this operation much more quickly than from Langenbeck's, owing to the much greater facility it affords for cleansing the wound. That perfect recovery may take place after division of the tendon of the biceps is well known, and indeed Esmarch relates three cases of resection in which it had been torn across by a ball, and yet in each case the patients recovered, with good use of their arms. It may also be gathered from similar evidence that transverse division of the fibres of the deltoid but little affects the ultimate success of the case. Whatever mode of incision be adopted, the deltoid, with few exceptions, becomes much atrophied after the operation; perhaps this is caused by the division of its nerve, which, with the posterior circumflex artery, are the only nerve and vessel of importance that are liable to injury.

The after-treatment of these cases is far more simple and more easy of execution than that of excision of some other joints. Absolute rest, cleanliness, and appropriate constitutional support constitute the principal measures to be adopted; but we will refer to the plan pursued during the Schleswig-Holstein war. Absolute quiet was maintained by bandaging the arm to the side. Ice was freely applied to the parts, and maintained there until suppuration was fully established. Bleeding, both constitutionally and locally, was unsparingly employed during the stage of reaction, and upon this Stromeyer strongly insists. The wound was never, if possible, disturbed, all cleansing was effected by allowing water to flow over the wound. Matter, if it formed, was let out by incisions, and not squeezed out. Cicatrization was promoted by dressings of nitrate of silver lotions; flannel bandaging was employed to consolidate the parts; passive motion was commenced as soon as the cicatrix had formed, and was continued at the discretion of the surgeon, and as the patient could bear it, until considerable voluntary motion of the extremity had been regained.

Stromeyer and Esmarch both agree as to the most favourable time for resection after gun-shot injuries. They divide the period after the receipt of a wound into three stages. The first stage lasts for twenty-four hours, during which time there is comparatively little or nothing going on at the seat of injury. The second or inflammatory stage extends from the second day until suppuration is established; and the third includes any period after the occurrence of suppuration. We think this division is of great importance, and far preferable to the old separation of operations into primary and secondary, which is a mere arbitrary division of time, irrespective of any changes which may be going on in the wound. In the class "primary" were included those operations performed, either before any pathological change has taken place at the seat of injury, or during the first stages of inflammation and reaction; two states which very differently influence the ultimate result to the patient for good or evil. The most favourable time for resection is either within the first twenty-four hours, or when suppuration is fully established. Out of six excisions of the shoulder performed during the former period two died. Ten were operated on after the occurrence of suppuration, of these two died. While three were operated on during the inflammatory stage, with two deaths. There was another inconvenience which attended operations in this stage—namely, the very free hæmorrhage that occurred at the time of their performance. The truth of these remarks, and the facts indicated by these resections of the shoulder-joint, were fully confirmed by the results of operations on other articulations, to which reference will be made hereafter.

Nineteen patients were subjected to resection of the shoulder-joint, during the Schleswig-Holstein war, of these seven died, and twelve recovered with useful and moveable articulations; of these latter it is recorded that one patient returned to his work as a farm labourer, and when last heard of, was engaged in threshing corn. The others, at the time when their histories cease, were either engaged in their ordinary employments, or were fast regaining the usefulness of their limbs. It has been used as an argument against resection in general, that the convalescence is tedious beyond endurance; but Stromeyer states that the average time occupied in acquiring firm cicatrization, was about three months, though of course patients did not regain the full use of their limbs in this time. Of the seven fatal cases two were primary operations, two were performed during the reactive stage, and three were secondary resections. They all died of pyæmia, and in most, secondary abscesses were found fully developed in the internal organs. In five of these, before death, profuse "pyæmic hæmorrhage" occurred, the result, as Esmarch states, of obstruction of the axillary vein.

The mortality here recorded at first sight appears high for such an operation as the one under discussion; but we must take into consideration the conditions under which these operations were performed. "They were done," says Esmarch, "under circumstances in which more than a third of all amputations of the arm died." Again, we must remember, that they died of pyæmia—a disease which, as far as our present knowledge extends, does not attach itself to any one of the capital operations in preference to another. In fact, so various are the conditions in military surgery that affect the well-doing of patients after operations, that we can scarcely with any justice compare the results of an operation as they occurred after two distinct battles; much less can we estimate the success and judge of the comparative fitness of two plans of operating, by arguing from their respective results in any two campaigns. For instance, after the battle of Fredericia, out of all the cases of amputation of the thigh, but one recovered; and yet out of 128 occurring in the whole war, sixty-seven survived. Nor can we compare the results of excision of the shoulder-joint in this campaign with those recorded by Larrey, who relates ten cases of excision of the same articulation for gun-shot fractures, and of these all recovered. Nor these latter with the results of the same operation performed in the Crimea. But we receive Esmarch's statements, and weigh them, bearing in mind, as far as we know them, the conditions to which the patients were exposed after the operation—"circumstances under which more than a third of all amputations of the arm died." And perhaps we gain some further knowledge of the comparative merits of the proceeding by referring to the histories of eight cases that were observed and noted by Esmarch. These were all injuries suitable for resection, but were, from insufficient experience of the value of the operation, "left to nature." Five out of these died, and the remaining three, after six months' time, had not recovered, but seemed rather still to need operative interference.

The results of our Crimean experience, so far as they are at present published, give a more favourable impression of the value of the operation under discussion. Before Sebastopol, twelve excisions of the shoulder-joint were performed, with but two deaths, while the survivors all regained more or less motion in their limbs. For injuries to the same articulation, sixty suffered amputation at the joint. Out of these, nineteen died, being 15·0 per cent. in favour of the former operation.*

We can scarcely with justice compare the mortality after resection with that after ex-articulation in former wars; for in these it appears that amputation of the joint was adopted for almost all gun-shot fractures occurring high up in the humerus. This is true with regard to the practice of Larrey in the earlier part of his career, and no less of Pirogoff, who, in his work published in 1840, makes the inquiry, whether, in cases of gun-shot fractures of the humerus, it is allowable to make an attempt to save the limb?† The statistics afforded by Larrey of his success in ex-articulation of the upper limb, give a more favourable impression of this operation than the statements of any surgeon we are acquainted with. In 1817, Larrey and his assistants had amputated at the shoulder-joint upwards of one hundred times, and of these ninety recovered.‡

Mr. Guthrie's statistics, taken from the hospitals in the Peninsula, give us a far greater mortality. Of the fifty-six cases submitted to amputation at the shoulder-joint, thirty-three recovered. We must regret that at present we possess no sufficient number of cases of resection of this articulation to admit of our making any fair comparison on a large scale between the mortality after excision and after amputation of the shoulder-joint. As far as we can decide from the information we at present possess—namely, from the experience gained in the Schleswig-Holstein and Crimean wars—the evidence clearly shows resection to be the less dangerous operation. There are but few recorded instances of complete resection of the shoulder-joint. MM. Moreau, Syme, Heyfelder, Larrey, and Lauer have each removed the entire articulation. These five cases were all perfectly successful, except Heyfelder's, whose patient sunk on the twenty-first day, apparently as the direct result of the operation. Larrey's case is well worthy of remark, showing,

* Medical Times and Gazette, Sept. 20th, 1856.

† Rapport Méd. d'un Voyage en Caucase.

‡ Cooper's Surgical Dictionary, Article "Amputation."

as it does, the power of recovery after most extensive mutilation. The operation consisted in the removal of the head of the humerus, the acromion, the acromial end of the clavicle, with the glenoid fossa, and part of the spine of the scapula; and yet the patient recovered with considerable use in the upper extremity, the end of the humerus forming for itself a bed against the ribs, deep in the axilla.

The results of resection when put in practice for disease of the shoulder-joint are, as might be supposed, more favourable than those performed for gun-shot injuries, though, on account of the comparative immunity this joint enjoys from disease of its articular extremities and synovial membrane, the operation is but seldom required. We have abundant proof that the curative effects of this operation are for the most part permanent, and there cannot be brought against it the objection that is frequently urged against resection of other joints—viz., that the cure effected is not lasting. Moreau, Syme, Textor, Larrey, and others, have had patients for years under their observation. Textor, indeed, examined a patient's body from whom he had resected the shoulder-joint nineteen years previously, and here he found the new articulation in a perfectly healthy condition. The great importance of attempting to save the upper extremity by all means in our power, owing to the inefficiency of any artificial substitute for the fore-arm and hand; the simplicity of the operation in question, its ease of execution, and its comparative freedom from danger; the great power of restoration possessed by the parts about; together with the unsatisfactory termination of most of these cases if left to nature,—these considerations, together with the encouraging results of those cases where the operation has been put in practice, forbid us from condemning any upper extremity to amputation before we have well weighed the question of resection.

The Elbow-joint.—Among the larger articulations, there is none to which the operation of resection is so admirably adapted as to the elbow. Compared with other joints, our more extended experience of the safety of *this* operation, its general freedom from a protracted convalescence, and the brilliancy of its results, justify our performing it on the elbow, as a cure for disease, or as a substitute for amputation in cases of injury, with but little apprehension of danger to the patient's life, or little doubt of obtaining a successful issue. "In short," says Mr. Butcher, "those trembling and sceptical about the propriety of the more severe excisions of the hip, knee, and wrist-joints, yield their allegiance and assent tacitly in favour of excision of the elbow, and allow unsullied its accredited merits."

Mr. Park relates, that excision of a portion of this articulation was first performed by Mr. Wainman, who met with a case of compound dislocation of the lower end of the humerus; this he was unable to reduce, and would have amputated had not the friends of the patient refused their consent; unwillingly he was compelled to saw off the projecting end of the bone, and thus not only secured a useful arm to his patient, but suggested to Mr. Park the general applicability of the same treatment to other articulations. Moreau, senior, was the first to remove all the joint-ends at the elbow; this he did in 1797, with perfect success, and though many other surgeons, both in England and on the Continent, followed his example, yet the operation, in a less degree than the same proceeding on other joints, had fallen into disuse, and was in danger of being altogether neglected, until revived by Mr. Syme in 1830, and since practised by numberless other surgeons, both in disease and for the effects of injury.

The unfavourable issue of the expectative treatment of gun-shot injuries to the elbow-joint, is well known and acknowledged by all writers on military surgery; a clean-cut sabre wound opening this articulation and dividing the bone, may, indeed, be left to nature with a good prospect of a favourable termination; but so unpromising were the results of a similar plan of treatment in gun-shot fractures, that before the introduction of the practice of resection, nearly all such injuries were subjected to amputation. It is to Professors Langenbeck and Stromeyer that we owe, perhaps not the introduction, but the extensive practice, of this operation in military surgery; too much praise cannot be given to these gentlemen, both for their enterprising treatment during the Schleswig-Holstein war, and for the faithful records they have handed down to us of the results of that treatment.

The establishment of this mode of practice in military surgery, is the more important from the great liability to injury which is exhibited by the elbow-joint, and this principally for two reasons: the exposed position of this articulation in loading and firing, when, as is stated by Esmarch, it was most frequently injured; and again, from the great liability of implication of the joint from extension of fissures in fractures of the shaft of the humerus. These extend much more frequently downwards to the elbow, than in an opposite direction to the shoulder-joint; and this, according to the same author, is owing to the earlier union of the epiphysis at the lower end of the bone, which thus offers no obstacle to the extension of a fracture into the neighbouring articulation.

The elbow-joint, in cases of injury, may be resected in part or entirely, for gunshot or other compound fractures affecting it, where the injury to the bone is too extensive to permit of extraction of the fragments, and where no extensive destruction of the soft parts or lesion of the great vessels necessitates amputation.

It is also applicable to those cases where bullets have lodged in either of the articular extremities entering into the joint, and no less to cases of compound dislocation where the most favourable result of the expectative plan of treatment will but restore a stiff joint to the patient. It is in the elbow-joint alone that we are justified in applying resection as a cure for simple ankylosis; and here any such condition of the joint, either the result of injury or disease, which so impairs the usefulness of the upper extremity as to prevent the sufferer from gaining his livelihood, is without doubt a case to which excision is applicable, provided that the patient is desirous of submitting himself to the operation.

In cases of disease, excision of the elbow may be practised with great success for caries or necrosis of any of the joint-ends of the articulation, and indeed for any affection of the synovial membrane or articular cartilages which renders the joint useless and is beyond our curative aid. Carious disease or necrosis of any considerable portion of the shafts of the bones of the fore-arm and humerus, or fractures which extend for more than four inches either above or below the articulation, all alike forbid resection of the joint. By performing the operation in these cases, or in any other to which it is inapplicable,

> "Either the wretched sufferer may be doomed to mutilation for life, or resection, a most valuable operation, may be brought into discredit by its performance where not at all suited, and where it was never intended by its warmest supporters."
>
> "Again, it should be remembered that a patient may not have the power to bear an amputation after failure by an ill-advised resection, when he might have made a good recovery from amputation if performed at first."*

The condition of the integuments in long-continued disease of the joint—we mean their swollen, sodden, and inflamed state—need form no obstacle to the performance of this operation; this quickly subsides on removing the real cause of the disease.

The object of this operation, in the case of the elbow-joint, is to restore, if possible, a thoroughly moveable articulation, such an one as is fitted for all the ordinary avocations of life; but if mobility is unattainable, a firm ankylosis at a suitable angle is the most desirable result. As in resecting the shoulder-joint, it is not necessary to remove all the joint ends entering into the articulation, though one would have supposed that recovery would be expedited by the removal of the opposed cartilaginous surfaces. Stromeyer gives it as his opinion, and that the result of experience, that it is of no advantage purposely to destroy the articular cartilage.

The rule of practice, in this respect, in the Schleswig-Holstein war, appears to have been either to resect the articular extremities of both radius and ulna, or only the end of the humerus; of course, where the severity of the injury required

* Mr. Butcher: Dublin Quarterly Review, Nov. 1855.

it, all the joint-ends were removed. In no case was it thought expedient to leave the opposed ends of the bones in contact after the operation; this later proceeding results, as one might expect, in bony ankylosis. Stromeyer maintains, in opposition to Esmarch, to whose opinion we have just alluded, that the presence or absence of ankylosis is chiefly dependent upon the plan of after-treatment adopted by the surgeon; and indeed, from the perusal of Esmarch's table of forty cases of resection of the elbow-joint, it appears that the amount of bone removed but little affects the ultimate mobility or stiffness of the arm. In one case related, four inches of the end of the humerus were removed, and still no movement of the joint was ultimately obtained, and yet another case is given where all the joint-ends were resected with the same result; again, from a third patient, four inches and a half were removed from the end of the humerus with a directly opposite result, great mobility being eventually obtained. In fact, so anomalous are the results in this respect, of removal of more or less extent of bone from the articulation, that we are inclined to believe with Stromeyer, that judicious after-treatment chiefly contributes to the restoration of a moveable joint. Esmarch strongly condemns partial resection, or the removal of broken fragments of bone from the articulation, and in proof of this opinion he quotes two cases of Stromeyer's where this plan of treatment was adopted; in one, the external condyle alone was removed, in the other, only the olecranon; both cases were followed by secondary abscesses in the neighbourhood of the joint, and after a tedious convalescence of four and seven months respectively, both patients recovered with bony ankylosis. In oblique compound fractures of the bones of the elbow-joint, it was found sufficient to truncate the projecting angle of bone, and not necessary to cut through the shaft so as to include the whole of the fractured surface; in this way many cases were successfully submitted to operation which otherwise would have involved too great a loss of bone to allow of subsequent repair.

In all the various plans of operation for resection on the elbow, the main object to be attained is the sufficient exposure of the bones without injury to the ulnar nerve, or undue division of the soft parts around the joint; the ulnar nerve is to this joint what the tendon of the biceps is to the shoulder in the operation for resection; though the preservation of the nerve is of much greater importance, yet neither nerve nor tendon should in either case be divided.

For performing the operation under consideration, various plans of incising the integuments have been adopted; those most in use at the present day are the H-shaped incision, used by Mr. Syme; Liston's mode of operating, and Langenbeck's. In the H-shaped incision, the longitudinal cuts run at the back of either condyle, and are connected by a transverse division of the integuments just above the olecranon. Langenbeck's operation consists in a single longitudinal incision running down the inner side of the posterior part of the joint, just to the radial side of the ulnar nerve; the joint is opened, and the soft parts and ulnar nerve are dissected *en masse* from the subjacent bone; the coverings of the joint on the outer side are in the same way separated, the lateral ligaments divided, and the ends of the bone thrust out by flexion. Of these two modes of operating, the former, without doubt, fulfils the great object of the undertaking—viz., the free exposure of the joint-ends; but it does this in an unnecessary degree, or rather, sufficient exposure may be attained by less extensive division of the soft parts. Langenbeck's method of operation, on the dead body or on an arm where the integuments are in a healthy condition, will with little difficulty quite sufficiently expose the joints; but in an arm where the integuments are tense and swollen, it does not accomplish this to the requisite extent. We have ourselves seen, in an operation for disease where the soft parts were in their usual state of tension and succulence, the outer flap tear transversely, owing to the great strain upon it in drawing it aside to bring the joint fairly into view. In our opinion of Langenbeck's operation, we are supported by Esmarch, who says of this proceeding:

"As soon, however, as considerable serous or inflammatory infiltration has come on, as generally occurs after gun-shot injuries, it is necessary to lengthen this incision too much, to prevent tearing and bruising the skin."*

And for this reason it was that, in the Schleswig-Holstein campaigns, Liston's operation was generally adopted. Out of forty cases, Langenbeck's operation was put in practice three times, while Liston's was employed in twenty-seven cases. This latter operation consists, as is generally known, in a ⊣ shaped incision. The longitudinal cut is commenced above the olecranon, between it and the ulnar nerve, and is continued downwards for three inches or more, along the ridge on the back of the ulna; a transverse incision is now made across the radio-humeral articulation, which joins the first at its centre; by this the joint is opened, a hook or a finger is placed within the joint, and used to retract the soft parts with the ulnar nerve to the inner side; this is facilitated by separating them with the knife from the surface of the bone, taking care to carry with them as much periosteum as it is possible to procure; the lateral ligaments are divided, and the heads of the bones turned out by flexion. In this operation the ulnar nerve runs but little risk of being divided, as, in separating it from the bone, there is the capsule of the joint, the lateral ligament, and its own sheath to defend it from injury,—in fact, the nerve should not be seen during the whole operation. In gun-shot fractures, however, the existence of bullet-wounds often necessitates the deviation from any rules of operating, and obliges the surgeon to make a skilful use of existing apertures by including them in the line of his incisions.

In this operation, although the preservation of the ulnar nerve may appear to be easily attained, provided ordinary care be exercised, yet we have recognised its division by the subsequent loss of sensation, when at the time of the operation it was apparently uninjured, and indeed was not seen throughout; that union may take place and sensation be restored, even after its division, is well exemplified in a case related by Mr. Syme, where, although it was divided at the time of the operation, the patient subsequently regained sensation and voluntary motion in the parts supplied by it. If possible, in removing the ends of the bones, the insertions of the brachialis anticus and biceps should be preserved, as their integrity adds much to the strength of the future articulation; still, the removal of the coronoid process and the tubercle of the radius has often been effected with the ultimate restoration of a useful joint.

On the completion of the operation begin most of the anxieties of the surgeon, and the chief responsibilities of the case. Absolute and painless rest must now be maintained for some time by placing the arm on an angular splint, well padded, and in such a position as is most comfortable to the patient. The position which is found, from experience, to be most free from pain, is an angle of 140°; and in this position it was that Langenbeck's and Stromeyer's cases were treated. The plan of treatment pursued in the forty cases recorded by Esmarch, consisted in maintaining absolute rest, closing the extremities of the wound, cold dressings of ice or evaporating lotions were applied until suppuration was fully established; during the stage of reaction vigorous antiphlogistic measures were employed by leeches and venesection, and all collections of matter were evacuated by free openings. In the later stages, oiled charpie was used as a dressing, flannel bandaging employed to consolidate the parts, and when cicatrization had been accomplished, passive motion, friction of the joint, and warm water-baths were assiduously made use of. Great value is attached by German surgeons to the use of ice in moderating the inflammatory stage after resection or compound fracture; of the five deaths which occurred out of the forty cases submitted to operation, four happened in the year 1849, and are attributed to the want of ice, which could not at that time be procured. Stromeyer ranks the local application of ice as next in importance to venesection, a position which, to our ideas, implies its general inutility rather than its importance. Still we would not venture to call

* Statham's Translation, p. 85.

in question the efficacy of bloodletting in military surgery, when such surgeons as Guthrie, Langenbeck, and Stromeyer so strongly advocate its use and testify to its advantages, which must depend upon the general physical condition of the wounded, the nature of the climate to which they are exposed, and the type of endemic disease prevalent.

The value of passive motion, frictions, and warm water-baths for restoring free movement to the articulation, is well illustrated by the results of some cases where these means were not employed. After the battle of Idsted, several patients who had been submitted to the operation of resection fell into the hands of the Danes, who, according to Esmarch, do not perform resections of joints; in the beginning of the year 1851, when these patients were released, they returned with their fore-arms for the most part wanting in sensibility, and destitute of motion; and "these evils," says Esmarch, "could only be partially bettered by means of warm water-baths and methodical passive motion."

In his valuable records of the surgery of the war in Schleswig, Esmarch furnishes us with the results of forty cases of resection of the elbow-joint for gunshot injuries. Of these, six have died; thirty-two have recovered with a more or less useful arm; one had not yet recovered when last under observation, and in one the arm was subsequently amputated. Among the thirty-two recoveries, eight are said to possess very extensive motion in the arm; nine a fair amount of mobility, which in many of them is still increasing; thirteen have a more or less complete ankylosis of the joint. In two cases, the amount of motion in the joint is not mentioned. The average time required for complete cicatrization in these cases was from two to three months.

The most favourable period for operation in cases of injury was found to be either within the first twenty-four hours, or after suppuration is fully established. Stromeyer and Esmarch give us the result of their experience on the point, which agrees in every respect with the comparative mortality of the primary, secondary, and tertiary resections performed on the shoulder-joint. Of eleven excisions of the elbow, performed within the first twenty-four hours, but one proved fatal; twenty secondary operations were performed on the same joint with four deaths; nine were operated on after the eighth day from the receipt of the injury, and of these, one died.

The cause of death in five out of the six fatal cases was pyæmia; and this occurred at periods varying from the third to the thirtieth day after the operation. In all these, the presence of pyæmia was verified by the discovery of secondary abscesses in the internal organs; the remaining case died from tubercular disease of the lungs and mesenteric glands. One case is recorded in which the fore-arm became gangrenous, and was eventually submitted to amputation. This unfortunate event was in no way attributable to the operation, but to the fact that the brachial artery and ulnar nerve had been divided by the bullet which injured the articulation, though by some oversight the absence of pulsation at the wrist was not observed until after the operation.

One case was submitted to amputation by Danish surgeons, but on what account has not been ascertained.

It appears that nearly all the ankylosed joints were flexed at an obtuse angle of from 130° to 140.° This most inconvenient and undesirable result was owing to their being put upon splints fixed at that angle, and it is stated that in no other position could the patient obtain complete rest and freedom from pain. We shall on this point venture to disagree with Esmarch, having ourselves witnessed the successful treatment of resected elbow-joints on splints fixed at a more acute angle, and, indeed, in a position in which, had they eventually become ankylosed, the patient would have possessed a useful arm.

Having in our possession the account of the resections performed on the elbow during the Schleswig-Holstein war, as well as the statement of the results of the same operation in the Crimea, we may form a fair estimate of its comparative mortality. In the former campaign, 54 amputations of the arm were performed, with 19 deaths; while 40 elbow-joints were resected, with 6 deaths. In the Crimea, 153 arms were amputated, and of these 29 died; 17 elbow-joints were excised, with 2 fatal cases. In all these we have 207 cases of amputation, with a mortality of 48, or about 23 per cent. Of resections we have 57, with 8 deaths, or 14 per cent. Thus, then, in comparing amputation of the upper arm with resection of the elbow-joint, we obtain a per-centage of nine in favour of the latter operation.

No large collection of cases is requisite to prove the advantages of this operation, as applied to the treatment of articular caries, or other incurable joint-disease. General experience has long ago decided in its favour in suitable cases, and we believe that at the present day there is no surgeon of eminence or consideration who does not appreciate the advantages it offers in comparison with amputation. But surgeons still differ as to the advisability of submitting certain cases to operation, and especially whether it is justifiable to resect the joint for permanent ankylosis in a position which renders the limb nearly useless.

> "To many persons this state of things might be productive of no inconvenience sufficiently serious to warrant an operation; but there are others on whom its effect would be to deprive them of their livelihood and to ruin every prospect in life."*

Under these circumstances, and where age and constitutional peculiarities do not forbid it, we have no hesitation in recommending resection of the joint, with a view to restore, if possible, a moveable articulation, or at any rate to obtain ankylosis at a suitable angle.

The new articulation resulting from this operation appears to suffer in no respect, but rather to gain strength from daily use. Many patients have been under observation for years, having passed through the wear and tear of a laborious occupation without injury. Relapse and extension of the original disease may take place a short time after the operation, either from a want of discrimination in selecting the case, or from some constitutional defect. Indeed, this has sometimes occurred; but we are acquainted with only one, and that a doubtful case, in which after the cure had been fully completed, the new tissue between the divided ends of the bone became the seat of disease. This instance is recorded by Heyfelder, who relates a case where the entire elbow-joint had been resected from a patient suffering from painful ankylosis with articular caries. Three months after the operation, she had recovered good motion in the articulation. Flexion, supination, and pronation could be effected with ease, but there was no power of extension. After relaxation of the flexors, the arm fell into the extended position. In addition to this, some pain always remained about the joint, so that it could not be used for work. Some months afterwards, an abscess appeared at the cicatrix of the operation, and subsequently, from time to time, others formed at the same spot. Eight years afterwards, her arm had become perfectly useless, very painful, and she could not accomplish any voluntary movement of the fore-arm, though it could be flexed so as to touch the humerus. In this condition, and suffering as well from spasmodic movements of the muscles of the whole limb, Dr. Heyfelder amputated the arm. On examination, the muscles and nerves, together with the newly-formed fibrous bond of union between the bones, were found softened, and everywhere infiltrated with fat. About the ends of the humerus and ulna were circumscribed purulent deposits, while the shafts of the bones themselves were in a state of inflammatory softening. Heyfelder himself inclines to the belief that this was a case of secondary disease, originating in the newly-deposited material between the resected ends of the bones.

The Wrist.—We pass from the consideration of resection of the elbow-joint, to the less favourable and indeed almost discouraging results of the same operation on the wrist. In all operations for excision, a general and indispensable condition must be fulfilled—namely, that together with the partial or complete removal of

* Blackburn on Excision of Joints: Guy's Hospital Reports, vol. i., first series.

the articular extremities of the bones, there should be a free exposure or destruction of the synovial cavity of the joint; any operative proceeding which leaves the articulation in a condition approaching to that of a wounded joint will lead to no good result, but will rather thenceforth be exposed to the dangers attendant on joint wounds, and will terminate as such accidents are wont to do.

In excisions of the wrist, owing to the complexity of the joint, it is seldom that we can fulfil these conditions; in removing disease here, while we take away the carious bone, or destroy the useless joint, in all probability we partially open or puncture one of the neighbouring articulations, which may be healthy or otherwise, and thus we leave a wound from which the subsequent phenomena of inflammation, profuse suppuration, and ulceration of the cartilages, will but too surely result. There are other, but minor, objections to this operation, such as the difficulty of its performance, and the injury to the tendons which is often unavoidable, though these form no serious objection to the operation. Moreau the younger seems to have been the first to perform this operation on the wrist; the result he reports as successful, but we have been unable to meet with any distinct history of the case. In 1849, Heyfelder operated on the same articulation with equal success; five years afterwards this patient had regained perfect use in the hand and fore-arm, a result but rarely attained in similar cases; more recently the operation has been performed by Messrs. Fergusson, Stanley, Erichsen, and others.

In operations on the dead subject it is sufficiently easy to remove the extremities of the radius and ulna without injury to the tendons or vessels in the neighbourhood, and indeed this has been accomplished by more than one surgeon on the living body; but very often, in disease, it is impossible, and indeed useless, to attempt to save the extensor tendons, matted together as they may be by inflammatory deposits, and closely adherent to the bone. The principal modes of performing this operation are three:—1st, by lateral longitudinal incisions running along the ends of the radius and ulna respectively; 2nd, by a semilunar or conveniently-shaped flap, formed from the back of the wrist; and 3rd, by making two long incisions, the one in the palm, and the other on the posterior aspect of the joint. This latter operation has been performed by Mr. Simon, but we should imagine that the great length of the incision would form a serious objection to this method. By the first and last modes of operation, the tendons may without difficulty be saved; whereas, by forming a flap on the dorsum of the hand, though a good view of the joint may thus be obtained, yet some of the extensor tendons will necessarily be divided. Mr. Butcher maintains, with great justice, the importance of avoiding, if possible, any interference with the carpal articulation of the thumb, or its extensor tendon. This he effects by commencing his incision just to the ulnar side of the tendon of the extensor secundi internodii, cutting a semilunar flap from the back of the wrist, and ending his incision on the ulnar half an inch higher up the fore-arm than the point where it commenced; thus the extensor tendons of the fingers only are divided, while those of the thumb are turned aside; this operation has also the advantage of being easy of execution in nearly all cases.

Resection as applied to the wrist is justifiable in young patients, as a substitute for amputation in caries, or in cases of general destructive synovial disease; it has also been performed with success for osteoid disease of the carpal end of the radius. Probably, the cases to which it is most applicable are those of compound dislocation of one or both bones of the fore-arm, where the protruded portions of bone may be sawn off with a good prospect of success.

We subjoin the results of fifteen cases of resection of this joint for disease; of these more than one occurred under our own observation, but the majority are collected from the published accounts contained in the medical journals of the day. Operations on the carpus, which do not involve the wrist-joint, are purposely excluded from this account.*

* The operators in these cases were—Moreau, Heyfelder, Fergusson, Dr. Green of Bengal, Mr. Simon, Mr. Erichsen, Mr. Stanley, Mr. Butcher, Mr. Cock, Mr. Page.

Out of the whole number subjected to operation, three died—one of continued fever, another from carious disease of the vertebræ, and the third with some cerebral affection. Five are reported cured, with more or less useful hands; three are said to be in progress of cure when their history ceases. Of three it is stated that the prospect for one was hopeful, one unsatisfactory, and the third is said, nine months after the operation, to "have some chance of recovery." The remaining patient never gained any use in his hand, though the wound eventually healed perfectly.

The three deaths that occurred can scarcely be attributed to the effects of the operation, though Mr. Butcher's patient,* who died with cerebral effusion, probably had her death hastened by the shock to her system. Still we venture to suggest that fifty-eight is not an age at which to undertake resection of the joint with a very hopeful prospect of success. The same remark may be applied to Mr. Page's case, where, although the wound healed, the patient could make but little or no use of the hand. This patient was sixty-two years of age. Of the five cases that recovered, all regained a useful hand, though Dr. Green's patient,† as shown by the woodcut in the Indian journal, appears to have acquired by no means an ornamental member. In this case, a considerable portion of the radius was removed, while the ulna was left entire, and thus the hand became drawn to the radial side of the fore-arm, and there fixed at a right angle.

To take the most favourable view of these cases, it may be said that eight of them were successful—that is, if we include with the cured those in progress of recovery. This is but a small proportion among fifteen, and though from such insufficient evidence it is unsafe to draw general conclusions, yet we cannot but think that these results will be found to represent pretty accurately the comparative value of this operation on the wrist-joint—an articulation unfitted as it is by its complexity for the favourable performance of resection.

The Hip-Joint.—This bold and hazardous operation was first recommended by Mr. Charles White of Manchester, in 1769, whose suggestion was put into practice by Mr. A. White of the Westminster Hospital. He in 1818 excised the head of the femur for carious disease, with a more fortunate result than has since attended many operations on the same articulation. Subsequently, in 1844, Mr. Fergusson excised the head of the bone for caries with equal success, after having, as he says, meditated for more than ten years the performance of this operation, without meeting with a single case in which its execution could be deemed justifiable.

The introduction of this operation into military surgery, and its substitution for disarticulation in cases of injury, is of much more recent date. Mr. Guthrie, writing in the year 1853, says:—

> "This operation must some day be done in cases of fracture of the head or neck of the bone, caused by an external wound—cases which have hitherto been invariably fatal, or in which life has been preserved by amputation at the hip-joint."‡

The first operation of this kind for gun-shot injury appears to have been performed by Oppenheim, and more recently, under Stromeyer's direction, by Dr. Schwartz, in May, 1849. The last case is recorded by Stromeyer in his account of the surgery of the Schleswig-Holstein war. From this patient the head and neck of the femur were removed for a comminuted gun-shot fracture of the upper end of the bone.

In considering the merits or the demerits of this operation, we must widely separate the cases of disease from those of injury. Of the results of excision for the former we have some experience, while of the practice of excision for the latter, as yet we possess insufficient data to form any decided opinion, either in favour of or against the operation.

Excision of the head of the femur is, without doubt, best adapted for those cases of disease of the femur where the carious head of the bone, having

* Dublin Journal, Nov. 1855. † Indian Annals of Medicine, April 1855. ‡ Guthrie's Commentaries, 5th edition, p. 88

suffered spontaneous dislocation, lies as a foreign body on the ilium, and by the irritation it causes, endangers the life of the patient. Such cases resolve themselves into ordinary operations for necrosis or caries, and can scarcely with justice be called excisions of the hip-joint.

Less hopeful are the results of this operation, when undertaken for disease of the head of a bone which lies yet in the acetabulum, and still less encouraging are the cases where it is performed for general caries of the articulation—i. e., where both pelvis and femur participate in the disease. In no case is it justifiable unless the life of the patient is in danger from the excessive discharge or from the irritation of the carious head of the bone.

It should be undertaken in no case as an operation of expediency, but only when absolutely necessitated by the constitutional condition of the patient, and by the incurability of the local disease. In the most favourable cases, when the patients are young and otherwise healthy, the bone dislocated, and the pelvis free from disease, we may hope not only to save the patient's life, but to restore to him a useful and moveable limb. Still in many cases, and especially in those of gun-shot fracture, in which it is no less our duty to perform this operation, we can only afford to the patient an uncertain chance of escape from certain death.

It is only applicable in injuries, when the head or neck of the femur is broken by a musket-ball of moderate dimensions, in such a way as to implicate the joint. Extensive fracture of the pelvis or injury to the great nerves or vessels, of course forbids the performance of the operation.

> "Picture to yourself (says Mr. Guthrie) a man lying with a small hole either before or behind in the thigh,—no bleeding, no pain, nothing but an inability to move the limb, to stand upon it, and think that he must inevitably die in a few weeks or months, worn out by continual pain and suffering, unless the thigh be amputated at the hip-joint, or he be relieved by the operation which I insist ought first to be performed" (i. e., resection).*

Military surgeons all agree with Mr. Guthrie in his estimate of the general fatality of gun-shot wounds of the hip-joint. In such injuries, three courses are open to the surgeon—first, to leave the case to nature; secondly, to amputate at the hip-joint; thirdly, to resect the joint. Left to nature, they are invariably fatal; submitted to disarticulation, they rarely survive; the treatment by resection, though by no means eminently successful, is probably most applicable to such cases, and is justifiable not because of its good results, but rather on account of the well-known inefficiency of the two former modes of treatment.

There is yet another class of cases to which resection of this joint is probably applicable; we mean to such as are described in Sir B. Brodie's work 'On Joints,' at page 71 of the last edition, where death occurred within a few days after a wrench to the joint. "In these cases," says Mr. Hancock, "I have long since made up my mind, that should I meet with such, I would give the patient the benefit of the operation."

Mr. Hancock subsequently states that he once had an opportunity of examining a recent specimen, where a patient died comatose a week after wrenching his hip-joint. There was a longitudinal rent, an inch long, in the synovial membrane, while the joint contained rather more than a tablespoonful of matter and blood. At present, our knowledge of the effects of this operation is derived from few cases. Hereafter, more extended experience may decide as unfavourably of resection as it now does of disarticulation. Still, in cases to which (in our present state of knowledge) it is applicable, it possesses this decided advantage over ex-articulation, that it for the most part puts the patient's life in no immediate danger.

The head of the thigh-bone may be removed without any considerable difficulty, and it is usually done from the posterior and outer side of the joint. A longitudinal, semilunar, or crucial incision over the great trochanter, will in all cases sufficiently expose the parts without any danger to the great nerves or vessels. The

* Commentaries on Surgery, fifth edition, p. 88.

head of the bone can be thrust out of the wound by rotating the knee inwards, when it can easily be removed by forceps or an ordinary saw. Mr. Guthrie advised, that in cases of doubt, where the extent of the disease or injury is unascertained, the incision should be made in such a way that should it be necessary, exarticulation may be performed by extending the first wound. This may easily be attained if the joint is exposed by an incision from the anterior superior spine of the ilium downwards to the great trochanter. We have ourselves seen this plan of operation adopted with good success.

The ordinary indications for after-treatment may best be fulfilled by the application of Liston's splint, well padded, and extending from the axilla to the foot, interrupted by an iron rod opposite the wound. Great care should be taken to promote the free escape of the wound secretions, and to this end the leg may be raised somewhat above the level of the thigh—a plan adopted in more than one case with good effect. Some surgeons have treated cases of resection of this articulation without the assistance of a splint, and have, by means of weights attached to the foot, maintained the limb in good position. Mr. Shaw has put this treatment into practice in two cases with a successful issue.

The operation for resection of the hip-joint has, we believe, been put into practice in this country about twenty-seven times, with the result, as stated by Mr. Hancock, of fifteen recoveries. These operations, it must be borne in mind, were all performed in cases where there was no hope of ultimate recovery, and, as far as the judgment of the surgeon extended, death would have soon taken place had not the operation been performed. This proportion of successful cases, though abundantly sufficient to warrant the attempt thus made to save life, yet affords too slender a stock of information to generalize upon; still we venture to say that, by performing the operation in suitable cases, we may give the patient at least half a chance for his life, bearing in mind that in no case should we operate unless, humanly speaking, no other means of recovery remain.

Dr. Heyfelder gives us histories of two cases of excision of this articulation for disease, the one performed, in 1838, by C. von Textor, on a patient aged twenty-eight; the other in 1848, by himself, on a patient aged twenty.

Textor's case was one of carious disease of femur and pelvis, with partial ankylosis. The head of the bone was removed, and part of the acetabulum, and to conclude the operation, the suspicious portions of the pelvic bones were seared with a red-hot iron. This patient did not survive, but died four days after the operation.

Heyfelder's operation was performed for general caries of all the articulation, with partial dislocation of the femur. Nearly all the head and neck, and a part of the trochanters, were removed, portions of the acetabulum were cut away, and the surface of the bones was finally cauterized with the hot iron. After some time, this patient could get about with crutches, having his limb pendant, but partially united to the pelvis. A year afterwards, signs of returning caries showed themselves, and three years and a half afterwards, in consequence of the miserable condition of the patient—viz., his emaciation and approaching death from abscesses and continued discharge—Heyfelder amputated the limb at the joint, dividing the recent ankylosis, and at the same time removing two inches of the horizontal ramus of the pubes, with loose portions of the other bones. Much carious bone was left behind. The patient sank two hours after the operation. We have ventured to abstract the histories of these two cases, as they contain a trial of a plan of treatment never, so far as we know, adopted in this country;—we mean the application of the actual cautery to the divided surfaces of bones, and to any parts suspected of carious disease—a practice, we believe, adopted with some success in Germany.

It has been a subject of much discussion in this country as to whether, when both pelvis and femur are diseased, such a case is a fit subject for operation. Hitherto, surgeons have generally agreed that an extensively diseased acetabulum, or a pelvic abscess, if ascertained, was a good and sufficient reason to abstain from resecting the diseased joint, choosing rather to allow things to take their course, than to subject the patient to what was considered in such cases the gratuitous cruelty of the operation. In opposition to these views, Mr. Hancock, in his recent article on the subject,* thus expresses himself:—

"In deciding on this operation, we should be guided by the condition of the patient, and not by any arbitrary stages of the disease; and whilst I always have, and still continue to deprecate unnecessary ill-considered operations, I believe it to be our duty, when we have assured ourselves that a case is one of hip disease, that the patient is dying, and there is no hope of saving him by the ordinary means, to perform, or at all events propose, the operation—without reference to whether, pathologically speaking, the disease be in this or that stage, or whether the head of the bone be dislocated, the acetabulum healthy or not."

In support of his opinion, Mr. Hancock asserts that rarely, if ever, is the acetabulum entirely free from participation in disease with the femur. In nineteen out of the twenty-seven operations performed, the acetabulum afforded more or less evidence of disease at the time of the operation. In two there was scarcely a trace of acetabulum. In three the acetabulum was filled with a fibro-gelatinous mass. In six the gouge was employed for caries. In three it was perforated, and in the others it was more or less affected. After all, it is only in a very few instances that the surgeon can tell beforehand what is the state of the cotyloid cavity; and this uncertainty is of less importance if Mr. Hancock's views on the subject are correct. So far from perforation of the acetabulum being a barrier to the operation, that gentleman maintains that even under these apparently unpromising circumstances, excision may be performed with safety and benefit. In support of his opinions, Mr. Hancock brings forward his own case, where, from a boy, aged fourteen, suffering from carious disease of the whole joint, with pelvic abscess, he removed the head and neck of the femur, together with the floor of the acetabulum. The operation was followed by almost instant relief of his constitutional symptoms, and at the end of five weeks he could walk with a crutch and a stick. In only two instances recorded was the result of the operation immediately fatal; in most cases, whatever the ultimate effect of the proceeding, the immediate consequences were relief from pain, and a marked improvement in the constitutional symptoms.

It is but seldom that a case presents itself in military surgery to which this operation is applicable; the position of the head of the femur, deeply buried in muscles, surrounded by the acetabulum, and having the great vessels of the limb on its anterior aspect, renders an uncomplicated injury to the head or neck of the bone very rare. Stromeyer thus speaks of these injuries and the difficulty of their diagnosis:

"Left to themselves, all injuries of the femur close to the hip-joint must end fatally; but the diagnosis of the condition present is not without difficulty. When the bullet has struck the trochanter itself, it is not easy to tell whether the injury extends above that point or not. In all cases that I have seen, extensive comminution existed, and the joint had suppurated even in cases where the fracture did not extend as far as the capsule of the joint. The usual symptoms of fracture of the neck of the femur, the shortening and the eversion of the foot, failed in the cases I observed; this is due, without doubt, to the fragments hanging together better on account of the preservation of their fibrous covering. In one case, where death followed rapidly by pyæmia, the patient could perform considerable movements of flexion and extension with the leg, although the neck of the femur was broken; in another case, the fragments fitted so well together, that the patient did not experience the least pain, and the leg could be moved without causing crepitus, so that the surgeon considered that bony union had taken place, though I drew his attention to the fact that this could not be possible on account of the profuse discharge. On examination after death, it was discovered that the seat of fracture was necrosed, and that a portion of the man's trousers lay between the fractured ends."†

We have met with but eleven recorded instances of the performance of this operation for gun-shot injury. One was performed during the Schleswig-Holstein

* Lancet, April 25th, 1857.

† Statham's Translation, p. 28.

war,* one by Dr. Ross,† one by Oppenheim,‡ one by Mr. Seutin,‡ one by Textor; and six during the war in the Crimea.‖ Of these cases but one recovered, a soldier wounded by a shell before Sebastopol, and operated upon by Dr. O'Leary. From this patient, aged twenty-five, the head, neck, and trochanters of the femur were removed; after the operation, the limb was swung in a sling of strong canvas from a beam overhead; twelve weeks afterwards he was able to leave his bed on crutches; six months afterwards the wound was firmly united, two small sinuses only existing; he was gradually regaining power over his limb, and was able to a limited extent to flex the leg upon the thigh, and the latter upon the pelvis; shortening to about five inches, and very slight eversion, were the chief deformities consequent upon the operation.

The results of the above-mentioned cases are certainly not encouraging, nor indeed sufficient to induce any one to undertake the operation, were it not for the still greater fatality attending amputation at the hip-joint, the only other alternative in cases of this kind. In the Crimea, amputation at the hip-joint was performed in ten cases without a single recovery. In the Schleswig-Holstein war, the same operation was performed seven times, with but one recovery; results more unfavourable even than those of excision of the same joint.

The Knee-joint.—Mr. Butcher, the able and distinguished champion of resection of the knee-joint, by his valuable memoirs on the subject has left little to be desired, either in the collection of cases, or in the analysis of their results. Time only can add to our knowledge on this subject, by testing the permanency of the cure effected by the operation; and additional experience may guide us more correctly in our choice of cases, by teaching us the comparative advantages of the operation in the various affections of the joint.

The account of the first operation for resection of this joint is contained in a letter from Mr. Filkin of Northwich, to Mr. Park, in answer to an application made by Mr. Park for an account of the case:

"The patient," says Mr. Filkin, "was always of scrofulous habit, and had for many years a tumour on the knee, which gradually increased in size, and to which every topical application was made without effect. By accident falling from a horse, the patella was fractured, and from a small wound there was discharged a quantity of fetid, foul-coloured pus. Amputation was proposed, but the parents not consenting, my father was called in. Having frequently thought this method might sometimes succeed, and having performed it once on the dead body, he proposed it to the parents of the patient in this case, though it was an unfavourable one, the patient's general health being much impaired. The parents consenting, a day was fixed for the operation, which was performed August 23rd, 1762. The ligaments were found in a sloughy and suppurated state, with the cartilages greatly injured, and the heads of the bones much diseased, particularly the head of the tibia. The patella, head of the femur, and a portion of the tibia were removed; a good digestion came on; the limb was kept in a straight position; and on November 21st, 1762, the patient got so well as to require no further attention. The person is now living, and sometimes goes to Liverpool, where, if you will give me leave, I will direct him to call upon you."

In 1781, Mr. Park himself operated with success; not having heard of Mr. Filkin's case. In France and Germany it was, in a few instances, put into practice at the end of the last century; and again at the commencement of the present it was performed by Sir P. Crampton and Mr. Syme; but since the year 1830 it had been abandoned, until re-introduced by Mr. Fergusson in 1850. Since then it has been fortunate in possessing good operators and successful surgeons to put it in practice, and an able advocate to sound its praises, and keep us in memory of its successes; and thus it is at the present time recommended and practised by most of the leading surgeons of the day. Even now some surgeons stand aloof, and prefer subjecting a patient to amputation of the thigh, to exposing him to the, to them, untried dangers of resection. Curiously enough, foremost among

* Resections and Gun-shot Injuries, translated by Statham, p. 94.
† Deutsche Klinik, No. 41. ‡ Medical Times and Gazette, vol. i. p. 418' 1848.
§ Ibid., Sept. 20th, 1856.

those who hesitate to perform resection of the knee-joint, stands one or more whose very watchword is "conservative surgery," and who deservedly have the reputation of never, without good reason, resorting to operative interference.

The operation is only recommended, even by its strongest supporters, as a substitute for amputation in certain cases, and not as an operation of expediency. It may be performed as a substitute for amputation in any disease of the cartilages, synovial capsule, or articular extremities, which either by its severity endangers the life of the patient, or is in its nature incurable. Thus it has been put into practice with success by Dr. Buck, of the New York Hospital, in a case of angular ankylosis of the knee. On the other hand, just as in the case of the other joints, the disease, if of the bones, must be limited to their articular extremities; while of course visceral disease, or extreme age, alike preclude the performance of the operation.

This proceeding has but rarely been put in practice for injuries of the joint; indeed, it it seldom that the knee is exposed to an accident sufficiently severe to justify resection, which is not at the same time so dangerous to life as to necessitate amputation. It may, and has been, performed for gun-shot injuries to the articular extremities of the bones forming the joints, or for wounds of the capsule where the articular surfaces of the bones are bruised or lacerated by the bullet. It has also been put in practice with success in a case where, from an old fracture of the patella, with great separation of the fragments, the leg became useless, and mechanical appliances could not be worn.

The object to be attained by this operation is to free the patient from an incurable and often fatal disease, and to restore to him the power of progression, by giving him a firm and unbending, though shortened, limb, which shall have its axis of motion at the hip-joint. In fact, this operation, if successful, effects all that amputation can do; and in addition, while it endeavours to save the patient's limb, it is said to expose the sufferer to no greater danger of his life than amputation.

The various modes of resecting the knee-joint resolve themselves into two principal methods; first, the H-shaped; and second, the semilunar form of incision.

In the first operation, an incision three or more inches in length is made on either side of the joint, opposite the lateral ligaments, and these are joined by a transverse cut, extending across or below the patella, which bone is either thrown up in the flap, or removed, according to circumstances; forcible flexion is then used, and the lateral ligaments divided; the posterior part of the femur is carefully freed from its ligamentous covering; and with an ordinary saw the articular surface of the bone may be removed, first from the femur, and then from the tibia. The second mode of operating consists in forming a semilunar flap, with its convexity downwards, commencing just above one condyle of the femur, extending across the joint below the patella, and ending at the opposite condyle; the subsequent steps of these operations are the same in both cases.

There is yet another plan of excising the joint-ends at the knee, which was originated by Mr. Jones of Jersey, and has for its object the preservation of the ligamentum patellæ. It consists in forming two lateral longitudinal incisions, with a transverse connecting cut across the head of the tibia; the flap thus formed, of the integuments only, is thrown up, the synovial capsule cut through, and the patella with its ligament drawn to the inner side while the joint is in an extended condition; forcible flexion should now be used, the lateral ligaments divided, and the articular surfaces sawn off.

This operation, though admirable in its design, must at all times be difficult of execution, and in some cases impossible, though its performance may be much facilitated by the use of Mr. Butcher's ingenious saw. Indeed, Mr. Jones thus speaks of his operation:—"There are cases in which it is altogether inadmissible, and I feel persuaded that whoever adheres to one mode only, will often find himself wofully disappointed in the result."

To secure the general object in view—viz., bony ankylosis—it is in all cases necessary to remove the articular surfaces of both tibia and femur, whether diseased or not. The condition of the patella, as disclosed during the operation, will determine the course to be pursued with respect to this bone; any small and circumscribed spots of caries may advantageously be removed by the gouge, for in all cases healthy surfaces of bone should be left in contact.

In resecting joints, the seat of the particular affection known as "pulpy synovial disease," it is doubtless of great importance (as suggested by Mr. Humphry) to remove, if possible, all portions of the degenerated synovial membrane, as its presence in the wound cannot but prolong the subsequent suppuration and retard the healing processes. After removing the portions of bone from the articular extremities, much difficulty has occasionally been found in bringing the femur and tibia in apposition, and in maintaining the whole limb in a right line; to obviate this, Mr. Hutchinson has suggested the division of the hamstring tendons, a proceeding fully justified by the vital importance of the end in view. Heyfelder recommends, that even if the limb cannot at first be placed in a straight position, it should be put upon a splint; and a day or two afterwards straightened, when the muscles will be found to offer less resistance. In proof of this he quotes a case where, ten days after the operation, the limb could be straightened which had hitherto resisted the efforts made to place it in a proper position. Five days afterwards this patient died of pyæmia. We cannot but disagree with Heyfelder in this plan of practice, deeming it, as we do, of the highest importance that the limb be placed at the time of operation in a proper position, and there maintained until it no longer requires artificial support.

In no other resection is the after-treatment of such importance as in resection of the knee; on this point Mr. Butcher justly insists, and urges the personal superintendence of the surgeon in all the subsequent dressings and shifting, which should be few and far between.

A well padded McIntire splint behind and a long Liston's thigh splint outside are almost indispensable to the well-doing of the case. The former should be applied before the patient leaves the operating-table, and the latter on his removal to bed. After no operation is the judicious administration of stimulants and opiates, together with a skilful management of diet, more necessary than after this; indeed, in reading over any number of cases of recovery from this operation, though a few patients seem to suffer but little from the immediate effects of the operation and subsequent suppuration, yet in the many cases one cannot but be struck with the fact, that where the event was successful, the after-treatment showed a well-contested and hardly-won struggle between the surgeon on the one hand, and the flagging powers of the system on the other.

We pass on to consider the results of this operation, as furnished us by Mr. Butcher in his memoirs on the subject; omitting the records of cases he gives us operated upon prior to 1850, we find recorded the abstracts of 82 cases where the operation has been performed. Of the 82 cases subjected to operation, 57 have recovered with either useful limbs, or progressing rapidly towards the same result; 15 in all have died, and one of these after amputation; 8 have been subsequently subjected to amputation, with the one death above mentioned. Mr. Butcher relates that of the 57 recoveries, at the time his account ceases, 36 had regained "perfect use of limb"—i. e., a strong and unbending limb, fitted for all purposes of locomotion; the remaining 21 were in progress of perfect recovery. That recovery, when established and complete, will (setting aside accidents) be found permanent, we have little doubt; though at present we have no living instances of cure extending over more than six years, yet we can see no reason why, when tibia and femur are firmly united, there should be any more risk of relapse than after the union of a fractured femur.

In addition to the well-known case of Mr. Park, where the patient followed his avocation as a seaman for many years after the operation, we have a case before us related by Heyfelder, where the knee-joint was resected, in 1830, by W. Jäger; the patient was twenty-eight years of age at the time of the operation, and was seen thirteen years afterwards by Heyfelder, who reports that at that time the limb was slightly bent laterally, but in a perfect state of extension, the patient enjoyed "complete" use of the limb, and could make pedestrian journeys without the support of a stick.

A year after this he died of phthisis, and the limb was examined by Reid, who found perfect osseous union between the femur and tibia. The tibio-fibular articulation was unchanged. In some cases, the symmetry of the lower extremity has been remarkably restored by the operation. Mr. Butcher's patient appears, by the drawing furnished us, to have recovered the normal appearance of his limb. The same gentleman also gives a woodcut, representing the altered appearance of the limb after the excision of the knee-joint. The patient was one of Mr. Jones's, and, says Mr. Butcher—

"This case affords such an admirable illustration of the powers of conservative surgery when directed by an able surgeon, that it is a pleasant and instructive thing to contemplate and dwell upon the great change, as represented in the annexed woodcuts of the limb, taken before and after the operation.*

The aforesaid cuts show indeed a change—the effect of the joint efforts of surgeon and artist; for not only has the "able surgeon" restored beauty and symmetry to the limb, but the change has been materially assisted by the artist, who has drawn a right limb before the operation, and a left after. It has been objected to this operation, that the period of recovery is long and tedious. This we by no means deny. But surely the object to be gained is well worth the time consumed in attaining it. How willingly do patients submit to two, three, or more years' treatment of a diseased knee-joint before abandoning it to amputation; and, after all, though a patient often quickly recovers from the immediate effect of amputation, yet we question if there is much difference in the time after operation when a patient can comfortably wear an artificial leg, and when he can walk upon the restored limb from which the knee-joint has been resected. Again, it has been urged against resection, that by performing this operation in the young, the growth and development of the limb are arrested. This objection has principally been put forward by Mr. Syme of Edinburgh, who himself met with a case which, so far as it went, fully justified this opinion. More recently, Mr. Jones of Jersey, Mr. Page, Mr. Keith, and others, have had cases where the same operation has been performed in youth without its affecting in any way the subsequent growth and development of the limb.

Mr. Jones operated on two boys in the years 1851 and 1852 respectively, who are now both possessed of useful limbs, and have in no way suffered as Mr. Syme threatened. Mr. Page of Carlisle reports a case where, during four years, the growth of the stiff limb has quite kept pace with that of its fellow, and there are not wanting other cases which tend to confirm our belief that, provided the line of epiphysis is uninjured by the operation, the subsequent growth of the bones will continue as before. Mr. Syme,† in his operation, sawed off the femur as high as the tuberosities, and afterwards removed half an inch more from the femur, as well as the head of the tibia. It is not unlikely that in this case the epiphysis was injured or removed.

The shortening—a necessary consequence of the removal of the articular surfaces of the bones—if slight, will be found rather advantageous than otherwise in a limb where the knee-joint is stiff, and any greater loss in length may be supplied by a piece of cork worn in the shoes.

Among the 82 cases subjected to operation, 15 died—3 from phlebitis and pyæmia; 2 from dysentery and diarrhœa; 2 from phthisis and organic disease; 6, more immediately from the operation; and 2 sank at a longer interval. Mr. Butcher separates from these fifteen, a case of Mr. Cutler's, where partial resection was performed, the articular end of the femur only being removed; thus the first

* Dublin Quarterly Journal, Feb. 1857, p. 52.

† Syme's Cases of Resection, p. 186.

principles of resection were unfulfilled, and a successful result could scarcely be expected. Two of the fatal cases suffered from severe hæmorrhage, which carried one of them off on the fourth day, while the other lived to have his limb amputated, and died five days after the second operation.

Mr. Butcher labours hard to prove that the mortality after amputation in the thigh is greater than after resection of the knee-joint. That the success of the latter operation is abundantly sufficient to justify its performance in suitable cases, there is no question; but that amputation is a much more dangerous operation we hesitate to affirm, although we have no doubt but that the immediate effects of amputation are more dangerous to life. Still the exhausting effects of the subsequent processes after resection fully counterbalance the early dangers of the former operation. We find that out of the cases related by Mr. Butcher, eight were subjected to amputation. Seven of these recovered. Now, it may be without injustice concluded that had not amputation been performed, these cases would in all human probability have been added to the number of fatalities, making twenty-two deaths out of eighty-two cases, or about twenty-six per cent. And again, the very fact that amputation was performed in these cases to save life, opposes the notion of the greater danger of this latter operation.

We cannot conclude our account of Mr. Butcher's memoirs without noticing the great difference in success and comparative mortality of the two series of cases published by him. In the first table, containing cases "operated on from July, 1850, to December, 1854," we have 31 cases, with 25 recoveries, 5 deaths, and 1 amputation. In the second table we have recorded 51 cases; out of these, 9 died, 7 were subjected to amputation, with 1 fatal case; 1 remains in a precarious condition, and the remaining 33 recovered. The comparative want of success in the cases contained in Mr. Butcher's second memoir, together with the much greater number that were subjected to amputation, suggests forcibly that the difference between the results of the two tables is not a mere matter of accident —indeed, more than one instance related in the second memoir was obviously unfitted for the operation of resection.

"In my former essay (says Mr. Butcher) I forcibly dwelt upon the necessity of selecting the cases for excision, and pointed out the prominent features which should influence the surgeon; but I fear the caution has not been applied in every instance; I fear the panting after *éclat* has charmed away some from the stern dictates of judgment."*

The well-known danger of wounds of the knee-joints, but especially of gun-shot injuries, where with laceration of the capsule there is fracture or bruising of the bones, had hitherto led to the practice of performing amputation in these cases, and it remains for additional experience to determine the advisability of attempting resection of the joint in such cases. Stromeyer says of these injuries,—

"I have not undertaken resection of the knee-joint, because it affords little hope, even under favourable circumstances, and because, in the majority of cases, it cannot be certainly known how much of the bone should be cut off."†

So far as our information extends, there are but two cases recorded where this operation has been performed for gun-shot injuries. The one occurred in the Schleswig-Holstein war,‡ and the other during the Crimean campaign.§ Both were fatal. The former was performed for a bullet-wound of the capsule, with fracture of the external condyle. Three days after the injury, the articular ends of the femur to the extent of an inch and a half, were removed. The bullet, and a portion of cloth, were found loose in the joint. The result of this case was, as one might have anticipated, that the patient sank a month afterwards, with secondary pyæmic deposits in the lungs and elsewhere, and tubercular disease. The cartilage of the tibia was only partially thrown off, and a portion of the same still hung in shreds from the bone; the granulations from the femur, which had

* Statham's Translation, p. 31. † Dublin Quarterly Journal, p. 58. ‡ Ibid., p. 95.
§ Medical Times and Gazette, Sept. 20, 1856.

been "luxuriant," were shrunk and discoloured, together with the semilunar cartilages. Here it appears as if the patient's strength was inadequate to support the ulceration and discharge necessary for the separation of the extensive cartilaginous surface of the tibia—a process which might have been avoided had the articular surface of the tibia been removed at the time of the operation. There is yet another case mentioned by Mr. Guthrie in the last edition of his Commentaries, where the knee-joint was resected for a shell-wound by Staff-Surgeon Lakin. This case is said to have recovered, though we have been unable to find any subsequent account of it.

The Ankle-joint.—Excision of the ankle-joint, originated by Moreau, strongly advocated and successfully practised by Sir Astley Cooper, has in these days, for some insufficient reason, met with but little countenance; and in comparison with the effects of the same operation on the knee, its advantages seem to be by no means adequately appreciated. Resection is applicable to the ankle-joint in its diseases, but still more so in its injuries, and this both on account of its structure and position. The synovial cavity of the ankle, unlike that of the wrist, may be laid open or destroyed by the operation, without implicating the surrounding articulations; there is less danger here of inflicting an injury on a neighboring joint, and of setting up a disease no less severe than that for which the operation was undertaken. Again, after removal of the articular extremities, the appearance of the wound contrasts most favourably with that left after resection of the knee-joint, where the exposed surfaces of bone are very extensive, and the cavity of the wound large and deep. In addition to this, the position of the ankle-joint, far as it is from the centre of the circulation; while, on the one hand, it is perhaps unfavourable for the healing process, yet, on the other, it enables us to put excision into practice with but little danger to the patient's life; indeed, we are not acquainted with a single case where the operation was followed by fatal consequences.

The diseases of the joint under consideration present considerable difficulties in the diagnosis of their extent; it not only requires great care to decide whether the disease is entirely limited to the tibio-tarsal articulation, but it is sometimes nearly impossible to distinguish between caries of the astragalus and the same disease of the os calcis. For this reason it is, as well as on account of the great difficulty of exposing the articular extremities when the bones are in their normal position, that the operation is best adapted for injuries of the joint, and especially for compound dislocations. In these the amount of mischief is more easily ascertained, and the protrusion of the bones, rendering their section easy, obviates the greatest difficulty of the operation. Sir Astley Cooper, in speaking of the compound dislocations of this joint, states that he knew of no fatal case where the operation was performed, though he had met with several where it was not put in practice.*

Resection may be substituted for amputation in any synovial or carious disease of the ankle-joint which does not extend among the other tarsal articulations, or involve any considerable portion of the shafts of the bones of the leg. It may be performed in rare cases of gun-shot wounds of the joint, with fracture of the bones; but in these accidents there is too often such considerable injury to the soft parts and vessels as to demand amputation. In compound dislocation,

> "If the dislocation can be easily reduced without sawing the ends of the bones, if the bone be not obliquely broken, but remains firmly on the astragalus when reduced, if the ends of the bone be not shattered, if the patient be not excessively irritable, the bones should be returned to their place; but rather than amputate the limb where the above-named circumstances were present, one would certainly saw off the ends of the bones."†

The same author transcribes a letter from Dr. Kerr of Northampton, who, after sixty years' experience, writes:

> "Several such cases have fallen under my notice, and it has been uniformly my practice to

* Sir A. Cooper on Dislocations, p. 301. † Ibid., p. 302.

take off the lower end of the tibia; in my early life I have seen many attempts to reduce compound dislocations without removing any part of the tibia, but to the best of my recollection they all ended unfavourably, or at least in amputation; by the method which I have pursued, I have generally succeeded in saving the foot and a tolerable articulation."

From the results of several operations on this articulation it appears that in many cases a considerable amount of motion has been restored to the patient, and also that this mobility has been found quite compatible with the necessary strength of the limb. It may also be noticed, that in those cases where complete ankylosis has taken place, the loss of motion in the ankle-joint has been greatly compensated by the increased mobility in the other tarsal articulations, and especially by the movements of the astragalus upon the os calcis.

In performing the operation on this joint, there is said to be no absolute necessity for removing the opposing cartilaginous surfaces: in diseases, the affected surface of bone should be sawn off; in injury, the protruded or fractured portions of bone need only be excised. Many instances have occurred where, after sawing off the protruded ends of the tibia and fibula, the cartilaginous surface of the astragalus has been left untouched, and with the happiest result. In this particular we cannot but notice the much greater proneness to ulceration displayed by the articular cartilages of the long bones entering into the knee-joint, where exposure of the cartilage but too surely leads to its ulceration and separation; still we have no doubt that the safest course, and that which is most likely to promote recovery, is to remove the cartilaginous surfaces of the joint, whether healthy or diseased.

The plans of incision used for excising this joint have been various, having for their object the adequate exposure of its cavity without injury to the tendons; this can be best accomplished by the operation recommended and described by Mr. Guthrie in his Commentaries, at p. 99, to which we refer the reader, as the limits of this paper will not permit us to transcribe it. After the operation, the limb may be best secured on a back splint and foot piece, having moveable sides, which can be let down to wash and dress the wound.

We have collected and have before us the histories of ten cases where resection has been performed for disease of the ankle-joint; they occurred in the practice of Messrs. Hancock, Wakley, Statham, Teale, Humphry, Ure, and Hutchinson. Of the ten cases, seven are reported cured, and the remaining three are said to be respectively "under treatment," "slowly recovering," and "going on well." The cures were certainly rather tedious, but appear to have been in most cases complete; one, Mr. Wakley's patient, was shown three years afterwards at the Medical Society, and could walk well, with a slight halt, and possessed some motion in the joint. All these patients could walk; of four it is reported that they possessed motion in the ankle-joint; of one that the ankle was very weak, and liable to bend. In two cases the account merely states that they were cured and could walk. We have omitted in this account two operations quoted by Heyfelder; in both these the tarsus was extensively diseased, and in consequence one or more of its bones were removed at the time of operation, thus constituting an operation like those on the wrist-joint, where the first conditions of successful resection are not fulfilled. The results of these two cases were such as might be anticipated: the one, Heyfelder's patient, lived to have his limb amputated, but died seven days after the second operation, of pyæmia; while the other, a case of Dr. Roberts', eventually recovered so far as to be able to walk with a stick. Of all serious accidents to which the ankle-joint is exposed, there are few more liable to occur, or more dangerous in their consequences, than compound dislocations; and it is fortunate that in resection we possess an operation which in most cases will not only preserve life, but secure to the patient the possession and use of his limb. We refer for confirmation of our remarks, to Sir A. Cooper's account of his experience of this operation; in his work 'On Dislocations,' nine cases are quoted as having occurred either in his own practice or that of his friends. In all these resection was performed for compound dislocation, with or without fracture; the fractured or protruded ends of bone were alone removed, and in no case does it appear that the cartilaginous surface of the astragalus was purposely destroyed. All recovered, five even retaining motion at the seat of operation. The only case we are acquainted with where the result of this operation, when performed for compound dislocation, was unfavourable, is one that occurred in the practice of Mr. Hey of Leeds, who removed, from a man, aged thirty-five, the lower end of the tibia, for compound dislocation of that bone. This case appears to have gone on badly, and the last report we have been able to meet with states that the ultimate prospects of the patient's recovery was doubtful.

There are yet other points of importance connected with the subject of resection of joints to which we would wish to advert, but we must defer their consideration to another opportunity.

Review II.

Bijdrage tot de Kennis der Spijsvertering van de Plantaardige Eiwitachtige Ligchamen. Door Dr. Rinse Cnoop Koopmans. ('Nederlandsch Lancet,' No. 7, p. 385. 1855.)

A Contribution to the Knowledge of the Digestion of Vegetable Albuminous Bodies. By Dr. Rinse Cnoop Koopmans.

The object of Dr. Koopmans' investigations was to ascertain, with more exactitude than had previously been done, the changes which vegetable albuminous matters undergo in the stomach during digestion. The author points out, that while the corresponding class of bodies in the animal kingdom and the non-nitrogenous constituents of vegetables had been duly studied in this respect, the substances alluded to had received but little attention. His first inquiry was, whether pepsin is necessary for the solution of the albuminous matters occurring in the cerealia and leguminosæ; consequently, whether these substances, during their solution in gastric juice, undergo a peculiar metamorphosis (pepton-formation), and what properties they thereby acquire. Lastly, whether a difference in the amount of acid contained in the digestive fluids exercises an influence on the solution, and whether this is the same as for other substances.

The quantity of albuminous matters in the seeds of most of the cerealia varies not only in different species, but also in the same species according to the place of growth. This difference was particularly remarkable in preparing gluten from Hungarian and Dutch wheat. In the Netherlands it was necessary to use much more flour in order to obtain the same quantity of gluten than was required in the experiments the author performed in Vienna. Moleschott, in his 'Physiology of Aliments,' has collected numerous analyses of species of grain exhibiting this fact.

Gluten was the first substance examined by Dr. Koopmans in order to ascertain whether it is capable of solution in a dilute acid alone, or whether the co-operation of pepsin is necessary to produce this result; he comes to the conclusion, that while it is not perfectly soluble in dilute acid alone, it does not occur therein entirely unchanged, or merely in a state of mechanical suspension, since otherwise more accurately-defined forms than he observed should be visible under the microscope. He then asks whether a molecular swelling may have taken place? But no visible swelling of the whole mass, such as is met with in fibrin, is visible. In fine, he does not fully decide the question, but contents himself with intimating, that when in the sequel he speaks of a solution of gluten in an acid, he does not mean to refer to this condition in its strictest sense.

With reference to the second branch of his inquiry, the author feels justified in drawing the conclusion that during digestion in the stomach, gluten is not merely dissolved, but that it is at the same time modified in its properties in like manner as the other albuminous bodies, and that consequently, a "gluten-pepton" also exists.

One of the principal differences said by Mialhe to prevail between albuminous bodies dissolved by acid alone, and through the influence of pepsin, the author did not find in gluten—viz., that on the addition of rennet, they coagulate from the latter acid solutions like milk, to be again subsequently dissolved.

This is said to obtain for all albuminous bodies, as well for dissolved albumen as for fibrin and gluten; but in the author's experiments on the latter substance he never observed a coagulum such as is produced by the action of rennet on milk. On the addition of acid gastric juice to the hydrochloric solution, a precipitate in most instances occurred, but this took place also when an acid alone was used, so that there is in this case no ground for attributing an active part to the pepsin. But Longet gives a distinctive mark, which Dr. Koopmans also observed. Longet states that when peptons are present, sugar cannot be indicated by Trommer's test, as the oxide of copper is then not reduced, while albuminous matters dissolved in acid alone do not hinder this reaction; and he further says that the absorbed peptons retain this property in the vena portæ. Hence sugar ought to be conveyed, without being capable of demonstration, from the intestinal canal to the liver. As a proof of the non-existence of a glycogenic function of the liver, this has, however, absolutely no value, as Bernard has plainly pointed out. Bernard always examined the blood after repeated filtration through animal charcoal, by which all albuminous matters are removed, and as a controlling test, fermentation was employed. Longet himself admits that peptons do not hinder fermentation. After some details, into which it is unnecessary to enter here, the author remarks that the more or less perfect reduction of the deutoxide of copper from Trommer's test ought to enable us to distinguish albuminous bodies from one another. Thus ordinary albumen is coagulated by a boiling heat, while albuminous bodies dissolved in a dilute acid, remain in solution at 212°, but are precipitated by sulphate of soda. Peptons, on the other hand, are rendered completely insoluble neither by a boiling heat nor by sulphate of soda, but are retained by animal charcoal.

"Accordingly, if a small quantity of grape sugar, added to an acid fluid, is not indicated on the application of Trommer's test, but if the reaction takes place after boiling, ordinary albumen is present; but if this does not ensue until after treatment with sulphate of soda, an albuminous matter modified by acid has been removed; but if filtration through animal charcoal is necessary in order to make the precipitation of deutoxide of copper evident, the presence of a pepton may be assumed, at least if no other matters capable of disturbing the reaction be present in the fluid." (p. 400.)

Physiologically speaking, the most important change albuminous bodies undergo is, that they are no longer precipitated from the solution in gastric juice when the fluid is neutralized. If they are only molecularly altered by the action of a dilute acid, they cannot be taken up into the blood, since the alkaline reaction of the latter is sufficient to prevent their passage into it. The author details an experiment to show that the fluids of the small intestines have the same influence in preventing the absorption of such a fluid.

From experiments undertaken by Dr. Koopmans, with a view of determining the degree of acidity most effectual in producing the solution of gluten in artificial gastric juice, it was at once evident that raw gluten requires a different amount of acid in order to its perfect solution from albumen. Boiled gluten appeared to be not so exclusively limited to a definite degree of acidity. Thus it was found impossible to obtain the complete disappearance, in gastric juice of equal strength, of both raw gluten and albumen, although the test-tubes were allowed to stand for days together. This was equally the case at the temperature of digestion or at that of the apartment; and Schwann had already found that increase of the degree of acidity did not compensate for the retarding influence of a diminution of temperature.

The author found that between $\frac{1}{2000}$ and $\frac{1}{400}$ lies the degree of acidity at which most raw gluten, and between $\frac{1}{275}$ and $\frac{1}{60}$ that at which most boiled albumen, is dissolved. It also appeared that with the same proportion of acid which in artificial gastric juice best dissolved raw gluten, the latter became most broken up in distilled water.

In order to ascertain more accurately whether this great difference between these substances really exists, Dr. Koopmans instituted the following experiment; the middle part (containing the rennet glands) of the mucous membrane of the stomach of a pig having been well washed, was cut into small pieces, and digested for some hours with distilled water at a temperature of 100·4°. To portions of this neutral fluid, previously filtered through linen, so much hydrochloric acid was added as to give one part of the latter to 900, 500, and 100 parts, thus forming artificial gastric juice with $\frac{1}{900}$, $\frac{1}{500}$, and $\frac{1}{100}$ of acid. Similar well-closed, wide-mouthed, glass-stoppered bottles were now, three by three, filled with twenty-five cubic centimetres of these fluids. Four pieces of freshly prepared raw gluten, four of gluten boiled for fifteen minutes, and four of albumen boiled for ten minutes, were then weighed; nine of these pieces were introduced into the bottles, the three others were intended to ascertain, by drying, the original amount of solid matters in the substances used for the experiment. (In three other bottles the loss in legumin was determined in the same manner.) All these bottles were now kept for seven hours in a water-bath, at an uniform temperature of 100·4°. At the end of this period what remained undissolved was thrown on filters, and after twenty-four hours was dried equally with the other weighed pieces at 248°.

The foregoing will suffice to exhibit the manner in which the author conducted his experiments. Without reproducing the tables which he bases upon them, we may state generally that the numbers he obtained confirmed the results of the experiments already mentioned, in which he determined the most efficacious proportions of acid. In all the experiments, therefore, more albumen was dissolved in a relatively strong than in a weak acid; on the other hand, more gluten was digested in a dilute acid than where the amount of acid was greater; in other words, a fluid which attacks albumen most violently, dissolves least gluten, and *vice versâ*. Consequently, a weakly acid gastric juice is prejudicial to the digestion of coagulated albumen, while it may promote the solution of other substances.

The author found albumen to dissolve, without much difference in time, in artificial gastric juice prepared from the stomachs of a calf, sheep, pig, dog, rabbit, goose, hen, tortoise, frog, and bream. So far as this experiment goes, there would therefore appear to be but little modification of the organic constituent of the gastric juice in different animals, though a difference in the proportion of acid would appear to be capable of explaining much. Unfortunately but few analyses of the gastric juice of various animals have been made known; the gastric fluids of man, the dog, and sheep, alone have been more accurately examined. The author borrows from O. von Gruenewaldt the ascertained quantities of free acid in each of these:

	H Cl. in 1000 parts.
Gastric juice, free from saliva, of the dog, ...	3·050
Gastric juice, containing saliva, of the dog, ...	2·337
Gastric juice, containing saliva,* of the sheep	1·234
Gastric juice, containing saliva, in man, ...	0·200

Hence it would appear that much more acid is present in the stomach of the carnivorous dog than in that of the herbivorous sheep; accordingly, Bidder and Schmidt observed that boiled albumen dissolves much better in the gastric juice

* In the sheep, the food is always tolerably dry in the third stomach (psalterium), the greater part of the saliva having been absorbed in the ante-stomachs.

of the dog than in that of the sheep. The easy digestion of gluten in artificial gastric juice with a very dilute acid therefore affords reason for supposing that vegetable food should be best digested in the fluid of the stomach of the herbivora, but we must recollect that while gastric juice is the pure secretion of the rennet glands, it soon forms part of a very compound fluid, the constituents of which are never exactly similar; thus, though the quantity of acid in the original gastric juice is tolerably constant, in the stomach it may be very different. The same is true also of the *nature* of the acid: in the pure gastric juice, free hydrochloric acid seems always to occur, in the ordinary digestive fluid it very often cannot be demonstrated, but lactic acid is now present, which is especially indicated in the analyses of the gastric juice of man. In the ordinary digestive fluid, Schmidt found no hydrochloric acid, but lactic acid. On the contrary, the former was the acid present when the secretion of the pepsin-glands was excited, not by food, but by indifferent matter, which acted only mechanically.

O. von Gruenewaldt found the degree of acidity of the contents of the stomach extremely variable in man, and that this appeared to be much influenced by the acid present in the food itself. Thus, he observed that when a countrywoman labouring under fistula of the stomach, on whom he experimented, had eaten rye bread, her favourite diet, containing much lactic acid, there was far more acid in the stomach than during the use of wheaten bread, the reaction of which is neutral. He remarks, in reference to this point, that an egg eaten along with the latter bread remained longer undigested.

Schmidt also shows, by an experiment, how much the amount of acid in the stomach depends upon the nature of the food. Thus, the gastric fluid of a dog, fed exclusively upon boiled vegetables, required more potash to neutralize it than that of one which got meat; while, nevertheless, the proportion of hydrochloric acid was less. Schmidt assumes that the reaction described was owing to the development, under the influence of the saliva, of lactic acid. In the genuine herbivora, however, which use only raw food, such a high degree of acid does not occur, even if the gastric juice is mixed with saliva and food. In the ruminantia, the effect of the saliva does not extend to the contents of the fourth stomach, as the matters metamorphosed thereby are absorbed in the ante-stomachs (Schmidt); in the others, the formation of lactic acid from starch in the stomach is very improbable, as the saliva of man and of the dog cannot change at least raw starch even into sugar (Gruenewaldt), and gastric juice alone exercises no influence worth mentioning upon the amylacea.

The conclusion that vegetable food is best dissolved in a gastric juice with dilute acid, depends, however, on the transfer of the properties belonging to gluten to the other albuminous matters found in plants; and the assumption that the mode of their occurrence therein does not essentially alter this relation. Thus, being enclosed in cells is of the greatest influence on the entire solution of albuminous matters; bran, for example, is capable of yielding suitable nutriment only in the digestive apparatus of some animals. The herbivora alone extract the albuminous matter, and the fat which is contained in it in great quantity from its thick-walled cells; dogs restricted to its use die of starvation.

At all events, therefore, vegetable food in general will not be dissolved by the same gastric juice in the same proportion as gluten, except when this substance constitutes the principal ingredient.

Even in this more limited sense, however, this property of gluten is of great importance, not only in reference to the herbivora, but especially to man. The cerealia occupy a prominent place among our articles of food; even if, therefore, all other albuminous matters be less perfectly digested in the human stomach, that of bread may find precisely the conditions most favourable to its solution, in the, under ordinary circumstances, very weak digestive fluid it there meets. The observations of Gruenewaldt and Beaumont have distinctly proved that it is only through the acid occurring in the food, or formed therefrom, that the acid reaction of this fluid becomes in some measure stronger; while in the fasting condition it even passes through the preponderance of gastric mucus and saliva, into an alkaline state.

It is principally Frerichs who has examined the contents of the stomach under pathological circumstances, and directed attention to the modified digestion of starch, which mostly consisted in the formation of lactic acid, and even of acetic acid; this increased development of acid is generally recognised as pyrosis, through the obstructing influence it exercises on the digestion of food; Frerichs observed a total deficiency of formation of lactic acid in the stomach where much sugar was present, in two patients labouring under diabetes mellitus. It is worthy of note, that precisely in this disease Bouchardat's gluten-bread is borne for a long time with good result.

In order to ascertain whether the great difference in solubility between gluten and albumen, already described, really existed during the process of digestion in different animals, the author undertook the following experiments on pigs as representing the omnivora, and dogs as representing the carnivora. He had hoped to have procured horses, as being the most suitable among the herbivora, but failed. The compound nature of the stomach in the ruminantia, and the small size of rabbits, with the fact that some portion of the food always remains, even after long fasting, in the stomachs of the latter animals, he considered would seriously interfere with the accuracy of the investigation. In order that the substances experimented on should be brought under all the conditions which could influence their solution in normal digestion, and that these conditions should operate on all at the same time and in the same degree, the following plan was devised. Bags of muslin were filled with weighed quantities of freshly prepared raw and boiled gluten and albumen, and these, carefully closed, and distinguished from one another by the introduction of little glass beads of different colours, were passed immediately after one another through the mouth into the stomach.* After some time the animal was killed, the bags were taken out, washed with distilled water, opened, and the contents carefully collected, dried, and weighed. Other portions of the same egg and the same gluten were also dried, and by a comparison of the weights the loss the substance had undergone in the stomach was ascertained.

The results of experiments on six pigs and three dogs are given in a table, and from them the author draws the general conclusion—

> "That the same digestive fluid does actually in the living animal dissolve unlike quantities of albumen and of gluten, so that if much of the albumen be digested, the loss of weight of the gluten is but small, and *vice versâ*. So far as can be deduced from the experiments made, the quantity of the boiled gluten dissolved is, as was the case in the experiments on artificial digestion, proportionate neither to that of the raw gluten, nor to that of the albumen." (p. 420.)

In some of the pigs the albumen dissolved better than the gluten; in others, precisely the reverse obtained; but in no case did an uniform solution of both substances occur. In one dog in which the bags had, at the end of four and a half hours, penetrated to the commencement of the large intestine, the quantity of matters dissolved was beyond all comparison the greatest; in this case alone was the albumen entirely dissolved, and the other substances very much diminished —a result strongly in favour of the digestive power of the smaller intestines for albuminous bodies also, which has been very prominently put forward, especially by Bidder and Schmidt.

These experiments further show how important this intestinal digestion is for the metamorphosis of a sufficient quantity of the albuminous matters of the food. In the stomach of animals more of one element will be taken up at one time, and again, more of another—a circumstance which, resting on experiments on artificial digestion, we might be inclined to attribute to a variation in the proportion of

* It appears to us, however, that by this mode the important element of due insalivation was dispensed with, an omission which may have affected the solubility of one substance more than another.

acid. This is, however, adjusted in the small intestines, by which arrangement but little of the substance is left unused.

On the Digestion of the Albuminous Matters of the Leguminosæ.—In reference to this subject, it is of importance to know how the natural properties of the matters in question are altered by the influence of boiling heat, of acids, and of pepsin. Among the results of his experiments, the author states that the contents of the cells are molecularly altered by a boiling heat—perfectly coagulated if they have not become sufficiently diluted by previous solution in water, imperfectly if this has been the case. By treating pea-meal with water, we get, after separating the starch, a solution of legumin as it occurs in peas. This has always, even though freshly prepared, a weakly acid reaction. Authors are divided as to whether this fluid is or is not coagulated by rennet. Dr. Koopmans often added a neutral infusion of a calf's stomach to a fresh solution of legumin, and a firm coagulum always occurred after it had been for but a very short time exposed to a temperature of 100° Fahr., while no thickening took place in the same fluid under similar circumstances, but without gastric juice. By experiments upon a rabbit, Dr. Koopmans proved that a fresh infusion of peas is coagulated in the body also by acid gastric juice.

A solution of legumin in water is precipitated by dilute hydro-chloric acid, and the precipitate is re-dissolved by the addition of more acid. On an average, the proportion of acid necessary to produce this result is $\frac{1}{70}$. The acid in the stomach is, however, never so concentrated; immediate solution is, therefore, not to be expected; but by continued action a more dilute acid is capable of bringing about the same result.

On comparison with the solution of boiled albumen in artificial gastric juice, it appeared that the degree of acidity which digested this substance best dissolved the most legumin precipitated by a dilute acid from fresh infusion of peas, and that even after a very long time, just as with albumen, no complete solution ensued when the quantity of acid was too small.

Although legumin dissolves both in dilute acid alone and in gastric juice, the fluids thus obtained differ in their reactions, precisely as is the case with the other albuminous bodies soluble in acid alone. In the one instance peptons have formed, in the other they have not. The greatest difference between these two solutions consists, in the case of legumin also, in its being wholly or not at all precipitated on neutralization. This metamorphosis is of the highest importance for it also, in reference to its reception into the alkaline blood. If not changed into a pepton—that is, if not so modified as to remain dissolved when neutralized—legumin is unfitted for absorption.

In peas, legumin is coagulated by boiling. This is further almost always the case with its solution in water, but if the latter be made slightly alkaline, it will not coagulate completely on boiling, but will form a pellicle during evaporation. Towards gastric juice the coagulum does not, however, absolutely differ from coagulated animal albumen; by acids alone it is not dissolved, though it is by pepsin and a dilute acid; and this is best accomplished when the acid possesses the strength most suitable for the solution of albumen.

A principal essential condition for the digestibility of peas in the digestive apparatus of man, is the removal of the epidermis. If this be present, the digestive fluids cannot act at all on the contents of the cells; by long-continued boiling, however, the latter burst, like most cells; the starch swells up, and the legumin, partly coagulated, partly dissolved in water, is now rendered capable of metamorphosis. In some districts of the Danubian Principalities the inhabitants live exclusively on peas, and enjoy good health on this diet.

At all events, the fact is established, that for most men the seeds of the leguminosæ, provided they are well prepared, afford an excellent article of food. Still more than for gluten, therefore, in the relation of the legumin to the ordinary small amount of acid in the human stomach, must the solvent action of the fluids of the small intestine be taken into account, in order to explain the occurrence of a sufficient absorption.*

Review III.

1. *Small-pox and Vaccination.* Copy of Letters from Dr. Edward Seaton to Viscount Palmerston; with enclosed copy of a Report on the state of Small-pox and Vaccination in England and Wales and other Countries, and on compulsory Vaccination; with Tables and Appendices. Presented to the President and Council of the Epidemiological Society by the Small-pox and Vaccination Committee. (Parliamentary Paper, 3rd May, 1853.)
2. *On the Protective and Modifying Powers of Vaccination.* By Edward Cator Seaton, M.D.—*London*, 1857. pp. 26.
3. "*Small-pox*" *and* "*Vaccination.*" Articles in 'Copland's Dictionary of Practical Medicine.' Parts XVI. and XVIII.
4. *General Board of Health. Papers relating to the History and Practice of Vaccination.* Presented to both Houses of Parliament, by command of Her Majesty.—*London*, 1857.

Since the Arabian physicians first described the ravages of small-pox at Mecca, the history of this once formidable and still loathsome disease may be considered in three great periods, each impressed with distinctive characters; and now the epoch of a fourth may be said to have commenced.

The first of these periods is marked by an improvement in the treatment of small-pox, in which, more than in most diseases, medical opinion has undergone an obviously beneficial change. To Sydenham the merit of this change is due.

The second era is characterized by the discovery of a most singular custom practised in some countries, by which the severity of small-pox to the individual appears to have been greatly lessened. It was observed that the virulence of this poison was mitigated by introducing or engrafting the disease artificially into the system, through the cutaneous tissue. This was effected by propagating the disease from one person to another by the operation known as *inoculation*, in which the specific poison contained in the fluid of a mature small-pox vesicle was inserted into the skin of a healthy person. To Lady Mary Wortley Montague, as all the world knows, is due the merit of having introduced such a practice into this country, in 1722; an achievement on her part, when measured by our knowledge in those days, which ranks with deeds of the greatest heroism.

"To the present time," however, as Simon writes, "it remains one of the most interesting and least explained facts in pathology, that the specific contagion or ferment of small-pox, so uncontrollable in its operations, when it enters a man in the ordinary way of his breathing an infected atmosphere, becomes for the most part disarmed of its virulence when it is artificially introduced to the system through a puncture of the skin; so that a person exposed to this artificial infection very generally contracts the disease in its mildest and most tractable form."

We need hardly here allude to the deficiency of information possessed on this point. We have in fact no accounts of the virulence of the effects of this poison if introduced artificially by *punctures of the mucous membrane.*† It may be that

* [We regret to have to add to the analysis of the above memoir the announcement of the early death of its talented author, who, at the commencement of the present year (1857), at the age of twenty-three, fell a victim to typhus, taken in the active discharge of his duties as assistant-physician to the General Hospital of Utrecht.—Ed.]

† The only experiment on mucous-membrance inoculation is that performed by Mead: "In 1721 (at the command of the Prince of Wales) he made the famous experiment of inoculating some condemned criminals for the small-pox. On six of the prisoners he tried the ordinary method, on the seventh, he carried out the Chinese plan, slightly modified—by introducing into the nostrils a pledget wetted with matter taken from a ripe pustule. This succeeded. All the criminals contracted small-pox, except one, who had the disease previously. They all, too, recovered, and saved their lives; going out of prison even safer than when they went in. The success attendant on this trial being thus eminently successful, the two young princesses, Amelia and Caroline, were inoculated on April 17th, 1722." ("Our Great Ones of the Past," Med. Times and Gazette, 1856.)

a less dose of poison is introduced artificially in either case than finds its way into the system when small-pox is contracted in the natural way.

The epoch which commences the third era in the history of small-pox is marked by a not less remarkable discovery, which has gained for the name of EDWARD JENNER an immortal and a well-merited fame. He discovered the protective power of *vaccination*, and how its influence modified the course of the disease which the poison of small-pox produced. In June, 1798, his great discovery was first communicated to the world in the form of a thin quarto of scarcely more than seventy pages; and dedicated to his friend, the late celebrated Dr. Parry of Bath. When still an obscure apprentice with a surgeon at Sodbury, near Bristol, he caught a glimpse of a great pathological truth, and became impressed with the conviction that a disease existed, communicated by contact from the cow, which was capable of protecting the affected individual from the occurrence of small-pox. Amongst the gossip of cowherds he had heard of a vague, obscure, but popular belief regarding the possible existence of such an occurrence, a belief which prevailed in the rural districts of Gloucestershire, and on the Continent in some districts round Göttingen; but it was by the life-long efforts of Jenner that this discovery was elaborated, and its importance to the welfare of the human race clearly demonstrated.

During the past few years, and especially since about 1830, a fourth era in the history of small-pox may be said to have commenced. A single definite characteristic can scarcely be assigned to the pathological belief entertained from that period till the present time regarding the relation of vaccination to the arrest of small-pox. The characteristics are different, according as we regard the progressive history of small-pox and vaccination in our own country, or in other countries of Europe.

The contrast which such an history presents in Great Britain and on the Continent does not show that in this instance ours has been "a highly-favoured land." The comparison which some of the works at the head of this article have instituted on the protective and modifying influence of vaccination, as shown in the statistical records of this and other lands, is not honourable to our country—to its reputation as a nation, jealous and watchful of its national health, strength, enterprise, and wealth. Were we asked to give a character to this commencing era in the history of small-pox and vaccination, we should say that in this country the epoch is one of "*transition*;" and that according as Government will organize and efficiently enforce those measures which Science has shown to be of the most powerful protective and sanative kind against the occurrence, the spread, and fatal character of small-pox, this transition will be for good or for evil.

A feeling of doubt and of scepticism has prevailed to some extent as regards the efficacy of the protective power of vaccination, with an ill-defined belief in some pernicious influence inherent in the practice of the operation, and with a tendency to propagate erroneous and absurd popular notions as to the nature of these supposed deleterious effects.

In some countries of continental Europe, on the other hand, especially in the central and northern parts, this epoch is now marked by implicit faith in the protective and modifying influence of vaccination, and by the consequent successful and efficient legal enforcement of the measure as a part of State Medicine.

Between twenty and thirty years ago (1820–1835) a combination of circumstances paved the way for the scepticism which unfortunately gained ground with some, not only in this country, but then also on the continent of Europe; but it was not till during the past fifteen years that undercurrents of doubt found expression in no unmistakeable language, and which the renewed prevalence and spread of small-pox previous to 1840 tended to strengthen, or rather furnished an excuse for giving renewed expressions of opposition to the practice of vaccination, and misrepresentations as to the protection afforded by the measure. Besides the fact observed, and to a certain extent explained by Dr. Jenner himself, that all who were vaccinated were not completely protected, there were a host of theoretical objections propounded and urged with arguments of various force against vaccination. Physicians opposed the practice, and wrote pamphlets against it. It was written in those days that *a bestial humour* implanted in the human race would inevitably produce new and dreadful diseases; *horns* would grow on the vaccinated, and a *brutal fever* would excite incongruous impressions on the human brain; these and such-like pyrological doctrines were dreamt of in their philosophy. Religion and morality no less gravely and absurdly denounced the "greatest physical good ever yet given by science to the world." Leviticus was quoted by learned divines, with dark insinuations against "contaminating the form of the Creator with the brute creation." And, to perpetuate the prejudice in another way, it is said that over the remains of Mr. John Birch, surgeon of the time to St. Thomas's Hospital, may still be seen, within one of the City churches in Rood Lane, a monument erected by his sister, which commemorates that "*The Practice of Cow-poxing, which first became general in his day, Undaunted by the overwhelming influence of power and prejudice, And the voice of Nations, He uniformly and until death* (1815) *perseveringly opposed.*"

To the uninitiated the fight and the controversy was Doctor against Doctor; and inquiries of an official kind were called for and successively instituted. The Royal Jennerian Institution made a solemn declaration, expressing their full belief in the incalculable benefit already derived from vaccination, and the advantage and security still in prospect from its use. The subject was discussed in Parliament, and by command of His Majesty the Royal College of Physicians of London were ordered to inquire into the virtues of vaccination. To carry out efficiently this royal command, they put themselves in communication with each of the licentiates of their college; they corresponded with the Colleges of Physicians of Dublin and Edinburgh, and with the Colleges of Surgeons of London, Edinburgh, and Dublin; they called upon the societies established for vaccination for an account of their practice; and they record their belief in the following remarkable paragraph, which closes their Report to the House of Commons in Parliament assembled in July, 1807:

> "'The College of Physicians feel it their duty strongly to recommend the practice of vaccination. They have been led to this conclusion by no preconceived opinion, but by the most unbiassed judgment, formed from an irresistible weight of evidence which has been laid before them. For, when the number, the respectability, the disinterestedness, and the extensive experience of its advocates is compared with the feeble and imperfect testimonies of its few opposers; and when it is considered that many who were once adverse to vaccination have been convinced by further trials, and are now to be ranked among its warmest supporters, the truth seems to be established as firmly as the nature of such a question admits; so that the College of Physicians conceive that the public may reasonably look forward with some degree of hope to the time when all opposition shall cease, and the general concurrence of mankind shall at length be able to put an end to the ravages at least, if not the existence, of the small-pox.'"

Discussion was now at an end for a time. Henceforth the public mind was apparently quite satisfied on the subject, and from this period vaccination became universal among the children of the educated classes in this country.

> "The commencement of this century," writes Dr. Copland, "was remarkable for the progress of vaccination. In 1801 upwards of six thousand persons had been vaccinated, and the greater part had been tested with small-pox. In 1800, 1801, and 1802, vaccination was introduced into France, Germany, Italy, Spain, and the East Indies. In 1802, Parliament voted Dr. Jenner a reward of 10,000*l.* for the discovery; and, in 1807, the additional sum of 20,000*l.*; and in 1808 vaccination was taken under the protection of Government."*

The general assent of the medical profession appears to have been given at a much earlier period, at least fifty years ago, and was all but unanimous. There were, and there still exist, as we learn from Mr. Simon's papers, three sources of

* Dictionary of Medicine, art. Vaccination, p. 1286.

opposition. These are (1) hereditary opposition among the descendants of those whose fathers, friends, and teachers were contemporaries of Jenner, and among whom the old allegiance finds here and there a surviving follower of Birch, the surgeon of St. Thomas's Hospital; (2) the operation of personal eccentricities against the common convictions of mankind; (3) persons who for sinister purposes are always ready to oppose any project, independent of its merits.

Another source of opposition, however, appears to us to have been entirely lost sight of by Simon—namely, (4) that there are men impressed with the sincere conviction that neither vaccination nor inoculation affords that security and protection from small-pox which is generally believed to follow—a conviction impressed on their minds by a concurrence of what may be termed untoward instances within the limits of their own observation and experience.

It is now fifty-nine years since Jenner first promulgated his discovery to the world, and modestly said "he would continue to prosecute his inquiry, encouraged by the hope of its becoming beneficial to mankind." He felt a holy reliance in the truth of his discovery, and as he himself said, the keenest of all arguments which will be used against those who opposed the practice of vaccination, will be *those which are engraved with the point of the lancet!* This prediction has been fulfilled, as the works we have been privileged to review, enumerated at the head of this article, abundantly testify.

With one exception, in the writings of Dr. Copland, their conclusions are unqualified in favour of vaccination. He stands aside, along with Dr. Hamernjk of Prague, and to the opinions expressed by both of them we shall by and bye refer.

One would think it, at first sight, almost an insult to the human understanding to be obliged to collect statistics at this period to *prove* that vaccination confers a large exemption from attacks of small-pox, and almost absolute security against death from that disease. But so it is, and independent of the information which such statistical inquiry is calculated to convey to those who advise our lawgivers in high places, it is an eminently useful instrument of research in relation to everything which bears upon the nature of this disease.

The highest medical authorities, to which we can refer, recommend

> "That all views and facts objected to vaccination be rigorously inquired into, and that there be published annually, in some journal of large circulation, a true account of such inquiries, and an elucidation of whatever has seemed doubtful or contradictory; together with instances in which vaccination, or more particularly re-vaccination, has given protection against the contagion of small-pox, when this disease has been in the neighbourhood."

So writes Dr. Sigmund, Professor in the University of Vienna, and a no less distinguished authority and most amiable man, Dr. Alison of Edinburgh, the first Physician to Her Majesty for Scotland, and Emeritus Professor of Medicine, writes as follows:

> "It is to be observed, however, that the poisons producing all epidemic diseases are subject to variations, sometimes rapid, sometimes very gradual, both as to intensity and to several of the effects they produce; which makes it right to have the evidence of the efficacy of any such protecting power as cow-pox has shown now for years, subjected to examination from time to time, with the view of ascertaining whether any such modification of its usual power has taken place."

While, therefore, the subject of vaccination has had a large share of our attention—as the pages of this Journal and that of our predecessor can testify through a series of years—still, for the sake of the principles advocated by Dr. Sigmund and Dr. Alison, we willingly accord to the subject any aid and countenance which a prominent notice of the works before us can confer.

In 1841 the Vaccination Act was passed by the British Parliament, which rightly made the practice of *inoculation* unlawful. In 1853 another Act was passed with the view of rendering the practice of *vaccination* compulsory. This later Act is known as Lord Lyttleton's Vaccination Act. During the interval between the first and second reading of the Bill, "the Small-pox and Vaccination Committee of the Epidemiological Society" completed a report on the prevalence and mortality of small-pox in different countries, and on the means taken to guard against its propagation, and to diminish its mortality through vaccination. This Report (understood to be especially the work of Dr. Seaton) was enclosed in a letter to Lord Palmerston, with the view of strengthening the hands of Government, and encouraging Her Majesty's Ministers to pass an efficient measure relative to vaccination. The conclusions arrived at in this Report are deduced from the largest and most accurate mass of statistical evidence which had ever been brought to bear upon the question. We shortly noticed its appearance at the time in our Bibliographical Record,* Government having considered it so important as to cause its being printed as a Parliamentary paper, but we were unable to enter then upon a review of the important subject on which it treats. We now place it first on the list, not only because it is so chronologically, but because we believe it to be a most valuable record, and because the material it contains does not seem to have received that attention which the importance of the subject, the care and the labour brought to bear on its elucidation, deservedly ought to command. The Report, too, appears to have been prepared in so short a time that the information and statistical data it contains have not been made so much of as they deserved, by a more lengthened consideration of the subject; and the valuable little pamphlet of Dr. Seaton may be read as a supplement to the Parliamentary Report, for it demonstrates in a stronger and more forcible manner than had been done in the Report, the modifying and protecting powers of vaccination.

However satisfactory it may have been to have passed Lord Lyttleton's Act, it is now well known that the Act has proved but a very imperfect measure, and one which has fallen far short of accomplishing all that is yet required. The inefficiency and imperfect working of the Act is fully shown in the Reports of the Registrar-General for 1854, by the medical profession generally, by the medical registrars in particular, and by the public, as expressed now and again in the public newspapers. Much might have been done to remedy the existing defects of the Act and facilitate vaccination to the people by the Government measure of last year, which the ill-directed influence of erroneous popular belief caused to be postponed. No one, perhaps, was more active in defeating its progress than Mr. Mitchell, whom the recent appeal to the country has deprived of his seat in Parliament, and who appears to have had a virulent hatred to the memory of Jenner, for he believed that vaccination had introduced "no end" of diseases into the human body! and that the existence of cow-pox had been a curse to mankind!! It is lamentable that the progress of medical science should be obstructed in the aid it might receive from Government by the influence of such representatives of the people. Such displays only cause us to lament that the medical profession has no representative in our legislative assemblies, while the law, the church, and the profession of arms are fully represented; and while many of these representatives are permitted and called upon to fill high offices of state, hardly even a seat in the House of Commons can be secured for a member of the medical profession; and when one does find a place in that honourable House, he takes his seat as the representative of a civic community, not of a learned, enlightened, and humanizing profession. Hence it is that questions which involve an accurate and extensive knowledge of the science of Medicine, do not receive that unbiassed, careful consideration and elaborate discussion which they demand, and that all questions which affect the interests of public health are virtually determined by men who have no voice in Parliament, but whose professional opinions must be obtained and instilled into those who are to settle the most abstruse questions in State

* British and Foreign Medico-Chirurgical Review for 1853.

Medicine, and to legislate accordingly. We again enter our protest against this state of things. We desire the medical profession to be fairly represented in Parliament, and we wish to see some distinguished physician the President of the Board of Health, if that institution is to be continued; at all events let him have a seat in the Privy Council of Her Majesty's Government, and a voice in the legislative assemblies of our country.

To the existing state of things, however, we owe the appearance of the work which stands last in the list, and which, in June last, emanated from the Board of Health.

The medical officer of the General Board of Health (John Simon, Esq., F.R.S.) having been desired by the Right Honourable the President of that Board "to lay before him such medical facts and considerations as may assist him in estimating the hygienic value of vaccination, and the strength of any objections which may have been alleged against its general adoption," we have accordingly before us a large mass of most important material bearing on the history and progress of vaccination, and its influence in arresting the ravages of small-pox during the past half century. Most valuable documents have thus been collected and printed as an Appendix to a letter written by the medical officer, and addressed to the President of the Board, in which the following questions are considered in an interesting narrative style—namely:

"I. What kind of an evil was small-pox before vaccination arose to resist it?

"II. What facts and arguments led to the first sanction of vaccination, and to what sort of inquiry were they subjected?

"III. What further knowledge, at the end of half a century's experience, has been gathered on the protective powers of vaccination.

"IV. What evils have been shown to attend its practice, and to counterbalance its alleged advantages?

"V. How far are there realized, in this country, those benefits which can reasonably be expected from the general use of vaccination?"

Under each of these heads, the honourable President of the Board of Health has presented for his consideration some of the most abstruse questions in pathology; but the state of our knowledge is made as plain to him as it possibly can be by the comprehensive descriptions and forcible illustrations of Simon. It is not to be understood that the topics discussed in this admirable work are at all novel, or that anything has been brought forward on the subject of vaccination which has not been already noticed and discussed in the pages of this Journal or in that of our predecessor, as far as existing information would admit.*

Nevertheless, this work from the Board of Health is especially valuable now, because it brings together a body of evidence, down, as it were, to the present day—evidence of a statistical kind,—the records which have been engraved by the lancet's point, and the names of men and bodies of men as societies, who have done the most for vaccination, are brought prominently forward.

Passing over the history of small-pox before Jenner's great discovery, the great dangers of its attacks in the *natural form*, and the different circumstances under which it has prevailed as epidemic, let us review the statements of Mr. Simon, contained in the last three topics of his inquiry.

"Tested by half a century's trial on millions of civilized Europe, what has vaccination achieved? Comparing the small-pox mortality of the last forty or fifty years with that of as many years in the last century, do we find a sensible difference? Has progress been made towards that final result which Jenner anticipated—the annihilation of the most dreadful scourge of the human race?"

The immense amount of important material, already condensed in tabular forms in the works before us, renders it difficult to give a readable extract of them in a

* See British and Foreign Medical Review, vols. vi. to ix.

review. Accordingly, we shall attempt to illustrate only the more important topics from the combined researches of all.

Let us consider first the nature and extent of the evidence now existing which demonstrates the *protective influence* of vaccination, and the causes which have combined to impair its *protective power*. Of these in their order, and:

I. *Of the Protection conferred by Vaccination—its Nature, and the Evidence of its Existence.*

The main features of the Report of the Epidemiological Society illustrate how small-pox diminishes in its mortality *in proportion as* EFFICIENT *measures are adopted to ensure perfect* vaccination. To demonstrate this, the progress of vaccination in Great Britain and in Germany is first compared as to its influence on mortality generally; and secondly, it is shown by comparing the statistics of vaccination from various German states with the statistics from different districts in Great Britain and Ireland, that where vaccination is most perfectly carried out, small-pox is least mortal. The following is a general record of the statistics which the committee of this society have collected:

"1. To prove the influence of vaccination in England:

Out of every 1000 deaths in the half-century from 1750 to 1800, there were of small-pox	96
Out of every 1000 deaths in the half-century from 1800 to 1850, there were of small-pox	35

"2. To prove the influence of vaccination on the Continent:

In various German States, sufficient evidence can be obtained to show that, before vaccination was used, out of every 1000 deaths, there occurred from small-pox	66·5
After vaccination there occurred	7·26

"3. To prove that in countries where vaccination is most perfectly carried out, small-pox is least mortal:

(*a.*) In this country, where vaccination is voluntary, and frequently neglected, the deaths from all causes being 1000, the deaths from small-pox in the following towns are as follows:

London	16	Glasgow*	36
Birmingham	16·6	Dublin	25·66
Leeds	17·5	Galway*	35
England and Wales	21·9	Limerick*	41
Paisley	18	Connaught*	60
Edinburgh	19·4	All Ireland	49
Perth	25		

(*b.*) In other countries where vaccination is more or less compulsory, the deaths from all causes being 1000, the deaths from small-pox in the following towns are as follows:

Westphalia	6	Bohemia	2
Saxony	8·33	Lombardy	2
Rhenish Provinces	3·75	Venice	2·2
Pomerania	5·25	Sweden	2·7
Lower Austria	6	Bavaria	4

Mr. Simon has adopted another mode of illustration. He gives the following table calculated by Mr. Haile, which contains two series of facts side by side, and shows, 1st, how many persons in each million of the population annually died of small-pox *before* the use of vaccination; and, 2nd, how many persons in each mil-

* We beg to direct special attention to the high rate of small-pox mortality in Glasgow, Galway, Limerick, and Connaught. Before any of the papers appeared which are noticed at the head of this article, Dr. Steele, the present superintendent of Guy's Hospital, London, then of Glasgow, called the attention of the profession to the great increase of small-pox in Glasgow as being chiefly from the Highland and Irish population. See Glasgow Medical Journal, No. 1 for 1853.

lion of the population have annually died of small-pox *since* the use of vaccination.

Terms of years respecting which particulars are given.	Territory.	Approximate average annual death rate by small-pox per million of living population. Before introduction of vaccination.	After introduction of vaccination.
1777—1806 and 1807—1850	Austria, Lower	2,484	340
1777—1806 and 1807—1850	" Upper, and Salzburg	1,421	501
1777—1806 and 1807—1850	Styria	1,052	446
1777—1806 and 1807—1850	Illyria	518	244
1777—1806 and 1838—1850	Trieste	14,046	182
1777—1803 and 1807—1850	Tyrol and Voralberg	911	170
1777—1806 and 1807—1850	Bohemia	2,174	215
1777—1806 and 1807—1850	Moravia	5,402	255
1777—1806 and 1807—1850	Silesia (Austrian)	5,812	198
1777—1806 and 1807—1850	Gallicia	1,194	676
1787—1806 and 1807—1850	Bukowina	3,527	516
1817—1850	Dalmatia	—	86
1817—1850	Lombardy	—	87
1817—1850	Venice	—	70
1831—1850	Military Frontier	—	288
1776—1780 and 1810—1850	Prussia (Eastern Provinces)	3,321	556
1780 and 1810—1850	Prussia (Western Provinces)	2,272	356
1780 and 1816—1850	Posen	1,911	743
1776—1780 and 1810—1850	Brandenburgh	2,181	181
1776—1780 and 1816—1850	Westphalia	2,643	114
1776—1780 and 1816—1850	Rhenish Provinces	908	90
1781—1805 and 1810—1850	Berlin	3,422	876
1776—1780 and 1816—1850	Saxony (Prussia)	719	170
1780 and 1810—1850	Pomerania	1,774	130
1810—1850	Silesia (Prussian)	—	310
1774—1801 and 1810—1850	Sweden	2,050	158
1751—1800 and 1801—1850	Copenhagen	3,128	286

Confirmatory evidence is also adduced from the records of Dr. Balfour relative to the army and navy, reprinted from the 'Transactions of the Medical and Chirurgical Society of London,' and the combined weight of testimony from all sources of evidence is to show, *that in the proportion as vaccination is general and* EFFICIENT, *so is the exclusion of small-pox from the community, and mortality greatly lessened.*

There does not appear to be sufficient evidence to illustrate, as Dr. Seaton observes, the comparative protective power of *vaccination*, as compared with the protection from *inoculation*, or a previous attack of *variola*, in adults under ordinary exposure. Dr. Balfour's statistics appear to give an approximation, but it is very correctly remarked by Dr. Copland, with reference to the statements of Dr. Balfour, that the results subsequently are not known, and cannot well be ascertained; and it is important to observe that the liability to small-pox, modified or otherwise, becomes greatest *after* the ages at which Dr. Balfour's statistics are framed. Supplementary to Dr. Balfour's interesting observations, the records of experience during the epidemic at Marseilles in 1828 may be also referred to as quoted in Mr. Simon's 'Letter.'

From all these observations, it is probable that *variola* or *inoculation* affords a greater certainty of immunity from small-pox than *vaccination* does; but the evidence in the records before us appears, on the other hand, to show that just in proportion as vaccination is *badly* performed so will the proportion of *post-vaccine* cases of small-pox be greater compared with those after *variola* or *inoculation;* and, as already appears, there are abundant proofs of the enormous amount of protection afforded by vaccinia. We have other evidence given by Mr. Bedford in the 'Indian Annals of Medicine,' relative to Bengal, where it appears that no less than 82·12 per cent. are protected by inoculation, and 5·6 per cent. by vaccination, yet the records are not such as would enable us to estimate the comparative protective power of variola and vaccination.

One obviously beneficial result of vaccination as regards its protective influence to the multitude has not, we think, been appreciated or illustrated with sufficient force—namely, "that the *epidemic influence*" of small-pox greatly *increased* during the practice of *inoculation*, and greatly *decreased* since *vaccination* has been adopted. Dr. Hebra, Professor on Diseases of the Skin at Vienna, incidentally remarks, and simply alludes to the fact, "that epidemics of small-pox have been more rare, and are less malignant, since the introduction of vaccination." But definite material for the following statements are to be found in the 'Report of the Epidemiological Society,' in illustration of our remark.

(1.) During ninety-one years previous to *inoculation* there had been *sixty-five* distinct and well-marked epidemics, which is a ratio of 71·4 epidemics in 100 years.

(2.) During sixty-three years in which *inoculation* was practised, and that to a great extent, there were *fifty-three* distinct and well-marked epidemics, which is a ratio of 84 epidemics in 100 years.

(3.) During the last fifty years since *vaccination* has been practised and *inoculation* been declared illegal, there have been twelve epidemics of small-pox, which is a ratio of 24 epidemics in 100 years.

This kind of testimony is also greatly enhanced by the fact that epidemics never occur in the army or navy of our own country. The experience of other countries is equally satisfactory. The Danish army and navy almost entirely escaped contagion during several epidemics of small-pox. The two epidemics in Malta during 1830 and 1838, to which our soldiers were exposed, also afford an illustration of the protective power of vaccination when tested by epidemic influence.

Another topic to which we claim the attention of our readers, concerns one of the most important ways in which the *protective* power of vaccination exhibits itself. "It is in *the mildness of the disease in the vaccinated as compared with the unvaccinated, and the almost absolute security against death from small-pox which perfect vaccination confers*." With few exceptions, this appears to be the universal opinion of the medical profession. At various times the opinions of large masses of our medical brethren have been specially obtained on this point. Three distinct and comprehensive "*polls*" may be referred to—namely, (1) that by the College of Physicians of London, eight years after vaccination had been adopted; (2) an almost national "poll" taken by the Epidemiological Society of London, about the years 1852 and 1853; (3) a "poll" of which a list is published in the Appendix to Mr. Simon's 'Letter,' and it comprehends not only members of the profession generally, but also the medical department of the public service (army and navy) together with the opinions of foreign Governments. As Dr. Alison remarks:

> "The question whether successful vaccination gives security to a great majority of mankind against any attack of small-pox in future life, and to a much greater majority against fatal small-pox, has been generally regarded in this country for the last half-century as practically decided in the affirmative; and since the date of the papers which were held decisive of the question fifty years ago, there has been quite sufficient evidence collected to show that the same inference is still inevitable, and that he who disputes it is equally unreasonable as he who opposes in like manner any proposition in Euclid."*

With two eccentric exceptions, the "poll" taken by Mr. Simon is unanimous. How extraordinary, then, must be that obliquity of judgment which advises the giving up of *vaccination* to recur to *inoculation!* And when we find so eminent a writer, and a physician so deservedly respected as Dr. Copland giving expression to such sentiments, we feel it our duty to enter a strong protest against them, and

* Notes, by Dr. Alison, Appendix, p. 119, of Papers collected by the Board of Health.

prominently to put before our readers the interesting facts contained in the volumes which have emanated from the Epidemiological Society and the General Board of Health respectively. We see no notice in Dr. Copland's bibliographical references to the important documents prepared by Dr. Seaton and printed by Government; and we regret to notice his advocacy of doctrines which are at variance with the written opinions of two thousand of his medical brethren in this kingdom, not to mention those in Bengal, Bombay, Mauritius, the West Indies, and various other places, all which passed through the hands of the Secretary of the Epidemiological Society. An inexplicable *animus* obscures some parts of his writings on this topic, which betrays itself in expressions such as the following:

"After a quarter of a century of most transcendental laudation of the measure, with merely occasional whisperings of doubt, and after another quarter of a century of reverberated encomiums from well-paid vaccination boards, raised with a view of overbearing the murmurings of disbelief among those who observe and think for themselves, the profession is still doubtful as to the benefits to be derived from inoculation or vaccination."

Under this same criticism must come the extraordinary "opinion" expressed by Dr. Hamernjk, of Prague, in the Appendix to the papers "On Vaccination," emanating from the General Board of Health, and especially the following paragraph:

"Confidence in vaccination has been much shaken in England by the epidemics of 1825, 1838, 1840 and 1841, and I cannot understand how Dr. Brown could propose the use of inoculation after having so warmly and correctly spoken respecting the giving up of vaccination; just as if Nature could be sported with in this (to say the least of it) harmless manner! Vaccination proved likewise useless in the epidemics of Paris (1825) and Marseilles (1828); and it is hardly to be doubted that vaccinatlon would long ago have been abolished if people could, in other countries, as is the case in England, freely express their opinions; and if it were not the interest of appointed vaccinators and other officials to keep the practice of vaccination *in statu quo.*"

These and such-like sentiments, we repeat, are inexplicable and illiberal.

Not a few of those who have been requested to express their opinions take exception to the phrase—"*almost absolute security against death by that disease.*" But since such is the expression of a fact, to whom now can the expression be objectionable, unless to a Turk or a fatalist? It is worthy of remark that there is perhaps no topic in the wide range of medical science on which the opinions of the profession as a body have been more definitely expressed or more extensively obtained, and on which there is so much unanimity on the whole, as regards the influence of vaccination.

The following illustrations, selected by Mr. Simon from various sources, show *the extent of the security against death* enjoyed by vaccinated compared with unvaccinated persons. The extent of this security cannot be expressed otherwise than by the terms, "almost absolute."

Places and times of observation.	Total number of cases observed.	Death-rate per 100 cases. Among the unprotected.	Death-rate per 100 cases. Among the vaccinated.
France, 1816—41	16,397	$16\frac{1}{8}$	1
Quebec, 1819—20	?	27	$1\frac{2}{3}$
Philadelphia, 1825	240	60	0
Canton Vaud, 1825—29	5,838	24	$2\frac{1}{6}$
Darkehmen (*Dürkheim?*), 1828—29	134	$18\frac{4}{5}$	0
Verona, 1828—39	909	$46\frac{2}{3}$	$5\frac{2}{3}$
Milan, 1830—51	10,240	$38\frac{1}{3}$	$7\frac{2}{3}$
Breslau, 1831—33	220	$53\frac{4}{5}$	$2\frac{1}{9}$
Wirtemberg, $1831\frac{1}{3}$—$5\frac{1}{2}$	1,442	$27\frac{1}{3}$	$7\frac{1}{10}$
Carniola, 1834—35	442	$16\frac{1}{4}$	$4\frac{2}{5}$
Vienna Hospital, 1834	360	$51\frac{1}{4}$	$12\frac{1}{2}$
Carinthia, 1834—35	1,626	$14\frac{1}{2}$	$\frac{1}{2}$

Places and times of observation.	Totalr numbe of cases observed.	Death-rate per 100 cases. Among the unprotected.	Among the vaccinated.
Adriatic, 1835	1,002	$15\frac{1}{5}$	$2\frac{1}{5}$
Lower Austria, 1835	2,287	$25\frac{4}{5}$	$11\frac{1}{2}$
Bohemia, 1835—55	15,640	$29\frac{4}{5}$	$5\frac{1}{6}$
Gallicia, 1836	1,059	$23\frac{1}{2}$	$5\frac{1}{7}$
Dalmatia, 1836	723	$19\frac{3}{4}$	$8\frac{1}{4}$
London Small-pox Hospital, 1836—56	9,000	35	7
Vienna Hospital, 1837—56	6,213	30	5
Kiel, 1852—53	218	32	6
Wirtemberg, no date	6,258	$38\frac{9}{10}$	$3\frac{1}{2}$
Malta, no date	7,570	21.07	4.2
Epidemiological Society Returns, no date	4,624	19.7	2.9

It thus appears that the death-rate amongst the vaccinated varies from an inappreciably small mortality to $12\frac{1}{2}$ per cent.; that amongst the unprotected it ranges from $14\frac{1}{2}$ to 53[4] per cent.

This great range appears to be due not alone to the severity of the epidemic influence, but to the badness and insufficiency of vaccination.

The per centage mortality stated by Mr. Marson to occur amongst the vaccinated over all is $5\frac{1}{4}$ per cent.; but when vaccination is known to have been perfectly performed, as shown by the cicatrices, the mortality is uniformly found to be reduced to less than *half of one per cent.*

Another interesting phase in which vaccination may be viewed as exercising a protecting influence *indirectly* over the health of the community, may be studied in those statistics which show that the *general death-rates* from other diseases have diminished, and more especially as regards *scrofulous* diseases, since vaccination became more universal. To the labours of Dr. Greenhow and Dr. Farr in this country, and to the statists of Sweden, we are indebted for any accurate knowledge existing on this subject. Not only has the grand total of the death-rates been diminished, but there are two special classes of diseases whose death-rates have diminished in a remarkable degree. These diseases are (1), those of the tubercular order of the constitutional diseases—namely, scrofulous affections, including phthisis, or pulmonary tuberculosis; and (2) the continued fevers, and especially the typhoid form of typhus affections—diseases belonging to the miasmatic order of the zymotic class.

An abstract of Dr. Greenhow's elaborate statistics, printed in the Appendix to Mr. Simon's 'Letter,' shows that with the decline of small-pox, which has taken place through vaccination, the general death-rate of the population has decreased, even although epidemic influenza and two visitations of cholera are included in the calculations. The general death-rate per 10,000 of living population from 1846–55 was 25 per cent. less than in the decennial period 1746–55; and 40 per cent. less than the decennial period of 1681–90! having successively declined since the remoter period from 421 to 355, and from 355 to 249.

The following table by Dr. Farr regarding London, and that by the Swedish statists regarding that well-vaccinated country, lead to a similar result:

Average annual death-rates in London from all causes and all ages (Dr. Farr). Date.	Per 10,000 living.	Average annual death-rates in Sweden, from all causes and at all ages. Date.	Per 10,000 living.
1629—35	500		
1660—79	800		
1728—57	520	1755—75	289
1771—80	500	1776—95	268
1801—10	292		
1831—35	320	1821—40	233
1840—54	$248\frac{9}{10}$	1841—50	205

N.B. The *annual* small-pox death-rate in Sweden during the period 1841—50 averaged less than the *weekly* death-rate from small-pox and measles during the period 1755—75.

The statistics of Copenhagen also concur with those of London to show progressive improvement in the health of the population since vaccination became universal.

We do not entirely subscribe to the way in which Mr. Simon sums up the interpretation of these details in the following words:

"That while under the influence of vaccination small-pox has been diminishing its ravages, so, under other influences, have other diseases been diminishing theirs."

This is true; but it is only part of the truth. We *do* believe that whatever prevents small-pox, prevents the development of a specific miasm, and thus indirectly thwarts those conditions which are favourable for the development of fevers, of cholera, and the like, independent of any sanitary measures which may have been instituted by the Board of Health against special causes of disease.

It appears to us that Mr. Simon has given too much space to discuss what ought now to remain a literary fossil—namely, what is known theoretically as a "displacement of mortality"—a doctrine advanced by Mr. Carnot and Dr. Watt, of Glasgow. These writers believed that within the present century deaths which used to occur in early infancy have come to occur between the ages of fifteen and twenty, and so on; and that the mortality which has been removed from small-pox has fallen upon measles, scarlatina, and the like. The accurate death-rates of different ages in Sweden, as shown in the annexed abstract of the Swedish returns, prepared by Dr. Farr, is quite sufficient to controvert the statement of the French artillery officer, without bestowing six or eight pages of letterpress upon the question.

Annual Mortality in Sweden to 1000 *persons living.*

Ages.	21 years (1755—75).	20 years (1776—95).	20 years (1821—40).	10 years (1841—50).
0—5	90·1	85·0	**64·3**	**56·9**
5—10	14·2	13·6	**7·6**	**7·8**
10—15	6·6	6·2	**4·7**	**4·4**
15—20	7·6	7·0	**4·9**	**4·8**
20—30	9 2	8·9	**7·8**	**6·8**
30—40	12·2	11·6	11·8	**9·8**
40—50	17·4	16·1	16·7	14·5
50—60	26·4	23·9	26·0	23·6
60—70	48·1	49·3	49·4	46·3
70—80	102·3	104·1	112·9	102·8
80—90	207·8	197·4	243·7	228·5
90 and upwards	394·1	351·3	396·4	375·8
All ages	28·9	26·8	23·3	20·5

The figures which are put in larger type relate to that section of the population which has been born since the introduction of vaccination, and of which (persons under thirty in the fourth, and under forty in the fifth column) the greatest part is undoubtedly vaccinated. Of persons ten years older, especially in the last column, many are vaccinated; of persons still older a diminished proportion.

With reference to the alleged increased prevalence of certain specific diseases since the introduction of vaccination (and which was mooted by Dr. Watt, of Glasgow), we have now most satisfactory evidence to show that such a representation is inconsistent with facts. It is shown by Dr. Farr, in the second volume of M'Culloch's 'Descriptive and Statistical Account of the British Empire,'

"That fever has progressively subsided since 1771, and that the combined mortality of small-pox, measles, and scarlatina, is now only half as great as the mortality formerly occasioned by small-pox alone."

According to the researches of Dr. Greenhow, we find that during the middle of the last century, before vaccination was known, the scrofulous death-rate was more than five times as great as our present one; and that the pulmonary death-rate of the present time is seven per cent lower than the pulmonary death-rate of 1746–55.

But there occurs a check, as Simon laconically indicates on the margin of one of his pages. We should rather say there are a series of checks, a combination of conditions, which tend to diminish the *protective* power of vaccination. We cannot over-estimate the importance of a careful consideration of these conditions, and which are brought out and illustrated more or less forcibly by our authors. They are of extreme interest in a pathological point of view, and questions of administrative policy, which shall influence for good or evil the health, strength, and wealth of the nation, are wrapt up in their study.

We beg to lay before our readers the following remarks, after a careful perusal of the combined evidence, bearing on the question :—

II. *How the protective influence of vaccination has been impaired.*

(*a*) An interesting fact, of which we have now and then caught a glimpse, is clearly demonstrated by evidence in the works before us. It is, "*that the protective power of vaccination becomes gradually weaker, and at length apparently dies out in the individual.*" Dr. Seaton, however, does not subscribe to this belief.

In 1809, Mr. Brown, of Musselburgh, in Scotland, near Edinburgh, published the opinion that the prophylactic virtue of vaccination diminished as the time from vaccination increased. In 1818 and 1819 small-pox pervaded Scotland as an epidemic, and many vaccinated persons passed through a mild form of variola. The terms "modified small-pox" and "varioloid" now came into use, and two classic monographs on the subject made their appearance—one by Dr. Monro, in 1818, and one by Dr. John Thomson, of Edinburgh, in 1820. Dr. Copland also writes:

"I have seen and described, as early as 1823,* small-pox as it affected the members of the same family at different periods after vaccination, and in younger persons, or those who had been vaccinated only ten or eleven years; . . . . . in contrasting such cases the severity and fully-developed state of the disease was generally in proportion to the length of time which had elapsed from vaccination. . . . . . . I have, moreover, seen cases after undoubted vaccination—having been effected from thirty to forty years previously—that presented the most malignant states of the confluent disease, the pustules maturing imperfectly or slowly, or being filled with a black ichorous matter."†

From the evidence contained in the bills of mortality in 1825—the experience of epidemics in France and Italy in 1826, 1827, and 1829, and of Ceylon in 1833 and 1834, and from the admissions into the London Small-Pox Hospital in 1838, it has been long apparent that the susceptibility, which in vaccinated persons is destroyed for some years, returns with advancing age, and becomes greater as life advances within certain limits.

Some of the phenomena which the practice of vaccination has made known to us also tend to establish the doctrine of a gradual impairment of vaccine protection, due to *lapse of time*, and as a result of physiological changes in the healthy body. We refer to the fact that, in proportion (undetermined) to the distance of time that has elapsed from the first implanting of the vaccine virus, so is the better development of the secondary vaccine vesicle produced by a re-vaccination.

"Generally," it may be stated, as Simon writes, "that persons who had been vaccinated ten, fifteen, or twenty years, and who during the interval had perhaps repeatedly resisted small-pox, would at length, in a certain proportion of their number, yield to the infection. This had most frequently happened during times when small-pox was severely epidemic among the unvaccinated; and the first notice of the fact merely meant that then, for the first time, *large masses of persons* with *vaccination* of many years' standing were exposed to the

* See London Medical Repository, vol. xxi. † Dictionary of Practical Medicine, Art. Small-pox, p. 815.

test of a strong epidemic influence, and that under this ordeal it became evident that for some vaccinated persons the *insusceptibility conferred by cow-pox was not of life-long duration.*"

From careful analysis of cases, however, it was shown that this lesser protectedness of certain vaccinated persons bears at least *some* proportion to the number of years which in each case had elapsed since vaccination. There were not, however, materials to *prove* any uniform rate of increased susceptibility to small-pox *from year to year from the period of vaccination;* but the increasing susceptibility, such as it was, apparently continued up to about thirty years of age, after which period it seemed that the liability to contract small-pox underwent a continuous decline. Professor Heim, of Stuttgart, Mohl, of Copenhagen, Professor Retzius of Stockholm, and Mr. Marson, of London, have severally elucidated this interesting pathological phenomenon.

Mr. Marson's statements may be quoted as the common result of all:

"But few patients," he writes, "under ten years of age have been received with small-pox after vaccination. After ten years the numbers begin to increase considerably, and the largest admitted are for the decennial period from the age of fifteen to twenty-five; and, although progressively diminishing, they continue rather large up to thirty; and from thirty to thirty-five they are nearly the same as from ten to fifteen; but as in the unprotected, at this period of life, the mortality is doubled, showing the cause to be, probably, as much or more depending on age and its concomitants as on other circumstances. In still further advanced life, the rate of mortality will be seen to increase also, as in the unprotected state; but this tendency may be in a considerable degree counteracted, there is but little doubt, by giving more attention than has hitherto generally been given to the perfection of the process of vaccination."

Dr. Balfour, of the Army Medical Department, has also adverted to this important circumstance, but his data are not sufficient to found upon, nevertheless the fact appears to be as stated in the sentence with which we have introduced this paragraph; and there can be no doubt, from the evidence before us, that re-vaccination is a very necessary supplementary measure to vaccination; but it must also be remembered, as Mr. Marston again observes, that—

"Probably it does not afford the same amount of protection that the first vaccination, well performed, does. The great object to aim at is to vaccinate *well* in infancy; this should be looked upon as the sheet anchor; and therefore a careless vaccination should be deprecated at all times, practised under the belief that if it fails to take effect properly it will be of no consequence, as the operation can be repeated. By such proceeding the vaccination often takes effect *badly*, and will never afterwards take effect *properly*, and yet the individual may take small-pox *severely*."

In connection with the topic of *re-vaccination*, a very important question suggests itself. Is the lymph resulting from re-vaccinations proper to use for the purposes of vaccinating or re-vaccinating others? In reply to a question propounded by the Board of Health, to which we shall immediately refer, we find the following important paragraph:

"I have known matter taken from a re-vaccinated subject (having formerly had the regular cowpock) cause an irregular and unpleasant troublesome form of cutaneous eruption. Nor would I, under any circumstances, use such matter; as my experience tells me that (*as a rule*) a person having once had regular cowpock is not susceptible of the genuine disease again. But although you use good lymph you get a spurious pustule rising in three or four days, instead of eight or ten, putting on a different appearance, itching much, often causing inflammation of the absorbent glands and vessels, and much constitutional disturbance; in fact, acting as a morbid poison; consequently, I object to re-vaccination."

The question as to the value of re-vaccination matter (that is, of matter which has been taken from genuine cow-pox vesicles developed in a previously-vaccinated individual) for the purposes of further inoculation, is not now started for the first time. Nearly twenty years ago, objections were urged against the employment of matter of this description, and amongst others, the doubt as to its genuineness, and the danger of conveying other poisons into the constitution with vaccine virus taken from adults; for instance, syphilis, gonorrhœa, itch, and the like—attracted special attention, and became the subject of experimental investigation.* In the volume of papers before us from the Board of Health, Mr. Simon has widened this inquiry, and has called for the opinion of the profession at home and abroad on the general question.

"Have you any reason to believe or suspect, (*a*) that lymph from a true Jennerian vesicle has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection to the vaccinated person; (*b*) or that unintentional inoculation with some other disease, instead of the proposed vaccination, has occurred in the hands of a duly educated medical practitioner?" (Ques. III. App.)

To this question, let us quote the opinions of Hebra so logically stated, lucid, and conclusive.

"This widely-grasping question (says he) requires several separate answers, because queries are made,—

"1. *Whether the lymph of a vaccine vesicle may, besides its peculiar virus, contain another infectious principle—e. g., that of syphilis?*

"2. *Whether constitutional non-infecting diseases, as, for instance, scrofula, may be transmitted by the inoculation of cow-pox matter?*

"3. *Whether a vaccine vesicle possesses such characters that it may easily be distinguished from other similar vesicles, blebs, or pustules?*

"(I.) The transmissible infectious principles which have hitherto been recognised, by means of inoculation, are, the syphilitic virus contained in the pus of a chancre; and the virus contained in the cow-pox vesicle, and the small-pox pustule. The question therefore simply is, whether these morbid poisons have ever been mixed? Whether inoculation have ever taken place with such a mixture? And what results were obtained by such an operation? It is well known, that compendious answers have, for some time past, been offered to these questions, chiefly the result of Sigmund's experiments. These answers agree in the following respects:

"Inoculation with secretions of this kind—viz., containing, as it were, several special poisons, either produced no effect at all, or only generated a chancre, by inoculating a mixture of pus from chancre and vaccine lymph; and only cow-pox, by inoculating a combination of vaccine lymph and blenorrhagic matter. Hence one morbid state only was produced, either cow-pox or syphilis; the latter circumstance being a proof that both poisons are not *simultaneously* transmissible. This opinion is supported by the experience of Heim, Ricord, Bousquet, Taupin, Landouzy, Friedinger, &c.

"(II.) It is maintained in many quarters that the blood of persons suffering from secondary syphilis may serve as a vehicle to the infectious principle; but were even this theory found correct, it would have no prejudicial effect on the practice of vaccination; because we know, from experiments made for the purpose (Heim), and from accidental inoculation, that, regardless of the quality of vaccine lymph, the latter may be inoculated from syphilitic upon sound individuals; and, on the other hand, from sound subjects upon such as are under the influence of systemic syphilis, without propagating syphilis along with the cow-pox.

"What has here been proved of syphilis must, *à fortiori*, hold good as regards other constitutional morbid states, as direct inoculations with the secretions peculiar to these diseases have always yielded a negative result.

"But although it is abundantly proved that scrofula, tubercular affections, rickets, cancer, and other blood diseases cannot be transmitted by means of their own secretions, or along with vaccine lymph, we should nevertheless, if possible, avoid vaccinating diseased persons, because experience has taught us, as regards adults and children, that the phenomena of vaccination may awake—i. e., render worse—dormant affections, and that, moreover, the cow-pox vesicle easily degenerates upon such individuals. These latter vesicles are nevertheless adapted for further propagation, even when they take an imperfect development, because a positive result, a regular development of the vesicle, and sufficient protection against small-pox have been observed in cases where vaccine lymph was transferred from weakly, scrofulous, and rickety subjects upon perfectly sound individuals.

* Dr. Mead writes, "It is in my opinion more material into what kind of body the venom be infused than out of what it is taken." Mosely also is virtually of the same opinion.

"(III.) Every morbid appearance on the cutaneous envelope has its own peculiar characters, by which it may be distinguished from other similar phenomena; the vaccine vesicle presents, in the like manner, sufficiently striking peculiarities as to form, size, number, locality, and particularly as regards its course, to enable the observer easily to establish a distinction between the same and other vesicular, bullar, or pustular eruptions."

With a few exceptions, the replies from the medical profession relative to Question III. are wanting in exactness, and Mr. Simon does not inform us how he selected the men to whom the questions were put,—in other words, to how many members of the profession were the questions sent?—to whom were they sent? And as they do not seem to have been sent to every one mentioned in our medical directories, upon what ground was the selection made? Upon some, the ideas conveyed by the question seem to have dawned for the first time: and while there is abundance of evidence, in the replies of others, direct, conclusive, scientific, and positive, which shows that the general creed entertained in this country with regard to the points at issue in the question before us confirms the doctrines of the continental experimentalists, the replies from not a few are characterized by "doubts," "general beliefs," and "vague suspicions," *e. g.*—

"(*a.*) I have seen (says one) several instances of the transference of the syphilitic taint through the medium of vaccination, the lymph having been taken from a true Jennerian vesicle, or presumed to be so, at least, in a tainted infant; and I have known eczematous eruptions, apparently of a simple nature, in this way reproduced: but not scrofula. (*b.*) With respect to the latter part of this question, I can only answer as regards syphilis. I believe that the inoculation of matter taken from a syphilitic sore, in any of its stages, is capable of producing its characteristic phenomena in the inoculated."*

(*b*) It has been alleged, but sufficient proof has not been adduced, *that the vaccine virus becomes deteriorated by its passage through numerous human bodies*, or in other words, its protective influence is weakened by *length of time*, and the long succession of subjects through whom it has been transmitted, since its direct inoculation from the cow. Considerable differences of theoretical opinion prevail upon this point. The National Vaccine Establishment, in 1854, state in their Report, "that the vaccine lymph does not lose any of its prophylactic power, by a continued transit through successive subjects." Such an unqualified belief is not, however, by any means universal, as shown in various parts of Mr. Simon's work.

"From so long as forty years back, definite allegations have been made, purporting to prove that the power of vaccine lymph, as derived from successive contagions of the human subject, had progressively diminished; that for protective purposes it was now necessary to produce, instead of Jenner's two vesicles, eight or ten points of infection (Brisset); that the older scars of vaccination were much better marked than recent ones; that the proportion of unsuccessful to successful vaccinations was every year growing larger; that a regenerated supply of lymph acted with effect, producing cicatrices after the old normal type. (Meyer.)

"Successive comparative experiments by M. Bousquet, Dr. Gregory, Mr. Estlin, Professor Herring, M. Fiard, and Dr. Steinbrenner, have since established that certain original properties of the vaccine contagion have very generally declined, after its long successive descent from the cow.

"The lymph, when taken from the cow, seems to show an amount of infective power which is not usual in lymph of long descent. The former *takes*, as the phrase is, in persons with whom the latter has failed,—often, for instance, in *re-vaccination*. It excites local changes of an intenser kind, so active, indeed, as to render caution necessary in its use. The vesicle produced by it runs a full course, compared with which, the progress of common vaccine vesicles seems unduly rapid and their termination premature. Also it renders more certain, and apparently more characteristic, *that slight febrile disturbance which is proper to the action of cow-pox on the human system.*"

We print these words in italics, although they are not so at page 39 of Simon's

* "On receipt of these answers (Nov. 5th, 1856), I immediately," says Mr. Simon, "wrote to Dr. W., inviting further information on the statement made under No. 3. Hitherto none has been received. J. S. May 6th, 1857."

'Letter,' because we firmly believe that this febrile disturbance is an essential pathological phenomenon, demanded as well for the due protection of the vaccinated as for the perfect development of laudable and efficient lymph, at the spot where the lymph of vaccination was originally implanted. We believe that the more distinct the *specific febrile* action which follows the implanting of the vaccine lymph, the more certainly is the individual protected, and the more efficient is the lymph which is developed by the zymosis. We say *specific febrile action*, because it is well known that the development of any other idiopathic febrile state, such as from cold or other disease, destroys the progress of the vaccine vesicle altogether.

On referring also to the records of re-vaccination in the Prussian army, an extremely interesting fact is brought out by Mr. Simon confirmatory of the belief that by transmission the lymph has degenerated,—namely, "*that the vaccinations of* 1836, *as tested by eventual re-susceptibility to cow-pox, were not half so stable as the vaccinations of* 1813."

Such being the various statements on this important point, the question is put,—Is it then the case that an extensive use of degenerated or imperfectly developed lymph has determined that too frequent impermanence of protection against post-vaccinal small-pox?

"It is chiefly from national statistics (writes Simon) that the answer must be sought; and the critical question to be asked in any country where the vaccine supply has seldom or never been renewed from the cow, is this:—Assuming that, from 1800 to 1840, every year's vaccination has included *a certain proportion* of infants who eventually (15–25 years afterwards) have become re-susceptible of small-pox—*has this proportion from year to year, progressively increased?*

"In respect of small-pox itself, there are no facts, I believe, nearly sufficient in amount for even an approximative answer to this question; but in respect of a closely kindred issue there are some materials of a very suggestive sort. For, if it should appear that the *proportionate re-susceptibility of vaccination at a given age* were undergoing a uniform progressive increase, this, like a uniform progessive increase of post-vaccinal small-pox, would make it almost certain that primary vaccination had progressively become less effective. And it is difficult to conceive how the infantine generations of a country could, crop by crop, successively derive less permanent constitutional impressions from vaccination, unless the efficient cause of those impressions—the vaccine contagion itself—had year by year undergone enfeeblement of its powers."

We are extremely unwilling to believe that the enfeeblement of the contagion of the vaccine lymph has resulted, slowly but necessarily, from the mere fact of its many successive transmissions.

"A recent writer remarks (says Dr. Copland) that so far from believing in any deterioration of virus from successive vaccinations, there is reason to believe that by a careful selection of well-predisposed children, the pock may even be *restored from an imperfect to a perfect state*, and by proper care, therefore, may be retained indefinitely in that condition.* If children are successively vaccinated from each other, all of whom are from various causes ill-disposed to take on the perfect disease, the virus may unquestionably degenerate, and at length wear out altogether."

Seeing also that it is still a question undecided respecting the number of vesicles, and the degree of constitutional disturbance of a specific kind, which must attend the development of the contents of their little cells, so as to ensure and prolong the protective power of vaccination, we are rather inclined to ascribe any diminution of protective influence as due to personal carelessness in the selection of lymph, and the choice of cases, where lymph, for example, has been taken from a local vesicle developed in the absence of essential constitutional effects, and where not only the lymphy contents were impotent, but the anatomical development

* The statements made here may be considered as in some measure confirmed by an observation made by Heim about twenty years ago. He considers that vaccine matter, originally weak or imperfect, undergoes *progressive development* to perfect potency in persons of variolous susceptibility. A most interesting example of *Zymosis*.—Rev.

of the vesicle in respect of its little cells was at the same time incomplete or imperfect.

The following opinions contained in reply to specific questions put to medical men by the General Board of Health, shows we are not singular in our belief. Mr. Henry Ancell, of London, writes as one cause of imperfect vaccination:

"Carelessness in the operation by the use of foul or blunt instruments, producing an irritative wound, and modifying the specific influence by phlegmonous or erysipelatous inflammation. The vaccine vesicle runs a modified course; the lymph has a tendency to become quickly sero-purulent; the size, shape, and tint of the areola present shades of difference obvious to a practised eye, but I believe often overlooked by the careless operator.

"In this case, speaking theoretically, I should say the local disease runs a course which satisfies the operator; but the necessary molecular change in the blood, which constitutes the prophylaxis, is prevented."

Now, with reference to these two conditions under which the *protective power* of the vaccine virus may reasonably be believed to be impaired—namely (1), under the influence of "lapse of time in the individual" after primary vaccination; (2), under the effect of "continued transmission through a variety of individuals," it may be observed, that while the influence of the first appears to be fully proven, the influence of the second, as commonly understood, is by no means so; but, on the contrary, the impairment so explained may justly be in most cases referred to other circumstances to which we have alluded, and especially to one which we have yet to notice. Nevertheless, whichever view of the question is taken, it is, we think, a fair subject of inquiry, whether or not in the case of re-vaccinations, at least, the vaccine lymph should not be taken directly from the cow; or, whether it should continue to be taken from infants or adults?

If it is thought necessary to renew the virus directly from the cow, who and what proportion amongst us will be considered qualified to undertake this responsible duty, knowing that medical education at our schools of medicine is entirely *nil* in regard to this most important study? The only exceptions are those who have served apprenticeships to surgeons or general practitioners, for as yet our hospitals afford no instruction, except perhaps in provincial towns. The difficulties which surround this view of the subject to the uninitiated cannot by any means be dispelled by the knowledge that *four* sources of the *vaccine disease* in the cow are mentioned by Professor Hering, of Stuttgart—namely, (1) the *grease*, (2) *small-pox*, (3) *retro-vaccination*, (4) *spontaneous development of the vaccine disease.* We cannot now stop to inquire into this subject, so fully illustrated by the labours of Mr. Ceeley, of Aylesbury, and of Dr. Basil Thiele, of Kasan, in South Russia, nor can we inquire into the various values and properties of the lymph resulting from original sources; nor of the symptoms and progress of the disease of the cow, their irregularities, and the number of spurious and anomalous forms of pock which have been noticed in that animal. When we shortly enumerate these circumstances, to some we may convey a glimpse of difficulties attending the subject of vaccination, perhaps before unthought of.

(*c*) Another condition under which the *protective powers* of vaccination has been impaired, and that to an almost incalculable extent, an extent which is not yet duly appreciated by the profession, calls for most prominent notice; and the illustration of this fact is peculiarly due to the labours of Mr. Marson, who having already vaccinated upwards of 40,000 persons, the importance of his experience and evidence cannot be over-valued.

For more than twenty years he has been Resident-Surgeon of the London Small-pox Hospital, and founding his observations on many thousands of cases of which he has kept accurate notes, he has established that among vaccinated persons infected with small-pox, the danger of the disease is chiefly determined by the badness and insufficiency of their vaccination. He considers that the most trustworthy evidence we can have of the perfection of vaccination, after a long series of years subsequent to its performance, is from the appearance of the cicatrices. A good vaccine cicatrix he describes as "distinct, foveated, dotted, or indented, in some instances radiated, and having a well, or tolerably well, defined edge." An indifferent cicatrix is "indistinct, smooth, without indention, and with an irregular and ill-defined edge." The little pits, dots, and radiated lines of a good cicatrix, no doubt correspond to the sites of the minute cells which in the aggregate composed the vesicle. Now, it appears to us very clear from these observations, and from others, that all local vesicles produced by implanting vaccine lymph are not *perfect vaccine vesicles.* A vesicle having all the outward appearance of a genuine vaccine vesicle, may have been developed without any constitutional specific febrile action; its internal anatomical structure may be imperfect, its contents merely lymphy and impotent to protect from small-pox if implanted in another, and its existence on the individual under observation, no evidence that he himself is protected. When it is stated that nearly a third of all who are re-vaccinated—

"Give again exactly such local phenomena as arise in children when vaccinated for the first time; and it is said also by Dr. Thiele, referring to the re-vaccination of Russian soldiers, that perfect (we presume apparently locally perfect) vaccine vesicles would arise just as often on persons who once had small-pox, as on persons who had once been vaccinated;"

it appears to us that two kinds of susceptibility exist, both as regards variola and vaccinia. There is a local susceptibility, and there is a constitutional susceptibility. A very large proportion of mankind, perhaps nearly all, evince the local susceptibility by an apparently perfect vesicle either from cow-pox or small-pox virus; and persons who have once had small-pox, who have been inoculated or perfectly vaccinated, will not readily exhibit signs of any renewed constitutional susceptibility, although the local vesicle may appear to be perfect.

The very important question suggests itself—Can we always tell a perfect vaccine vesicle?—Can we always say of a given mature vesicle, that it is perfect in its internal organization; that it has been developed under the due amount of constitutional zymotic action; and that, consequently, its contents are potent as regards power to protect if implanted in another, and so capable of transmitting to generations still unborn an unimpaired protection from small-pox? We believe that these questions disclose to every thinking mind, that a wide field of research must still be carefully and industriously worked out, not to be completed for many years; and the more thought and study we have given to the subject of vaccination, the more does it appear to us that now we are treading on very delicate ground; that vaccination is an operation not to be trifled with, that the effects which it produces on the community must yet be watched for years, in order that its prophylactic powers may be efficiently developed; that many points in its pathology are still veiled in mystery, but that science has declared it to be a most powerful protection from small-pox, *if properly performed*, and that the guilt of the British Government is immeasurable, if it fail to secure, by efficient legislative enactments, complete and efficient vaccination for all, or if it tolerates its imperfect performance.

(*d.*) Regarding conditions which influence the *protective* powers of vaccination, there is yet another circumstance to be noticed—namely, the influence of idiosyncrasy and hereditary predisposition either to withstand the poison of variola or vaccinia, or to be unnaturally susceptible to its influence. We have heard of cases in which vaccination never would *take* at any period of life, and that the person, being exposed to small-pox on many occasions, had not been found to be susceptible, but seemed to have an immunity as perfect as any protected person could possess. Cases of a reverse description unhappily are not unheard of, as the following note at the foot of page 30 of Simon's "Letter" will testify:

"In a medical pamphlet of thirty-five years ago* I read: 'There are strong grounds for

* Address to Parents and Guardians on the Present State of Vaccination. By a Candid Observer, p. 47. London, 1822.

believing that this peculiarity of constitution, which disposes to attacks of modified or vaccine small-pox, is hereditary.' A medical friend writes to me, that he and his two brothers were vaccinated in infancy to the satisfaction of their then doctor; that some years subsequently, when they were severally aged thirteen, eleven, and seven, the second of them contracted small-pox, in a very severe form, and the other two caught the disease from him; that my correspondent himself, when twenty years of age, having occasion to attend the post-mortem examination of a patient who had died of small-pox, again contracted the disease, and in his turn communicated a second infection to both his brothers, who, like himself, had suffered it before. Dr. Arnott tells me, that he attended in Spain a case of post-vaccinal confluent small-pox, where the patient's father had had small-pox twice, and her uncle three times, another uncle having died with a first attack of the disease. I have notes of an instance—published, I believe, by Dr. Webster—where three brothers and sisters had had post-vaccinal small-pox; one of them once, another twice, and the other three times, including a last and fatal attack."

Such cases as these now noticed, however, are surrounded with many fallacies; and before we are justified in ascribing them to hereditary predisposition or idiosyncrasy, all the other elements we have already alluded to as modifying the *protective* power of vaccination must be strictly eliminated before such an explanation can be admitted. We are ourselves acquainted with an instance, where it was believed that vaccination, as performed by the doctor, "never took," and yet this individual was found never to be susceptible to the influence of the small-pox poison, although frequently exposed to it. On carefully sifting the history of this case, it was discovered that during early life the individual had been in the habit of handling and tending upon cows, and although he could not remember having sores on his hands from them, it is more than probable that he received his protection in this way.

III. *Legislative Measures to secure Efficient Vaccination.*

This topic is discussed both by the Epidemiological Society and by Mr. Simon.

The following advertisement, taken from one of the public newspapers, will show how these things are managed in this country. We contract for vaccination just as Government, the East India Company, or any large manufacturer would contract for a supply of coals! That such a course should ever have been sanctioned or necessitated is an insult to our profession, and that it should be responded to is no less disgraceful.

"——— Union.—Compulsory Vaccination.—The Guardians of the above Union will, at their meeting, to be holden on Friday, the 10th July instant, at the Union Workhouse, situate at , in the county of , receive Tenders from Medical Gentlemen duly qualified and willing to Contract for effectually Vaccinating in the District of this Union, comprising the following parishes, viz., , and also in the Union Workhouse. The Vaccinators to comply in all respects with the provisions and requirements of the Compulsory Vaccination Act of the 16th and 17th Vict. cap. 100; and all contracts will be made subject to termination at any time subsequent to the date thereof, by virtue of Legislative enactment. Particulars of the district, population, area, and other information, may be obtained on application to the Clerk.—By order of the Guardians, ——, Clerk."

We are surprised that the guardians do not pledge themselves to accept the lowest offer.

Two very obvious imperfections exist in the last Act of Parliament relative to this subject, the object of which, as the title informs us, is "To extend and make compulsory the practice of Vaccination." In the first place, the Act "has not provided competent machinery by which its enactments can be carried out." In the second place, it does not recognise "that there is such a thing as bad vaccination."

We admit that it may be difficult to organize machinery efficient to secure perfect vaccination; and there is evidence given by Mr. Simon which shows that every year the existing machinery has become less and less efficient. Looking, therefore, at the subject from a practical point of view, it is obvious to us—

1. That an efficient machinery should be organized.
2. That provision should be made for the education of the profession in all that relates to the practice and pathology of vaccination.

It is unsatisfactory, however, to find fault unless at the same time a remedy be suggested. The necessity for the education and practical testing of medical men in regard to vaccination is shown in sufficient force by Mr. Simon; but he leaves untouched any details of a plan to realize an efficient system of vaccination. In all plans to effect this object, sufficient advantage is not taken of existing national and local institutions, by calling in their aid as part of the machinery to carry out the spirit and letter of any legal enactment.

We submit the following outline of details, with the view of directing the attention of the profession to some specific means out of which an efficient machinery may, we think, be constituted to accomplish so desirable an end:

1. As suggested by the Epidemiological Society, it will be necessary to disconnect the practice of vaccination from the administration of the Poor-law Board.
2. Let a General Vaccination Board be constituted, under the auspices of the General Board of Health.
3. Let that Board be responsible for the organization of an efficient machinery which will secure universal and perfect vaccination.
4. Let the Board be responsible to Parliament, through the President of the Board of Health, who ought to be a medical man and a member of Her Majesty's Government.
5. Let the Officers of Health recently appointed undertake the inspection of the community within their districts, as to whether or not they are properly vaccinated. It is a legitimate subject for their investigation. Indeed, we conceive such to be a part of their duty by formulæ 18 and 19 Vic. cap. 120, of Sir Benjamin Hall's Bill.
6. Let one or more General Inspectors of Vaccination be appointed for each of the three parts of the British empire—England and Wales, Scotland, and Ireland; and let the General Inspectors be members of the General Board.
7. Let the existing vaccinators continue to do their duty; but let advantage also be taken of public institutions and local circumstances, as indicated above, to divide more and more the labour of vaccination, so that the operation may be more perfectly performed, and watched through its stages, in every case, as it ought to be.
8. Let legally qualified medical men be the only persons permitted to vaccinate.
9. Let additional public vaccinators be appointed where required. Let every union and workhouse employ a vaccinator. Let every hospital in the kingdom employ a vaccinator—a suggestion made in the last report of the General Vaccine Board, and which we particularly recommended in our last number.
10. In all towns where schools of medicine exist, let the practice and pathology of vaccination form part of the medical education, and let every student of medicine be required to produce evidence of having attended and studied as prescribed, before receiving a diploma or licence to practise.
11. Let uniform registers be kept by all who vaccinate, subject to inspection by members of the Board.
12. Let certificates of vaccination be granted to the vaccinated by the Inspectors or Officers of Health for the respective districts, and *not* by the vaccinators.
13. Let the vaccinators, local inspectors, and general inspectors be remunerated by fixed salaries.

The labourer is worthy of his hire: let him not be engaged by contract and paid with a grudge.

We have thus taken a comprehensive view of the important works before us relative to vaccination, the nature of the protection it affords, and the evidence of the existence of that protection. We have noticed how that influence has sometimes been impaired, and how more stringent legislative measures are imperatively demanded, not only to secure efficient vaccination for all, but to afford us the means of learning more accurately its pathology.

In now taking leave of our subject, let us formally express our profound respect for the memory of Jenner. We cannot help thinking that in this country we have paid but tardy homage to that illustrious name. Doubtless he has himself imprinted imperishable "foot-prints on the sands of time," which wave after wave of scientific research appears but to make more distinct; yet surely some lasting monument in this country ought to testify a nation's gratitude. A statue of the man, in honour of his great discovery, greets the traveller from the shore of a sister kingdom; and why should we be so regardless of one of our most illustrious dead? While monuments without number have been raised to perpetuate the deeds of warriors, whose fame has been achieved by the necessary destruction of countless human beings, surely we ought to cherish the memory and perpetuate the fame of one, by appropriate monumental honours, who, while he has been the means of subduing a disease, has been at once the means of improving the health of all civilized communities, and extending the duration of human existence.

Review IV.

Ueber die Bewegung der Iris: für Physiologen und Aerzte. Von Julius Budge, Professor in Bonn.—*Braunschweig*, 1855.

On the Motion of the Iris: for the use of Physiologists and Medical Practitioners. By Julius Budge, Professor in the University of Bonn. *Brunswick*, 1855. 8vo, pp. 206.

It is not alone in reference to the eye that the iris and its motions are of so much importance. The state of the pupil, it is well known, often affords valuable indications of much that is going on beyond the sphere of the organ of vision. Any work, therefore, calculated to extend and give precision to our knowledge of the subject must be acceptable to the physiologist and medical practitioner. Considering the work before us such an one, we have taken the pains to make an analysis of it. Comments and remarks which suggested themselves have been added, besides notices of observations on the subject by others, more recently published.

Muscular Structure of the Iris.—By means of the microscope, a muscular structure can be demonstrated in the iris. In man and in the mammifera the fibres are of the unstriated kind, like those of the walls of the stomach, intestines, &c. In birds they are transversely striated, like those of the common muscles of the skeleton.

In man and the mammifera the muscular fibres of the iris are divisible into two distinct sets; the one situated in the annulus major, and disposed in a somewhat radiating direction; the other situated in the annulus minor, and disposed round the pupil. It is by the action of these antagonistic sets of muscular fibres that the well-known variation in the size of the pupil is produced; the radiating fibres acting as a *dilator*, the circular fibres as a *sphincter pupillæ.* The state of relaxation of the iris is that in which the pupil is neither much contracted nor much dilated; a medium state into which the pupil falls some time after death, and to which, in consequence of an elasticity, the tissue of the iris is at the same time endowed with, it has a constant tendency to return after the dilating or contracting muscular force has ceased to operate.

In birds there is no *dilator pupillæ.* In these animals, dilatation of the pupil is the result of elasticity coming into play on the cessation of the action of the sphincter pupillæ. The state of relaxation of the iris in birds—the state in which it is found after death, therefore, is that in which the pupil is fully dilated.

In former times the muscularity of the iris was pretty generally admitted by anatomists, not so much, however, as Porterfield remarks, from their being able to demonstrate the muscular fibres, as from reason and analogy.

In the iris of man and the mammifera, indeed, there is not to be seen a tissue presenting any such appearances to the naked eye as those which used to be relied on by anatomists as characteristic of muscle. It was for this reason that Haller refused to recognise the muscularity of the iris, observing that we ought not to assume the existence of a structure which we cannot perceive by the senses. This philosophical reserve, however, Haller broke through when he attempted to explain the motions of the pupil by an afflux of blood to the iris, as in the case of erectile structures. This conceit found great favour with physiologists down to a very recent date, notwithstanding it involved much more improbability than the admission of simple muscularity. Though rich in vessels, the iris presents nothing like an erectile structure; and, supposing contraction of the pupil to be owing to vascular turgescence of the iris, dilatation would be the state of the pupil during collapse. But we know that in man and mammifera complete relaxation of the iris is attended by a medium width of the pupil—not by full dilatation, as in birds. In birds, the muscularity contracting the pupil, and elasticity dilating it, are too evident to be mistaken.

Nerves of the Iris.—In mammifera, the frog tribe, and fishes, the ciliary nerves come from the ophthalmic division of the fifth, the oculomotor, and the sympathetic. In birds, the sympathetic contributes no fibrils to the ciliary nerves.

Omitting what Budge says of the ciliary nerves as far as their entrance into the iris, in man, the dog, the cat, the rabbit, in birds, the frog, the barbel, we come to his account of their distribution in the iris. The substance of this is, that Budge never saw nerve fibrils in the iris with real free ends; they always appeared to form loops. He has also never found ganglions in the iris, either of man or the rabbit, which he has particularly examined, though Mayer had described and delineated ganglions in the iris of the whale, and had even found in the iris of man a few "small nodules" connected with the nerves. The fibrils from the oculo-motor, the fifth, and the sympathetic in the iris, Budge has not found to present such difference of microscopical characters as to enable him to distinguish one from the other.

1st. *Sympathetic.*—It is the cervical portion of the sympathetic which contains the fibrils destined for the iris. Budge calls these fibrils the *iridal sympathetic*; they cannot indeed be isolated from the other fibrils in the same nervous trunk; but by Budge's experiments, their origin and course appear to have been as exactly determined as the origin and course of any other nerves of the body.

The fibrils of the iridal sympathetic reach the trunk of the cervical portion of the sympathetic, from the spinal marrow, and medulla oblongata, through the communicating branches of the second and first dorsal, the eighth and seventh cervical, and the hypoglossal.

2nd. *Oculo-motor Nerve.*—In the rabbit's brain, Budge finds that the oculomotor nerve arises in five or six fasciculi, of which the strongest disappears in the crus cerebri of the same side, whilst the others turn inwards in order to pass into the crus of the opposite side. Lastly, anteriorly and externally the nerve is joined by more fibres, which run from before backwards, and appears to have a connexion with the anterior pair of the corpora quadrigemina.

3rd. *Fifth Nerve.*—At the origin of the portio major of this nerve, which alone comes under consideration here, there are an inner and an outer fasciculus, of which the former runs towards the *fossa Sylvii*, the latter towards the *corpus restiforme.*

Determination of the Nerves of the Iris.—On cutting the optic nerve, experimenters have not observed any symptom of pain; but at the moment of section of the ciliary nerves, the animal struggles. The seat of this sensibility are the fibrils from the fifth nerve.

Irritation of the optic nerve or retina directly excites the sensation of light and colour only. The pain extending to the head when the eyes are exposed to very strong light, or when mechanical impressions are made on the retina, is indirectly excited; being the result of irradiation from the optic to the fifth nerve.

When nervous irradiation is prevented by section of the optic nerve on the one hand, or of the fifth nerve on the other, exposure of the eye to intense light appears to cause little or no pain. On section of the oculo-motor nerve, neither Budge nor Longet have ever observed any indication of sensibility.

The cervical sympathetic also Budge has found to be insensible, but not so the superior cervical ganglion, which evinces sensibility when irritated.

As a result of Budge's experiments, it appears probable that the sensibility of the iris is dependent solely on the fifth pair.

Influence of the Oculo-motor and cervical Sympathetic on the Iridal Muscles.—Irritation of the trunk of a mixed motor and sensitive nerve causes muscular action *directly* through the centrifugal fibres, and by *reflexion* through the centripetal fibres. The reflex muscular action may be more extensive than the direct. If the nerve be cut, and the distal segment alone be irritated, direct muscular excitement will be the only result. When therefore a nerve has the same sphere of action after division as it has before, it is to be inferred that it is composed of centrifugal or motor fibres only.

Now Budge finds that the contraction of the pupil is as great when he irritates the oculo-motor nerve after section, as when it is still in connexion with the brain. From this it is evident that that nerve is composed exclusively of centrifugal or motor fibres.

When the oculo-motor nerve in mammals and birds is irritated, the pupil contracts; and when the cervical sympathetic in mammals (not in birds) is irritated, the pupil dilates. The experiment on the oculo-motor nerve Budge recommends to be performed immediately after death, but that on the sympathetic on the living animal.

A mistake has prevailed, that galvanization of the oculo-motor nerve causes contraction of the pupil at first, but dilatation afterwards. This Budge shows to be owing to care not having been taken to prevent the communication of the galvanism to the adjacent fifth nerve, which contains the sympathetic fibres, on the excitement of which the dilatation of the pupil depends, and that, too, at a period after death when the oculo-motor has already lost its irritability. In birds, in which the sympathetic has nothing to do with the motions of the pupil, the same mistake cannot occur. In short, the only effect on the pupil from excitement of the still irritable oculo-motor is contraction, never dilatation.

Galvanic irritation of the eye-ball itself excites contraction of the pupil, the oculo-motor being more irritable than the sympathetic. Again, in birds, in which the iris receives no branches from the sympathetic, galvanic irritation causes contraction; and when the galvanization is suspended, the pupil dilates by elasticity.

As transversely-striated muscular fibres are distinguished by the rapidity of their contraction, so in birds the sphincter pupillæ, the fibrils of which are transversely striated, contracts more rapidly when irritated than the sphincter pupillæ of mammals and frogs, the fibrils of which are not striated.

The sympathetic in the neck of a rabbit being isolated and galvanized, the pupil becomes dilated in from one to three seconds. The pupil is dilated also when the galvanic wires are applied to the uppermost cervical ganglion, the carotid twigs, the Gasserian ganglion, and ophthalmic branch of the fifth. Thus is indicated the course of the iridial sympathetic to the iris. The fifth nerve itself, as will be seen below, has no such effect on the motions of the iris—only the sympathetic fibres bound up in it.

Irritation of the Sympathetic in Birds.—In pigeons, Budge has found galvanization of the sympathetic in the neck produce no effect on the motions of the iris. And equally resultless was section of the nerve.

Irritation of the Optic Nerve, and its Influence on the Movement of the Iris.—From the experiments of Mayo on birds, and Longet on mammals, it is known that irritation of the optic nerve calls forth contraction of the pupil; and that when the optic nerve is divided this effect follows irritation of the end of the proximal segment, but not of the end of the segment in connexion with the eyeball. This is thus an example of reflex nervous action from the optic, through the brain, to the oculo-motor nerve.

The experiments of Flourens have shown that it is the corpora quadrigemina through which this reflection takes place. If the oculo-motor nerve be cut, irritation of the optic nerve is not followed by contraction of the pupil.

Influence of the Fifth Nerve on the Movements of the Iris, and Nourishment of the Eye.—After Magendie's experiment of section of the fifth nerve where it lies on the petrous bone, there was loss of common sensation in and about the eye. The vapour of ammonia no longer excited lachrymation nor winking. The pupil was contracted and immovable. The eye became inflamed, with a white puriform discharge. Opacity of the cornea supervened; but before this, iritis was observed to have taken place, with exudation of lymph into the anterior chamber. On the eighth day after section of the nerve, the cornea sloughed away, the humours were evacuated, and the eyeball became collapsed.

Magendie afterwards found that these effects did not, except in a slight degree, ensue when the nerve was divided close to its escape from the pons. On the seventh day the cornea was only slightly opaque, and some exuded lymph was visible in the anterior chamber.

Magendie's experiments have been repeated, and the same results observed, by Eschricht, Schöps, Baker, Longet, and Valentin.

The two effects of section of the fifth nerve, which call for particular notice here, are the contraction of the pupil, and the disturbance of the nutrition of the iris.

The first of these phenomena—the contraction of the pupil—the above-named experimenters have observed in rabbits; pathological cases have shown that it occurs also in man. According to Longet, on the contrary, dilatation of the pupil is the result of section of the fifth nerve in dogs and cats.

Budge has observed contraction of the pupil in the frog after section of the fifth nerve. But as the pupil in the frog will continue to contract and dilate under the influence of light, not only after section of all the nerves of the eye, but even after the eye has been extirpated, not much weight can be laid on his experiments on the frog as regards the point which Budge had in view—viz., to ascertain whether the fifth contains motor fibrils which determine the contraction of the sphincter pupillæ. Section of the fifth nerve in the frog, Budge has always found followed by contraction of the pupil. It makes a well-marked difference whether the fifth be cut behind or in front of the Gasserian ganglion. In the former case, the contraction of the pupil is not so great, and, what is to be particularly noted, not permanent. This difference, Budge thinks, is probably owing to implication of the sympathetic fibres in the section in front of the ganglion.

After section of the fifth nerve in the rabbit, whether before the Gasserian ganglion or behind it, or at the medulla oblongata, the pupil contracts, but subsequently dilates, with the difference, that when the section is behind the ganglion or at the medulla oblongata, the dilatation ensues much earlier than when the section is in front of the ganglion. In this latter case the pupil never attains the diameter of that on the sound side.

By the third day, the supervening opacity of the cornea prevents any further observation of the state of the pupil. The phenomenon is explicable by the fact that in the ganglion the sympathetic joins the fifth, and that therefore section in front of the ganglion implicates the two nerves. The two effects, however, as above mentioned, are in their nature quite different, contraction of the pupil after section of the sympathetic being permanent—that after section of the fifth, temporary.

But whichever be the place where the fifth nerve is cut, the consequent contraction of the pupil takes place very slowly—often not until a minute after, or even longer. When it is established, however, the contraction is very considerable, and when the operation is performed behind the Gasserian ganglion or at the medulla oblongata, twenty-four to forty minutes not unfrequently elapse before the pupil recovers its former diameter.

Contraction after irritation of the oculo-motor is quite otherwise. When the oculo-motor is cut, Budge has observed that the pupil contracts at the moment, and when the section is completed, the diameter of the pupil becomes as before.

Budge concludes that we cannot explain the action on the sphincter which takes place after the section of the fifth by supposing it to be through the medium of the oculo-motor. He has in rabbits repeatedly detached the oculo-motor at its origin, and then cut the fifth, but nevertheless observed contraction of the pupil take place as usual. He cannot therefore admit a connexion between the fifth and oculo-motor through the central organ to explain the influence of the fifth on the sphincter pupillæ. If in the section of the fifth the sympathetic fibrils were involved, the dilator pupillæ would be paralysed. The sphincter having been already paralysed by the detachment of the oculo-motor at its origin, the result on the pupil would be neither contraction nor dilatation.

It might be said that the ciliary ganglion is a reflecting centre between the fifth and oculo-motor. But to this Budge objects, that in rabbits in which the pupil contracts on section of the fifth, this nerve has no connexion with the ciliary ganglion at all. Budge therefore concludes that the temporary contraction of the pupil which supervenes on division of the fifth nerve is not owing in any manner to the oculo-motor.

By these experiments it is, indeed, not fully established that the fifth contains motor fibres exciting the contractility of the sphincter. A remarkable circumstance is, that immediately after general death, no reaction of the iris is to be observed on irritating the fifth. It is also remarkable that the contraction takes place so slowly, and when once established, does not so readily cease. A third remarkable phenomenon is, that even long after death this contraction of the pupil does not again cease. Lastly, it is worthy of remark, that in some cases of anæsthesia of the fifth, the pupil was found contracted. In the cases related by Serres and Meyer, the ganglion was much diseased.

If, now, on the one hand, these phenomena are not of a kind which we are in the habit of recognising as actions of motor nerves, so, on the other hand, we cannot, says Budge, set aside those marks which argue in favour of such an admission. Other nerves are known, after irritation of which, the action only slowly ensues and continues long. And it is a character proper to irritation of motor nerves, that the action again ceases.

In the pathological cases in which, with anæsthesia of the fifth, contraction of the pupil is also mentioned, the Gasserian ganglion was found morbidly softened and changed in composition. We may therefore presume the existence of some co-affection of the sympathetic nerve, and thence explain the contraction of the pupil. To this head, lastly, belong the cases in which, after irritation of other branches of the fifth in man, contraction of the pupil takes place. If, according to the above recorded observations, there be no reflexion from the fifth to the oculo-motor, no other way remains through which this contraction of the sphincter (*e. g.* after the application of irritants to the conjunctiva) can take place, but through the fifth nerve itself. It is consequently, Budge thinks, quite probable that the first branch of the fifth contains motor fibres for the *sphincter iridis*, though further researches are required to warrant a decision on the point.

We would remark that in inflammation of the iris the pupil contracts. Now, after section of the fifth, inflammation of the eye comes on, in which the iris is involved. It is therefore very likely that one cause of the contraction, of the pupil under consideration, is the inflammation, especially as, according to Budge, the contraction does not cease after death.

The cause of the contraction of the pupil which supervenes on irritation of the conjunctiva remains to be explained. The phenomenon has very much the appearance of being the result of reflexion from the fifth to the oculo-motor. But if there be, as Budge's experiments seem to indicate, no connexion between the fifth and oculo-motor, whereby reflex action may take place, it might be conjectured that irritation of the fifth reacts on the optic nerve (as no doubt the optic nerve can re-act on the fifth), and that from the optic, reflexion takes place to the oculo-motor.

More striking even than the contraction of the pupil after section of the fifth nerve, are the inflammation and exudation of which the conjunctiva, cornea, and iris are the seat. These effects, Budge observes, might be attributed to the influence of the Gasserian ganglion, though he thinks this is far from being proved. It appears to him that the bleeding which takes place in the experiment has an important share; having observed that when he wounded the ophthalmic veins in the orbit, without cutting the ciliary and optic nerves, dimness of the cornea still ensued. Of two rabbits, Budge cut, in one the optic with the ciliary nerves; in the other, he made all the preparations to do so, but did not. In both experiments the bleeding was considerable. In both cases the cornea became opaque, though in the one in which the nerves were cut the opacity was much greater than in the other.

Budge conjectures, therefore, that the opacity of the cornea after section of the Gasserian ganglion depends partly on the nerves, and partly on the loss of blood. It might with greater probability be said that the opacity partly depends on the interruption to the free return of blood from the eye, occasioned by the section of the ophthalmic veins. Still there can be little doubt that the principal cause is the section of the sympathetic fibres.

CENTRE OF THE IRIDAL NERVES.

Determination of the Centre of the Iridal Sympathetic, by artificial Excitement of Power.—That the pupil contracts when the sympathetic in the neck is cut, and that the pupil dilates when the end of the upper segment of the divided nerve is irritated, had been already established as physiological facts; it had moreover been rendered probable by Valentin's experiment of dividing the connecting branches between the spinal nerves and sympathetic in the neck, that the iridal sympathetic springs from the cervical portion of the spinal marrow, when, by a series of experiments performed partly in conjunction with Dr. Waller, and partly by himself alone, Budge claims to have succeeded in establishing that the iridal sympathetic does spring from the spinal marrow.

By irritating one part of the sympathetic after another, down to the lowest cervical ganglion, dilatation of the pupil was always called forth. From the place where the thoracic sympathetic lies behind the second thoracic ganglion, there was never the slightest action in the *dilator pupillæ* to be observed in numerous experiments. Here, therefore, were the limits to the centre of the iridal sympathetic.

It was accordingly to be inferred, that the iridal sympathetic comes either from the ganglionic chain or from the spinal marrow. But as section of the nerve behind the ganglions was followed by contraction of the pupil, it became evident that the spinal marrow must be the centre whence the iridal sympathetic springs.

The following is Budge's *experimentum crucis*:—Having cut through the spinal marrow immediately behind the fifth cervical vertebra, he applied the galvanic wires in such a way that the galvanic stream might pass through both halves of the exposed part of the spinal marrow. The result was that both pupils immediately dilated exactly as if the sympathetic nerves in the neck had been irritated. Having next isolated the two halves of the spinal marrow, by division, from each other, and galvanized one of them only, the pupil of the corresponding side was alone observed to dilate. If the sympathetic on *one* side was previously divided, and the galvanic stream sent through *both halves* of the spinal marrow, the pupil on the side on which the sympathetic had been cut did not dilate; the pupil on the sound side alone did.

In addition to the section immediately behind the fifth cervical vertebra, Budge now cut through the spinal marrow close in front of the fourth dorsal vertebra, and galvanized the piece of spinal marrow thus separated from all connexion with the rest. The effects on the iris were the same as those above described. By irritation of the other parts of the spinal marrow—above as well as below—no such effects were transmitted to the iris.

It thus appears that the *centrum cilio-spinale inferius*, as Budge calls it, is comprised in the portion of the spinal marrow between the sixth cervical and fourth dorsal vertebra. Having determined this, Budge considered it further necessary to experiment on the roots of the nerves coming from the iridal centre of the spinal marrow—the posterior roots by themselves, and the anterior roots by themselves. The following were the results which he found to be constant:

1st. Dilatation of the pupil when the two roots of the seventh or eighth cervical, or of the first and second dorsal nerves, were galvanized separately.

2nd. The same result, when the posterior root was first divided, and only the anterior root irritated, and that even when this root (the anterior) was detached from the spinal marrow.

3rd. From irritation of the posterior root also, dilatation of the pupil took place, but it was neither so great nor so permanent as that excited by irritation of the anterior root. It is, however, to be observed, that never the slightest effect on the pupil was noticed if the posterior root had been first detached from the spinal marrow, and then galvanized; showing that the irritation of the posterior roots acts most probably by reflection to the anterior root through the spinal marrow.

Determination of the Centre of the Iridal Sympathetic by Artificial Suspension of Power.—By extirpation of the *centrum cilio-spinale inferius*, its limits admit of being more exactly determined than by the above described method. After this operation the pupil contracts.

Budge extirpated successively on one side only different parts of the spinal marrow, and noted which of them no longer showed any influence on the pupil.

The parts of the spinal marrow in rabbits covered by the third, fourth, and fifth cervical vertebræ were thus operated on, without any effect on the pupil being observed. But when one half of the portion of the spinal marrow corresponding to the sixth cervical vertebra was extirpated, the pupil on the corresponding side was observed to contract.

In one case, having removed a small piece close behind the anterior end of the exposed portion of spinal marrow, Budge observed no change in the pupil; but, on the contrary, when he cut away a piece posteriorly in the same animal, the contraction of the pupil was quite evident; so that the portion of spinal marrow intermediate between the sixth and seventh cervical vertebræ may be considered as the place which corresponds to the anterior (in man the upper) limits of the ciliary centre.

From similar experiments, the posterior boundary of the ciliary centre appeared to be between the third and fourth dorsal vertebræ. The arch of the third dorsal vertebra having been removed, and the half of the exposed part of the spinal marrow cut out, a very distinct contraction of the pupil on the corresponding side took place. On the contrary, not the slightest effect was perceived when, in another animal, the spinal marrow under the fourth dorsal vertebra was treated in a similar manner. It may hence be concluded that the nervous centre which governs the dilatation of the pupil (*centrum cilio-spinale inferius*) begins behind (in man under) the giving off of the sixth cervical nerve and ends before (above) the giving off of the third dorsal nerve. At this place, therefore, a force is generated, which, through the medium of the cervical sympathetic, extends to the *dilator pupillæ*, and of which we observe the suspension after destruction of the part mentioned.

Which of the two columns of the spinal marrow—the anterior or the posterior—must be destroyed in order to abolish the action of the iridal sympathetic? and which of the two roots of the spinal nerves must be cut in order to isolate the centre from its periphery (in the iris)?

For the solution of these questions by experiment, Budge found frogs better adapted than mammals. In reference to the experiments on the frog, however, it is well to remind the reader that movements of the pupil in that animal, in obedience to variations in the degree of light, still take place after section of all the nerves of the eye, and even after extirpation of the eye. The difference between the pupil on the side operated on and that on the side not operated on, may, however, be admitted as the real effect of the experiments.

The limits of the ciliary centre in the spinal marrow are in frogs the same as in mammals. When from this portion of the spinal marrow, the upper half (the posterior columns) only was removed, a contraction of the pupil did take place, but it was only transitory, and not very well marked. When, on the contrary, the whole half was extirpated, the contraction of the pupil began as soon as the etherization passed off. The difference between the two pupils was then very evident.

In order to demonstrate more fully the action of the anterior and posterior columns, Budge extirpated in a frog the right half of the centre wholly, but of the left half only the posterior column. The result was, that the pupil of the right eye became contracted, while that of the left remained unaltered. A corresponding result was obtained when the anterior or posterior roots alone were cut.

Budge has made the remarkable observation, that as soon as the posterior root only was cut, the pupil began to contract in half a minute, or even earlier; but that, on the contrary, a longer time elapsed before the same effect was produced when the anterior root only was cut. The contraction after the first operation, however, was only transitory, whereas that after the second was permanent. From this it would appear that the fibrils which the anterior roots give to the iridal sympathetic spring from the spinal marrow, and not from the spinal or any other ganglion. As these fibrils show the most enduring action, it is to be inferred that the anterior column of the spinal marrow at the place indicated is the most essential part of the *inferior ciliary centre.*

Everything, it may be concluded, speaks for the proposition that the iridal sympathetic consists of centripetal and centrifugal, or excitor and motor fibrils; that the former go in the direction from the iris to the spinal marrow, and the latter from the spinal marrow to the iris; and that as yet no relation has been proved to exist between the ganglions through which the iridal sympathetic passes, and the nerve fibrils which govern the dilatation of the pupil.

Relations of the Inferior Centre of the Iridal Sympathetic to other Organs.—Besides the contraction of the pupil, there takes place, according to the discovery of Bernard, as a result of section of the sympathetic in the neck, increase of heat on the side of the head corresponding to that operated on. This phenomenon of increased heat appears to be owing to an acceleration of the circulation,* and not, as Budge thinks, to a stagnation of blood in the vessels of the head. By section of the sympathetic, the contractility of the arteries, which receive their nerves from it, is suspended or impaired. The result is dilatation of the calibre of these vessels, and consequent acceleration of the flow of blood, not only through them, but also through the capillaries and corresponding veins.

When the *centrum cilio-spinale inferius* on one side is extirpated, Budge has found a similar result on the vessels to that arising from section of the sympathetic in the neck. In respect to its relations to the arteries, the centrum, according to Budge, is bounded by the same limits and seated in the same columns of the spinal marrow and in the same roots of the nerves, as it has above been shown to be in respect to its relations to the iris. Consequently, the apparatus of fibres which in the cervical sympathetic acts on the vessels, springs in the spinal marrow at the above-mentioned place. It may therefore be said that the cervical sympathetic, so far as it has been investigated by Budge in reference to its function, has its origin in the spinal marrow between the sixth cervical and fourth dorsal vertebræ.

* See a paper, by the writer of this article, On the State of the Blood and the Bloodvessels in Inflammation, in the Transactions of the Medical and Chirurgical Society of London, for 1853.

According to Dr. Brown Séquard's experiments, the centre of the fibrils which go from the sympathetic nerve in the neck to the arteries of the corresponding side of the head, extends as far as the ninth or tenth dorsal vertebra.

After section of the sympathetic in the neck, the pupil on the same side is smaller than that on the other, even in the shade, and still more so in the light. The influence of light on the pupil is thus not lost, nor is it altered. In other words, what Budge means might be expressed as follows:—When the dilator of the pupil is paralysed by section of the sympathetic in the neck, the unrestrained action of the sphincter does not take place to its fullest extent, just as when the internal rectus is cut or paralysed, the eyeball is not much turned towards the temple by the external rectus; the sphincter pupillæ, equally with the external rectus, retaining the power of contracting more fully under the usual conditions.

Section of the Sympathetic in different parts of the Neck.—If the trunk of the sympathetic in the neck be cut, and some five or six days thereafter subjected to galvanism, no effect is produced on the pupil; but galvanization of any part of the nerve above the superior cervical ganglion—the carotid branches, the Gasserian ganglion, or the eyeball itself—still excites dilatation of the pupil. It is quite otherwise if the superior cervical ganglion be extirpated, and the animal killed a week after. Not the slightest dilatation of the pupil is observed on the operated side in answer to the galvanic irritation immediately after death, whether applied to the Gasserian ganglion or to the eyeball.

The cause of this difference, Budge thinks, can only be, that in addition to the source which the iridal sympathetic has below the superior cervical ganglion, there is another source in the situation of this ganglion, so that if even the sympathetic below the superior cervical ganglion were compressed by a tumour, or otherwise injured, dilatation of the pupil might still be possible.

The Upper Centre of the Iridal Sympathetic.—Having shown that the iridal sympathetic receives a second supply of power in the situation of the superior cervical ganglion, Budge next proceeds to ascertain by experiment the exact centre from which this is derived. From numerous experiments he has found that section or irritation of the branches of communication between the sympathetic and upper cervical nerves has no effect on the pupil; but that, on the other hand, the effect is very striking when the communicating branch between the hypoglossal nerve and sympathetic is cut. Accordingly, if this communicating branch be divided, as well as the trunk of the sympathetic below the superior cervical ganglion, both sources of the power of the dilator pupillæ are cut off, just as when the superior cervical ganglion itself is extirpated. In one case Budge succeeded, soon enough after the death of the rabbit, to isolate and galvanize the communicating branch between the hypoglossal and sympathetic, and found the result to be dilatation of the pupil.

It thus appears made out, that the communicating branch between the hypoglossal and superior cervical ganglion gives to the iridal sympathetic a second set of motor fibrils.

After having ascertained this, Budge searched for the same action at the origin of the hypoglossal itself. Immediately after death he laid bare the nerve in a rabbit, and irritated it, but could observe no effect on the pupil, owing probably, he thinks, to the speedy extinction of the nervous irritability. In the frog he cut the hypoglossal, whereupon a transitory dilatation first occurred, as when the sympathetic is cut—the effect of the irritation caused by the section. To this dilatation succeeded contraction of the pupil, sometimes sooner, sometimes later, but which contraction was also only transitory. In the neighbourhood of the hypoglossal nerve, therefore, it is not to be doubted that the iridal sympathetic has a second centre—*centrum cilio-spinale superius* (*anterius*).

Centre for the Optic and Oculo-motor Nerves.—By Flourens it was shown that removal of the corpora quadrigemina is followed by blindness. Destruction of them on one side only induced blindness on the opposite side. Flourens also observed contraction of the sphincter iridis of one, and even of both, sides when the corpora quadrigemina were irritated. These observations have been confirmed by Hertwig, Longet, and others.

In his experiments on the corpora quadrigemina, Budge has obtained the following results, in reference to the iris and sight. Suppose each of the corpora quadrigemina be divided into an outer and an inner half, the whole of the outer half of one of the anterior pair may be taken away without motion of the iris of the opposite or corresponding side being thereby necessarily destroyed. In white rabbits, which are well adapted for the experiment on account of their irritable eyes, Budge has seen that both pupils become smaller in the light, although on one side the whole outer half of one of the anterior of the corpora quadrigemina was removed to the very bottom.

The inner side of the corpora quadrigemina, on the contrary, stands in close relation to the iris. Thus, in a rabbit in which Budge destroyed it, complete insensibility to light on the part of the sphincter of the opposite side was remarked. On the side of the wound, the iris continued to react as usual.

The sight, on the contrary, was not abolished. Whether after complete extirpation of the corpora quadrigemina of both sides total blindness results, Budge cannot from his own experience say, as the animals experimented on were never in a condition, after the operation, to admit of any opinion being formed on the point.

Centre for the Iridal Fibres of the Fifth Nerve.—By section of the spinal marrow on one side, between the atlas and dentata, Budge found contraction of the pupil temporarily ensue; in a manner, he supposed, similar to what happens when the fifth nerve is cut.

Section of the spinal marrow on one side, at the point of the calamus scriptorius, is followed by loss of sensation in the domain of the fifth of the same side, along with contraction of the pupil.

When the inner part of the medulla oblongata was alone divided, the corpus restiforme being untouched, Budge found that sensation in the face and eye was not abolished, and that there was not much contraction of the pupil.

Although further research is necessary in order to determine accurately the origin of the portio major of the fifth, and particularly that of the ophthalmic branch, it may, Budge thinks, still be conjectured that the fibrils (motor) having relation to the pupil, spring in the spinal marrow above (before) the second cervical nerve, and that the rest of the fibrils join them in the *corpora restiformia* and *locus cœruleus*.

Influence of Light on the Pupil.—Three effects—viz., special sensation, common sensation, and motion—are produced by light entering the eye. We perceive the light, we have a feeling of pain or the opposite, and the size of the pupil is altered.

By the perception and sensation, ideas and impulses are often awakened. The motion of the iris may therefore be either directly excited by the light, or only indirectly called forth through the ideas and impulses.

The reaction of the iris to light is not quite the same in warm and in cold-blooded animals. Immediately after section of the optic nerve in mammals and birds, the pupil is no longer affected by light. In a rabbit, the optic nerve within the skull having been exposed, both eyes were tested to see if the pupil of each was equally affected by the light. One optic nerve was then divided, and the two eyes again tested, when it was found that the brightest light produced no effect on the pupil of the side operated on, whilst the pupil of the uninjured side remained obedient to light as before. In pigeons, the removal of the cerebral hemispheres with the optic tubercles does not alter the action of light on the pupil. After separation of the optic nerve from the corpora bigemina, however, the iris immediately becomes immovable to the brightest light. The iris of the opposite side reacts as usual.

A second condition on which the susceptibility of the iris to light depends, is the integrity of the corpora quadrigemina in mammals and corpora bigemina in birds. According to the experiments of Fleurens, Hertwig, Longet, Magendie, &c., when the anterior of the corpora quadrigemina in mammals, or the corpora bigemina in birds, were removed on one side, the iris of the opposite side was no longer obedient to light, while that of the same side was less so than before. When the anterior pair of the corpora quadrigemina or the corpora bigemina were extirpated, complete immobility of both pupils resulted. Lastly, in complete paralysis of the oculo-motor nerve, whether from disease or section, light has no influence on the pupil. When the fifth nerve is cut the iris is often motionless, but again becomes obedient to the light.

Contraction of the pupil by light, it is generally acknowledged, is not owing to direct action on the iris or its sphincter in mammals and birds. In these animals an essential condition for the action of light on the pupil, is that the path from the retina to the iris, through the optic nerve, to the anterior of the corpora quadrigemina, and thence to the oculo-motor nerve, be not interrupted. It has lately been, however, asserted by a Dutch physiologist, Ruiter, that he has observed light act on the pupil of the dog after death. And Dr. Brown-Séquard also affirms that he has observed contraction of the pupil after death in mammifera, and even in man, excited by light. Brown-Séquard admits, however, that the movements of the pupil during life are not due to the direct action of light on the iris.

In frogs and fishes it has been discovered that light by its direct action on the iris excites the sphincter iridis.

Influence of the Will on the Activity of the Iris.—Cases have been related in which it was alleged that the motion of the iris was subject to the will. In reference to this, Budge relates the following observation. Professor Beer, of the University of Bonn, is able in the same light to contract or dilate his pupil at will. This change in the size of the pupil, however, he brings about only through certain ideas. When, for example, he thinks of a very dark space, the pupil dilates. When, on the contrary, he thinks of a very light place, the pupil contracts. He finds it more difficult to induce contraction than dilatation. Budge has met with several other persons who can dilate the pupil in consequence of such ideas, but not another who can contract it also. From such cases we must conclude, not that the motion of the iris is voluntary, but that the idea of a sensation can bring forth motions, as well as the actual sensation itself.

Professor Allen Thomson, of Glasgow, has lately published, in the "Glasgow Medical Journal," some remarks on the case of Dr. Paxton, of Kilmarnock, who possesses an unusual power of contracting and dilating the pupil, alleged to be voluntary and independent of any effort at adjustment of the eye.

Dr. Paxton showed Dr. Thomson the motions of his iris, "alternately contracting and dilating the pupil to a great extent with apparent ease at will." And he informed Dr. Thomson—

> "That although in producing the motions of contraction or dilatation of the pupil, he did not actually make an effort of adjustment, or attempt to fix the eye alternately on a near and distant object, yet the effort to make either of these motions seemed to him, as it were, very similar to the motions for adjustment."

Dr. Paxton further stated to Dr. Thomson, in proof of his possessing a greater than usual power of moving the iris independently of adjustment, that he

> "Can fix the eyes upon a near object, and, while steadily looking at it, dilate the pupil without any effort for adjustment for distant vision, and while continuing to look at a distant object, he can still further dilate the pupil and contract it at will, without any attempt at adjusting the eye for near vision."

In short, as Dr. Paxton himself informs us in a letter, "he can alternately dilate and contract the pupil with as much facility as he can open and shut his hand," and that without the slightest mental effort at adjustment. This he can do also more rapidly than the pupil can adjust itself for near and distant vision.

The pupil, Dr. Paxton says, has the ordinary action under the influence of light and shade, but he can always at will dilate it, whether the eyes be exposed to light or shade. It is by dilating he must always begin the movements in question. By a slight effort of what appears to him to be relaxation, he dilates the pupil, and when the pupil is dilated, he can, by a slight effort of bracing up, contract it. Furthermore, Dr. Paxton says that it is not by raising up any idea in the mind, such as thinking of light and shade, that he calls forth the movements of his pupils; but by distinct efforts, and that he is always conscious, both by the state of *vision* and by the *sensation* in the eye, whether the pupil is in its normal condition or not.

Though it is thus evident that Dr. Paxton has the power voluntarily to dilate and contract the pupil in some manner or other, it is not clear that he has the power to do so directly in the same manner that he can will the opening and shutting of his hand. The power which Dr. Paxton has of bringing on a temporary dimness of sight may have something to do with the dilatation of the pupil; whilst, on the other hand, the bracing-up effort (which is most likely exerted on the muscles of the eyeball) by which distinct vision is brought back, may be the immediate cause of the contraction of the pupil.

Action of Belladonna on the Iris.—Two principal opinions have prevailed in regard to the mode in which dilatation of the pupil is caused by belladonna. According to the one, thes phincter pupillæ is paralysed, and the dilator being thereby no longer restrained, freely contracts. According to the other, the dilator pupillæ is directly excited. A third opinion might be mentioned, according to which belladonna acts by both paralysing the sphincter and exciting the dilator.

In agreement with Bernard, Budge has found that after section of the oculomotor nerve, full dilatation of the pupil does not take place, but that this effect is produced by belladonna as usual. Moreover, it is known that in cases of complete paralysis of the oculo-motor in the human subject, the pupil is usually only somewhat dilated, but becomes still more so by belladonna. This result is not consistent with the opinion that belladonna acts merely by paralysing the sphincter; but it is consistent with either of the two other opinions—viz., that belladonna acts by exciting the dilator, or by both paralysing the sphincter and exciting the dilator.

As was first observed by Kieser, belladonna does not cause dilatation of the pupil in birds. The explanation of this Budge thinks must be sought for either in the transversely striated structure of the muscular fibres of the bird's iris or in the absence of an iridal sympathetic. These, however, are not the only alternatives, for supposing that belladonna causes dilatation of the pupil in man and the mammifera, not by paralysing the sphincter, but by exciting to contraction the dilator pupillæ, it might be said that the reason why belladonna does not cause dilatation of the pupil in birds is that there is no dilator pupillæ to be excited to action by the belladonna, the pupil in birds being dilated by elasticity coming into play on the cessation of the action of the sphincter.

In agreement with Biffi, Cramer, and Ruiter, Budge has found that after section of the sympathetic in the neck, and even after extirpation of the superior cervical ganglion, belladonna still exerts its dilating influence on the pupil, though in a less degree. Dr. Harley,* however, has recently made the important observation that, after section of the sympathetic in the neck, by continuing the application of the atropia the pupil becomes at last fully dilated.

If belladonna acted merely by paralysing the sphincter, we could not have such a result as this, seeing that the dilator, already so completely paralysed by section of its nerve, would not be in a condition to act spontaneously on the cessation of the antagonism of the sphincter. The result, however, is consistent with the opinion that belladonna excites the dilator pupillæ, if we admit that the drug

*** Additional Experiments on the Action of Atropine in Dilating the Pupil, in Edinburgh Medical Journal for February, 1857. See also a previous paper by Dr. Harley, in the same journal, for Nov. 1856; and two papers by Mr. Benjamin Bell, in the same journal, for July and December, 1856.**

comes by absorption to act on that muscle. The result is also not inconsistent with the supposition that belladonna acts both by paralysing the sphincter and exciting the dilator.

Budge has cut both the oculo-motor nerve and the sympathetic; nay more, he has cut all the ciliary nerves, together with the optic, and still found the pupil to dilate distinctly under the influence of atropia. This result is entirely consistent with the opinion that belladonna acts by exciting the dilator pupillæ, if we make the admission above stated—viz., that the atropia by being absorbed is brought into contact with that muscle. The result is at the same time not inconsistent with the opinion that belladonna acts both by paralysing the sphincter and exciting the dilator pupillæ.

Budge thinks, however, that his observations make it very probable that belladonna does not act on the nerves of the iris at all in dilating the pupil; but that, on the contrary, it directly paralyses the fibres of the sphincter muscle. And in favour of this opinion, he adduces the fact of the non-action of belladonna on the pupil of birds, as it is improbable, he thinks, that the oculo-motor in birds would be unaffected by the same agent which paralyses it in mammalia.

It has been above shown that the non-action of belladonna on the pupil of birds is susceptible of a different explanation from that of Budge, and is not therefore of any weight as regards the view of the question Budge has here broached—viz., whether belladonna acts directly on the muscular fibres, and not through their nerves. It would have been more reasonable, perhaps, in Budge to have concluded from his observations, that belladonna directly stimulates the dilator—a conclusion to which Dr. Harley is disposed to come, from his observation above cited, that by continuing the application of atropia to the eye, in a case in which he had divided the sympathetic in a cat's neck, the pupil at last became completely dilated. The theory that atropia acts by paralysing the sphincter muscle through the third pair, Dr. Harley now considers overthrown by that observation. His other experiments he appears to believe had disposed of the theory that atropia acts by exciting the dilator pupillæ through the sympathetic. What is now to be done? he asks. Both theories are upset, and yet the action of atropine is not explained.

> "The only other view of the action of atropine in dilating the pupil," continues Dr. Harley, "with which I am acquainted, is that held by Ludwig and some other German physiologists—that the action of atropine depends on its directly stimulating the radiating muscle of the iris to contraction."

The new question which has thus been raised, as to whether belladonna exerts its action through the medium of the nervous, or directly on the muscular fibres, belongs to the more general question of the nature of muscular irritability, and need not be entered on here. An experiment, however, which Dr. Harley performed to test the correctness of this view is worth quoting. He placed the eyes of a cat immediately after death, the one in a solution of atropia, the other in water, and found that the pupil of the former became dilated, while that of the latter contracted. The same experiment was tried on the eyes of two dogs, but with no result. It may be mentioned that Ruiter had before observed that atropia still dilates the pupil if applied to the eye immediately after death by decapitation, in the calf, the rabbit, and the frog.

In arguing that belladonna dilates the pupil by exciting to contraction the dilator pupillæ muscle, the writer of this article* has always meant by *muscle* the proper muscular fibres, together with the fibres of the iridal sympathetic distributed among them.

* See two clinical lectures On the Use of Belladonna in Ophthalmic Practice, in the Medical Times and Gazette for Jan. 10th and 24th, 1857.

Review V.

1. *A Treatise on the Diseases of the Breast and Mammary Region.* By A. Velpeau, &c. Translated from the French by Mitchell Henry, &c., for the Sydenham Society.—*London*, 1856. 8vo. pp. 608.
2. *A Treatise on the Cancer of the Breast and of the Mammary Region.* By A. Velpeau. Translated from the French by W. Marsden, M.D., &c.—*London*, 1856. 8vo. pp. 293.
3. *A Treatise on Cancer and its Treatment.* By J. Weldon Fell, M.D., &c.—*London*, 1857. 8vo. pp. 95.

We have carefully perused the work of M. Velpeau, and regard it, on the whole, as a valuable contribution to surgical literature. The author is at times wordy and prolix, and evinces indecision on some important points of doctrine; his work, nevertheless, gives evidence of remarkable industry in accumulating and assorting facts, and of great experience in clinical research. He has done good service, by showing that an overweening reliance on the microscope in surgical diagnosis, to the exclusion of practical tact and observation, is fraught with error. We have no hesitation in commending the translation by Mr. Henry, with the useful notes which he has supplied. We regret our inability to speak in equally eulogistic terms of the translation of the latter part of the work, executed by Dr. Marsden.

Diseases of the female breast are divided by Velpeau into two principal classes—1st, diseases of an innocent kind, inflammatory or non-inflammatory; 2nd, diseases of malignant or cancerous nature. After treating minutely of diseases and deformities of the nipple and areola, the author proceeds to describe inflammation of the breast. Premising that here, as elsewhere, inflammation is modified by age and constitution, by normal and abnormal conditions of the functions of the part, by the state of the general health, and by the nature of its exciting cause, he lays great stress on the varieties dependent on anatomical structure—varieties which have been admitted by some of the leading surgeons of Paris.

> "Regarded in this point of view, three classes of cases must be established. The first originates between the gland and the skin, in the subcutaneous cellular fatty tissue; the second is seated, either primarily or secondarily, beneath the breast, between the gland and the chest; and the third class has its principal focus in the interlobular network, or in the glandular structure itself.*

Each of these branches off into several subdivisions. Some interesting observations are made on lymphatic inflammation, not previously described as a distinct affection, doubtless, he believes, because it has been confounded with certain varieties of erysipelas or phlegmon, for it is not very rare.

> "The symptoms which usher it in are—
>
> "1st. Irregular chills, sometimes a downright shivering fit, a rapid and tumultuous pulse, which is unequal rather than strong, heat of the skin, restlessness, loss of sleep and appetite, and sometimes nausea.
>
> "2nd. In the breast there are pain, great heat, swelling, and red patches irregularly scattered about, coming to a focus in painful spots, more or less raised from the surface.
>
> "3rd. Generally there is a painful state of the axillary glands, and sometimes reddish striæ lead from the breast to the armpits. Chaps, excoriations, eczematous eruptions, abrasions of the skin, and all affections of the nipple and areola, are the ordinary causes of lymphatic inflammation of the breast."†

The author states, that when left to itself, this kind of inflammation sometimes ends by resolution and rapid and complete cure. If suppuration result, the abscesses terminate by opening, contracting, and eventually cicatrizing. Sometimes, how-

* Sydenham Society's edition, p. 20.

† Ibid., p. 22.

ever, the disease is tedious and painful. The treatment which he recommends, should the surgeon be called in, during the first two or three days, is heroic—viz.,

"To bleed from the arm to the extent of from seven to twelve ounces, and the next day to apply fifteen or thirty leeches to the breast. If the inflammation does not now abate, mercurial ointment should be freely applied to all the affected parts, and renewed three times a day, without discontinuing the employment of poultices. If there is no reason to the contrary, the bleeding may be repeated, and a purgative administered, should the digestive organs not be disturbed."

Now, concurring with M. Sappey, who has discovered several strata of lymphatic vessels in the breast, and according to whom the majority of abscesses in this situation are only the results of lymphatic inflammation, or, as it is called, angio-leucitis, we consider the above practice too energetic. By the employment of saline purgatives combined with antimony, and oft-renewed poulticing at the outset, followed by the topical use of tincture of iodine, and those means best adapted to support the patient's strength, the disease will be found to yield in numerous instances.

For each kind of inflammation the author has a different line of treatment. Thus, for subcutaneous phlegmon he advises leeches in large number to the affected part, mercurial friction, and compression; for deep-seated, as also parenchymatous inflammation, general bleeding, with leeches around the breast, and large poultices with mercurial ointment; for glandular inflammation, purgatives, alterative drinks, and emollient topical applications—a line of practice which it will be somewhat difficult to persuade English surgeons to adopt, inasmuch as we do not believe that such vigorous measures of depletion are in general needed, or that the resulting lowering of the system at all countervails any presumed influence they may have in controlling the disease.

With respect to deep-seated abscesses, with sinuses in the breast, the author advocates the plan recommended by the late Mr. Hey, of Leeds—namely, that the breast should unhesitatingly be divided in its entire extent through the whole length of the sinuses, with the view of healing them. (p. 81.) This proceeding, although condemned by Sir A. Cooper, is, in the opinion of the author, the most certain of success—sometimes the only one that can produce a radical cure, and which would be generally adopted were it not for its severity, and he might add, the unseemly scars which are left behind. Before resorting to such an extreme measure as the above, we suggest the trial of moderate, well-adjusted compression, conjoined with repeated dilatation of the orifice or orifices with a pencil of lunar caustic, applied once or twice a week, the part being in the interval covered with lint smeared over with compound mercurial cerate surmounted with a piece of leather spread with soap plaster, and a roller employed in order to maintain the dressing in its place, and also exercise a suitable amount of pressure. This has answered admirably in the instance of deep and circuitous sinuses.

Under the head of Chronic Abscess the author refers to some difficulties of diagnosis which seem entitled to notice. Those which assume the form of cysts are met with not only under the breast and in the substance of the gland, but also all around it.

"At the circumference of the breast, it is hardly possible to mistake them for tumours, of malignant or cancerous nature; but beneath or in the substance of the gland, the error is not always easy to avoid. Such a mistake was committed a few years ago in one of the great hospitals of Paris, by one of the most eminent and experienced practitioners."

Sir A. Cooper cites a similar case, and we know that a like occurrence has taken place since his time in at least one London hospital. Surely the introduction of an exploratory trocar, or even the plunge of a bistoury at the outset of the operation, would be conclusive.

There are two therapeutic agents connected with phlegmon and abscess on which the author lays considerable stress—namely, blistering and compression. Blisters are not indicated in cases of lactiferous engorgement or parenchymatous inflammation before the formation of pus, but chiefly in deep-seated inflammation of the breast. In subcutaneous inflammation, also, they frequently prove successful. According to the author, the blister acts,

"When pus has not yet formed, as a powerful resolvent, and when resolution is no longer possible, as an energetic means of maturing the abscess. It delays suppuration, and hastens the resolution of the inflammation, if applied at an early stage. At a later one it expedites the formation of the abscess, by softening the tumour, and diminishing the sensibility, tension, and thickening of the parts around. In cases of open abscess, when the cure is tedious, blisters are also useful. Applied over the whole extent of the swollen surface, comprising the ulcers and the open wounds, they tend both to close the depôts, to heal the fistulæ, and to resolve the concomitant engorgement."*

The author most properly takes exception to the indiscriminate employment of the word *engorgement*, as being a source of serious mistakes in the prognosis and treatment of diseases of the breast. Many practitioners confound under this appellation affections of the most opposite character—such as colloid, melanotic, scirrhous, encephaloid, tuberculous, and fibrinous tumours, and simple sub-inflammatory thickening. Velpeau would apply it only to

"A lesion characterized by thickening, with a lardaceous condition, and loss of a part of the elasticity and porous lamellar structure, and of the extensibility natural to the tissues, together with the absence of any heterologous formation."†

The consideration of tumours properly so called, extends over nearly 160 pages. These are disposed of group by group. The first group comprises tumours of an innocent nature, made up of the natural structures of the mamma—such are, hypertrophies, fatty growths, and neuromata. With respect to neuromatous tumours—those small painful indurations in the breast which often harass patients for years—the author suggests by way of cure subcutaneous incision, in despair of tracing the cause of the disease. This plan has been resorted to by M. Rufz with success in two cases. He introduced a tenotomy knife between the centre of the pain and the root of the nerves or vessels, and then divided the tissue of the breast; he thus cut through several structures at one sweep; in one case, where all the nervous filaments had not been freely divided, and there was still pain, he repeated the operation, and the result proved successful. These deep subcutaneous sections determined sometimes a slight hæmorrhage, which, however, was readily stayed by pressure; also extravasation of blood underneath the integuments of the breast, but never any accident of moment. M. Rufz was indeed surprised at the rapidity of cicatrization.

The author has devoted a long chapter to the consideration of imaginary pains and tumours. With regard to the former, we can scarcely conceive it possible that a woman should complain of pain which does not exist. The question of imaginary tumours is important, because it has happened that even medical practitioners have been led to believe in the existence of tumours where the mammary region was perfectly free from any morbid alteration. The source of error has been clearly pointed out by M. Velpeau. It may depend, 1st, on inequality in the form and consistence of the lobules of the breast; 2nd, on a prominence of a rib, in consequence of being larger and more convex than natural, a circumstance which may induce the belief of there being a deep-seated and adherent tumour; 3rd, the exaggerated arching of the upper part of the chest; 4th, lastly, on the manner in which the examination is made: it happens, for example, when the surgeon seizes a portion of the breast between his fingers, he perceives a resistance proportionally great, because no portion of the gland can escape being firmly held between two solid planes—namely, the fingers; and if, at the same time, the

* Sydenham Society's edition, p. 124. † Ibid., p. 164.

patient should complain of a swelling in the breast, the sensation experienced by the surgeon will persuade him as to the presence of a tumour, which has really no existence. To avoid this mistake, let the surgeon support with the fingers of one hand the circumference of the mammary gland, while with the other he will press on the different regions, resting the fingers on the anterior or cutaneous surface. M. Velpeau insists on these modes of exploration in order to establish an accurate diagnosis of tumours of the breast.

The author gives a careful description of tubercular tumours, designated by Sir A. Cooper as scrofulous tumours, but whose account of them is brief and incomplete. He is decided as to their close affinity with tubercular deposit in other textures of the body. In the same chapter he furnishes details respecting the tumours formed by cysts filled with purulent or semi-purulent matter, and a half concrete substance resembling that of tubercle. It is difficult to determine the origin of these kinds of collections; in one case it was attributed to some affection of the sternum, to one of the cartilages of the ribs, or to a gland contained in the anterior mediastinum; in other cases the cyst was moveable, and did not appear to have connexion with any other structure but that of the mamma; in another case, again, it was attached so closely to the walls of the chest, as to convey the impression of being a symptomatic abscess.

Under the head of Galactocele the author treats of "tumours formed of milk, or by some of its constituents, accumulated either in the natural ducts of the gland, or amidst the organic structures of the mammary region."* This class of tumours, of which but few examples have been published, never, he says, received any special description, until he wrote a short account of it in 1838. It occurs in several forms—as swellings, that may be acute, chronic, liquid, solid, inflammatory, indolent, temporary, or permanent. It has, the author believes, been frequently confounded with tumours of an entirely different nature. With respect to treatment, he enjoins resort to incision or a seton:

> "Rather a large seton is best for large cysts, and an incision of some extent for cysts of moderate size. In the one case, as in the other, it is necessary for the whole interior of the galactocele to suppurate, and for its cavity, no longer distended by the fluid, to retract and become reduced to a sort of sinus or fistula. Afterwards, it is sufficient to keep the lower opening patulous by means of a thread, so that it may close finally from the anterior towards the exterior."†

Velpeau attaches but little value to the efficacy of injections of iodine or wine in such cases, because from investigations which he has made into the effect of medicated injections into closed cavities, it has resulted that irritating injections are only efficacious when they are applied to cavities which are *bonâ fide* serous. He observes, however, as they may perchance succeed, he should not hesitate to try them. He treats serous cysts, of course, with iodine injections. After emptying the tumour by means of a small trocar, he immediately injects into the sac a solution composed of one-third of tincture of iodine to two-thirds of water, and the operation is complete. He has employed it on ten occasions in the female.

Upwards of fifty pages are occupied with the consideration of adenoid tumours, signifying thereby *gland-like* tumours; termed by Cruveilhier *fibrous tumours;* by Sir A. Cooper *chronic mammary tumours;* and by Vidal, in common with most microscopic observers, *tumours from partial hypertrophy of the mamma.* Allusion is made to the confounding together of tumours of this kind with "scirrhus," "encephaloid," or "blind cancer," but such cannot apply, as the translator justly observes, to this country, and we agree with him as to its certainly appearing strange, that after the descriptions of Sir A. Cooper and Mr. Lawrence, such a mistake should be common, even in a foreign land; still more strange, as shown by a remark of M. Velpeau, that even so late as the year 1844, during a discussion at the Académie de Médecine, several medical men maintained that the innocent were undistinguishable from the malignant tumours of the breast.

* Sydenham Society's edition, p. 232. † Ibid., p. 245.

"It is impossible (continues Mr. Henry) to read over the admirable account of the chronic mammary tumour in Sir A. Cooper's work, which was published in the year 1829, without acknowledging that he has almost exhausted their history; and in all points, save in their microscopic characters, that he has anticipated what has been since said about them. One of the cases quoted in that book had been operated on by Sir Astley in the year 1815, whilst M. Velpeau's earliest case appears not to have occurred until the year 1824."*

The principal diagnostic signs of these tumours are, their mobility, their absence of adherence to the integument, a condition not met with in scirrhus, which is always more or less closely united with the skin and the mammary gland. The slowness of their development, at variance with the rapid evolution of encephaloid disease, which advances so rapidly to the surface, that it is not long of implicating the integuments; the absence of indurated glands in the axilla and in the vicinity of the breast; finally, the state of the general health, which continues unaffected, even where the tumours have attained a large size. These signs will enable the surgeon in most instances to form an accurate diagnosis.

M. Velpeau fully discusses the notion entertained by some surgical writers concerning the transformation of benign into malignant tumours. This notion, which was controverted by Mr. Pearson in his work on cancer, published in the year 1793, has been frequently urged as a pretext for resorting to the use of the knife in cases where no operation was required. Nervous women, terrified into the belief that some simple swelling in the breast, if not removed, will eventually become cancerous, submit to a painful mutilation, the result of which has sometimes been most disastrous. M. Velpeau states that M. Martin Solon, seconded by Blandin, published in 1844 the account of a tumour in a woman aged forty-five, which had remained in a benign condition nearly twenty years, and then became transformed into encephaloid. Similar examples have been reported by others, especially by M. Roux.

"To the first case two things may be objected: 1st, There is nothing to show the innocent nature of the tumour in the first period of its history. 2nd, Neither is there any absolute proof that it was really encephaloid at the time of its removal. To say that women advanced in life, that those who are married or have borne children, are more subject to cancer than to innocent tumours, does not authorize the conclusion that innocent tumours become cancerous in this class of persons. Moreover, adenoid tumours have remained innocent, as my observations show, to the very last, in a vast number of married women, mothers of children. If it be true that tumours which seemed to be innocent, have taken on the characters of cancer, at a more advanced period of life; after the cessation of menstruation, for instance, it does not by any means follow that they were primarily adenoid in nature. It may be that tumours cancerous in nature from the very first do not display themselves, or show their malignity, until after the lapse of a considerable time."†

After proceeding at some length to show that the differences are as well marked in respect to return as in all other points of view, between adenoid tumours and cancers, the author subjoins:

"Although hitherto the cancerous degeneration of adenoid tumours has never been demonstrated, I cannot absolutely deny the possibility of its occurrence;—some facts appear to me to call at least for reserve on the subject."‡

Now, we regard this as simply a gratuitous assumption. The facts to which he refers are chiefly the case of a tumour composed entirely of caseous material, ending by undergoing the cancerous transformation; that of another patient, in whom the tumour, under the microscope, was found to be cancerous in one half and simply adenoid in the other. We admit that the germ of cancer may originate and be developed in a tumour of an innocent nature; there may be interposition, but no transformation. Cancer is a disease *sui generis*. We deem the question, indeed, one of such importance in a practical point of view, that we venture to quote the passage of Mr. Pearson above mentioned, as well entitled to notice at the present day:

* Sydenham Society's edition, note, p. 281. † Ibid., p. 302. ‡ Ibid., p. 305.

"Writers have indeed said much about certain tumours changing their nature and assuming a new character, but I strongly suspect that the doctrine of the mutation of diseases into each other stands upon very uncertain foundation. Improper treatment may without doubt exasperate diseases, and render a complaint which appeared to be mild and tractable dangerous or destructive; but to aggravate the symptoms and to change the form of the disease, are things that ought not to be confounded. I do not affirm that a breast which has been the seat of a mammary abscess, or a gland that has been affected by scrofula, may not become cancerous, for they might have suffered from this disease, had no previous complaint existed; but these morbid alterations generate no greater propensity to the cancer than if the parts had always retained their natural condition. There is no necessary connexion between the cancer and any other disease, nor has it ever been clearly proved that one is convertible into the other."*

We therefore reiterate our conviction, that wherever a cancerous tumour is met with in connexion with a growth of a different nature, it has pre-existed, but in a comparatively quiescent state. In short, cancer is always an original disease, and never appears as the sequel of any morbid affection whatever.

A large proportion of the volume (260 pages) is occupied with the study of diseases of a malignant nature. Cancer in the breast shows itself in three principal varieties, as scirrhus, encephaloid, and fibro-plastic cancer. These seem occasionally to be associated together, but usually from the commencement to the termination preserve very dissimilar clinical characters. Melanosis, keloides, and epithelial cancer, are rare in this situation. Under the head of scirrhus, the author includes ligneous scirrhus, lardaceous scirrhus, diffused scirrhus, and scirrhus in the form of plates. All these are fully described.

In order to distinguish scirrhus from encephaloid, it is sufficient, he considers, to attend to two observations: the one is, that scirrhus almost always tends to involve the skin in its neighbourhood, from the moment that it attacks or invades the part; the other, that encephaloid pushes before it, and makes this membrane prominent in front, at the same time that it thins it, and endeavours to perforate or destroy it. (p. 365.) Some misconceptions which the author labours under with respect to colloid tumours and to epithelioma are pointed out by Mr. Henry.

We now approach a highly important subject—the diagnosis from microscopical anatomy. It must be allowed, that in as far as the blood is concerned, the researches of observers have remained as fruitless as those of chemists, in discovering a trace of the elements of cancer in that liquid. It may nevertheless happen that the so-called blastema of cancer may be circulating in the blood, and yet elude the subtlest means of investigation.

"In the meantime (observes M. Velpeau) we must thank microscopical observers for the exertions they have made in another direction, and for certain important results already attained by them. Their investigations have been directed to the intimate composition of tumours themselves. After much groping in the dark, and after oscillations which, perhaps, have not yet ceased, they have been enabled to demonstrate, in cancerous tumours, certain forms of cells, which are never, or at least rarely, formed elsewhere."†

After alluding to the inquiries of Gluge, Lebert, Robin, Broca, and others, he gives a *résumé* of their observations by M. Follin, who has accomplished his task in an able and complete manner. M. Velpeau agrees with M. Lebert that it is generally impossible to confound the cancer-cell with any other, when it is perfectly developed or unaltered; but it also seems to him, as to M. Virchow—

"That certain cells of epithelium—of pavement epithelium in particular—have in some cases a considerable resemblance to it; that the numerous alterations of its circumference, the variable number of its nucleoli, and of the granules which are infiltrated or contained within it, may in many instances render confusion easy. Moreover, we cannot help coming to the conclusion, that different microscopists are far from agreeing as to the characters and the nature of this cell. The opinion of M. Vogel differs markedly from that of M. Lebert, and M. Virchow, on his side, does not hold the same language as M. Müller. In France even some differences are already perceptible between the views of M. Follin, M. Kuss, and M. Robin, for instance, and M. Lebert, and M. Courtz."‡

* Lib. cit., p. 8. † Sydenham Society's edition, p. 398. ‡ Ibid. p. 399.

Velpeau then proceeds to detail instances of tumours of a malignant nature, in which the most expert observers were unable to detect the so-called cancer-cells, and likewise of those of an innocent nature, which teemed with acknowledged cancer-cells. We concur, therefore, in opinion with the author, that it would be imprudent—at least, up to the present time—to consider the cell, upon which so much stress is laid by microscopic observers, as the absolute characteristic of cancer. As far as concerns the demonstration of the normal tissues of healthy structure, the microscope is of great value, but with respect to morbid tissues it is far otherwise. In regard to the latter we must rely mainly on clinical tact and experience.

Cancer, according to M. Velpeau, results from a morbid exudation. This exudation, which is well marked, and he believes incontestable, in cases of encephaloid cancer, in cartilaginous cancer, and in fibro-plastic cancer, is less manifest, but still pretty well marked, in the greater number of cases of scirrhus. It seems to occur in two different forms; in that of deposit, and in that of infiltration. Is it not rather a cell-growth in a fluid or fibrous matrix, devoid of healthy plastic properties, evolved and interposed amid the tissues?

With respect to comparative diagnosis, special symptoms demand notice. One to which importance is attached, in the investigation of cancer, is pain. It is a common notion that a painless tumour of the breast cannot be cancerous. This is an entire mistake. Almost all innocent tumours of the breast are at times accompanied with pain; and women have been tormented for months, or even years, with acute pain at one spot in the breast, without the slightest tumefaction being discoverable. On the other hand, says M. Velpeau, enormous encephaloid tumours occasionally pass through all their stages without causing any pain to the women in whom they have occurred, and who, from this circumstance, scarcely considered themselves at all ill. Scirrhus itself, especially lardaceous scirrhus, and scirrhus *en masse*, often exists for several months without being accompanied by pain. Indeed, the majority of cancers are painful only at an advanced period of their development. The author considers the contagious nature of cancer, not as demonstrated, but as possible. As regards prognosis, he asserts that, abandoned to the resources of nature, cancer never disappears. Those who believe or affirm the contrary are mistaken. Their assertions depend upon errors in diagnosis, or, at any rate, from their confounding tumours of different kinds under the title of cancer.

> "A scirrhus, an encephaloid, a napiform or fibro-plastic tumour, or a well-marked epithelial or melanotic cancer, fatally follow their destructive evolution until the death of the patient. When once it has commenced, cancer never retrogrades." (p. 447.)

He properly repudiates the notion entertained by some pathologists, that there may take place in cancer a spontaneous cure through a process of retraction analogous to what occurs in the tissues of a cicatrix:

> "According to M. Virchow, the object of this process is to eliminate the cancerous material by a profound interstitial or molecular action, so as to cause the gradual disappearance of the tumour, and, in fact, to cure it. Any one who can satisfy himself with such notions, or fall into such strange delusions, can scarcely have followed the history of patients, or observed the course of cancers, otherwise than very superficially."*

We now arrive at the treatment of cancer of the breast—a subject which has engaged the attention of the professors of the healing art for centuries past. Incapable of spontaneous disappearance, endowed with destructive energy, cancer invariably terminates, sooner or later, by overcoming the powers of the constitution, and consigning its victim to the tomb. Hence the incessant endeavours that have been made to find a remedy for this ruthless malady. Have the lights of modern science brought to bear on therapeutic art at all deprived it of the attributes assigned to it by the Roman poet—

* Sydenham Society's edition, p. 448.

"Utque malum late solet immedicabile cancer
Serpere, et illæsas vitiatis addere partes;"

or does it still continue, wrapt in mystery, to baffle human skill, and constitute one of the most distressing modes in which man is doomed to "shuffle off this mortal coil?"

"Cancers of the breast," says M. Velpeau, "like cancers in general, have been treated by all sorts of remedies and applications. It would require a volume to name or simply pass in review what has been proposed for this purpose. Like most surgeons, I have, at first, admitted the efficacy of some of these measures, but, after submitting them to a rigorous trial in a great number of patients, I have come to the mortifying conviction that not one of them is capable of curing genuine perfectly characterised cancer. The contrary opinion is maintainable only in consequence of errors of diagnosis."*

With respect to general treatment, he has come to the conclusion that when the cancerous nature of the disease is well marked, we do not, up to the present day, possess a single remedy—a single constitutional or internal application—that has ever succeeded in curing it. His condemnation of external applications in the form of ointments, plasters, &c., is equally sweeping. As for compression, which he has employed in the treatment of cancer of the breast, "with a hearty and sincere desire of finding it efficacious," he merely found that

"It may flatten or depress the tumours in the midst of the tissues, or into the intercostal spaces, and thus in some measure mask their existence, which no doubt has imposed on some persons, but it never leads to their resolution." (p. 460.)

Compression may induce absorption of effused fluid and of fat, and thus determine diminution of the swelling, but can exercise no material influence on the disease. The notion of its favouring the disintegration of the cancer cells, leading to their re-entrance into the blood, and their subsequent excretion through the emunctories, is simply a pathological chimera.

Under the head of surgical measures, the author manifests the utmost reluctance to admit that cancer is an affection primitively constitutional, or depending on a pre-existing diathesis. We would willingly, for the sake of humanity, that an opposite doctrine could be substantiated. M. Velpeau adduces, in corroboration of the above view, the results of cases in which he had performed amputation of limbs for tumours seemingly malignant, and where no recurrence of the disease followed the operation; but these, as Mr. Henry points out, belonged, in all probability, to the class of myeloid or myelocystic tumours. He further refers to cases of genuine cancer, in which a considerable length of time has elapsed since the removal; but these cannot be accepted as instances of definite cure, for we know that many years may in some instances intervene between the date of the operation and the reappearance of cancerous disease in its former seat.

We agree with Velpeau that statistics are of very little value; and, upon his own showing, it is evident that his grounds for forming any valid opinion as to ultimate results are imperfect. He says:

"I have certainly seen more than a thousand cases of tumours of the breast. It is certain, however, that I have only been able to trace a very small number to the end. There is a large proportion of them, both public and private, which were seen only on one or two occasions. . . . . . The same difficulty existed in those cases in which I have had to perform an operation. When once the tumour has been removed, and the wound has cicatrized, the surgeon and patient readily lose sight of each other. Many of mine came from the country for the operation, and returned when it was completed. Some, in fact, did not reside or remain in France, and those who lived in Paris it was not always easy to see again."†

M. Velpeau lays it down as a rule, that scirrhus in plates, whether scattered or in one mass, should never be submitted to operation; the like applies to tubercu-

* Sydenham Society's edition, p. 453.

† Ibid., p. 489.

lar pustular cancer, whether discrete or confluent. Stony scirrhus *en masse*, and diffused lardaceous scirrhus, stand in precisely the same position.

> "Whenever the scirrhous mass and the skin are confounded together in a diffused form, when the scirrhus is rather protuberant than depressed, and the tegumentary degeneration has no appreciable limit, and the whole mamma is more or less thickened, there is also nothing to be hoped for from the operation." (p. 497.)

He points out other varieties where the use of the knife is inadmissible.

With respect to the risk of danger, M. Velpeau has had 32 deaths in 167 operations—that is, about one in six. It may, we think, be laid down as a safe rule, that where the tumour is circumscribed, making slow progress—where the lymphatic glands in the neighbourhood are unaffected—and the general health of the patient good—resort may be had to the operation. We thus, in the majority of cases, procure the patient respite from pain, remove a source of continued anxiety of mind, so as, at all events, for a term of years, to enable her to enjoy a fair share of health and spirits. The withdrawal of the morbid mass, moreover, may, in a great measure, prevent a painful, a loathsome, and a lingering death.

Velpeau advocates early operation. So soon as the tumour declares itself, and he recognises the characters of genuine cancer, he ceases to attempt its resolution, and recommends its removal—in other words, does not believe that we are justified in submitting tumours of the breast to curative treatment, from the moment we can with certainty discriminate their cancerous nature. Some ten pages are occupied with details of the operative proceedings. Among the untoward results which occasionally ensue, are death from inexplicable causes—pleurisy, phlebitis and purulent infection, cancerous infection, erysipelas. The last is unquestionably the most common accident after the removal of tumours of the breast—a dangerous malady, which, besides favouring pleuritic effusions, may give rise to peritonitis.

The author is disposed to view the employment of congelation in a favourable light. Anaplasty he denounces as wholly valueless.

The question of caustics is duly considered. He thinks that they preserve the lymphatic glands from secondary cancer more than extirpation. At all events, he noticed this on two occasions. After pointing out some of their disadvantages, he says it cannot be denied that they possess some advantages.

> "As they do not give the idea of an operation, they are less terrifying to the patients, and their application will be permitted much more cheerfully, and with infinitely less effort, than the knife. As they destroy the tissues bit by bit, they occasion no loss of blood, and they disturb the system less than operation properly so called. Patients who submit to this treatment are not compelled to keep their beds, or to consider themselves ill. The after-dressings require less care, and do not absolutely call for the assistance of the surgeon. The wound generally becomes cleansed rapidly, after which cicatrization proceeds without delay. Without also obviating erysipelas, phlebitis, or purulent infection, as has been stated by some surgeons, there is nevertheless reason to believe that they are somewhat less liable to these unfortunate complications than cutting operations." (p. 549.)

Caustics he deems preferable to the knife, when the cancer is ulcerated in patches, and is rather sprouting than narrowed; when, even with the knife, it would be impossible to preserve a portion of the integuments of the part invaded by the tumour; in all cases in which the cancer is fungous, circumscribed, and the patient dreads the knife; in all ulcerated, cavernous, and disseminated scirrhus; and in ulcerated cancers, which are adherent to the summit of the axilla, or to the clavicle, or extend to the neighbourhood of the bones.

The caustics principally in vogue are arsenic and the chloride of zinc; sulphate of zinc, lately recommended by Dr. Simpson, constitutes, according to Mr. Spencer Wells,* the active ingredient of a nostrum employed for the cure of cancer by a Dr. Pattison. Arsenic has been largely used by cancer-curers, but the great

* Medical Times and Gazette, July 11th, 1857.

risk of danger from absorption, and which has occasionally led to the death of the individual, has caused surgeons to be very chary in employing so potent a poison. We have certainly found it useful in small cancerous sores upon the face, applied according to the formula of M. Manec. Chloride of zinc is a caustic which has rapidly acquired a certain reputation. It was employed by Hancke of Breslau, by Canquoin of Paris, and extensively in this country by Mr. Ure, who published a series of researches on it, several years ago, in the seventeenth and eighteenth volumes of the 'Medical Gazette.' Mr. Ure used it mixed, in various proportions, with anhydrous sulphate of lime. He says,

"The superiority of this phagedænic paste (for so the preparation may be justly named) over every other caustic, consists in its susceptibility of being applied over very extensive surfaces without any risk of injury from absorption, and in its being available wherever the surgeon's hand can reach. The depth to which it will corrode the morbid texture can always be estimated beforehand; its action is unfailing; the separation of the eschar is prompt; and it imparts an excellent character to the sore, and soundness to the suppuration. The favourable modification of the tissues, the rapidity with which cicatrization follows, and the mildness of the general phenomena that accompany its action, are additional recommendations."*

We have introduced the above quotation, because this caustic has lately gained considerable notoriety on account of its forming the basis of a remedy, till lately secret, employed in this metropolis by a Dr. Fell of New York. The secret, which was oozing out, has at length been divulged by the Doctor, in what he calls a 'Treatise on Cancer and its Treatment;' the object of which is to bring before the medical profession a new, and what he believes to be "an entirely original mode of treating the disease of cancer in all its various forms." (*Preface.*) We learn from the same source that Dr. Fell received a communication from the Board of Governors of the Middlesex Hospital, respecting his making a trial of his method of treatment in the cancer wards of that charity on the following terms:—

"1st. That Dr. Fell should, in *confidence*, communicate to the surgical staff the nature of the remedies employed by him, the method of their preparation, and the mode of using them. 2nd. That twenty-five cases should be subjected by him to treatment during a period of eight months. 3rd. That Dr. Fell should pledge himself to publish the full particulars of his system of treatment within a period of six months; and that in case of his failing to do so, that duty should be undertaken by themselves." (p. ix.)

Now the gentlemen forming the surgical staff allow that Dr. Fell has fulfilled the obligation contracted by him, frankly and without reserve. After some commendatory observations on the plan of treatment, and its immediate results, these gentlemen conclude their report as follows:—

"That the undersigned have not as yet had time to ascertain the average duration of the benefit conferred by the treatment, nor have they any means of knowing whether, in the event of a return of the disease, there be any difference observable from what is known to take place after excision."

Be it remembered, the first applications were made to patients in the hospital on the 22nd of January last, and that the Report bears the date of the 18th of March, so that the period of trial did not extend to nine weeks. We took up the work with avidity, but great was our disappointment to find the preliminary portion a mere *réchauffé* from the writings of Walshe, Paget, and others. At length we arrived at the plan of treatment thus magnanimously disclosed to the surgical staff of a metropolitan hospital, our expectations being raised to a high pitch by the astute remark of the Doctor: "any man who, at the present day, can believe that because a cancer is removed it is cured, is worse than simple." (p. 56.) Assuming this to be Dr. Fell's creed, let us briefly inquire into his curative means. There is a plant, he tells us, whose large white blossoms and snow-white flowers

* Lib. cit., p. 392.

enamel the wild forests and plains of the Far West, and whose bruised stem exudes a red blood-like juice, recalling Virgil's account of the myrtle by the tomb of Polydorus, "huic atro liquuntur sanguine guttæ." It is called by botanists Sanguinaria Canadensis, by the Indians puccoon. The Doctor found this plant capable of removing tumours and effecting a cure, but requiring months of continued application. He therefore deemed it expedient to quicken its action by the addition of chloride of zinc, employing a considerable dash of this powerful escharotic, as may be gathered from the subjoined formula.* As a preliminary step in cases of non-ulcerated tumours, he destroys the cutis by nitric acid, and then applies the paste; but finding the eschar produced by each application so thin as to require a long time to get rid of a large tumour, he resorts to incisions through the eschar about half an inch apart, avoiding the living tissues, and into these inserts daily the paste spread on strips of linen. He asserts,

> "That although the action of the puccoon was much hastened by the addition of the zinc, yet it was slow enough to allow its complete absorption, thereby enabling it to exert its peculiar constitutional effects, and at the same time removing the diseased mass in a few weeks." (p. 59.)

In certain cases he employs an ointment containing sulphate of zinc, alternated with one containing iodide of lead. He exhibits internally iodide of arsenic, as suggested by the late Dr. A. T. Thomson.

We maintain that the whole efficacy of Dr. Fell's treatment is due to the chloride of zinc; the only originality consists in scoring the eschar, and thus allowing the liquefied chloride to percolate through to subjacent morbid textures. We consider the puccoon to have no more virtue, as far as cancer is concerned, than so much licorice powder. Not being imported into this country from the "Far West," it served the Doctor as a convenient vehicle for blinding some of his credulous visitors.

Those of our readers who desire further information respecting cancer-curers, past and present, we would refer to Mr. Spencer Wells' ingenious and interesting Lecture, already quoted.

M. Velpeau, in a few pages at the end of his work, treats of diseases of the male breast. He expresses a belief that the extirpation or destruction, by caustics, of cancer in the mamma, affords a better chance of success in the male than in the female.

Review VI.

Practisches Handbuch der Gerichtlichen Medicin. Nach eigenen Erfahrungen von Johann Ludwig Casper. Thanatologischer Theil. Mit einem Atlas von neun colorirten Tafeln.—*Berlin*, 1857. 8vo. pp. xxxi. 860.

Practical Manual of Forensic Medicine. From Personal Observation by Johann Ludwig Casper. Necroscopic Division. With Atlas of nine coloured Plates.

The name of Dr. Casper has been long familiar in the literature of medicine. It is now more than twenty years† since, in speaking of his "Weekly Journal of Medical Science," we accounted for the value of that periodical by referring, as a leading cause, to the great talents and respectability of its chief editor. Then already extensively, and by no means newly, appreciated for his successful efforts for the advancement of science, Dr. Casper has never since relaxed in his exertions; and he has remained too conspicuous among the strenuous cultivators of medicine in his own country not to have frequently occupied a place in our pages.

* "℞ Sanguinariæ Canadensis, ℥ss vel ℥j; chlor. zinci, ℥ss vel ℥ij; aquæ ℥ij; pulv. sem. tritic. Hib., q. s. Mix, and form a paste the consistence of treacle." (p. 59.)

† Brit. and For. Medical Review, vol. iii. p. 461.

For an extensive series of years, it has been chiefly to points of statistical inquiry, or still more in relation to medico-legal topics of investigation, that he has directed his attention: while, with reference to the latter especially, it has been the advantage of his position, through the strict organization of the criminal code of Prussia, to have enjoyed peculiarly ample opportunities for observation, sustained, wherever necessary, by the most efficient co-operation on all points of more exclusively technical inquiry on the one hand, and constrained into careful elaboration by a system of organized checks and revision on the other; and thus singularly favourable, because admitting the greatest possible scope for the due application of the existing resources of science, while presenting the greatest devisable resistance to the ingress or the persistence of error.

It becomes daily more and more a matter of duty and necessity, to examine closely into the position of those who venture to proffer instruction to us, in a science so vital in its interests as that of medicine; because the easy opportunities, now presented everywhere by a teeming press, almost invite to the promulgation of opinion, however hasty and crude its formation, and however scantily based on reflection and experience. An eager passion for competition, more clamorous for notoriety than solicitous for repute, rushes prematurely into the field; and, with rash statements, the intrepidity of which secures a temporary assent from the unthinking, and crude methods of treatment, whose novelty obtains the plaudits which should have been reserved for their fitness, the way is prepared for that wearying task of the reflecting physician, which forces him too often to occupy himself more with the demolition of error than with the establishment of truth. It is, therefore, with a sincere respect that we receive the present summary of the long experience of a well-occupied life, bearing on a large department of that science of forensic medicine which the author has cultivated with so distinguished success. It may be comparatively a slight matter that he has been a public teacher of the science for a period of upwards of thirty years, because Adolph Henke, the twelfth edition of whose "Manual of Legal Medicine" now lies before us, taught it also for a still more ample period, yet was, after all, but a judicious commentator on the opinions of others, and was himself actually destitute of a practical knowledge of its subjects. But when an author illustrates his book with the present details, always pertinently, and often minutely recorded, of 346 medico-legal cases which he has selected from those which have come under his own immediate observation; when he refers, under one important department, to his having examined 1605 bodies of children, either still-born or who had perished shortly after delivery; when he speaks of the actual dissections of such infants as constituting only a fourth of his general experience in obductions; when we learn that, as the medico-legal physician of an extensive capital, all its crimes of violence, and its physical disaster, have been subjected to his inquiry for a time nearly as ample as that of his experience as a lecturer; and when we add to this that, as a member of the Supreme Medical Board for Prussia, he takes a share in the supervision of all reports and protocols of whatever case of importance may occur throughout the rest of the country, we cannot be otherwise than conscious of his right to instruct us, and we almost feel even the impossibility of looking elsewhere for the results, or at least for the opportunities, of an equally extensive and discriminating knowledge.

Dr. Casper has grouped his subjects under two main divisions—a general and a special. Under the *general* division he embraces a short view of the aims of medico-legal obductions; first limiting the legal definition of a dead body by briefly setting forth the conditions of viability. He then enters upon the discussion of the actual signs of death; upon the question of the priority of the fatal issue where more than one individual has been involved; and upon an examination of the outward and inward phenomena, whether merely of universal occurrence, or as demanding to be contrasted with others of special import, because indicating a peculiar cause or form of extinction. Under these appearances are included the condition of the blood within the vessels, the period and degree of cadaveric rigidity, and the phases of the subsequent process of putrefaction, with its modifications and varying grades of rapidity, in relation to the individual organs, to the constitution of the body, or to the position and circumstances of its exposure. After a few general considerations as to the secondary causes of death, he enters more immediately upon the subject of necroscopic examinations, considering first those necessarily undertaken upon bodies in an advanced stage of decomposition. Proceeding to the more ordinary cases, and pursuing the usual course, he indicates carefully the various points to be noted with reference to the external surface of the body, whether to identify the individual, to detect marks of violence, or to throw light otherwise on the topics to be investigated. He then considers the different possible modes and instruments of injury, and the information to be sought through an examination of the apparel of the deceased, and the constituents of any matters with which it may be stained. The internal inspection of the body follows; and he concludes with an account of the method of framing the protocol, as well as the consequent report, with its detailed conclusions, for which the former is the authorized foundation. The whole is accompanied by an ample exposition of the different regulations of the legal authorities, fixing the strict order of these proceedings; and he further introduces a few important hints as to the conduct of oral examinations. All protocols of such examinations, as we may remind the readers of our last October number, and all reports of the Prussian forensic physicians, are transcribed, and, without exception, transmitted for revision, first from the local to the chief provincial authorities, and thence to the provincial medical board. Finally, they are submitted, with the remarks of the latter, to the supreme State department; by a scientific commission of which the whole are subjected to a supervision, the result being communicated back to the subordinate officials. Hence a certainty and stability of decision and practice of which we discern plainly the beneficial results, and which we must look for in vain under what has been hitherto our own more faulty and fortuitous system. It is scarcely with more than an allowable satisfaction, therefore, that Dr. Casper quotes the testimony of Mittermaier, that the practice of medical jurisprudence in Prussia has reached a higher standard than in any other country.

In the *special* division of his subjects, Dr. Casper includes a particular account of the different modes and descriptions of violent death; taking care throughout to point to the necessity of vigilantly distinguishing between the proper and natural appearances resulting after death, from whatever internal cause, or through the more or less advanced stages of decomposition, and those really dependent upon extraneous lesions. Under a first sub-division he places the kinds of death through mechanical injuries, such as severe blows, falls, concussions from objects in motion, crushing by machinery, &c. Then follow wounds by fire-arms and death by burning. Under a second sub-division are ranked the more dynamic forms of death, as by loss of blood and exhaustion, starvation, poisoning, suffocation, hanging, choking and strangling, drowning, excessive cold, and the employment of anæsthetics. An interposed portion treats of death from medical mal-practice. He concludes his volume with a consideration apart of the subject of infanticide, regarding which he enters into copious and valuable details; thus completing a monograph which may be regarded, in so far, as perfect in itself, but which we are glad to learn is designed to be followed by another part, so as to constitute ultimately a comprehensive work, embracing the entire field of forensic medicine. The volume before us he has designated as the "thanatologic" division, because confined to the examination of the dead body, and of inanimate matters: that which we are promised is to discuss all questions relating more especially to the living individual, and will appear as the "biologic" portion.

It is of course impossible to present a detailed analysis of the contents of a treatise so voluminous, and so replete with matter, as that before us. To snatch a portion here and there, whether because it conveys some important instruction, or suggests some cursory animadversion, will be all which our design contemplates, or which our limits will admit. Neither is it necessary that we should seek only for novelty, which, if it be always attractive, is not less generally precarious. A confirmation of the most familiar truth is not without its value, when it proceeds upon an enlarged experience; and science has alike a gain, where such an authority as that now before us confirms the recent doctrine which was yet but waveringly established, or rejects the opinion which had been imprudently accepted without its fitting warrant. The whole tenor of the work of Professor Casper has the evident, and, we fear we must add, the somewhat rare, merit of evincing a greater regard for truth and justice of doctrine in themselves, than as the means of fabricating a reputation for their exponent. We are never sought to be surprised by a hardy innovation, or startled by a rash hypothesis, framed for the public wonder; yet, in the midst of all this forbearance from self-seeking, there is no want of a due consciousness of what ought to be the value and authority of his opportunities and experience.

In discussing the significance of the signs of death—a topic of importance everywhere, but which had at one time acquired a special intensity of interest in Germany, from the then generally diffused terror of premature interments*—Dr. Casper refers, in terms somewhat too absolute, to the altered transparency of the eye. It is familiar to every one, that in nearly all cases of instantaneous death, where the circulation and the resulting vital actions have undergone no gradual modification before the fatal termination, the eye retains for some time its almost natural brilliancy. We have had occasion ourselves, like most others, to remark this strikingly in cases of suicide by hanging, and by prussic acid: and it may be recollected, that the often-observed glistening look in poisoning by the latter was some years ago adopted by a notorious medical speculator as an appropriate analogy, that bane of shallow reasoners, and consequently as a just ground, for resorting to the employment of its vapour in certain descriptions of blindness. We need not add that he was not destitute of his group of intrepid believers, who lauded his ingenuity, as they attested his miracles, during the usual period of attraction of such prodigies of rationality. Dr. Casper points out that there are individuals of more than ordinarily vivid complexion, in whom the pallor of death also is more slowly established than in others; and he justly directs attention to the various grades of persistence of the temperature of the body, according to the measure it possessed at the period of dissolution. In sudden death, as the heat is in greater quantity, so it is longer retained: in gradual death it is already largely dissipated before life has ceased. In referring to the process of decomposition, the last and decisive proof of the close of existence, he details the various conditions under which putrefaction may be either accelerated or retarded. Those who perish suddenly, and with the body full of juices, decompose more promptly than the extenuated victims of a lingering disease; and the author hence especially refers to the bodies of women who have died in childbirth, from whatever cause, as uniformly prone to pass into rapid decomposition. But this can scarcely be said, with any special truth, of those at least who have perished from profuse uterine hæmorrhage; just as extensive loss of blood, generally ensuing from any description of disease or from violence, by draining away the fluids, places the body in a state analogous to that condition of extenuation by which the putrefactive process is checked.

Facts of this kind may often assume a character of essential importance in medico-legal investigations. A case presented itself, in which a female of the middle classes, who had previously been known to have exhibited signs of melancholy, had attempted to stab herself in the region of the heart; and afterwards, passing into another apartment on a different floor, had succeeded in terminating her

* Dead-houses, with their apparatus of nicely-adjusted levers,-wires, and bells, responding to the slightest movement, were constructed at Weimar, Dresden, Erlangen, Halle, Leipzig, Frankfurt, Munich, Bamberg, and many other places in Germany; but no record tells us that, even in a single instance, these had startled the watch by any sound, or other token of re-animation of the corpse to which they were skilfully attached. The idea of an apparent death, and of a re-animation in the grave deprived of the access of air, becomes, after this experience, a subject for the most extreme credulity, and not for reason.

existence by cutting her throat, the wound being accompanied with the usual excessive hæmorrhage. On the instant that she was discovered, a neighbouring surgeon was called in; an act of suicide was said to be recognised, and interment took place after the usual interval. Some time afterwards, however, it was found that the surgeon had been expressing his conviction that the heart had been penetrated; and the question arose, whether a person, having wounded her own heart, could proceed down a staircase to another room, and complete destruction by a new injury, or whether the case was one of murder. A disinterment was ordered by the authorities, and the case was committed to ourselves and to an eminent colleague for investigation; when, notwithstanding the lapse of a fortnight, in the height of a warm summer, we found the corpse nearly untainted, presenting no obstacles to the most searching examination, and admitting us easily, among the rest, to reach the conclusion, that there existed nothing to impugn the idea of an act of suicide. The wound of the chest had not even penetrated the pericardium.*

It is worth noting here, that our author has repeatedly remarked a proportionately great retardation of the process of putrefaction in those who had perished during fits of excessive drunkenness. Keeping in view such facts, while guardedly attending to the condition of the body as to exposure and other circumstances, there will always be difficulty in determining the probable period of decease from the state of the remains; yet it will be frequently approached with considerable accuracy, and, as an occasionally paramount question, will demand the scrupulous care of the jurist. With reference to the influence of different media, Dr. Casper thinks himself justified from his experience in proposing the approximative rule, that, with corresponding average temperatures, the results of one week or one month's exposure to the air are equivalent to that of twice such lapse of time under the surface of the water, and to eight-fold that where a corpse has been deposited in the earth.

The observations of the author as to the manner of making necroscopic examinations, whether with reference to a late or early period after death of their performance, or to the other special circumstances of an individual case, are exceedingly interesting and valuable; and, like all his other opinions and rules of procedure, are well illustrated by appropriate examples. The authoritative directions under which such proceedings are regulated by the department of police, and by the administration, are presented in full detail; and it would be well if we were enabled to refer in this country to any body of rules so judicious, because fitted to give that precision and uniformity to our practice, the want of which has subjected us abroad, and often justly, to severe animadversions. Under these regulations it is directed, among other prudent injunctions, that even where a sufficient cause of death has been discovered in one portion of the body, the other divisions must still be opened and examined also. The value of this rule is obvious, because it prepares for us a double surety, by demonstrating not only an efficient cause of death, but, if that have been really an individual and sole cause, by definitively excluding the existence of all others. The regulations prescribe, besides, the scrutiny of all apertures, channels, and cavities of the body, to determine the possible presence of foreign substances, of whatever description; and all spots of lividity are to be tested by incision, to distinguish absolutely between those which result from blood which has been actually extravasated, or which has merely gravitated, or otherwise become accumulated in the vessels. The order of procedure in the necroscopy is strictly defined, and general rules are laid down for the framing of the protocol of the merely descriptive *res gestæ* at the time, as well as for the preparation of the definitive report afterwards, it being requisite that the latter should be so reasoned and expressed as to adapt itself to the comprehension of persons beyond the profession.

The rich store of experience by which Professor Casper exemplifies the applica-

* Casper (p. 536) introduces a case which he believed to be an undoubted suicide, where a female had stabbed herself in the left side, penetrating the pericardium twice, and slightly touching the heart, and who had afterwards completed destruction by hanging herself in her shawl.

tion of these regulations, the candour with which he gives weight to difficulties inducing uncertainty, and the caution with which he uniformly arrives at his conclusions, or scrutinizes his doubts, are exceedingly instructive. Where his conviction is firm, it is expressed without hesitation; where it is necessary to limit its import, he points to that necessity; and where the grounds for a decision are wanting, he confesses his incompetency. It may be easier to pursue this course in Prussia than with us, where an able, but on such points an imperfectly instructed, newspaper press rushes along with the popular sympathies, judges nearly every important case before it appears in court, and is ever ready to brand the medical jurist for the imperfections of his science or of his judgment, when the defect was but in his means. It is easier also for one in the position of Professor Casper, than for by far the greater portion of those who appear as medical witnesses in our own courts. But, be this as it may, it is the only wise and conscientious course for the witness; and it is one from which no vanity of skill should tempt the experienced to verge a tittle, and no publicity or prodigality of taunt, or anticipation of applause, incite the comparative novice to a like default. To the jury and to the public the accused is an identity; to the forensic physician he is merely an abstraction; and it is with the crime alone that science has to concern itself. There thus arises a fence against all surmises and prepossessions, and against all imaginary views as to modes or motives of perpetration. Such counsel may be trite, but we have scarcely yet reached the period when it can be accounted superfluous.

In the course of his remarks upon the inspection of the dead body, Dr. Casper justly points to the difficulty, if not the occasional impossibility, of discriminating between excoriations produced after death, and those inflicted during life. He even insists, and doubtless with propriety, upon the difficulty which may occasionally present itself in determining whether a wound had been received during life, provided it had been inflicted so immediately before death that there was no sufficient interval for the establishment of a re-action. In his illustrations of the effects of different instruments of violence, he mentions the instance of a quarrel between two drunken rustics (Case 56), in which a blow was inflicted by a scythe, causing a wound eight inches in length, which commenced in the region of the lower false ribs on the right side, and passed outwards to the spine, penetrating the chest without having wounded the lung, and terminating in death; yet where the circumstance of the intoxication of the perpetrator was, contrary to the rule of our law, which protects the drunkard for his civil but not for his criminal actions, admitted as a palliation of his crime, and the punishment was restricted to a two years' imprisonment.

At a later portion of his work, Dr. Casper alludes to the risk attendant upon inferences attempted to be drawn from comparisons between the results of apparently equivalent violence, inflicted upon the living or the dead body. Thus, if we attempt to injure the skull of a dead adult, we shall find that the blows which in the living would unquestionably have at least produced a fissure, if not a fracture, or even a complete comminution, will leave the dead cranium unharmed. In the same way our author, while he unhesitatingly admits the occurrence of rupture of the structures of the larynx as an occasional result of manual strangulation in the living, has found himself unable to break either cartilage or hyoid bone in the dead body of the adult, under the most powerful pressure; whence, thus judging by his own experience, he does not hesitate to vindicate the conclusion, that wherever a fracture of these parts is discovered after death, and the tissues have passed into such a state of decomposition as to render otherwise the evidence of a vital re-action inaccessible, the very existence of the fracture must alone be held to give assurance of an injury inflicted during life. Though this may possibly be said with nearly the truth, and in the concrete, of every actual example in practice, still that it has been really advanced too absolutely is shown by the well-devised experiments of Dr. Keiller,* who succeeded in fracturing the larynx, both by severe pressure applied from before backwards, and by violent lateral com-

* Edinburgh Medical Journal, December, 1855, p. 527, and March, 1856, p. 824.

pression through manual grasping. Under such circumstances, moreover, it is to be remembered that it is possible that a fissure or rupture of one or other of the cartilages, though really existing, may escape detection, especially where there has been no osseous transformation, owing to the exceedingly resistent perichondrium retaining its integrity and concealing the injury, as we have had occasion to remark in trials made by ourselves. Remove the perichondrium, however, and the disruption becomes apparent. It is obvious, therefore, that a close scrutiny may be sometimes requisite, in order to detect a rupture of the larynx; and that without such a scrutiny it is not prudent to deny its existence, and so to exclude the important inferences which its presence would enforce. A further extension of such researches into the vulnerability of the dead body has shown also, that missiles from fire-arms penetrate into them with less ease than through the living tissues.

The remembrance of the remarkable case of the Countess of Görlitz is still too fresh with the chemists and medical jurists of Germany to permit our author to pass over, without a brief discussion, the question of the possibility of a really spontaneous combustion ensuing in the human body. The solution at which he promptly arrives is to regard the allegations of such cases as ridiculous fables, a belief in which, in any one of scientific pretensions, would be little creditable to the enlightenmennt of the nineteenth century. Yet admitting unreservedly, as we do, the justice of this conclusion in its absolute sense, we still think that Professor Casper joins too readily with Liebig in assigning so unconditional a value to the experiments and reasonings of that distinguished chemist. Liebig,* in repelling rightly the notion of a spontaneous combustion, and rejecting it as a plea in the special case of the Countess, attaches too slight importance to the relative fact of the possibility of a contingent condition of at least an increased combustibility of the organized tissues; the occasional development of which, and the remarkable results to which it has led, in the event of the implied application of fire through casualty from without, but with the presence of singularly limited amounts of extraneous materials, has been verified by too many recent observers, including a Dupuytren and a Devergie, to admit easily of challenge or cavil. We have already seen that Professor Casper himself records, as his not rare experience, that the bodies of drunkards perishing in an excessive debauch show an extraordinary resistance to the progress of decomposition; and he agrees with Percy, as with the more recent observations of Buchheim † and Duchek,‡ in asserting the customary imbibition of alcohol into the tissues. "Here," he remarks (p. 36) of such cases, "the whole body is, as it were, immersed in spirits." Let us add to this the equally often recognised existence of the fatty conversion of the tissues, so conspicuously observed in the bodies of drunkards, and we have the conditions for an increase of combustibility to which we cannot reasonably hesitate to assign consequence, and the coincidence of which, in an individual case, we are entitled to expect. Dr. Casper relates a fatal case (No. 128, p. 342), in which a man of eighty-three years of age had been sitting in front of a stove, when his clothes had apparently caught fire, and were found thoroughly consumed. The corpse of the man lay in a bent position, and was completely charred, with the exception of the lower extremities, which were scorched to a dark brown, but not incinerated. The back was especially consumed, so that the body broke asunder in attempting to raise it. On the right side, the cavities of the chest and abdomen were laid open, and the seared liver was plainly visible. No further examination was attempted, and nothing is told of the previous history of the sufferer, deficiencies which are to be regretted. But let the circumstances have been what they may, such a case has a bearing on the general argument of Liebig, and on his calculations as to the large amount of combustible matters absolutely necessary for

* Our acquaintance with the disquisition of Liebig is through its Dutch edition: Is de Zelfverbranding van het menschelijke ligchaam al of niet mogelijk? Haarlem, 1850.
† Deutsche Zeitschrift für die Staatsarzneikunde, Band iii. 1854, p. 381.
‡ Prager Vierteljahrsschrift, Part iii. 1853; Annali Universali di Medicina, vol cli. 1855, p. 89.

the consumption of the human body, which must show the propriety of limiting his conclusions, and which we are surprised that Dr. Casper has not seen cause to make the topic of animadversion. We may mention here, not certainly as an illustrative, but as a readily associated idea, that the author relates a case (No. 141, p. 354) where he took occasion, in the body of a female at an advanced stage of putrefaction, and with the abdomen largely inflated, to repeat an experiment which he had often previously tried, but in this instance with a rare degree of success. On making a minute puncture into the abdominal cavity, and applying a light, the issuing gas caught fire, and burned for a couple of minutes with a bright flame two inches and a half in length.

Although Professor Casper conveys much important information on the subject of poisons, there are obvious reasons why his plan should have excluded the full consideration of that extensive and complex body of science which has now given to toxicology so eminent a position among the departments of medical knowledge. Yet, in this very extent and complexity it is that we have the sources of those difficulties, which the experienced forensic physician approaches always with a deep sense of doubt and responsibility, and which are only glibly and peremptorily discussed by the voluble lawyer for the purposes of a defence, or by the newspaper writer whose prestige is in his assurance. By the rule of practice in Prussia, the chemical investigations in cases of poisoning do not rest in the hands of the ordinary medical jurist, as one of the many departments of inquiry with which he is expected to be conversant; but, in consideration of the intricacy and peculiarity of the manipulations, they are required to be committed to chemists habituated to such researches, yet still acting in each case under the general superintendence of the proper forensic physicians. Hence it is that Dr. Casper, like the generality of German writers, has left the full discussion of the appropriate methods of investigation to the works of the special medico-legal chemists, and the consideration of the wide varieties of the qualities and actions of poisons to the special toxicologists, contenting himself with merely the broader classification and the more general results. Our author does not insist upon the uniform detection of the deleterious substance by the chemical test; but, while he awards to this its great and due importance as a criterion, he asserts its frequent unattainableness, and claims for the morbid symptoms, the necroscopic appearances, and generally the complexion of the external circumstances of the sickness and death, a not rarely perfect efficiency for the substantiation of the proof. On the other hand, he naturally does not fail to advert to the possibility of various poisons, as arsenic, morphia,* &c., having been introduced medicinally into the system; and even, in the form of secret nostrums, fever-drops, &c., without the knowledge of the medical attendant. Singularly enough, sulphuric acid appears to be the poison most ordinarily employed by suicides in Prussia:† while for murder its intense causticity renders it unavailable, unless in young children, for whose destruction it is frequently resorted to. Chloroform has been used as a means of wilful poisoning. In March, 1856, a dentist of Berlin, reduced to extreme penury and distress, thus destroyed at Potsdam his wife, his two children, of ten and eight years of age, and lastly himself.

Of poisoning by phosphorus, usually procured as an ingredient of a paste sold as a rat-poison, and similarly employed in this country, Dr. Casper relates several remarkable instances, powerfully illustrating the peculiar and super-eminently deleterious qualities of this substance. Colchicin, familiar to us as a poison as well as a medicinal agent of great power, evinced its energies, within the author's experience, in the form of a very singular and equally decisive retribution. Four housebreakers had obtained possession of a large bottle of the tincture of colchicum

*** No resource in medicine is insignificant which has a distinct practical result, if through a means more efficient, yet less harassing, than others in ordinary use. We may here notice that, in following the usual injunctions to prevent sleep in narcotic poisoning, we have tried profusely the cold affusion, held strong ammonia to the nostrils, or even to the eyes, or placed vinegar between the eyelids, and pinched sharply under the nails, with other modes of benevolent torment, but with none so successfully or with such trifling and easily obviated inconvenience, as by the smart spirting of water into the face from an ordinary syringe.**

† Hanging is the most common mode of suicide, generally, in Germany.

seeds, and, taking it for brandy bitters, swallowed each a wineglassful. One perished the same evening, under violent diarrhœa; and the three others on the second day afterwards, all similarly affected. The catastrophe recals an event in our own early experience, but one with a happier issue, where an Irish reaper, returning from labour, found under a hedge a bottle containing a transparent liquid. It held a saturated solution of corrosive sublimate, placed there, but insufficiently concealed, by a shepherd, who had used it as an external application in the care of his flock. Mistaking it for water, or more probably for whiskey, the man, parched with thirst, gulped down a considerable quantity; and was instanty thrown into intense suffering, with agonizing pain, vomiting of a frothy fluid tinged with blood, extreme depression, and the other usual symptoms of this description of poisoning. The albumen of half a dozen eggs, mixed with water, and given in two successive portions, according to the plan of Orfila, seemed to afford the most prompt and conspicuous relief, though other remedies were not neglected, and the man speedily recovered. On the second day, the tongue, palate, and fauces were covered with membranous sloughs. It was difficult to estimate the quantity of the poison swallowed, but it was supposed to have been somewhere about two drachms. The author, with a degree of seeming incongruity which it is scarcely possible to lay to his charge elsewhere, appears to consider the use of certain of the alkaloids necessarily excluded as a means of murder by poison, on the score that their intensely bitter taste would prevent them from being swallowed. But this is to forget the facility with which such substances can be exhibited in pills: and to ignore, though but for an instant, and by an inadvertence, the memorable case of Palmer, of whom Dr. Casper himself speaks subsequently as having received his merited fate; although certainly the formal steps of his conviction have been sharply censured by many abroad, not without a far better right than its actual justice has been challenged by a few among ourselves.

It is scarcely a digression to proceed from Palmer, the subtle destroyer of his friend, to another recent and remarkable case, where the charge pointed to the violation of a still nearer tie; but where the issue, clear as to the fact of a poisoning by arsenic, was more favourable for the accused. We advert to the case at Darlington now, chiefly on account of the opportunity it affords us of urging the necessity for extreme vigilance on the part of the attending physician, in all instances where the symptoms of his patient depart from the usual aspect and sequence of any ordinary form of disease; yet, certainly, by no means to abet in the slightest degree the grave and unmerited charges made in the court, and by the common press, against the medical attendants of the unfortunate lady on the occasion in question, because they did not save her life by sooner arriving at their suspicions, or sooner disclosing them. It is to be trusted that it will be long indeed before our national morals have become so prevailingly ruthless, that the ordinary practitioner, on entering into the privacy of domestic life in apparently the usual execution of his humane office, shall be called upon to do otherwise than exhaust every imaginable means of explaining the phenomena of a given illness, however extraordinary or anomalous, before seeking, in the midst of the sacred relationships of the family, the motives or proofs of the practices of the poisoner. It ever should be, and we trust it ever will be, that suspicion will the most shrink from visiting where crime should have been the least willing to enter. But the suspicion once unavoidable, and gradually increasing in strength, how is the medical attendant to proceed? To attain entire conviction in his own mind, in one direction or another, must be his first object; but that may be neither easy nor timely for the exigence. He may inquire, as it were vaguely, whether the patient can have taken anything injurious; but there would obviously still be risk of terrible outrage, were he to insinuate directly, among those surrounding the sick bed, the supposition of positive crime, at a mere hazard as to its reality and its agent. Till a decision can be reached by some crowning test, it appears to us that the really prudent course would be to adopt one, and one only, of the alternatives which, according to the newspaper reporters, seem to have been recommended on the occasion of this trial by Baron Martin, the judge who presided; and to proceed to lay before a magistrate, or any adequate legal authority, under the evidently necessary seal of confidence, the nature of the facts elicited and their tendencies, the further inquiry contemplated, the best means for its accomplishment, and the aid which might possibly be required. Thus the medical attendant will escape from the position of acting as a self-constituted agent of police; and he will escape equally from all possibility of a charge of concealing crime, or of conniving at the destruction of another by too protracted silence; while he will throw the responsibility, where that of the detection of guilt ought really to rest, upon the magistrate, under whose directions he ought to proceed until the facts develop themselves into certainty. It is true that a highly distinguished authority, in commenting on this case in our Edinburgh contemporary, is inclined to reject this course, and points to another as in his opinion, and it is one well entitled to respect, the fittest of all. It is that, when the medical attendant is satisfied of the fact of poisoning, (although the context would lead us to believe that it is merely the strong suspicion which is here implied) he should communicate his conviction directly to the patient. But, if there be conviction, the limit of suspicion is already passed, and the affair is still more exclusively that of the civil authority: and if there be not, into how unhappy a predicament may we cast the supposed victim. Is the supposition false? What darkness and horror have we mischievously thrown round the closing hours of the sufferer! Is it true, and yet has the victim no certainty of knowledge of the possibly guilty individual—and does every officious care, every proffer of food, every drop of medicine, by whatever hand administered, now become the suspected vehicle of the consummation of injury, how much here have we certainly lost, and how little truly gained? We may, though not safely, disregard the consequences to ourselves of a false suggestion inconsiderately promulgated, but we cannot use too much forbearance in imposing them upon another. "The cautions which you have as yet given," said King William III. to Prendergrass, under an analogous warning, "can only make me suspect everybody that comes near me. They are sufficient to embitter my life, but not sufficient to preserve it. You must let me know the names of these men."

Even in cases of suicide, we shall not always be free from attempts to mislead us, not only on the part of the victim himself, but on that of relatives and bystanders; and here also it becomes sometimes necessary to use vigilance in attaining and announcing our conclusion. In a recent example, a druggist's assistant returned home at his usual hour of dinner, and a few minutes afterwards was found extended upon a bed, insensible, speechless, and breathing slowly and sonorously; life becoming speedily extinct, and without any preceding convulsions. On seeing him, and making inquiry as to his previous circumstances, it was averred to us that these could afford no cause of suspicion of a tendency to self-destruction; as well as that no phial or other indication of possession of poison had been discovered, nor any other means of proof of suicidal design. Groping in the sink of the apartment, and among the ashes, as well as searching elsewhere, we ourselves could discover nothing; and there was no smell of prussic acid perceptible at the mouth of the corpse. On being asked as to the cause of death, it was replied that the circumstance gave suspicion of poisoning by prussic acid; but that, as no odour of that acid was perceptible, and there seemed to have been no cause to anticipate an act of suicide, the death might possibly have arisen from some internal source, and most probably from an affection of the heart. It was added, however, that it was not possible to give a positive opinion without a post-mortem examination, and it was directed that information should be conveyed to the police. It was after this that a paper was produced which an aged relative had kept concealed in her pocket, and which was now found to be a confession of intention of suicide, dated the day before. Renewing our examination, and again moving the body, a small phial cork was found under it by a colleague; and lastly, our search being further stimulated, on raising the bed so as to allow the access of light into a dark and remote corner, we found an empty half-ounce phial, labelled "Hydrocyanic acid." Evidences of a previous condition of melancholy now easily accumulated. On an examination of the contents of the stomach three days afterwards, we failed in eliciting proofs of the presence of the poison by the customary tests either in the filtered liquid, or by inversion of re-agents so as to receive its vapour; but, by means of distillation, a quantity was readily obtained, amply sufficient to show its undoubted presence. Independent of the unusual reticence of the friends, the medico-legal reader will perceive that this case is not destitute of one or two other points of value in illustration.

The whole of our author's details regarding the subject of asphyxia, or suffocation, evince the same distinct and practical character which belongs to his other observations. While adverting to the remarkable fluidity of the blood, as common to all kinds of death by asphyxia, whether drowning, hanging, throttling, or garrotting, he points out that it is by no means peculiar to this form of extinction of life, but occurs also in other circumstances, as in putrid fevers and narcotic poisonings. In considering as necroscopic phenomena the results of the gravitation of the blood in this condition, we think that scarcely sufficient attention has been paid to the mobility with which it sometimes continues to vary its localization, at even remote periods from that of death; and to the risk that occasionally medico-legal inferences of an erroneous description, as to the posture of a body when a murder was perpetrated, or its situation afterwards, may have been thus deduced. In an individual killed by throttling, we have had occasion to remark that the blood, on the third day after death, continued so mobile in the vessels, that it gravitated readily in various parts, as in the face, according to the change of position in the process of examination. Again, those who have heard how closely the notions of our courts of justice still adhere occasionally to Warwick's description of a strangled man, as one of all but unconditional reality, and who are yet able to appeal to their own experience, will not object to Professor Casper's assurance that this picture is of only exceptional truth. The tongue, moreover, is by no means always protruded between the teeth in cases of the most indubitable strangulation; while it not rarely thus projects in examples, as the author points out, where the death was, equally unquestionably, from other causes. The death is even, he asserts, often here a neuro-paralytic one, and the internal indications of a true asphyxia are actually wanting. He has observed the yellowish-brown marks, with condensation of texture, similar to those caused by the cord in suspension, after compression by the hand in manual strangulation; a remark which is not unimportant, and which our own experience inclines us to verify. The author's comments on death by drowning are also comprehensive and valuable. He estimates that the proximate cause of the fatal issue arises here almost as frequently from neuro-paralysis as from proper asphyxia, marked by hyperæmia of the thoracic organs. Hypostasis in the brain he takes the opportunity of warning us from confounding with congestion. Nearly two hundred pages on infanticide complete the treatise; and in no portion of its contents has the copious experience of the writer been employed to more striking advantage. The accompanying Atlas of plates supplies occasionally some useful illustrations. The appearances of the fœtal and infantile lung have especially been carefully and instructively represented; and the insular marbling is pointed out as the only sure criterion, from this description of inspection, that the child has breathed.

The concealment of the bodies of infants, who have perished naturally, is mentioned as a common delinquency in Berlin, merely to escape the cost of interment. Each country, as it has its national habits and manners, has its idiosyncrasies of crime; as even different periods have their different fashions in atrocity. The calculating villany of a Palmer, to whom murder was but gambling for a higher stake, will not easily be surpassed anywhere; but it is probably among our excitable and ingenious kinsmen in the United States that we are to look for the quintessence of the romance of felony. A Professor of Chemistry, like Webster, slaying his creditor, and charring and consuming the corpse in his assay furnaces, was a hitherto unequalled climax of peculiar horror; for science, through the agency of one of its higher experts, had never so before abused its instruments. A Baptist preacher, like Carawan, waylaying his victim from an ambush in the woods, and carrying the corpse, "tied hog fashion," with the assistance of his slave, to a remote and skilful concealment; and then, on his discovery and conviction, striking the prosecuting counsel with one pistol-shot in the open court, and piercing his own brain with another, and this in North Carolina, and so recently as 1853, is another accumulation of ruffianism, so singular as to push credibility to the utmost verge. "The defendant," says our authority,* "was convicted; but scarcely had the jury returned their verdict, when he drew from his breast a single-barrelled pistol, rose from his seat in a half-sitting posture, leaned forward, and thrusting his arm between two attendants, took deliberate aim at Mr. Warren, one of the counsel engaged in prosecuting for the State, and fired. The ball struck just above the heart, and passing through the lappel of his coat, and cutting the cloth on the breast, struck the padding, and fell to the floor. He then dropped this pistol, and instantly taking another, applied it to his own forehead. One of the officers observing the movement, seized his arm, and pulled it down to the railing of the box, but could get it no further. During the struggle, the prisoner, with great coolness, leaned his head against the muzzle of the pistol and fired, the ball entering the right side of the skull, considerably behind, and somewhat above the ear, and traversing the brain until it lodged just over the right eye. He then dropped on his seat senseless, and died shortly afterwards."

We could willingly have dwelt longer on the work of Professor Casper, and on the allied topics which it suggests to us, but our space is not inexhaustible. We trust we have said enough to manifest our high appreciation of its value, and to induce our readers to search for themselves among its ample stores of apposite fact and inference. Its chief distinction is the all but exclusively personal nature of its means of observation and experience: its chief merit, its strictly practical tendency. In his remarks upon what may be termed the chronology of the process of putrefaction, in those on injuries, on poisons, on burns, on the comparative effects of violence applied to the dead and living structures, on medical mal-practice, and on injuries to the fœtus *in utero*, we probably find the most distinct traces of his habit of original inquiry, though these traces are deficient nowhere. As a thinker, he is what his countrymen would term essentially objective. Depending upon original observation, possessing for it evidently a peculiar aptitude, but not neglecting the views of others, if now and then he cuts the Gordian knot, it is to proceed more habitually by a careful and cautious deduction, to arrive at a distinct truth or a consistent interpretation. Yet he is not likely to please the more sanguine lovers of hypothesis; for a fact to him is something even before it is explained, and a speculation is nothing where the facts must be constrained to it. The transcendentalism so much in honour with many of his countrymen, and which we ourselves never encounter without recalling the witty alternative of Julius Weber, that here a man must either begin, or leave off, thinking, has evidently no charms for him. To reduce a reality to its most comprehensible form, or an idea to its simplest expression, is the exposition fittest for medical jurisprudence, where a basis of sound judgment is to be prepared for a court or a jury, to whom a subtlety, if not even a technicality, might be a bewilderment or a source of misconception, and it is this method which Dr. Casper has adopted. Obviously, therefore, he never suffers himself to forget, that it is luminousness of arrangement, accuracy of inference, and copiousness of illustration, but not fertility of invention, which can justly be required of him. With an unvarying candour, he never hesitates his doubts, or conceals his ignorance, or shrinks from the consequences of his opinion; while maintaining conscientiously the precept that the medical jurist, in offering his decision, has no further concern with its results, than that he shall not rashly involve the innocent in disaster. As a writer, Dr. Casper never leaves us in difficulty as to his meaning. His style is clear and animated, with an occasionally happy sententiousness, which is not the least pleasing to us because showing that

* Wharton and Stillé: Treatise on Medical Jurisprudence. Philadelphia, 1855, p. 763.

his ripe years have brought with them no diminution of cheerful vigour. When to the complete treatise before us he shall have added another analogous treatise, as he has projected, and so has developed his labours into an entire System of Forensic Medicine, the work will constitute a valuable whole, to which we shall look forward with hope and interest.

The perusal of this volume, with the consideration of its topics generally, cannot fail to recal forcibly before us the defective organization, in kindred matters, subsisting in our own country. The foremost of qualities in a civilized nation is its justice. Without the solicitous and thorough protection of public and private rights, and of the personal freedom and safety of the individual, the rest is but the gaudy decoration which masks a structure of no real worth or solidity. The careful, and wise, and enlightened administration of its justice, therefore, has a primary claim upon the attention of a community; and wisdom and enlightenment can be regarded as but in a narrow sense the possession, as they can be no future acquisition, of those who choose to exult only in the knowledge which they have inherited. The law must assuredly have its settled principles, but it must not the less have its varying adaptations; and wherever science developes new and improved resources, justice must have recourse thither for new and improved appliances. Hence conservatism in law, so inherent to it, and so precious in the abstract, lapses into but a stronghold of defects in its relations. Unquestionably our country is not niggardly in its appointment and remuneration of such legal officials as it has been taught or habituated to consider necessary. On the contrary, the continental judge multiplies with astonishment the pounds sterling of the salary of his ermined brother in Britain into thalers, guilders, or francs, and smiles resignedly at the unapproachable distance between the result and his own pittance; as it is with a marvel not less beyond the limits of envy that his subordinates compute the stipend and perquisites of their own more immediate counterparts, who aid conformably to sustain the sumptuousness of English jurisprudence. But, in the midst of all this liberality, he is startled to find no position and no allotment for an official who is to serve as a permanent and authorized representative of that which has grown into an indispensable body of science in every cultivated people. The broad and open-handed munificence which reached, in one direction, even beyond his yearnings, presents to him no satisfactory equivalent for that narrowness of spirit elsewhere, which he can still afford to contemn, and which recognises in the merely legal functionaries the only ministers of justice, assigning no place, therefore no assured opportunities of experience, to the forensic physician, who is to be not the less their sole safe guide in the determination of the most momentous questions. In short, the continental governments have considered the forensic physician a portion of their apparatus for the administration of justice as essential as any other of its officers; and, seeking to derive from him the fruits of his knowledge, they have rationally prescribed to him his course of training and his duties, and allotted to him the field for perfecting his capabilities. With more general lavishness hitherto, we have been less specially provident.

In a recent article upon State Medicine, published in our eighteenth volume, we introduced much information, and propounded several views, towards which we beg once more to direct the attention of our readers. We then took occasion, among other matters, to refer almost incidentally to the really able and earnest pamphlet by Mr. Craig, in relation to the system of criminal prosecutions in Scotland, and we now willingly renew the expression of our approval of the general tenor of its remarks. Yet it may not be inappropriate to recur to it again, in so far as to mark out somewhat more fully than was then our design, how wide is really the distinction between the scope of the office and the duties of the Procurator-Fiscal in our sister country, and those of the coroner in England, in giving an initiative to the investigations of the forensic physician. It is true that Mr. Craig points out that the coroner's office, now unknown in Scotland, formerly existed there; but he does not appear to have sufficiently kept in view, or at least has not expressly stated, that its functions in no respect corresponded with those of the English charge, as they have been transmitted from even the earliest periods of our law. Thus in the 'Practicks of the Law,' by Sir James Balfour, a Scottish judge writing nearly three centuries ago, we find the coroner merely described as an officer of the pleas of the Crown; but without a reference to any power of holding inquests, as either possessed by him then, or having been charged upon him at any prior time.* Indeed, the duties assigned to him seem to be such as were even incompatible with those proper to the English office. Then, with regard to the actually existing rule of proceeding in Scotland, we suspect that Mr. Craig's description of that which subsists in Midlothian affords no absolute criterion for what is habitual elsewhere. Instead of the constable being an all but arbitrary investigator and virtual authority, as Mr. Craig depicts him, he is merely, it is but just to state, in more than one district with the practice in which we have chanced to become intimately acquainted, the intelligent and authentic messenger who carries the information of any imputed crime or sudden catastrophe to the Procurator-Fiscal; to whom, however, the report may have already found access from other sources, when that official, should he see cause, would necessarily assume the initiative himself. Assuredly the whole armoury of the law is not brought into requisition on every trivial occasion: but wherever the case affords a reasonable pretext for interference, the fiscal instantly enters upon the investigation, scrutinizing all departments of inquiry, taking what are termed written precognitions or details of evidence, and visiting the localities where necessary; and this often with the aid of the sheriff, and of one or more medical men where the assistance of these is requisite. This inquiry, which we know is frequently conducted with singular discretion and ability, and with the hand of even justice between the weakness of the accused and the strength of the law, is then reported, if it have led to any palpable results, to the Crown lawyers in the capital; and the case proceeds to a trial or not, according to the scope and quality of the facts elicited. That a system, which admits of being so conducted, possesses many real excellences, must be readily conceded; while it would be unjust to charge upon it the derelictions of one or more of its individual agents.

Several such derelictions, reported by Mr. Craig upon his own knowledge, are unquestionably flagrant enough; and to these, to the account of which we attach entire confidence, we fear that not a few might still be added from different quarters. But in at least one case, which he cites upon the testimony of another, and to which we referred on the authority of the pamphlet, where a child and mother both died after the obstetric administration of chloroform, more recent intelligence entitles us to question his accuracy. The facts he has given are too peculiar, and intrinsically too correct, to permit the identity of the case itself to be mistaken; but he has erred in stating that it gave rise to no effective inquiry. That inquiry is surely effective enough, by which the ends of justice are amply vindicated, while other sanctities of social life, less paramount it is true, yet still willingly respected, are preserved from outrage. As the rumour and suspicion oozed abroad, exciting doubt and surprise from the station and previous repute of the parties implicated, the fiscal, a gentleman of great intelligence, immediately proceeded to a precognition; and among others, the medical man who had acted on the occasion, with the view of unhappily aiding in a culpable and calamitous concealment of a lapse of honour in a recently married young lady, was examined at length. The case, and from our knowledge of its circumstances, we believe with perfect propriety, was not brought to a trial, but both the practitioner and the husband, who had also been the seducer, withdrew from the district. We revert to this, because we are desirous that, by those who demand justice, it should be first scrupulously rendered; and because, at a time when it has been contemplated to introduce the office of public prosecutor into England, of which, in the article already referred to, we have signified our approval, it is further not unimportant to remember, that the Procurator-Fiscal in Scotland is not a mere analogue of our English coroner, but has duties far beyond him, taking cognizance of every

* Balfour's Practicks. Of the Justice Air, chap. vi.: *The Coroner's office and fie.* The details are curious.

grade and variety of crime, whether against property or person. He is, in the strictest sense, a public prosecutor; and by no means irresponsible, but removable for default, if he commit default, by the Lord Advocate, or on complaint at his instance to the High Court of Justiciary. In no case could he be justified in receiving, as Mr. Craig indicates, the mere report of a constable as definitive; unless, indeed, to that extent to which Chitty has given his sanction in English law, and certainly what has been allowed by that eminent authority might be vindicated without discredit by our Scottish neighbours.* With more immediate regard to the comparative efficiencies of the English coroner and the Scottish fiscal, where their functions become really analogous, perhaps we could resort to no better authority than that of an intelligent practitioner, residing at the limit of either kingdom, and occupied extensively in both; and such a one has expressed to us an opinion strongly in favour of the Scottish system. But to have public prosecutors in England, it is not necessary to supersede the office of the coroner. We merely design to show that in Scotland, where these already exist, and where they discharge a double function, there may be faults in individual officials where none are justly chargeable upon the system itself: that for all proved derelictions of duty there is a remedy at law: and that the scope of the office is really excellent, and its authority, or the means of sustaining it, ample, although capable, doubtless, of being regulated, as to both, into greater uniformity and certainty of procedure.

As to what is truly wanted besides, throughout the United Kingdom, to add safety and precision to the march of justice, and therefore to confer upon it dignity, by surrounding it with the best, and readiest, and most ripely versant aids of applied science, we have little to subjoin to the remarks in our eighteenth volume. There should be at least one forensic physician appointed for the assize town of every county in England and Ireland, and for every place of circuit in Scotland, with a greater number wherever required for any special density of population. The whole should be constituted into a kind of college or society of experts; and there should be a central board in the capital, to which all reports should be transmitted for supervision, and all doubtful or disputed points referred for determination. No nomination into the body should be held as legalized, unless the individual, in addition to a complete medical education, with the attesting diploma, should have undergone a special examination before the central board, and should be in possession of its testimonial of his peculiar qualifications for the description of functions upon which he is to enter. Should the forensic physician be also the Officer of Health of the district, there should then be appointed a pharmaceutic or medico-legal chemist, to aid him in that class of investigations which will necessarily prove one of the most intricate departments of his duty. Were he to be required to act as forensic physician only, the entire responsibility of the chemical examinations might, perhaps, be reasonably imposed upon him: but the arrangement which unites the medico-legal and the sanitary duties in the same individual, and sustains him in both through the assistance of the practised chemist, would, we doubt not, be by far the more efficient and beneficial for the public. It would be unprofitable that he should be distracted with too multifarious labours, such as have been imposed upon him in certain of the German states, where even the observation of the progress of the crops, the numbers and condition of the flocks and herds, and the registrations of sickness and mortality amongst man and beast, all leading to the framing of interminable tabular reports, have been adjudged as within his proper sphere of usefulness. On the other hand, the possessor of the joint office should not be in practice, yet he should have enjoyed reasonably prolonged and extensive general experience in practice; and, in particular, he should have had large opportunities of attaining familiarity with the appearances of normal and morbid structures, as

* Coroners ought not in general, where a party dies by the visitation of God, as from apoplexy or the like, nor in any case, unless a very doubtful one, unnecessarily to obtrude themselves into private families for the purpose of instituting inquiry. They should in general wait until they are sent for by the peace-officers of the place where the violent or unnatural death occurred, before holding an inquest." Chitty's Burns' Justice, vol. ii. p. 32.

with the phases and phenomena of the course of ordinary decomposition. Who can question that, under the eye of the common medical witness, there has been frequently no proper distinction between the extrinsic effects of poisons, and of certain classes of injuries, and the spontaneous results of natural pathological changes, strictly so called; or even between these and the customary and necessary products of the death agony, or such as arise beyond death as the results of the progressive grades of putrefaction? We have little guarantee that it is rarely now, as it was certainly frequently once, that a passive vascular accumulation, the result of the flagging energies at the close of life, circumscribed, and without products of exudation, or that the mere hyperæmia of gravitation, or ecchymoses into the serous and mucous membranes, with the blood unaltered, and therefore recent, or the imbibition of hæmatin into the tissues, or the alteration of colour or consistence of the stomach as a post-mortem phenomenon, has failed to be discriminated from the imputed results of wilful injury, and that hence error and confusion have been mischievously propagated. Let us suppose an officer, otherwise duly instructed, to be also, as he ought to be, an accomplished medical psychologist, and it would be difficult to over-rate the valuable assistance which he must render to every department of the magistracy, whether in civil or in criminal procedures; as well as the authority which could be added to his testimony on all points of perplexity, through the co-operation, where requisite, of his more immediate colleagues, or the revision of the superintending board.

We have no desire to assail our brethren of the robe with a single word of attack or recrimination, for it is a gratification to us to recognise in many of them those talents and acquirements which demand from us our sympathies as men of science, if we could have expected sometimes that these might have been rendered more uniformly reciprocal. Yet it must be owned that our profession has appeared but too frequently, and too recently, to be thrust on some points into an unseemly antagonism with both the bench and the bar. Courts which unsettle each other's judgments, and pleaders who thrive by disputing each other's facts and controverting each other's opinions, are angry and petulant when they meet an occasional diversity of conception among medical witnesses; though on matters where these must have decided on the instant, and with the intervention of many obstacles to embarrass and obscure. But we have neither wish nor right to shelter ourselves, as to the uncertainty of our results, by an appeal against those who cannot appreciate our difficulty in arriving at them. It is our evident duty to struggle towards greater perfection and greater certainty: nor has this duty been neglected, for it has ever been a pride in our profession to welcome every opportunity of giving precision and stability to knowledge, by a real extension of its basis; though compelled to advance slowly, like men constrained to feel that science is rather the child of time than of genius. Such an added opportunity we now seek from our legislators. It is indeed, and most confessedly, desirable to obviate all flagrancy of dissent among medico-legal witnesses. But what is this but to desire, in these witnesses, equal instruction, on which to ground equal capacity of observation, equal power of appreciation of facts, and equal capability of judgment: and how is this to be attained but by selecting men of originally adequate endowments, preparing them by the relevant training and study, and securing for them the necessary range of experience? In short, we require here, as exists elsewhere, an established corps of medico-legal physicians and of their subordinates, acting for the public service with a regulated and mutual responsibility towards each other, and to a superintendence over all. With such men labouring impartially, whether to demonstrate innocence or to expose crime, and appearing as the rule, few matters would be left as grounds for contention; and even the witnesses for a defence, themselves sharing generally in the influence of the better instruction, would lose, with the opportunity, the inclination for cavil. Till such are appointed, we are amenable to no challenge, unless it be from our own conscientiousness and zeal.

Review VII.

On Stricture of the Urethra. By Henry Smith, F.R.C.S., Surgeon to the Westminster General Dispensary; formerly House-Surgeon to King's College Hospital. *London*, 1857. pp. 280.

Mr. Smith commences his work by a consideration of some points connected with the anatomy of the urethra. Those which he particularly dwells upon have an immediate practical bearing: thus, an accurate division of the urethra into different portions is naturally connected with the ordinary seat of stricture. Upon this point there can be no doubt that up to a very recent period most surgeons would at once have said that stricture was generally met with in the membranous portion of the urethra. This opinion Mr. Smith combats; and he gives the results of eighty-five examinations of preparations in the museums of Bartholomew's, Cambridge, King's College, and the College of Surgeons. The seat of stricture was found in the membranous portion in eighteen cases only, "whilst in fifty of the specimens the disease is situated either in the bulb itself or just in front of it." (p. 30.)

In Mr. Henry Thompson's work "On Stricture of the Urethra," published three years before Mr. Smith's, there is an analysis of a much more extended series of observations. "I have personally," says Mr. Thompson, "submitted to a close and careful inspection not less than three hundred preparations of stricture of the urethra. I possess notes made on the spot of two hundred and seventy." (p. 87.) The conclusion arrived at by Mr. Thompson is, that the junction between the spongy and membranous portions is the point at which stricture is most frequently situated, but, he adds, that a spot about an inch in front of this point is almost as frequently affected. It must, however, after all, be doubted how far the evidence adduced from the examination of preparations is available in determining such a point; because few pathologists will preserve specimens unless they illustrate something which has not, up to that time, generally been received—and consequently during the period in which stricture of the urethra was regarded as occurring usually at the anterior point of the membranous portion, a specimen of this disease would not be so likely to be preserved as a specimen of a stricture found in any other situation: one important and practical fact has, however, been clearly demonstrated, namely, that stricture does occur in a large number of cases in the bulbous portion of the urethra; and to this we shall again more particularly refer when we consider the subject of perineal section. Another point of practical interest arising out of the consideration of the anatomy of the urethra is the existence throughout the whole length of the canal of a distinct set of organic or involuntary muscular fibres.

> "The occurrence of spasmodic stricture without the existence of any organic disease, has been by most surgeons attributed to the action of the muscles which surround the back portions of the urethra,—viz., the accelerator urinæ, the compressor urethræ, and the anterior fibres of the levator ani. . . . Experience teaches that in the majority of cases of spasmodic stricture an instrument is obstructed at the bulb or membranous portion, where the muscles above mentioned exert their action. . . . But it is occasionally noticed that the spasmodic action is particularly appreciable at the anterior portion of the canal." (p. 24.)

The difficulty of accounting for spasm in the anterior part of the urethra has been removed by Mr. Hancock's discovery that the urethra is muscular throughout. In his work lately published, Mr. Hancock "has related some very interesting cases in which spasmodic stricture seemed to depend entirely upon the organic muscular fibres of the urethra." (p. 25.) Upon this point we cannot forbear extracting the following from a MS. copy of notes of Sir B. Brodie's Surgical Lectures, delivered in the year 1810, now before us. It cannot fail to be read with interest, as affording an instance of that intuitive insight which discerns the subtle workings of nature, even before the mechanism by which those actions are performed has been either demonstrated or discovered:

"There is no doubt (says Sir B. Brodie, in a lecture delivered now nearly half a century ago) "that the whole of the canal is muscular. Though such fibres cannot be distinguished, they may be distinguished in larger animals. In hydatids no muscular fibres can be seen, but by what other means can the animal contract and dilate itself? . . . The whole of the canal therefore, I say, is muscular."

A third anatomical point upon which Mr. Smith has dwelt, is the ordinary length of the urethra—a point upon which the late Mr. Briggs bestowed some labour. But as the urethra is surrounded by spongy and erectile tissue, it is evident that its dimensions must constantly be varying. The temperature of a room or the sight of a catheter would be sufficient to produce an alteration in the length of this sensitive part; we do not therefore think that much practical benefit has arisen from the measurements that have been made, nor are we surprised to hear that "anatomists and surgeons have differed" in the results which they have obtained.

In considering the nature and pathology of stricture, Mr. Smith reviews the effects on the canal in front of the stricture, the effects on the canal behind, perineal fistulæ, and various affections of other parts.

With regard to the mode of formation of perineal fistulæ, Mr. Smith says they are produced in two ways—

"Sometimes an abscess is formed external to the canal, and at length opens into it. The other method in which the same condition is produced is as follows: the urethra behind the stricture ulcerates, urine escapes from the canal, becomes diffused into the loose cellular tissue of the perineum and scrotum." (p. 36.)

Mr. Smith surely does not mean to say that whenever the urethra ulcerates, the urine becomes infiltrated in the cellular membrane, as in the formation of a common abscess. The adhesive inflamation may, and does generally precede the ulceration, so in ulceration of the urethra a similar action may and generally does take place; and by means of this the urine, after it has escaped from the urethra, may be prevented from becoming effused into surrounding parts. Under such circumstances we have an abscess formed which will probably open externally, but being everywhere surrounded by its layer of lymph, the contents of this abscess are circumscribed, like those of a similar affection situated in any other part. In either case, should the adhesion not be sufficiently firm to circumscribe and limit the disease, effusion will take place, and we then have extravasation of urine from an ulceration connected with the urethra, from the same cause as we have diffuse cellular inflammation occurring in conjunction with suppuration elsewhere.

By far the greater part of the work before us is devoted to the treatment of stricture, and the mechanical means employed for this purpose are considered under three heads:—1. Dilatation; 2. Cauterization; 3. Incision.

With regard to the first, the preference is given, as a rule, to the use of the common wax bougie. Upon this point, we believe that the conclusions avowed are based upon sound reasoning and experience. We would only remark, that it appears to us that the consideration of the use of bougies with a bulbous extremity is somewhat summarily dismissed. These instruments are much used in France, and from the flexible nature of their extremities are often preferred to others. The objection urged by the author, that in some instances, even if such an instrument could be passed through a stricture, "there might be some difficulty in withdrawing it" (p. 80), scarcely requires a serious examination.

"Another mode of treating stricture, by a species of rapid dilatation, has of late been practised by Mr. Thomas Wakley. It is, indeed, chiefly a modification of the plan devised by the French surgeons. . . . The conducting rod is first passed through the stricture into the bladder; and upon this one of the small silver tubes is carried, so as to dilate the stricture forcibly and rapidly." (p. 113.)

Some of the French surgeons have of late years much insisted upon this forcible dilatation of a stricture; and M. Maisonneuve, two or three years ago, invented a sound composed of two halves. These were connected by little metal bars, with a hinge at each extremity, and were so arranged, that by withdrawing the upper half of the instrument it was separated from the lower half for a quarter of an inch or more. By this means, after the instrument was once passed, a stricture could be dilated instantaneously to any requisite extent. But in these various modes of producing rapid dilatation we see no advantage over the ordinary conical bougie, or the sound with an egg-shaped enlargement at one part. Instruments of the latter kind were many years ago kept by Messrs. Savigny and Co., of St. James's-street. The anterior part of the sound might be made to pass readily enough through an ordinary stricture into the bladder, and the enlarged portion following would necessarily dilate the passage according to the degree of force employed. It can matter little whether, in producing this result, a solid instrument is employed as of old; or an instrument composed of two parts, as suggested by M. Maisonneuve; or of many tubes, as recommended by Mr. Wakley.

Seventy-six pages of Mr. Henry Smith's work are devoted to the subject of the treatment of stricture by external section:—a rather large proportion, compared with the small space given to the consideration of constitutional treatment of the patients in these cases.

After a lengthened consideration of the objections to the operation of external section of the perineum, Mr. Henry Smith, although evidently adverse to the operation, arrives at the conclusion that—

"If proper attempts have been continued for a length of time in vain, it is justifiable to make a fine opening into the urethra, either cutting through the strictured portion, or *penetrating the membranous part*, if there is not much appearance of stricture." (p. 253.)

The words in the above quotation which we have put in italics appear to us important. They show that Mr. Smith contemplates the division of the membranous portion of the urethra in certain cases of perineal section. We dwell upon this point from a conviction that many operating surgeons in common with Mr. Smith consider this as a part of Mr. Syme's proceeding. If, however, we turn to Mr. Syme's description of his own operation, we read—

"In regard to extravasation of urine, there can be no doubt that the circumstances most favourable to its production are openings through the deep fascia of the perineum. . . . But, according to my proposal, the only fascia concerned is that which lies immediately under the integuments."*

Now it must be evident that Mr. Syme's operation, as he describes it, can apply only to cases of stricture situated in front of the membranous portion of the urethra. It would be impossible to divide a stricture in the membranous portion without interfering with the deep fascia. Here, then, two distinct classes of cases present themselves, corresponding to the different situations in which the stricture is commonly found—namely, in the bulb and in the anterior part of the membranous portion. The seventy cases to which Mr. Syme refers in the paper above alluded to, and which he says were performed without any fatal or even alarming symptoms, must, we should think, have been cases of stricture in the bulbous portion of the urethra. We have observed in practice the very slight irritation that has followed a division of this part when the incision has not been carried backward. The possibility of this is prevented in Mr. Syme's mode of performing the operation, in consequence of the back of the knife being towards the deep fascia.

From the cases related by Mr. Henry Smith, as well as from those which we have ourselves observed, we would venture to say that the operation in question

* Medical and Chirurgical Transactions, vol. xxxvi. p. 259.

cannot with impunity be performed in cases where the stricture is situated in the membranous portion of the urethra. The operation then would be either useless or dangerous—useless, if confined to the bulb; dangerous, if extended farther back. The want of clear statement of the situation of the strictures operated upon, has probably been the cause of much misunderstanding and apparent contradiction with respect to this operation; and we hope in future works upon stricture of the urethra, that this point will receive the attention which it appears so justly to deserve. It is not a little remarkable that Mr. Smith should have failed to have noticed the great difference which appears to us to exist between an external section of the bulbous and of the membranous portions of the urethra, while he so cleverly notes this difference respecting internal section of the same parts:

"It is almost universally acknowledged (he says) that when stricture is situated anywhere in the straight portion of the urethra, internal incision may be performed with safety; but that, when it is met with beyond the bulb, internal section may be followed by very serious results, such as hæmorrhage, inflammation and abscess, infiltration of urine, false passage, and death." (p. 171.)

In concluding our necessarily brief review of Mr. Henry Smith's work, we feel convinced that it has, as he states, been his endeavour to investigate this subject without any prejudice, and that he has related his cases with the utmost candour and fairness.

Review VIII.

The Midland Quarterly Journal of the Medical Sciences. Vol. I. Part I. pp. 192. *Birmingham. London.* May, 1857.

The present is an age of public journalism. Every morning ushers in a rapidly increasing series of papers, containing an account of the current events of the day, and advocating opinions to suit every grade of political creed. Every profession, and every branch of literature, science, and art, are represented by numerous periodical publications. In no profession is this multiplicity of journals more remarkable than in that of medicine. Every week, month, quarter, and half year beget a host of them, treating of every topic upon which medicine can be brought to bear.

It is not long since we had occasion* to notice the first appearance of the 'Liverpool Medico-Chirurgical Journal,' and we have now to introduce a new claimant for the support of the profession, under the title placed at the head of the present article. It may be considered invidious in us to pronounce an opinion on the necessity or propriety of thus multiplying the already too numerous sources of periodical medical literature; nevertheless, we would venture to suggest, that it may be questionable whether the true interests of medical science are promoted by this multiplication, or whether these may not occasionally be sacrificed to others of a more private nature. The great number of journals of the present day affords a facility for the publication of many papers which only encumber medical literature, and which in some instances hardly deserve to have seen the light. Another consequence is, that good papers often appear in journals which must necessarily have but a limited sphere of circulation, and they too often become in a measure buried in oblivion from the fact that the journals themselves are apt, after a few years, or after they have served the purposes of their original promoters, to die a natural death. Those who are acquainted with the periodical literature of our

* Brit. and For. Med.-Chir. Review. April, 1857.

profession in this country during the last half century are well aware that many journals have been started and conducted for a few years with vigour, which have then dwindled down and ultimately ceased to exist. We have before us the titles of at least a dozen such, which are scarcely known, even by name, to the majority of the profession. With every wish for a more prosperous career to the journal, the appearance of which has elicited these remarks, we proceed to say a few words on its objects and contents.

The journal is arranged somewhat on the plan of the "Edinburgh Medical Journal," consisting of three parts: Original Communications, Reviews, and Periscope. Its principal object, however, appears to be for the publication of original communications, which extend over 166 pages; the Reviews occupying twenty-one pages; and the Periscope, which in this number has reference solely to Midwifery, only five. The original communications are eleven in number. Some of them contain the details of interesting surgical cases; others are little more than essays on the subjects treated of, while one or two appear unnecessarily prolix. They are as follows:—

I. *On the Nature, Causes, Statistics, and Treatment of Erysipelas.* By P. H. Bird, F.R.C.S., London.—Mr. Bird was the author of the Jacksonian prize essay on erysipelas. The results of his investigations, in which there is nothing very original, may be summed up in the following general conclusions:—

"To conclude, then, erysipelas is merely an example in the skin of that diffuse inflammation, which in other tissues constitutes diffuse inflammation of the mucous membrane, diffuse phlebitis, puerperal fever—all of which have a common origin, a poison in the blood, are infectious and contagious, and may mutually produce each other.

"Erysipelas is best treated by stimulants and support, and when complicated with inflammation of the subcutaneous cellular tissue, by early incisions, which should extend to the depth of the disease." (p. 15.)

The author confirms the observations of Williams and Gulliver as to the occasional presence in erysipelas of pus corpuscles in the blood of parts remote from the affected textures. When, however, we remember the difficulty, if not the impossibility, of distinguishing pus-corpuscles from the white corpuscles of the blood, the observation loses much of its force. At all events, however, we question the correctness of his explanation, that these corpuscles result from "blood globules becoming converted into pus."

II. *On the Radical Cure of Inguinal Hernia.* By Holmes Coote, F.R.C.S., &c., London.—Mr. Coote advocates a more frequent recourse to a radical cure, in preference to the more common palliative treatment by pressure. He details the various operations which have been proposed by different surgeons, but recommends the plan adopted by Wutzer (not Wützer), of simple invagination of a portion of the scrotum into the inguinal canal. Numerous cases in which this has been successfully practised, are alluded to. The author does not appear to be aware of the very elaborate memoir on this subject, published during the present year at Stockholm by Dr. Carl Benedict Mesterton, Lecturer on Surgery in the University of Upsala, and of which we give an abstract elsewhere. Dr. Mesterton, after a most careful investigation, advocates Rothmund's modification of Wutzer's operation as most certain and permanent in its results.

III. *Case of Cæsarian Section.* By Dr. Charles Clay, Manchester.—The operation was performed on account of a fibro-cartilaginous tumour, weighing from three to four pounds, originating from the upper and central portion of the curve of the sacrum, and filling the cavity of the pelvis, so as with difficulty to admit the finger between its surface and that of the pubes. The patient sank on the nineteenth day after the operation: but this was not undertaken till three days after the cessation of labour. The word Cæsarean occurs eleven times in the communication, and is invariably spelt Cæsarian.

IV. *Observations on Syphilitic Retinitis.* By Dr. W. F. WADE, Birmingham.—This paper contains the record of a single case of retinitis, which the author believed to be of a syphilitic origin.

V. *On Conservative Surgery.* By F. C. SKEY, F.R.C.S., London.—Mr. Skey takes a retrospect of the history and progress of surgery during the last three-quarters of a century, and shows that the object of modern surgeons is "to repair and to restore, rather than to mutilate and to remove." In the propriety of always having this object in view, we entirely concur with him; but we do think that his anxiety to annihilate surgical operations has carried him too far when, in speaking of the treatment of popliteal aneurism, he makes the following assertions:—"The compress has taken the place of the knife. The 'operation' for popliteal aneurism is about to be numbered among the relics of the past." We are sure that the success of compression has not been established in the conclusive manner that Mr. Skey would have us believe, and are convinced that the treatment by ligature is not attended by a mortality of twenty-five per cent. It is well known that the distinguished Professor of Clinical Surgery in the University of Edinburgh has performed the operation twenty-five times with invariable success.

VI. *Case of Excision of the Knee-Joint.* By Dr. WILLIAM MASFEN, Stafford.—The operation was undertaken for extensive disease of the knee-joint, of some years' standing. It was followed by irritative fever, and the patient died exhausted on the twenty-first day after the operation.

VII. *On Rational Therapeutics.* By Dr. R. C. R. JORDAN, Birmingham.—This paper consists of some sensible observations on the mode of action of drugs. These are divided into three classes: Eliminants, Tonics, and Specifics. The last class, according to Dr. Jordan, would "include not only counter-poisons, if such exist, but those agents which, uniting with the tissues of the body, or helping in its chemical processes, thus supply the deficiencies produced by disease." Dr. Jordan appears to doubt if we have any specifics in the sense of counter-poisons to disease. Quinine in ague he considers to act through the nervous system as a tonic; mercury and iodine in syphilis, as eliminants. Under the head of specifics, which supply deficiencies in the body, we have iron in anæmic diseases, and others. There is no proof, however, that the antiscorbutic virtues of the Cruciferæ are to be attributed to their containing sulphur. It has not yet been shown that in scurvy there is a deficiency of sulphur in the system; and how would this theory explain the action of lemon-juice and other known remedies?

VIII. *Contributions to Surgery*, by EVAN THOMAS, M.R.C.S., Manchester.—This paper contains three cases of considerable interest.

The first is one in which the knee-joint of a lad, aged twelve, was excised, on account of disease of four years' standing. The patient was dismissed cured from the hospital two months and a half after the operation; and four years after, the following note was taken of his condition:

"He is in the enjoyment of perfect health, and is in good condition. The stiff leg is perfectly straight, and its muscles almost as well developed as in the other leg; on admeasurement, it is shorter by an inch and a half, but there is nothing discoverable in his gait but a stiff knee." (p. 63.)

The second is a case in which the knee-joint was excised, but in which amputation of the thigh had subsequently to be performed.

The third case is one in which the right femoral artery was successfully ligatured, on account of a popliteal aneurism. About six months after, a second aneurism appeared in the left ham, for which the left femoral artery was also tied, with success.

IX. *Case of Femoral Aneurism.* By W. Sands Cox, F.R.S., Birmingham.—In this case compression was first resorted to, but on the fifth day was discontinued, in consequence of its producing tenderness and very severe pains in the groin. This pain continued, and the aneurism increased in size. Twelve days after the removal of the compression, a ligature was applied to the external iliac; but gangrene set in, and the patient died on the twelfth day after the operation. It is but fair to add, that Professor Cox had not a sufficiently good instrument to compress the artery alone, without exerting pressure on the veins and nerves.

X. *The Principles of Treatment of Chronic Phthisis Pulmonalis.* By Edward Smith, M.D., London.—This paper, which extends over upwards of fifty pages, is rather a general treatise on the whole subject of phthisis. As such, it would be apart from our object in this place to enter into any detail concerning it. There are one or two observations, however, contained in it, which we cannot pass without comment.

In the first place, the chief point on which the author insists throughout the paper, is the necessity of recognizing a "pre-tubercular stage" of phthisis. Every one admits that in cases of phthisis there has been a state of body predisposing to the deposition of tubercle; but few, if any, we think, will agree with Dr. Smith in making this predisposition the first stage of the disease; still less in believing that we have any evidence by which the existence of such a stage is clearly indicated; in other words, that in a given case, we can say that tubercle is *about to be* deposited in the lungs, before any deposition has actually taken place. Indeed, it is a fact established on the highest authority (Skoda and Louis), that tubercle may occasionally exist in the lungs without any possibility of its detection. How, then, does Dr. Smith venture to assert that he can, from any signs or symptoms, *predict* its deposition, when sometimes it may be impossible to diagnose its actual presence by all the tests known to the best observers? The symptoms of the "pre-tubercular stage," mentioned by Dr. Smith, are very vague and unsatisfactory. They are principally "shorter breathing, less breath-motion;" "expiration quick, forcible;" and a diminution of "the quantity of tidal air in ordinary breathing." Seeing that Dr. Smith lays claim to such accuracy of diagnosis in the early stage of phthisis, we are at a loss to understand how he objects to Dr. Pollock's conclusions as to the period of the year in which phthisis originates, on the ground that "we cannot determine the moment of origin of the disease." (p. 81.) But not only is lessened respiration an unmistakeable symptom of "pre-tubercular stage" of phthisis; according to Dr. Smith, it is also an essential *cause* of the disease. (p. 117.) How, then, does he explain what, at another place (p. 80), he is unable to deny—the comparative infrequency of consumption in tropical countries, where the respiratory function is known to be much less active than in colder regions?

Dr. Smith makes some novel observations on the various conditions of the throat met with in phthisis. Three different varieties are mentioned. 1. A state of pallor, without injection of the vessels; 2. A state of pallor, with injection of defined vessels in some parts, and enlarged mucous follicles in the pharynx and uvula; 3. A state of desquamation, of fibrinous deposition, or of ulceration, generally met with only in advanced cases. Allusion is also made to the appropriate treatment in each of these varieties.

The author does not think that much assistance can be derived from the microscopic examination of the sputa in the diagnosis of phthisis. The discovery of tubercular matter in the sputum he views with mistrust; and as to the presence of elastic fibres, he observes, this is certainly an evidence of destruction, but not of the nature of the destruction, although in a case of bronchitis masking tubercular disease it may render essential service. These, however, are precisely the cases in which the test has been recommended.

As to the treatment of phthisis, believing that the disease mainly depends on diminished respiration, Dr. Smith recommends voluntary deep inspirations and exercise. We have not space here to consider the propriety of such a treatment, but would merely observe, that it is purely a theoretical one, and that no cases are adduced to prove its efficacy.

Dr. Smith's opinion as to the proper diet in phthisis is expressed in the following very vague sentence:—

"As a rule it may be affirmed that much of the hydro-carbonaceous food lessens respiration, and hence such are not the most fitting articles of food; but, at the same time, it is equally certain that they are necessary to the system, and must be supplied to it; whilst their effect in this direction is counteracted by the administration of other kinds of food." (p. 123.)

As to cod-liver oil, he at one place (p. 77) couples it with "the little idol of mesmerism, magnetism, table-rapping, homœopathy," &c.; and at another (p. 127) he regards it as being only of use when the patient is poor, and cannot obtain suitable food, including fats, or when he cannot be induced to take a proper quantity of food.

We have already had occasion to allude to Dr. Smith's voluminous writings on the subject of phthisis, and have come to the conclusion that, while they indicate much labour and industry on his part, they yet contain much that is perfectly theoretical and unwarranted by the facts which he adduces; and that while he accuses others of worshipping "little idols," he is not altogether guiltless of this species of idolatry himself.

XI. *Observations on the History, Pathology, and Treatment of Melanosis.* By Oliver Pemberton, M.R.C.S., Birmingham.—This is a lengthened memoir on the subject of melanosis, and contains a notice of all the recorded cases of the disease to which the author has had access. It is illustrated by some good coloured lithographs. Mr. Pemberton regards melanosis, and we think rightly, as merely a variety of encephaloid cancer, distinguished by the deposit of pigment granules in the interior of the cancer cells, or interspersed amongst the other elements of the growth. These pigment granules, on microscopic examination, we have generally found to be of a dark brown colour, not "black."

Melanotic cancer is considered by the author under the two heads of primary and secondary. The primary form originates in the skin or eyeball, more rarely in the subcutaneous cellular tissue. As regards secondary melanosis, a very interesting table is given (p. 145), showing the comparative frequency of the disease in the internal organs; out of thirty-five cases in which the post-mortem appearances have been recorded, the liver and lungs were the organs most frequently affected. The questions of age and sex, of the total duration of the disease, the length of life after operation, the frequency of recurrence, with the period at which this or secondary deposits took place, are carefully examined. Two cases also are recorded of the disease, which came under Mr. Pemberton's own observation. On the whole, the paper is one of great merit.

Of the twenty-one pages occupied with "Reviews," the greater number are taken up with notices of Dr. Laycock's 'Principles and Methods of Medical Observation and Research,' and Professor Bennett's 'Introduction to Clinical Medicine,' no fewer than fifteen pages being allotted to the former work. These reviews appear to have been written with the object of canvassing the merits of the present rival professors of clinical medicine, and the former rival candidates for the chair of Practice of Physic in the University of Edinburgh. While we fully concur with the Reviewer, that "few men are so well qualified as Professor Bennett to write an introduction to clinical medicine," still, we think that under the circumstances it was scarcely necessary to adduce, as a reason for noticing Dr. Laycock's work, that it emanates from a school which has of late years acquired great pre-eminence in the teaching of clinical medicine, owing mainly "to the zeal, energy, and ability of Professor Bennett." We have already* expressed our own views on the merits of Dr. Laycock's Lectures, which were far from being in terms of approbation; yet we cannot agree with the Reviewer in his sweeping conclusion, that "his doctrines carry with them their own condemnation." On the contrary, as we have already stated, we are of opinion that they contain much that is sound and good, and only regret that the work bears the stamp of too hasty composition. Dr. Laycock is accused of inculcating too strongly the study of logic by the clinical student, and at the same time of being illogical in his own arguments; but we cannot avoid the impression that the Reviewer has made a very bad attempt at logic himself. There are two points also in his remarks to which we must take decided exception. First, Dr. Laycock's "astounding announcement" that "man is but a link" in the chain of created beings, is spoken of in terms of derision. The author seems not to be aware that this opinion has been supported by many of those most competent to judge (such as Linnæus, Cuvier, Illiger,) and that Dr. Laycock is backed up by the first comparative anatomist of the age. Professor Owen, in a recent paper read to the Linnæan Society, makes man a distinct subclass of the class mammalia.† Secondly, it is stated as a "scarcely credible fact, that a Professor in the University of Edinburgh, in the year 1856, should think it necessary to advocate what he terms "physiognomical diagnosis," as worthy of the serious "attention of the student." There are few, we believe, of much experience in the profession, who are not in the habit of attending more or less to this subject. The peculiar physiognomy in phthisis and in cancerous diseases is generally acknowledged; its important indications in the different species of renal disease have recently been pointed out by Dr. Todd;‡ and to these examples we might add a host of others. We should be the last to detract from the value of physical diagnosis; yet, while we admit that perhaps Dr. Laycock hardly assigns to it the position which it deserves, we do think that the present tendency is to place too much confidence upon it, to the neglect of those general symptoms and phenomena of disease on which our forefathers had solely to depend, and which are often of the greatest importance.

The "Reviews" also contain short notices of Dr. Clay's "Handbook of Obstetric Surgery," "The Treatment of Cancerous Diseases by Caustics," by Mr. Langston Parker; Dr. Fuller's work "On Rheumatism," and Griffiths and Henfrey's "Micrographic Dictionary."

Review IX.

Des Anévrysmes et de leur Traitement. Par Paul Broca, Agrégé à la Faculté de Médecine de Paris, Chirurgien des Hôpitaux, &c. *Paris*, 1857. pp. 938.
Aneurisms and their Treatment. By Paul Broca, &c. &c.

A new work of some nine hundred pages, on the nature and treatment of aneurism, will perhaps be considered by some surgeons in the present day, an unnecessary and superfluous addition to the monographs we already possess on this important and oft-occurring malady. It is therefore our duty to examine how far M. Broca has proved his labour not to have been superfluous, while we also endeavour to ascertain whether the volume before us, the result of that labour, can be considered unnecessary.

"I have been obliged (says our author in his preface) not only to illustrate the modern methods, but to reconsider every method that relates to the treatment of aneurisms. This

* Brit. and For. Med.-Chir. Review, vol. xix. p. 159.
† Journal of Proceedings of Linnæan Society, vol. ii. p. 37. 1857.
‡ Clinical Lectures on Urinary Diseases, p. 120.

will account for the unusual length of the work. To be certain not to travel out of facts, I have collected, analysed, and grouped together nearly 1100 cases of aneurism. I have given special attention to conditions observed after death. I have thus acquired accurate notions on the modes of action of the different methods of treatment, and I have been able to describe many important phenomena with which authors until now have not even been occupied."—

M. Broca almost apologises for bringing himself to the notice of the profession in a form so massive; and though some of his chapters might certainly have been curtailed without diminishing the value of his volume, we have every reason to rely upon the industry of the author, to feel satisfied that his conclusions have been matured in a large field of observation, and to pronounce that his industry and observation have produced a work the practical nature of which must make it one of high standard reference.

M. Broca divides his volume into two portions. The first relates to the Pathology of Aneurism; in the second, by far the largest share of the book, he most fully investigates the various methods of treatment.

Commencing with a wish to define what lesions are to be considered aneurisms, in their pathology and in their treatment, M. Broca naturally investigates what should be intended by the word aneurism, and to what conditions of diseased or altered bloodvessels the term ought to be strictly confined.

The confusion that has arisen in the definition of aneurismal tumours of which M. Broca complains, is owing, he thinks, to the circumstance that surgeons have rather been guided in their nomenclature by the symptoms of a tumour, than by its pathological conditions; and consequently any tumour possessing pulsation concurrent with the heart's action, has been considered aneurismatic in its nature. He wishes, therefore, to limit the term "aneurism" to tumours which are circumscribed, filled with liquid or coagulated blood, communicating directly with the canal of an artery, and limited by a membrane usually termed the sac. And though he states that this definition is alone capable of distinguishing aneurisms from other tumours of an arterial origin, he candidly confesses it will not sufficiently answer all practical purposes,—because certain sanguineous and pulsatile tumours may possess no sac at a certain period, but may become ultimately encysted—because it is often impossible *during life* to know whether such tumours are diffuse or surrounded by a membranous sac—and because, in either case, the therapeutical indications are the same as in diffuse aneurism.

Diffuse aneurism is the variety M. Broca first considers, dividing it into three kinds, the "true diffuse form" of Chelius being entirely excluded from this division—

1. The first, resulting from wounds of arteries, he terms "Anévrysmes faux primitifs."
3. The second, occasioned by simultaneous rupture of all the arterial coats, "Anévrysme par déchirure ou par épanchement."
3. The third, resulting from rupture of an aneurismal sac and extravasation of a portion of its contents, "Anévrysme diffus des auteurs Anglais."

He says, with respect to the first, that to apply the term "diffuse aneurism" to the mere escape of blood from a wounded artery into the surrounding tissues, is confounding the wounded artery, the hæmorrhage which follows, and the infiltration of blood which takes place subsequently, with the traumatic aneurism which takes place sometimes—but *sometimes only*—after the accident. In fact, that after the wound of an artery, he observes, "it is true the pulsations of the extravasation continue, or reappear after having ceased—the external wound has cicatrised—and the tumour presents ultimately the progress and anatomical characters of an aneurism." "This only occurs when the escaped blood becomes circumscribed in a cavity, and enclosed in a membrane newly formed; previous to this the tumour constitutes but a simple escape or infiltration of blood." The second variety, resulting from laceration of the coats of an artery, resembles in many respects the first, but there is no external wound, and the extravasation occurs among the deeper and the more superficial structures—*c'est là toute la différence.*

These two divisions M. Broca excludes from the class "Aneurisms." For as in the one, so in the other—to speak correctly—if there is not yet a sac, there is then no aneurism. This is, however, about to occur, and ultimately the extravasation becomes a definite aneurism, by the surrounding tissues forming a circumscribing wall, which limits the extent of the cavity, and occasions a boundary to the extravasated blood. And to these species of aneurism M. Broca proposes to apply the term "primitive diffuse aneurism" (anévrysme diffus primitif).

The third variety, the diffuse aneurism of English authors, M. Broca considers analogous to the foregoing, and refuses the name of aneurism to this secondary condition. For, says he, reasonably enough, when the rupture of an aneurism gives place to an extensive extravasation—when an aneurism of the aorta bursts into a serous cavity—the aneurism is extinguished (*éclipsé*) by this grave complication; and no one can view the distension of the pleura or the pericardium by blood, resulting from the rupture of the aneurism, as the aneurism itself. So when an external aneurism bursts, one should not give to the diffused extravasation the name of aneurism.

"Cette tumour, dans laquelle l'anévrysme n'occupe plus désormais que la seconde place, ne peut pas être considérée comme un anévrysme. . . . But (he continues) the rupture of the sac may be very limited—blood escapes in a very small quantity—and we see succeeding all the phenomena which accompany a very slight laceration of an artery. A second tumour, of variable size, and also of variable shape, but always circumscribed, now surmounts the primitive aneurismal tumour." (p. 8.)

Then, he asks, ought we to refuse the name of aneurism to a tumour modified by this one circumstance, that at a single point of its boundary it is deprived of its sac? No; English surgeons say in such a case that an aneurism has become diffuse. M. Broca adopts this distinction; and to mark the variety from the primary diffused form, he designates it the consecutive diffuse aneurism (*anévrysme diffus consecutif*). Diffuse aneurisms, he adds, differ from other aneurisms in one respect only—in the absence of a membranous sac; in all other respects they approach each other in resemblance; and though pathological anatomy rightly distinguishes between them, in practice it is but proper not to reject them altogether from the class of aneurisms.

To complete our observations upon M. Broca's ideas relative to diffuse aneurism, we must follow our author a little further.

Diffuse aneurisms, he says, resemble ordinary hæmorrhages into the cellular tissue in origin and in nature; but they are distinguished by their circumscribed form, and by the presence of a tumour. It may appear strange at first sight to give the name of diffuse aneurism to a tumour which in its character is circumscribed. But, he adds, compared to ordinary diffuse hæmorrhages, diffuse aneurism, it is true, appears "circumscribed"—compared to aneurisms which are ordinarily surrounded by a membrane, it well deserves to be termed "diffuse."

The varieties of true aneurism are next considered, or rather we should say, in the words of the author, all other aneurisms which for their common character must be limited by a membrane termed the "sac." The nature and the connexions of this sac serve for the foundation of his classification; and after alluding to its communications either with an artery alone, or with an artery and a vein, he enters into the considerations of the varieties which result from these separate lesions.

Of cystic aneurism (*Anévrysme Kystogénique*), M. Broca mentions two interesting specimens which were presented by M. Leudet to the Anatomical Society of Paris. Corvisart was the first to describe this variety. It is said to commence as a cyst with dense cartilaginous or calcareous walls, containing fatty or atheromatous matter, and attached to the coats of an artery. In its growth, or after a certain duration, it opens into the artery; and as the orifice increases, the septum between the cyst and vessel becomes absorbed, and the aneurismal pouch is formed. M. Broca, sceptical himself as to their true character, had the opportunity of examining the specimens alluded to above—and which appear to illustrate the formation and progress of several of these cysts—but he adds, honestly, "Je suis disposé à admettre la réalité des anévrysmes kystogéniques;" though he objects to the term, and states his reasons against such a condition being looked upon as aneurism resulting from a cyst connected with an artery; he rather supposes that at one stage of the disease there had been a tumour produced by the solidification of an aneurism, and that this may have softened and broken down. He says, however, that there are a few facts tending to establish the primary existence of these cysts, and their subsequent communication with their corresponding arteries. Nothing in our experience enables us to confirm or condemn M. Broca's conclusions; we have not been able to satisfy ourselves that aneurism is ever the result of such cysts as he examined and describes.

The varieties of traumatic aneurism, and of spontaneous, true, false, and mixed aneurism, next occupy the attention of the author; but there is little here to detain us, however much we are inclined to recommend the perusal of the original to the reader.

Of varicose aneurism and its varieties, M. Broca speaks very fully. He points to William Hunter as having been the first to describe the true nature of the arterio-venous communication, and with true liberality pays him a well-merited compliment. It was to this arterio-venous communication that Cleghorn, of Dublin, first gave the name of aneurismal varix. One interesting case in illustration of the varieties of these traumatic conditions is cited by M. Broca. A sharp-pointed knife thrust into the thigh transfixed the femoral artery, and entered the vein, but did not pass through the latter. The openings in the vessels remained patulous, and produced two very different effects. The edges of the corresponding openings of the vein and artery united together, and formed by their union a permanent communication between the two vessels, as in simple aneurismal varix. The superficial wound in the artery also remained patulous, and was followed in time by the formation of an aneurism; consequently an aneurismal varix and an arterial aneurism co-existed opposite to each other, the result of one accident, and affecting the same artery.

M. Broca dwells upon the importance of distinguishing varieties of arterio-venous aneurisms from each other; for the particulars of the terms which he applies to them for this purpose, we must refer the reader to the original. Though all the distinctions, he says, are interesting in themselves, many of them lose their utility when we regard them practically. He does not forget to consider their clinical and anatomical differences, and especially the therapeutical indications attached to each variety.

The causes of aneurism—ably discussed in a very interesting chapter—are divided into the traumatic and the spontaneous.

Respecting the former there is not much to detain us. M. Broca speaks of the tendency in certain constitutions to the formation of aneurism after the application of ligatures; and he mentions a case in which, after amputation of the foot, the posterior tibial artery became the seat of aneurism in the stump; that ligature of the femoral artery was deemed advisable, and having been applied, succeeded in obliterating the aneurism.

This tendency to aneurism upon the application of a ligature to an artery at the part which is tied, he properly observes, is sufficiently rare and but little known; but he enters into an explanation of his views as to the manner in which it may and probably does arise.

While referring to the causes of accidental aneurism, M. Broca has very judiciously drawn attention to that rare condition which is the result of an abscess in contact with an artery. He observes:—

"One has often seen arteries denuded by suppuration, rupture after the opening of an abscess, and give rise to hæmorrhages of an alarming character. It is not impossible that an effect similar to the latter may take place previous to the opening of an abscess, and that the abscess invaded by the blood may suddenly be transformed into an aneurism." (p. 42.)

In speaking of the causes of spontaneous aneurism, M. Broca makes an observation which we think desirable to quote. He states that we rarely observe aneurism in the arteries transmitting venous blood; and he inquires whether the structure of the vessel itself, or the condition of the travelling fluid, is the cause of the almost universal occurrence of aneurism in vessels transmitting arterial blood?

He assisted at the examination of the body of an old woman who died in the Salpêtrière, under the care of M. Moissenet. This woman had been the subject of well-marked cyanosis, the result, as M. Broca states, of a patulous "ductus arteriosus;" all round the pulmonary orifice of this canal, the walls of the pulmonary artery, perfectly healthy elsewhere, were in their whole circumference the seat of an abundant deposit of calcareous matter, like that which existed in the aorta. He believes that this condition is to be attributed to the reflux of a certain quantity of arterial blood into the pulmonary artery at each systolic action of the heart.

After examining carefully the various points connected with the formation of spontaneous aneurism—causes, frequency, age, &c. &c.—M. Broca arrives at the conclusion, that in proportion as man advances in age, the disposition towards the formation of aneurism augments in those arteries which are situated above the diaphragm, and that it diminishes in those placed below the diaphragm. M. Broca arrives at this conclusion after a very careful analysis of no small number of cases. But to satisfy us of the fact, or to justify any such definite assertion being accepted as authority, it requires a very much greater number of very carefully recorded cases than he argues upon. Women are less liable to spontaneous aneurism, he finds, than men: habits of life, occupation, and other influences, he thinks, account for this.

Though M. Broca does not ascribe any importance to the general occupations of trade or the requirements of calling as the immediate causes of aneurism, the very frequent occurrence of popliteal aneurism—which constitute one half of surgical aneurisms, and occur chiefly in individuals by whom violent efforts are made in their lower limbs—he ascribes to the anatomical conditions of the soleus muscle. He thinks that its tendinous arch exercises, when in action, a considerable influence upon the circulation in the popliteal artery; and that the repeated contractions of the muscle upon the artery in constant and violent exertions, produce a tendency to aneurism.

This explanation is not to be entirely cast aside. That the action of the soleus may exercise some influence upon the walls of the artery is by no means improbable, but that the sudden straightening and bending of the latter, and the frequent obstruction to the circulation by pressure on the part, produce a greater influence, is much more probable.

In flexion of the leg, it must be remembered, that while the artery is bent, the tendon of the soleus is most relaxed; but as the latter becomes stretched and tightened, the artery assumes the straight position; the circulation is probably as free at this part when the soleus is contracted as when the artery is bent. M. Broca says, in his impartial manner, that his explanation is but an hypothesis; and if true, is only an additional cause to others already known to favour the production of popliteal aneurism; and that although mechanical causes may influence their immediate production, these would most frequently be insufficient, unless favoured by a previously altered condition of the coats of the artery.

The general causes, the symptoms, the progress, and the termination of common aneurism are carefully discussed, and we wish to draw especial attention to M. Broca's observations respecting the necessity of amputation when certain local complications render all other surgical interference not only entirely useless, but mischievous.

The characteristics and symptoms of aneurismal varix next come under consideration. M. Broca mentions a very remarkable case of venous dilatation, the result of an arterio-venous opening, which was communicated to the Surgical

Society of Paris, by M. Duménil. A thrust of a knife penetrated the abdomen about nine centimetres below the umbilicus, and six centimetres to the *right* of the median line. The point of the knife opened the *left* common iliac vein, and probably the *right* common iliac artery. The "bruit de souffle" was most distinct at this point; and the veins of the *left* lower limb, and of the *left* half of the abdominal wall, became varicose. The skin and cellular tissue of these parts became the seat of a kind of œdema somewhat resembling the condition of elephantiasis. The calf of the leg measured fifty-eight centimetres in circumference, and the thigh was eighty-eight round. M. Broca has observed, in limbs affected with aneurismal varix, and the accompanying varicose condition of the veins, that the growth of the hair is often affected—that it becomes thicker in consistence, darker in colour, and greater in length than that of the opposite limb. He also alludes to the circumstance of an increase in the length of a leg and foot in a growing youth who was the subject of aneurismal varix. In both conditions he attributes this hypertrophied state of the parts affected, to an impediment to the ready and uniform return of the venous blood in the implicated vein, occasioned by the arterial pulse.

"I ought to state (he says, in concluding his remarks upon varicose aneurism) that I know of no case of spontaneous cure of this accident, if of some days' standing. But, without denying the possibility of such an occurrence, I am not less authorized in saying that the varicose aneurisms have infinitely less tendency towards spontaneous cure than arterial aneurisms. At the same time, left to themselves, they are much less serious in their progress than the latter; but it is much more difficult to procure their obliteration." (p. 83.)

One of the most important chapters in the whole of M. Broca's work is that which deals with the pathological characters and conditions of aneurisms, or what he terms, "Physiologie pathologique des anévrysmes." The *form* of the sac, and the nature of its connexions with the artery, exert the most important influences on the circulation of the blood which flows through the former; and M. Broca says truly enough, that as far as spontaneous cure or recovery the result of treatment is concerned, the origin and mode of formation of aneurismal sacs are points of much less importance, practically considered, than their form. He therefore divides arterial aneurisms, for all practical purposes, into four varieties:—

1. The cup-shaped (*l'anévrysme cratériforme*), in which the sac is simply a depression in the walls of the artery, and through which the blood circulates almost as freely as in the artery itself.
2. Diffuse aneurism (*l'anévrysme diffus*), communicating with the artery by a fissure more or less extensive, having an irregular cavity, and its irregularity modifying greatly the circulation of blood through it.
3. Sacciform (*l'anévrysme sacciforme*), consisting of a cavity which communicates with the artery by an opening very much smaller in proportion to the size of the sac.
4. Fusiform (*l'anévrysme fusiforme*), in which the sac communicates by both its extremities with the artery.

In the first variety the blood is renewed so rapidly in the sac, that it has scarcely any time to deposit fibrine or form coagula.

In the second the blood circulates so irregularly in the uneven cavity of the aneurism, that it becomes almost completely stagnant at certain points, and has a tendency to deposit what M. Broca terms "*caillots passifs*," what we may term soft coagula. In the last two varieties, the blood circulates in the cavities of the aneurismal pouches in a manner regular, though not uniform. It moves much less rapidly than in the adjacent artery. It has a tendency there to part with a portion of its fibrin; this becomes deposited in layers, and constitutes, in contradistinction to the coagula already mentioned, "*caillots actifs*." We may perhaps be allowed to apply the terms "temporary" and "permanent" respectively to what M. Broca designates by the expressions "passif" and "actif."

The practical importance of this distinction cannot be too strongly urged, in connexion with the clinical history of aneurismal tumours. M. Broca deserves great credit for the masterly manner in which he has placed this portion of his subject before the profession. To use his own words, the permanent or fibrinous coagula are those which are formed under a vital influence. The temporary or passive clots are those which are formed when the blood ceases to obey the laws of life.

It will be observed, says M. Broca, that the following distinctions may be made as to the prospect of spontaneous cure in aneurisms:—

1. In the cup-shaped variety, the blood has very little chance of coagulation.
2. In diffuse aneurism, the soft temporary coagula are alone apt to form.
3. In the other varieties, all the conditions are favourable towards the formation of fibrinous deposits (*caillots actifs*).

In the cup-shaped aneurism (*anévrysme cratériforme*), art as well as nature will in all probability fail to produce fibrinous clots, and this form of aneurism must be regarded more with pathological than with clinical interest.

In the other varieties, coagulation more readily and more usually occurs. In the diffuse variety we generally find the soft temporary coagula. In the last two, fibrinous clots are constantly deposited by nature, or under the influence of art. In both cases, though not in equal proportions, the chances of recovery by obliteration are rendered by so much the more probable.

M. Broca devotes a long chapter to the consideration of the nature and mode of formation of the two varieties of clot. Though Mr. Hodgson was, we believe, the first to draw attention to the essential and important difference between ordinary coagula and the fibrinous deposits found in aneurismal pouches. Mr. Bellingham was the first, says M. Broca, to indicate their true origin; and it is but justice to the latter to state in passing, that he is ever ready and anxious to give the merit of any new idea or proposal to the individual with whom it may have originated, nor does he ever attempt to appropriate to himself the credit due to others.

The author very carefully discusses all that relates to the nature and character of the "passive" and "active" clots, and the differences between them, and enters into an analysis of the most recent investigations relating to them.

We wish our space allowed us to consider the causes of spontaneous arterial aneurism as fully as the subject would justify; M. Broca's remarks will repay perusal on this point. The practical result is, that they tend strongly to establish the importance of treatment by compression—they confirm the principles upon which it is based—and they mark the conditions which should allow it to be attempted, and also the conditions which will enable it to procure success. He points out why, in the one case, all the known evils attendant upon an aneurism may occur, and why these evils are to be attributed to the formation of sanguineous or "passive" coagula. He also points out why, in the other case, the deposition of fibrinous layers is always followed by a steady obliteration of the aneurismal cavity, and permanent cure of the evil existing, without accident or drawback, and without those numerous fears of relapse or retrogression which occur in the former case.

> "Fibrinous deposits (to use the words of the author) do not act as foreign irritant bodies, in the manner sanguineous clots often do. They retract, they condense, and, as cure is effected, become ultimately absorbed altogether or in part; and in the latter case, only leaving a tumour in the situation of the once increasing aneurism, hard and small, without pulsation, perfectly inoffensive, and quite compatible with the exercise of the functions of the limb." (p. 183.)

Thus far we have confined our remarks to the first portion of M. Broca's work—i. e., to the pathological considerations relating to aneurism. We now enter upon the second part. This embraces the question of treatment; and, as we have already remarked, comprises by far the larger portion of the volume. A more complete examination of the various methods of treatment has never yet been made.

Commencing with the ancient treatment of aneurism, M. Broca traces the history of the various measures for relief, from the period when excision of the sac or opening the aneurism was practised, down to the present improved methods of indirect modified pressure which have been adopted with such signal success, and with such general advantage.

M. Broca awards to Antyllus the credit of having applied for the first time a ligature to an artery for the cure of aneurism. There is some difficulty in fixing the exact period when "ce grand chirurgien," as he terms him, lived; but he concludes that it must have been about the time of Valerian. Antyllus applied the double ligature above and below the aneurism, and was in the habit of subsequently opening the sac. Minus the opening of the sac, the application of the double ligature was pretty generally adopted to the time of John Hunter.

For the relief of traumatic aneurism, the double ligature, according to M. Broca, was first applied in 1812, by M. Pasquier. Roux also used two ligatures in simple aneurismal varix, but if in such a case an aneurismal tumour existed in addition, his practice was to make an incision into it. A couple of pages are devoted to the ancient treatment by extirpation. M. Broca thinks that the only condition under which an aneurism could possibly be excised in the present day, is that of complete solidification mistaken for a fibroid or other growth. We may be permitted to express a hope that such a blot may never darken a page in the history of British surgery.

Of the actual cautery—of caustics, of styptics, and of moxas—we need not speak. We think, however, that M. Broca does not appear to be aware of all that has been done in this country, with platinum wire heated by galvanism in certain cases of nævi, the value of which, in our estimation, ranks before that of all other escharotic measures. He speaks highly of the application of perchloride of iron to the surface of superficial aneurisms of the skin—recommending, first, the removal of the integument by blistering, and then the application of a pledget of lint, wetted with the solution, to the raw surface. This treatment appears to have succeeded in his hands.

M. Broca has not overlooked the consideration of that treatment of aneurisms which he terms "malaxation," termed in this country "the treatment by manipulation," and which consists in disturbing the contents of an aneurism by external pressure and friction, lately advocated by Mr. Fergusson, in a paper read before the Royal Medical and Chirurgical Society. We have to turn back to a passage in a former part of M. Broca's work, to introduce our own observations on this proposed method of treatment. He says:

> "Many authors since Richter have spoken of a method of cure which is possible no doubt, but which appears to be as yet hypothetical. They suppose that by some accidental or other cause a portion of the fibrinous clot, which lines the inner surface of the sac, may become detached; float in the blood which fills the sac; become engaged in the distal orifice of the aneurism, and interrupt at last all communication between the latter and the artery beyond." (p. 161.)

On this principle it appears to us that Mr. Fergusson advocated the treatment by "manipulation," and that he contemplated the possibility of bringing about the conditions indicated in the foregoing remarks, in certain aneurisms, in which direct or indirect pressure, a single or double ligature, could not be safely applied, and in which mechanical interference with knife or cautery would be impracticable and dangerous.

A case presented itself to Mr. Fergusson, which appeared in all respects exactly suited to the adoption of this treatment. The aneurism was connected with the right subclavian artery, between and outside the scaleni muscles. By pressure of the thumb the sac was emptied of its fluid blood, and the inner surfaces of the sac were gently rubbed against each other. The pulse was immediately arrested in the vessels below the aneurism, and continued so for six or seven hours, when it returned. A similar operation repeated the next day, was followed by a more lasting effect on the pulse in the arm. For seven or eight days it could not be readily detected.

The tumour gradually diminished in size, the pulsation became less evident. The tumour was much diminished at the end of seven months: in the mean time a branch of the subclavian at the root of the neck gradually enlarged, and other indications gave hopes that recovery was taking place. But at the end of seven months, the patient was attacked with fever and excruciating pain in the tumour, and died after a few days' illness. The sac of the aneurism had ruptured in the direction of the axillary plexus. *The axillary artery was found blocked up.*

A second case, in most respects resembling the first, was treated in a similar manner. The aneurism in this instance disappeared between the 22nd and 24th month after the "manipulation" had been practised. The supposition that a clot detached in an aneurism might obstruct the distal orifice of the artery, and effect spontaneous obliteration of the sac, we have shown to have been long entertained; but no record had as yet proved its actual occurrence until Mr. Fergusson demonstrated it in his first case. He was also the first to attempt to copy the accidental displacement of the clot in an aneurism. The results he has placed before us.

M. Broca is very candid in his remarks upon these cases. But before alluding to them, let us consider what is due to Mr. Fergusson, and how much credit may be attributed to his proposal.

In the first place, Mr. Fergusson proposes that this treatment should be adopted only when all other known and useful measures are out of the question, either on account of the situation, or the nature or other conditions of the aneurism. So far, then, it would but be applying a method of treatment in cases in which no other form is available. The treatment itself is simple, and it does not appear to have been attended in these cases with any immediate evil consequences. In both cases there was distinct evidence of obstruction to the circulation, through the distal extremity of the tumour, following immediately upon the disturbance of the contents of the sac.

But as to ultimate results? We cannot feel satisfied that the first case offers any evidence in favour of the efficacy of the treatment. Nor is there sufficient accuracy in its record, nor sufficient detail of its progress, to allow us to discuss it fairly on its own merits. For Mr. Fergusson in his communication to the Medico-Chirurgical Society, says, after seven months the tumour was much diminished; the patient then had a feverish attack, accompanied with excruciating pain in the tumour, and died after a few days' suffering. In a letter addressed by Mr. Fergusson to M. Broca, and published in the volume, we read:

> "Le malade sur lequel vous me questionnez alla bien pendant quelque temps, la tumeur décroissait graduellement, mais vers le quatrième mois après les manipulations, il survint tout à coup, à la partie moyenne de la tumeur, une douleur tellement vive, que la malade y succomba au bout de trois jours." (p. 244.)

The fatal termination of the case *at the end of the fourth month after the trial of manipulation*, cannot in any manner justify us in receiving it as one offering the slightest evidence in favour of such treatment, though we must not lose sight of the evidence obtained during the treatment—viz., that the disturbance of the contents of the sac may act upon the distal orifice, by some portions being stopped there, and thus affect the aneurism in the same manner a ligature would, if applied to the artery beyond the sac.

As far as immediate effects and ultimate results in a single case can be any guide in treatment, we have in Mr. Fergusson's second case thus much to direct us. We have evidence of cessation of pulse in the arm, and we have entire obliteration of the sac following "manipulation," in some twenty-four months.

It may be argued that, as the entire obliteration did not occur until nearly the lapse of two years, the case can scarcely be received as evidence in favour of the proposed treatment; but as we know that many aneurismal tumours would be months disappearing after the application of a ligature to the proximal portion of an artery, it is not too much to grant, that some months must elapse before pulsation would entirely cease, and all tumour disappear, in the event of a clot blocking up the distal extremity of an aneurism.

No conclusions regarding treatment can be drawn from a single successful case, supposing this to have been one. Neither do we believe that Mr. Fergusson intended this; but rather, having met with these cases in his practice, he considered it right to place them before the profession, to illustrate, as he thought, a method of treatment, experimental rather than authorized, in a class of cases in which all other measures are impracticable or more dangerous.

M. Broca is of opinion that "manipulation" of an aneurismal sac cannot secure dislodgment of a clot, and its subsequent entanglement in the distal orifice, without extreme hazard. The clots, he observes, in the first instance must be sufficiently friable for the purpose in view, a condition which can never be foreknown. The layers are often of such density, that in the effort to detach them the sac may be ruptured. He considers the method uncertain in its results; and when it completely fulfils its object, it partakes of all the uncertainties of Brasdor's proposal—that of applying a ligature at the distal extremity of the sac. Above all, it is exposed to two serious evils—the rupture of the aneurism during the manipulation, and inflammation provoked by the bruising of the tumour. He adds, in conclusion, notwithstanding the result in Mr. Fergusson's second case, he cannot hide from himself that the method adopted does not inspire him with any confidence—that it is very uncertain and very dangerous, and more dangerous than uncertain. The impending rupture, or the consecutive inflammation of aneurisms of this category are so notable that he would give the preference in similar cases to galvanic puncture, coagulating injections, or to Brasdor's proposal—for, of all these, "la malaxation n'est qu'une dangereuse modification." The introduction of heated needles into the sac of the aneurism, recommended by Sir E. Home, and the treatment by refrigeration, are measures generally condemned by M. Broca.

The chapter on treatment by direct compression of aneurism is worthy the attention of the student in surgery; and creditable to the author, as containing an impartial, sound, and practical view of this once important question. We sum up his concluding remarks almost in his own words. To enumerate the principal points in the history of the treatment of aneurism by direct compression, we find that the method is always very uncertain in its results—it is entirely inefficacious in the great number of cases—it is often intolerable—it exposes the patient to the risks of inflammation or of rupture of the sac—to gangrene of the tumour or mortification of the limb—that it should be absolutely banished from our thoughts in the treatment of large aneurisms, and reserved alone for the treatment of aneurisms of small size and recent origin.

A work devoted to the consideration of aneurisms would be incomplete, did it omit an examination of the proposed treatment, of the injection into the sac of fluids which coagulate the blood. M. Broca has not been less careful in this than iu every other respect. He displays the merits and disadvantages of chymical agents thus applied. They are not, he thinks, to be entertained with excess of favour, nor should they be utterly condemned. We cannot yet estimate their value from what has been done; and he recommends, above all, that many more experiments should be made on animals to test their action and value. Though, he adds, they can but be applied with safety to the small aneurisms of the forearm, scalp, &c.

We have dwelt upon the foregoing chapters of M. Broca's work at some length, but we trust not longer than their importance deserves. We have now to notice the most practical, and consequently the most interesting and most important portion—viz., the treatment of aneurism by ligature, and by compression.

The application of a single ligature to an artery between the tumour and the heart was first adopted by Anel in France, to this extent, that he placed the ligature *immediately above* the aneurism—Anel operated thus in 1710. In 1784, Desault attempted compression of the subclavian in a case of axillary aneurism;

but the patient, unable to bear the treatment, left the hospital, and subsequently died under the care of another surgeon (Ferrand), who, mistaking the aneurism for an abscess, had the misfortune to open it, and saw his patient expire within a few minutes. Failing in his attempts at compression, Desault in the following year revived Anel's suggestion, and tied the popliteal just below the aperture of the adductor, and immediately above the aneurism. Notwithstanding subsequent rupture of the sac, the patient recovered from the immediate effects of the operation, and lived eleven months.

In the same year, 1785, but some months later, John Hunter performed his first operation of tying the femoral at a distance from the seat of aneurism. In this case, as in the next two which came under his care, Hunter included both artery and vein in the ligature. In his first case he applied four ligatures to the artery, and his patient survived fifteen months.

The question of priority between these two eminent surgeons, in reviving or recommending the application of a ligature above the aneurism, has already sufficiently occupied the field of discussion. But M. Broca, with a very proper feeling, endeavours to do honour to his countryman, Desault, for having been the first in his day to revive Anel's operation, and for having anticipated Hunter in this respect. The merit, however, should be shared, to a great extent, by these great surgeons. Desault certainly anticipated Hunter in the application of a ligature above the aneurism, without placing another below it, or opening the sac. But the ligature was applied *immediately above* the sac, and its application was successful.

Hunter tied the femoral in the middle of the thigh, but included the vein in the ligature; and he used four ligatures for the artery at the first operation. This case, however, recovered, and the man lived for several months. Desault's operation was so far preferable, that he only used two ligatures, and did not include the vein: Hunter's was so far superior, that the seat of ligature was far removed from the aneurismal sac. Desault did not live to improve upon his first attempt, nor to carry out the suggestions of Hunter: Hunter lived to see his practice perfected by experience, if such surgical interference with an artery in a case of aneurism can be considered to approach a state of perfection.

M. Broca has done well in advocating the claims of his countryman—the manner in which he has spoken of Desault's merits is most fair and praiseworthy.

Hunter's proposal was longer gaining universal adoption in France than elsewhere on the Continent. The first cases operated on in Paris appear to have been attended with singular fatality. Chopart, Guerin, Vernet, Pelletan—all appear to have practised it, but the conclusion they arrived at was, that in its results, opening the sac had not been more fatal.

The political state of France in the commencement of the present century did most in retarding its general adoption in that country. Roux tied the femoral with success in two instances in 1815; and M. Broca states that a few days after the second operation, Napoleon's abdication being followed by the peace of Europe, they learnt in Paris for the first time with astonishment, that inguinal, carotid, and subclavian aneurisms were no longer in England considered beyond the reach of treatment—that Abernethy had tied the external iliac, Astley Cooper the carotid, Stevens the internal iliac—that Ramsden had ventured to tie the subclavian beyond the scaleni—that Colles had dared to apply a ligature upon it within the scaleni. Subsequently they read that in New York, Scott had tied the innominata and common iliac—and that Cooper, "plus audaçieux encore," had not been deterred from placing a ligature round the abdominal aorta. It must be borne in mind—and M. Broca does not allow us to forget it—that Hunter's proposal is only applicable in certain aneurisms. Anel's can be adopted in all those in which little space is found for a ligature above the tumour.

The conditions which occur in the artery and the sac, subsequent to the application of a ligature, are considered with some originality by M. Broca. The course of the blood in this case, he states, is never entirely suppressed beyond the ligature—but that in a few minutes after its application, though suspended, the circulation is re-established to a slight extent. This, he observes, has been admitted by Hodgson, and that modern surgeons will not deny it; but its effects have never yet been sufficiently explained, which has left a blank in the history of the reparative process after ligature in aneurisms, which he endeavours to fill up.

The quantity of blood passing through the sac for the first few hours after the ligature is applied, will vary according to the peculiar features of the case, the number of the collateral vessels and their size, and the readiness with which they dilate. In one case ligature of the femoral may be followed by gangrene, while in a second ligature of the iliac will not compromise the vitality of the limb.

In the case of Hunter's operation, this return of the blood through the artery and sac, differs from the previous condition of the circulation through the aneurism, only in so much that it is reduced in activity, and passes but slowly through the vessel and sac. In Anel's method, such a return of blood will not occur, as no collateral vessel (as a general rule) will be found between the ligature and the sac—complete stagnation of blood takes place in the aneurism, with the prospect of soft sanguineous clots (*caillots passifs*) being formed.

After Hunter's operation, the course of the circulation is impeded, but not arrested, through the sac; consequently there arises a disposition towards the formation in it of fibrinous deposits (*caillots actifs*).

But there are circumstances under which either operation may be followed by the deposition of one or other variety of clot; for, says M. Broca, all other things being equal, the blood in certain patients readily deposits fibrin—in others, this deposit is effected with the greatest difficulty.

The real difference in the value of the two operations amounts to this—in Anel's proposal, if the aneurism is large, a large soft clot will be formed in the sac, with little prospect of any return of circulation to carry it away—it remains subject to all the ill consequences that M. Broca has demonstrated to follow depositions of the "caillots passifs." It is inapplicable in many cases, and it should only be adopted when there is not room to follow Hunter's plan. Need we dwell on the merits of Hunter's operation? Let us quote the words of the author:—

"Le procédé de Hunter possède ici une grande supériorité sur le procédé d'Anel, parcequ'il porte une atteinte beaucoup moins profonde à la circulation de l'anévrysme. Le procédé de Hunter lui-même agit d'une manière d'autant plus favorable, qu'on laisse un plus grande nombre de collatérales entre le sac et la ligature." (p. 515.)

In estimating the advantages of the two operations, M. Broca says, if the proceeding of Anel is inferior in its mode of action, it is superior in being less frequently followed by gangrene, and perhaps also by secondary hæmorrhage. The two proceedings have their advantages and their disadvantages, which in his opinion balance their merits. Hunter's operation is only applicable to arterial aneurisms, and to these alone when occupying certain positions. In any aneurism of the neck or groin, the ligature must necessarily be applied near the sac, and so far the operation approaches Anel's rather than Hunter's. With all the advantages of the treatment by ligature, it is a question of great importance to decide how far its general application is successful, and what are the results, when its effects are compared with those of the more modern proposal of compression. Mr. Benjamin Phillips collected 389 cases of aneurism treated by ligature, in which number 112 deaths occurred. This ratio of death agrees very much with what M. Porta observed—he found that in 418 cases of this operation there occurred 117 deaths.

After alluding to the results of operations on the different vessels of the body, M. Broca concludes with the following remarks on ligature of the aorta, which we recommend to the notice of any who may be tempted, in a fit of reckless enthusiasm, to entertain any interference with that artery:—"Je ne parlerai pas de la ligature de l'aorte, cette erreur du grand A. Cooper. Il n'y a que les chiens qui en échappent. Tous les hommes à qui on l'a infligée jusqu'ici ont promptement succombé, et il serait temps qu'on y renonçât;" and speaking of operations on the innominata, he says—"Treize opérations, treize morts. J'espère qu'on en restera là, et que ce chiffre malheureux servira d'avertissement."

Brasdor's proposal of applying the ligature beyond the sac, occupies next the attention of M. Broca. All who have considered this method of treatment will feel with him "that its effects are always uncertain, and that it only presents itself as an ultimate resource in cases which, without it, would be absolutely desperate." As this plan of treatment has cured some cases, and ameliorated the condition of others, it cannot be considered a useless remedy, though hazardous in its application, and doubtful in its results.

The most interesting, the most novel, and we think the most important, portion of M. Broca's work, is that which relates to the treatment of aneurism by indirect compression. We should not gain much by bringing the history of this method of treatment before our readers. We feel, however, that there has been a little unnecessary and ill-timed jealousy, on the part of some of those who, of late years, have directed much attention to the treatment by compression, and who desired to claim for themselves, the honour of priority for having brought it to the notice of the profession. An impartial examination of the works of their predecessors by the more modern authorities on this subject—such an examination as M. Broca has made of their own investigations—would have satisfied them that they were entitled to but slight claims to such an honour, whatever credit may be their due for improvements in the treatment by compression.

It is an interesting fact, in proof of M. Broca's great care in collecting information upon this method of treatment, to mention the following circumstance. In 1825, Guillier Latouche published a thesis at Strasburg, on a new method of making continued pressure on the principal arteries of the limbs, for the cure of aneurism. He considered in this paper, that the indication in treatment was to *diminish* the force of the circulation in the sac; and consequently, that it was not necessary for the obliteration of aneurism, to make pressure to such an extent as to stop all circulation through the artery at the compressed point. He concludes by stating, that as the course of the blood is rendered very feeble in its action through the sac, clots will be deposited, pulsations will cease, and the aneurism will be cured. His method was to apply several tourniquets to an artery, so disposed as to admit of one being loosened, while another was being tightened, over the vessel; and he gives minute directions as to their mode of application.

That this is the most effectual manner of applying compression, all surgeons will now admit; and that this was the first suggestion respecting this mode of treatment, we think few will hereafter be inclined to deny.

The inventor, or rather the proposer, of this modification in the treatment by compression, was not the author of the thesis. The latter modestly states that he received the idea from a young surgeon attached to his school, but he has quite forgotten to allude to him by name. M. Broca, with a very right and strong enthusiastic feeling of justice, encountered many difficulties, in order that he might discover and publish the name of this young surgeon, the first proposer of alternate continued compression in the treatment of aneurism. After much inquiry, he succeeded in ascertaining from Strasbourg, that a M. Belmas was the person alluded to by M. Latouche in his thesis, and we hope M. Broca will thus have succeeded in handing down to posterity this name in connexion with the history of the treatment by compression.

It is not in our province to discuss further the stages by which, or the periods in which, different men have improved the method of compression, and raised it to the position it now holds in the science of surgery; there only remain for our consideration the important points of the treatment itself.

It must be acknowledged by all who have paid the least attention to the subject, that since the year 1842, when Mr. Hatton applied compression for the first time in the Richmond Hospital, to the present period, greater success has been achieved in cases of aneurism thus treated in Dublin, than in those treated by compression in the London hospitals. There had been no want of cases in London to have fairly tested the merits of this treatment; though perhaps we may be told, that the London constitution is less capable, than individuals admitted into the Dublin hospitals, of supporting the continued pressure of tourniquets.

It appears to us, however, that the difference in these results may be attributed to the different methods of applying the pressure; and M. Broca, quite taking this view, endeavours, and we think with success, to prove its correctness.

We believe with our author, that the indirect compression treatment for certain aneurisms, is the most perfect of all known treatment in the present day—

> "It enables the surgeon to moderate at his will the circulation in the aneurism; to weaken, or to put an end to the pulsations; or to allow them, if such should be the indication, their full action; to induce gradually and methodically the dilatation of the collateral branches; to preserve in the end the permeability of the artery at the compressed point, and consequently to diminish as much as possible the chances of gangrene supervening." (p. 744.)

The treatment by compression in false aneurism is not so efficacious as in the true form; but the disadvantages following upon ligature of an artery in false aneurism are probably greater than those resulting from indirect *pressure*. As we cannot enter sufficiently into this part of the subject to do justice to M. Broca, we beg to refer our readers to his own remarks, to be found at pp. 757 to 760.

Under what circumstances should the application of indirect pressure be adopted? In what manner is that pressure best applied?

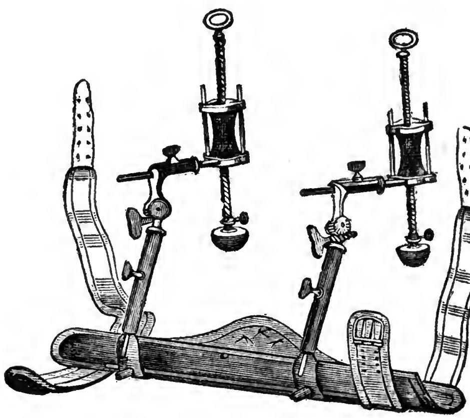
It is not necessary for us to answer the first question, nor has M. Broca pointed out anything that is new respecting it. Every surgeon will sufficiently recognise what would be the nature of the answer. But in replying to the second, we shall consider all M. Broca brings fresh to its illustration.

The best mode of ensuring compression, says M. Broca, is by the use of something that acts alone on the artery, similar to the finger of an assistant during the operation of amputation; and he adds, there may occur two instances in which the employment of the finger alone can be resorted to—viz., any deformity in which other mechanical pressure cannot be applied, or when excessive irritability of the skin forbids the use of every other means of compression. The use of the simple conical weight for the purpose of compression, M. Broca truly says, must only be considered auxiliary. It requires all the attention of the patient; he must, under its use, lie on his back, and he must lie still; it can only be applied at the groin, and it cannot be applied during sleep. But it is simply constructed, costs little, and may be useful in anticipation of, or in combination with, other measures. Various instruments for the purpose of exercising compression are spoken of, and their defects or good properties pointed out. The fundamental principle in all should be the formation and shape of the pad—so made as to press as much as possible on the artery, and to avoid the accompanying vein and nerves. It should vary in size according to the region, according to the depth or the calibre, or the degree of mobility of the artery. It should be convex rather than flat, but never conical. He prefers them made of vulcanized india-rubber, for it is firm in consistence and elastic, has neither the yielding of horse-hair or tow, nor the rigidity of wood or metal.

The method of effecting the counter pressure is almost as important in the practical management of a case, as the construction and application of the pad. M. Broca advocates a simple slightly convex bed of thin metal, well padded, ample in size, and fitted to the limb, so as to embrace one-third or one-fourth of its circumference; also sufficiently long to furnish a solid and firm base for the connecting arms which pass to the pad. Indeed, so strongly does M. Broca insist upon the importance of the latter condition, that his counter pad, or rather trough (*gouttière*), extends from near the buttock to within an inch of the knee, when employed for the thigh; and short in proportion when required for other parts.

We can thus obtain an invariable basis, and an apparatus which will not shift. It will restrain the movements of the limb, and in that lies its advantage. The absolute repose of the affected region is one important feature in favour of the obliteration of an aneurism. Above all, there is this advantage in it, that to such a basis there can be applied a series of arms and pads, through the means of which alternate pressure can be made at different points of an artery.

And now we have to decide, to what intermediate form of mechanism the pad should be fastened, so as to be applied in the most efficient, at the same time in the least painful, manner, to the surface over the artery. Ingenious experimenters have been fully employed in devising numerous instruments for this object—spring apparatus in great variety of shapes, rings fastening round limbs, and numerous modifications of steel bows, or iron arms broken and jointed at different angles to procure motion—all have had their trials and all their advocates. M. Broca deals with them in a sensible practicable manner—points out the advantages of some few, but condemns most. But, like a good workman, not content with exposing defects, he gives a model for an apparatus which, in our opinion, is a greater approach to perfection for ensuring equable, gentle, continued and alternate indirect pressure than any we have yet seen. The apparatus made by M. Charrière, of Paris, under the direction of M. Broca, is represented below.



With the use of such an instrument, pressure can be alternately applied at different points. The screw, passing through an elastic collar of india-rubber, renders the pad less liable to produce irritation of the skin, and less liable to occasion that pain which has so often made abortive the treatment by compression. And it enables us to keep up a gentle restraint on the circulation through the aneurism more easily than by any other form of instrument now in use.

The directions given by M. Broca respecting its application deserve attentive perusal, and will be serviceable to all who observe them in practice. He arrives at the following conclusion on the subject:—that when a rational and enlightened method of practice is adopted in the application of compression, the cases in which it will be inefficacious will become more and more rare.

But it appears to him certain that cases will always be found, which, notwithstanding compression—and even compression well directed—will baffle the efforts of the surgeon, and in which the ligature is all that remains for our use.

And what is the result of ligature after compression has been tried and failed? M. Broca thinks it as favourable as can be expected. In our experience, not any additional benefit has been noticed to accrue to patients, who had submitted to ligature after compression had been attempted.

M. Broca recommends that compression should be tried in all cases for two or more weeks, even supposing the ligature may be considered ultimately necessary. In the results of treatment by compression, we strongly advise our readers to turn to M. Broca's remarks. We can but relate them briefly. From 1842, to

May, 1854, compression had been tried in 163 cases. In 12 of these it could not be long maintained, in consequence of the pain becoming intolerable. There remain 151 cases in which compression was continued with sufficient perseverance. From these, 24 must be taken, as the compression failed from not having been properly applied. This leaves 127 cases to dispose of. Out of this number, 116 were successfully treated. The treatment was inefficacious in the remaining 11 cases; and every circumstance concurred to prove that, in 6 of these, compression failed in its object from a peculiar idiosyncrasy showing itself in the results of the subsequent application of the ligature.

The average of deaths in the 127 cases was not higher than five per cent. Six deaths were attributed to the treatment, four being after compression alone, and the other two subsequent to the secondary treatment by ligature. As to the relative success of treatment by compression or ligature, M. Broca thinks that about five per cent. die under the former, and nearly twenty-five per cent. under the latter. We cannot with accuracy travel into the field of statistics without a greater number of faithfully recorded cases than we possess or have the means of procuring; but there can be no reason to doubt the greater success from the treatment by compression, than from the application of the ligature. It must, however, be borne in mind, that to the ligature are turned over all cases in which compression is inapplicable or in which it has failed; and as it remains the only and ultimate resource, so it must prove the most fatal of the two.

Few surgeons will refuse to acquiesce in the obvious maxim practically obtained from this experience,—that in almost every case compression is at first not only justifiable but imperative, on the part of the surgeon, provided compression can be secured by appropriate appliances, and its effects watched with daily care.

There is one wholesome piece of writing which we cannot refrain from quoting before concluding our remarks. After having spoken of the advantages of compression over those of the ligature, M. Broca says:

"There is one circumstance which will appear in favour of the ligature, if science could countenance the selfishness and indifference of the surgeon; the application of the ligature is an operation brilliant, rapid, and apt to excite admiration in the looker on. When the vessel is properly tied, the surgeon has nearly completed his task; his mind is easy; he is not answerable for what may occur at a later period; whether his patient dies or recovers, his responsibility is protected. How much more delicate is the position of the surgeon who has recourse to indirect compression. Here, there is not much glory to be acquired; instead of an operation somewhat theatrical and made once for all, the measures are simple, slow, assiduous, and discouraging from the minuteness of their details. A thousand incidents may occur, and the obstacles which result from them demand incessant attention, a thorough fitness, and a perfect knowledge of all the features of the subject." (p. 876.)

And here we must conclude. In taking leave of M. Broca, we have to thank him most sincerely for the pleasure and for the information we have derived from the perusal of his volume. The industry, the talent, and the care exercised in its preparation we cannot speak of too highly; and we feel satisfied that it will long prove one of the most useful and necessary works of reference that can be studied by students and teachers in the science of surgery.

ART. X.

On a True Parthenogenesis in Moths and Bees: a Contribution to the History of Reproduction in Animals. By CARL THEODOR ERNST VON SIEBOLD, Professor of Zoology and Comparative Anatomy in the University of Munich, &c. &c. Translated by WILLIAM S. DALLAS, F.L.S., &c.—*London*, 1857. 8vo, pp. 110.

MOST of our readers are probably aware that there is a remarkable alternation of processes in the generation of many of the lower animals, characterized by the interposition, between two cycles of ordinary parental generation, of one or more cycles of non-parental multiplication. In the latter, reproduction is effected (not only independently of a male and a female animal, but even independently of the ovaries or testes, which sometimes represent the sexes in one individual) by the development of embryos within the body of an imperfect creature, devoid of true generative organs, and bearing to its embryonic contents the relation of a nursing individual, a germ-stock, or a pupa-sac.

It is to a reproduction of this kind that Owen some years ago proposed to apply the term *Parthenogenesis.* Limiting our attention to the fact, that it is a *lucina sine concubitu*, we may perhaps regard this term as applicable, though savouring rather of that peculiar variety of Greek adopted by advertising shirt-makers. But its inaccuracy in other respects is obvious. On the logical principle of *exceptio probat regulam*, by expressing the absence, it implies the possibility of sexual intercourse or impurity. Hence an animal which is neither a male nor a female, but a sexless neuter, cannot be termed a virgin in the true sense of this word. And any process of generation in which—whether by a fission of the original germ-mass or otherwise—the instrumentality of the sexes is dispensed with, is but very partially and inaccurately defined by the word *parthenogenesis*—a word which, foreign and uncouth as it is, ought at least to have the merit of really connoting its meaning.

The phenomena established by Von Siebold are, however, so much more strictly entitled to this name, that we may fairly expect it will henceforth be confined to them. Indeed, Owen appears almost to anticipate and acquiesce in such a transfer of its use; even while questioning the alleged inaccuracy of his own previous application of it. The constructor of that beautiful and philosophic system of knowledge summed up by the theory of the Archetype skeleton, can well spare such a trifling appropriation as that of a single inaccurate name.

The chief instances of true *parthenogenesis* detailed in the book before us are found in—1st, certain species of sac-bearing Lepidoptera; 2nd, in the ordinary silk-worm moth; and 3rd, in the honey-bee.

In all three cases, the proof of the process of course involves the disproof of ordinary sexual generation. And Von Siebold, while exposing the fallacies of some older statements of this kind, adds some curious information respecting the extraordinary acuteness of instinct by which the two sexes of some insects can discover each other under what would seem very unlikely circumstances. How, for instance, a number of (hitherto unnoticed) males of some insect become aware of the presence of a female, within a chamber from which they are shut out by doors and windows, is a mystery that almost drives one to certain Pythagorean theories of the grey old time to explain.

The process of parthenogenesis seems, in some of the Lepidoptera, to produce female individuals only. In the silk-worm moth, in which it appears to be more exceptional, it produces both sexes, in what proportions to each other, remains at present unknown. In this insect, however, a large number of the eggs laid by the virgin female shrivel up, and remain barren.

It is from the honey-bee that our author brings forward the most striking example of the process, and the most accurate history of its details. Aided by the observations of Dzierzon, as well as by the facilities which this celebrated apiarian placed at his disposal, he has arrived at a series of conclusions which open a new chapter on the physiology of generation to the scientific world.

The ludicrous contradictions and fables of some of the older bee-keepers are well known. The microscope, and the anatomical exactness which it permits, have, however, settled the leading points in the composition and polity of these wonderful communities. And the advocates of the "rights of women" must surely be delighted to find that what one of the oldest treatises* on the subject calls "the

* **Butler, 1634.**

feminine monarchie," not only really deserves this name,* but exemplifies a form of divorce, the simplicity and finality of which even the most strong-minded woman would scarcely wish to see exceeded.

As regards the impregnation of the queen-bee, this would appear to take place, once for all, during her single flight from the hive in youth—the wedding flight, as it is termed. The noise made by the wings of the young queen attracts the attention of some one of the vagrant bachelors who hover in mid-air above the hive, and give out a sound which reciprocally informs the queen of their presence. The copulation takes place during this flight, and the queen-bee returns to the hive not only with her spermatheca filled with the semen destined to impregnate all the progeny of her five or six years of life, but with more or less of the male organs of her consort broken off in her body.

The most singular fact, however, remains to be told. All those eggs which are destined to become workers (imperfect females), or queens (perfect females), undergo an impregnation in what seems to be the ordinary way, by means of spermatozoids, which, as each egg passes the spermatheca in its course towards the external outlet or vagina, enter the ovum at a micropyle, and remain for some time visible in its interior; while, on the contrary, all those eggs which are destined to become drones, are extruded without any such contact with seminal fluid, or penetration by the spermatozoids.

The presence and absence of spermatozoids are determined by the author from serial observations with the microscope. The destiny or character of the impregnated and non-impregnated eggs can be affirmed from their size and other appearances at the time, as well as from their subsequent development.

A variety of collateral facts are brought forward by Von Siebold, all of which illustrate and confirm the above more direct evidence of this extraordinary conclusion. The queen-bee that is crippled in her wings from birth, is thus rendered unable to take her wedding flight, and hence ruins her subjects by producing nothing but drones. In like manner, during her after-life, she may be pinched or frozen into a similarly one-sided (or rather unisexual) barrenness; these local injuries of the generative organs appearing sometimes to destroy the local conditions of impregnation, and hence thereafter to limit the act of generation to a reproduction of males. As the age of the queen advances, again her spermatheca becomes exhausted of its contents, and the number of eggs which receive spermatozoids undergoes a corresponding decrease, to end in an equally exclusive reproduction of drones. Lastly, the queen seems to have the power of producing either males or females at will; and this power, exercised as it is by the instinct of the whole community, appears to be specially evoked by the size of the cells into which the queen thrusts her body in the act of oviposition, so that the different size and shape of these cavities seems to be the stimulus or exciting cause of the impregnation or non-impregnation of the ovum. In this way the act of seminal impregnation is probably a strictly reflex one, called up by the mechanical excitement of the impaction of the queen's abdomen into a narrow cell.

The rarer instances in which a worker-bee produces drone-eggs are explained as probably due to worker-eggs having by some mistake received the royal food which is known to determine the perfect development of the female bee. And as the worker never copulates, the invariable production of drone-eggs, under these exceptionable circumstances, is referrible to the same law as the casual or permanent production of such eggs by the more perfect female, or queen-bee.

This brief outline of Von Siebold's chief results is all that our space permits us.

* So that the noble lines of Milton †—

> "Swarming, next appeared
> The female bee, that feeds her husband drone
> Deliciously, and builds her waxen cells
> With honey stored"—

remain unscathed by the fierce criticism of Dr. Bentley, who dogmatically corrects the supposed ignorance of Milton by asserting that "The drone is not the bee's husband; and that the bees are all females seems an idle and idiotica notion, against the course and rule of nature."

† Paradise Lost, Book vii. verse 489 *et seq.*

It is quite enough, however, to suggest various reflections which we shall leave our readers to make for themselves. With facts in such startling contrast to almost all that is hitherto known respecting the generative process, there is little need to recommend caution in implicitly accepting them. The less, indeed, that the balance of probability generally inclines in the opposite direction; rendering us more disposed to forget our own ignorance, and to challenge all new accessions of knowledge a little too strictly, when they unsettle our previous ideas, however vague and meagre these may be. But, even assuming the complete accuracy of these observations, it is impossible at present to foreshadow their relation to the generative process in the higher vertebrata. They appear to indicate a law of which, in these animals, there is no representative; a type of reproduction which can scarcely be linked with the really bisexual character of the mammalian fœtus, and which not even the teratology of the human or brute embryo seems likely in any way to explain.

The translator's work has been well executed by Mr. Dallas, and reproduces the original, evidently with faithfulness, and with all the smoothness which is really necessary to make it intelligible to the English reader. The judgment which led him to select this little monograph for translation is sufficiently shown by the value of the facts it discloses, facts which we shall be glad if this very scanty notice induces any of our readers to investigate for themselves.

PART SECOND.

Bibliographical Record.

Art. I.—*The Ophthalmia of Ireland: its Nature, Effects, and Treatment.* By John Williams, A.B., Trinity College, Dublin; Licentiate of the King and Queen's College of Physicians, and of the Royal College of Surgeons, Ireland; Surgeon to the Cork Eye Dispensary.—*Dublin*, 1857. 8vo, pp. 44.

From the title of this work we were under the impression that it was devoted to the consideration of the formidable disorder which, whilst never entirely absent from the pauper establishments of Ireland, has from time to time ravaged them with terrible results. The pestilential period of 1847 and 1848 was followed by an outbreak of epidemic ophthalmia, and between 1849 and 1852 no less than 118,835 cases occurred in the union workhouses of Ireland alone! The disease prevailed most in the unions of Tipperary, Cork, Limerick, and Clare, and principally attacked children under fifteen, the number of such sufferers being 84,136.

The author, however, touches but lightly upon this affection, devoting his pages first to an anatomical description of the palpebræ, conjunctiva, and cornea; and then considering, in order, simple inflammation of the conjunctiva, pure catarrhal ophthalmia, catarrho-rheumatic ophthalmia, scrofulo-catarrhal ophthalmia, purulent ophthalmia, granular lids, and affections of the cornea.

As the work is addressed to the members of the profession, we think it a pity that the author has indulged in such puerilities of description as stating that

"The palpebræ, or eyelids, are those moveable curtains situated in front of the orbit, and moulded with accuracy to the anterior surface of the eye, over which they slide with facility, and which is alternately concealed or exposed as they are in opposition or separated." (p. 1.)

And that

"The cornea is that highly-polished, strong, and transparent window in the front of the eyeball." (p. 5.)

Such passages are perfectly unnecessary, and only encumber a work like the one before us. To those who have neither time nor opportunity for studying elaborate treatises, the short account here given of the various affections would be useful; but there really is nothing in the description of the diseases, or of the treatment, which calls for particular comment. The author states that ophthalmia in Ireland is characterized by that peculiar condition of the conjunctival mucous membrane, termed "granular lids," more frequently and in a severer form than in any other country in Europe; and this condition of the conjunctiva is also the most frequent cause of blindness in the poorer classes. To this point therefore we turn, and find a description, good, without being too minute, of the morbid conditions constituting granular lids. He says truly that

"The hypertrophied papillæ are occasionally disposed in a row immediately behind the posterior edge of the tarsal border; or they may occur large, distinct, and pendulous from the superior palpebral sinus, which is a favourite retreat for them, and where they frequently remain undiscovered—I fear in some instances unsought for. The entire palpebral investment may present a uniformly florid and granular appearance—like the surface of a granulating ulcer—and in this case the granulations, which do not present a very irregular surface, are disposed in groups or packets." (p. 24.)

The rules laid down for the management of this troublesome disease are sound. The author justly remarks that, "although in this disease local applications cannot be dispensed with, I consider the employment of them very secondary to well-directed constitutional treatment;" and we concur with him in the disadvantages arising from the abuse of escharotics and stimulating applications. The treatment recommended is prolonged counter-irritation, snipping off the larger granulations, and the application of astringents to the smaller; internally, bark and the oxymuriate of mercury. Mr. Williams does not mention scarification of the lid, but we have seen great benefit from it, combined with the application of the undiluted liquor plumbi acetatis to the granular surface, and we are much in the habit of employing cold water freely to the eyes. The preparations of steel, either by themselves or with quinine, are valuable auxiliaries when prescribed with judgment.

It seems to be generally admitted that the main exciting causes of the various ophthalmiæ among the lower orders of Irish are their inattention to cleanliness, debility of constitution, and the derangement of general health induced by excessive privation. Mr. Williams mentions that want of covering for the feet is the commonest cause of all; it would be interesting to ascertain whether ophthalmia is very common in Scotland, where bare feet are the rule and not the exception among the lower orders.

Admitting that these opinions are correct, and that the excessive prevalence of diseases of the eyes and of their appendages are really due to such influences, it is not unreasonable to hope that the great social change now working in Ireland will sweep before it this, among the many evils arising from the curse of idleness and poverty; and that ophthalmia and its consequences will disappear under the benign influence of industry and prosperity.

ART. II.—*Elementary Treatise on the Wave-Theory of Light.* By HUMPHRY LLOYD, D.D., D.C.L., F.R.S. L. & E., &c. pp. 208.

WITH the exception of voltaic electricity and electro-magnetism, no branch of physical science has recently made such rapid strides as that of light; indeed, for nearly a century—viz., from the time of Newton to the time of Young, it was almost stationary; wonderfully advanced by the former, it received but few accessions till the investigation of it was renewed by the latter, to whom, indeed, mainly is owing the revolution of doctrine from that of the emission or projectile theory, advanced and elaborated by the genius of the first-named philosopher, to the undulating or wave-theory suggested by Hooke and more fully expounded by Huyghens. It is not a little remarkable that the projectile theory which had been adopted so long, and considered so well established, should now have hardly a supporter! How strongly does this change show the instability of hypothesis! And ought it not to be a lesson to us to adopt that excellent motto of a Society of which Newton was the greatest ornament, *Nullius in verba magistri*, and to follow the example of those able and zealous inquirers, the contemporaries and successors of Young, by whose brilliant discoveries the "wave-theory" of light has been confirmed?

The name of the author of the treatise before us—himself one of those inquirers to whom we have just alluded—is sufficient warrant for its sterling worth; and its being a second edition is a proof of the successful manner with which he has dealt with his subject. Expressly designed, in the first instance, for the use of the student—being, as he states, an extension of the lectures delivered by him in his professorial capacity in Trinity College, Dublin—it can hardly fail of being acceptable, and of proving highly useful to those anxious to indoctrinate themselves in the science. It is recommended by its logical precision throughout, and by happily combining the historical with the experimental and theoretical.

In concluding his preface, Dr. Lloyd states,

> "His only aim has been to present to those who were conversant with the elements of mathematics a clear and connected view of his attractive subject; and he has been compelled by this limitation to confine himself in many cases (as in all that relates to the dynamics of light) to a general account of methods and of their results. Those who desire a more exact acquaintance with the science will, of course, study it in Sir John Herschel's 'Essay on Light,' and in Mr. Airy's tract 'On the Undulating Theory of Optics;'"

And for a general knowledge, we would beg to add, in Professor Forbes's admirable dissertation 'On the Physical Sciences,' recently published in, and forming a part of, the 'Encyclopædia Britannica.'

It is foreign to our purpose to attempt an analysis of Dr. Lloyd's work, and we shall make only one extract, in which are clearly defined the characteristics of the opposite theories.

> "We have seen that light travels from one point of space to another in *time*, and with a prodigious velocity. Now, there are two distinct and intelligible ways of conceiving such a propagated movement. Either it is the *same individual body* which is found in different times in distant parts of space; or there are a *multitude* of moving bodies occupying the entire interval, each of which *vibrates* continually within certain limits, while the vibratory motion itself is communicated in succession from one to another, and so advances uniformly. These two modes of propagated movements may be distinguished by the names of the motion of *translation* and the motion of *vibration*. The former is more familiar to our thoughts, and is that which we observe when with the eye we follow the path of a projectile in the air; or about which we reason, when we determine the course of a planet in its orbit. Motions of the latter kind, too, are everywhere taking place around us. When the surface of stagnant water is agitated by an external cause, the particles of the fluid next the origin of the disturbance are set vibrating up and down, and this vibratory motion is communicated to the adjacent particles, and from them onwards to the boundaries of the fluid surface. All the particles which are elevated at the same instant constitute what is called a *wave;* and that this wave does not consist of the *same* particles in two successive instants, may be seen in the movements of any floating body, which will be observed to rise and fall as it is reached and passed by the wave, but not to advance, as it must necessarily do if the particles of the fluid on which it rested had a progressive motion. The phenomena of sound afford another well-known instance of the motion of vibration. The vibratory motion is communicated from the sounding body to the ear through all the intervening particles of the air, though each of the aërial particles moves back and forwards through a very narrow space.

"Each of these modes of propagated motions has been applied to explain the phenomena of light, and hence the two rival theories—the *theory of emission*, and the *wave-theory*. In the former, the luminous body is supposed to send forth, or *emit*, continually, material particles, of extreme minuteness, in all directions. In the latter, the same body is supposed to *excite the vibrations* of an *elastic ether*, which are communicated from particle to particle to its remotest bounds. This etherial medium is supposed to pervade all space, and to be of such extreme tenuity as to afford no appreciable resistance to the motions of the planets."

Art. III.—*On the Pathology, Symptoms, and Treatment of Ulcer of the Stomach.* By William Brinton, M.D., Fellow of the Royal College of Physicians, Physician to the Royal Free Hospital, Lecturer on Physiology and Forensic Medicine in St. Thomas's Hospital.—*London*, 1857. pp. 227.

Were it not that a good portion of this work has already appeared before our readers in the pages of this Journal, it would challenge a more particular notice than that which we are now able to afford it. The modest statement of the preface is more than justified:

"That it may be described as collecting and incorporating facts hitherto scattered and little accessible, and as recording the existing state of our knowledge respecting this disease, in a form calculated to facilitate those additions and corrections which time is sure to bring."

One of its chief merits appears to us to be, that all the particulars relating to gastric ulcer are examined into on the basis of a larger number of facts than has yet been done; the author having with great industry brought together and utilized the observations of those who have preceded him in this inquiry. Perhaps we may confess to a certain amount of distrust of some of the less obvious statistical calculations, the results of which, indeed, the author himself does not rank above "conjectures." These are, however, the fashion in physic at the present day, and can scarce be objected to. One point we must notice as well brought out—viz., the greater liability to perforation in the young female, contrasted with the greater frequency of occurrence of ulceration in advancing life.

The symptoms and concurrent conditions are exceedingly carefully and well investigated, and the reader will find a complete *résumé* of all our knowledge on these heads. We feel surprised that Dr. Brinton should lay stress on the effect of pressure in increasing pain (epigastric or dorsal) as a diagnostic sign ("a very important test") of gastric ulcer. To us it has appeared simply to indicate that the mucous surface was in a state of inflammatory irritation, tender and irritable, just as an inflamed conjunctiva. Of course this state may, no doubt often does, concur with ulcer, but it is not a sign of this lesion.

The etiology of the gastric ulcer is well discussed. The author objects to an explanation of its peculiarities founded upon the local circumstances, so far at least as accounting for the origin of the ulcer. In this we concur with him, as also with regard "to the remarkable influence of poverty and intemperance." Dr. Brinton considers that there is nothing specific in the ulcer of the stomach, any more than in ulcers of the leg, and he believes that either lesion may originate in very various ways. He cites Mr. Critchett's experience as to the existence of similar characters in the ordinary cutaneous ulcer at the epoch succeeding puberty, to those observed in the gastric at the same peried—viz., a more or less complete absence of inflammatory reaction at the base and margins. We would remind Dr. Brinton of the analogous instance afforded in ulceration of the cornea, to which Dr. Handfield Jones has drawn attention. In this we have a marked example of ulceration occurring in a tissue in a primary manner, uncomplicated often with any other morbid action. We observe it always in states of debility, and we watch its cure under an analeptic and tonic treatment. To comprehend aught of its origin seems altogether at present impossible, from our entire ignorance of the secrets of vital chemistry. If we knew anything of the power which originally constructed the tissue, and which continues to maintain it, we might look to understand the results of that power's failure.

We have next to notice the subject of diagnosis. Dr. Brinton, after posing the question, "What is the minimum of evidence that will justify our affirming the existence of an ulcer of the stomach during life?" replies, that he

"Is inclined to think nothing less than all the chief symptoms enumerated entitle us to pronounce a decided opinion. In other words, unless the pain possess the characters attributed to it, unless this pain be accompanied by vomiting, and unless there be evidence of hæmorrhage having occurred in the course of the malady, there is no sufficient basis for a definite diagnosis of the existence of a gastric ulcer."

He adds, however, that he has

"Not the slightest doubt that an absolute enforcement of this rule of diagnosis would lead us to overlook a vast number of cases, and might thus be the occasion of grievous errors in practice."

While we agree with Dr. Brinton, that in many cases we may and must suspect gastric ulcer, and should keep this probability steadily in view in our treatment, we must express our opinion decidedly that the occurrence of hæmorrhage, though succeeding such symptoms as pain and vomiting, is no sure evidence that there is any "breach of continuity" in the gastric mucous membrane, not at least such as we describe under the term ulcer. Our rule, until lately, used to be that if in a case of sudden profuse gastric hæmorrhage one could exclude cirrhosis of the liver, vicarious menstruation, purpura, and heart disease, one might feel pretty confident of its being the result of ulceration. But latterly we have met with so many instances in which the hæmorrhage concurred with symptoms of lowered nerve-power and aguish disorder, apart from any special gastric affection, that we have been much less ready to diagnose ulcer of the stomach than formerly. We would suggest to Dr. Brinton, that many of the cases he meets with among his out-patients may be of this kind, the gastric mucous surface being flushed with blood by reason of paralysis of the arterial nerves, and hæmorrhage then occurring by capillary rupture much as in a case of epistaxis. The prevalence of aguish disorders has been so considerable for some time, that we cannot think this cause should be overlooked. In fact, the author notices "ague" as a cause of gastric ulcer—in our view it is only of gastric hæmorrhage. The existence of hepatic cirrhosis, or other portal obstruction, ought also in all cases to be canvassed as a possibility. The former may occur, we know, in perfectly temperate persons, and in the young, and may declare itself by hæmorrhage without any dropsy.

The chapter On Treatment contains nothing particularly novel, except the recommendation (which we think very judicious) to give opium as a means of promoting the cure of the ulceration. The author looks to it not only to relieve pain and check irritation, but also "to support the strength, to buoy up the nervous system, and to check the waste or expenditure of the tissues generally." The subject of diet is most properly insisted on, the principle being to administer bland nutriment in very small quantities frequently repeated, so as to avoid distension of the organ. Some allusion should have been made to Dr. Budd's previously published observations upon this head. So also while a course of tonics is advised in the latter part of the treatment, it would have been well to have referred to Dr. Handfield Jones' remarks on that subject.* These, however, are mere accidental omissions, and do not detract from the value of the work.

The latter part of the volume is occupied with a series of reports, originally published in the "Association Journal." Of these we need only say that they are very complete, and will well repay perusal.

* Pathological and Clinical Observations respecting Morbid Conditions of the Stomach (pp. 212); and Lancet, March, 1856.

ART. IV.—*Report on the Formation and General Management of Renkioi Hospital, on the Dardanelles, Turkey.* Addressed to the Right Honourable the Secretary of State for War. By E. A. PARKES, M.D., late Superintendent of the Hospital.

IT is grievous to think that one of the greatest achievements that have resulted from the combination of engineering skill and medical science in behalf of our sick and wounded soldiers in the Russian war, should only have served to prove what we can do, and then have been doomed to destruction. The Renkioi Hospital, in which appear to have been united all the requirements of sanatory and sanitary science, was a thing of a day, and the necessities which called it into existence having ceased, it was—we would almost say ruthlessly—scattered to the winds. On admirable soil, on a tongue of land allowing of shelter to ships, in whatever weather they might arrive, shelving downwards to the sea in such a manner that drainage was much facilitated, while terracing and excavation for the wooden houses composing the hospital were unnecessary; supplied with pure water adequate for the purposes of economy; open to the invigorating breezes of the sea—all the conditions were united which could be desired to render the ancient Ophrynium a monument of modern civilization and philanthropy. It was only after much labour that this advantageous site was discovered. Its selection shows the spirit and the judgment which animated Dr. Parkes and his coadjutor, the engineer, Mr. Brunton.

The hospital, which had been constructed by Mr. Brunel, was sent out in parts from England, ready to be put together on their arrival at Renkioi. It occupied the tongue of land in such a way as to stretch down from the higher to the lower level—a space in all about half a mile in length. The main hospital consisted of thirty-four houses, capable of holding 1500 sick, arranged along the two sides of a long central corridor. Two accessory hospitals, on a similar plan, intended to hold 750 patients each, were nearly completed when the conclusion of peace put a stop to the works.

"On the sides of the hills in rear were numerous small springs of excellent water, which were collected together and conveyed in earthenware pipes to a large reservoir placed by Mr. Brunton 70 feet above the highest house, which was itself about 60 feet above the sea. From this reservoir the water was carried in iron pipes down the centre of the long corridor, and at every ward (which was placed at intervals at either side of the corridor) a leaden service-pipe came off, and led an abundant and never-ceasing supply into the ward cisterns, which supplied the baths, lavatories, and closets. By this arrangement, all necessity for pumping water was avoided, and the sewers were able to be flushed very perfectly. The lavatories and closets were placed at the ends of the wards most remote from the corridor, and immediately outside them ran the two main sewers, which at their sea terminations were carried some distance into the Dardanelles. The plan of the hospital may be at once understood by imagining a covered way, open at the sides, and 22 feet wide, running nearly east and west, and reaching for a length of more than a third of a mile, on either side of which stood, at intervals of 27 feet on the south side, and in most cases 94 feet on the north, the 34 houses, each of which, as already said, was 100 feet long, 40 feet wide, 12 feet high at the eaves, and 25 in the centre, and was capable of containing 50 patients, with an allowance of nearly 1300 cubic feet for each man. Some portion of this space was occupied by the closets, and some small rooms used as orderlies' and bath rooms. Thirty of these houses were used as wards; four were used as dispensaries and purveyors' stores. To the south of each division of ten houses was placed an iron kitchen, which afforded the necessary accommodation for preparing 500 diets. At the inland extremity of the corridor were placed two iron laundries, the water from which (some 4000 gallons daily) was passed into the sewers. Beyond the laundries were placed on either side the wooden houses of the medical and other officers, who were thus able to see down either side of this long line, and to preserve, to a certain extent, surveillance over the patients." (pp. 14, 15.)

The great recommendation of the system adopted was its simplicity, as it was a repetition of similar parts throughout; while the separation of the different buildings facilitated ventilation to an extent which rendered an accumulation of septic poisons and a communication of disease from ward to ward impossible. The facility with which extensive accommodation can, under such circumstances, be rapidly provided for an army in the field, is shown by the fact that the erection of the houses having been commenced on the 21st of May, 1855, Dr. Parkes reported the hospital ready for 300 sick on the 12th of July; on the 11th of August it was ready for 500; on the 4th of December for 1000 sick; and by January, 1856, or seven months from its commencement, the hospital could receive 1500 patients.

The principle of the construction of this hospital was one that we held up to admiration, some time since, in speaking of the Hospital of Bordeaux. It is one that we hope to see adopted before long in an English hospital, though necessarily modified in such a way as to meet the necessities of the climate. The subject of hospital architecture is one which merits our further attention. In the mean time, we briefly draw attention to Dr. Parkes' Report chiefly on account of the instruction afforded by the plans and descriptions of his magnificent hospital—magnificent in point of perfect adaptation to the proposed objects—and urge, also, upon all persons interested in these questions, the lesson to be learned from the success attending the harmonious co-operation of the arts and sciences.

Art. V.—*The Baths of Germany, France, and Switzerland.* By Edwin Lee, M.D., Corresponding and Honorary Member of the Medical Academies and Societies of Paris, Berlin, Madrid, Turin, Florence, &c. Third Edition. Two Volumes in One.—*London*, 1857. pp. 208.

The third edition of Dr. Lee's balneological work, though not including every watering-place in the three countries mentioned in the title, comprises faithful accounts of all the more important places of the kind resorted to by invalids of this country. In most instances the author speaks from personal knowledge, and always quotes from the best informed sources.

The work is one that may be safely consulted by medical men, as it contains reliable information on the therapeutic action of the various waters, as well as upon the localities in which they are found. The work recommends itself by the style in which it is written, as well as by the very suitable selection of the material placed before the reader.

Art. VI.—*Studier i Brackläran.* Af Dr. Carl Benedict Mesterton, Chir. Docens vid Universitet i Upsala. 1. *Om Radicaloperation för Ljumskbrack.* —*Stockholm*, J. och A. Rüs, 1857.

Studies of Hernia. By Dr. Charles Benedict Mesterton, Lecturer on Surgery in the University of Upsala. 1. *On Radical Operation for Inguinal Hernia.* With a Plate. 4to, pp. 100.

In our fifteenth volume we briefly alluded to the operation for the radical cure of reducible inguinal hernia recommended by Professor Wutzer of Bonn, and fully described by Mr. Spencer Wells in the thirty-seventh volume of the "Medico-Chirurgical Transactions." The operation in question, which consists in simple invagination of a portion of the scrotum into the inguinal canal, appears to have been successfully performed in a very considerable number of cases, and to have been attended, moreover, with the very important advantage of comparative freedom from danger. Thus, Mr. Holmes Coote states that "it is doubtful if any fatal accident can be reported as directly a consequence of this operation, performed now between one and two hundred times."*

The essay before us is a very complete and masterly history of the subject of operation for the radical cure of hernia, from the earliest period to the present time. During the first quarter of the nineteenth century, the question of the radical treatment of reducible inguinal or femoral hernia was readily answered.

* Midland Quarterly Journal of the Medical Sciences, vol. i. part 1, p. 19. May, 1857.

The experience of nearly two thousand years had passed a decisive sentence upon it, and assigned to such operative interference the most subordinate place. The ancient methods had long since been condemned and forgotten, and the more recent attempts deduced from them, or at least based upon the same principles, could not obtain the confidence of modern surgeons, who, with enlarged views of the nature of hernia, had learned from the experience of their ancestors to estimate the little efficacy and great danger of such operation.

The introduction of improvements has since, however, brought about a new era in the history of radical operation, and a minority of surgeons, in full conviction of the possibility and already established efficacy of operative radical treatment, now believe that they can offer to sufferers from hernia a certain and by no means dangerous mode of getting rid of their distressing ailment. The plan, which has been tried on the largest scale, and has afforded the most favourable results, is Wutzer's modification of the method by invagination, to which we have already alluded.

"An examination of the method by invagination will show that it is founded on three principles—viz.,

"1. Filling the entire hernial sac with an organic plug;

"2. Closing the neck of the hernial sac throughout its whole length; and

"3. Effecting these changes without injury of the hernial sac, and consequently without risk of peritonitis." (p. 66.)

The author reviews several of the various circumstances under which the operation may be undertaken, and concludes that it is perfectly admissible "in most cases of hernia, and that it ought to offer great advantages, especially for patients of the poorer *labouring* classes."(p. 94.)

As general contra-indications he would consider:

"1. Tender age, partly because at such a time the bloodless treatment by compression is most frequently fully sufficient, partly because the necessary rest and other precautionary measures cannot be enforced with very young children.

"2. A very advanced age, because in old persons the wound and long confinement are in themselves always attended with a degree of danger which is not counterbalanced by the diminished necessity for, and scanty prospect of, a radical cure which exist in the case of such patients.

"3. Enormous distension and relaxation of the abdomen, with numerous hernial protrusions of the walls, which are sometimes almost as thin as parchment. This rare and entirely incurable form of hernia, which may also occur in younger individuals, is especially met with in very old persons who, after having previously been very corpulent, have in advanced years become extremely emaciated: and most particularly in women who, in addition, have in earlier life had many children; this contra-indication hence most frequently coincides with that immediately preceding.

"4. Actual eventration, where the abdominal cavity becomes so contracted around its diminished contents, that the prolapsed viscera have lost their *jus domicilii*, and no longer find space in the abdomen.

"5 (and lastly). Established specific dyscrasiæ, or profound general cachexy, constitute a decided contra-indication to this as to all other operations." (p. 94.)

Dr. Mesterton appends an extensive bibliography of his subject, and his volume is illustrated with a plate representing the instrument employed by Gerdy, Zeis, Wutzer, Rothmund, Langenbeck, and Valette. The present essay appears to be intended as the first of a series on the subject of hernia, and is published in a style worthy of what will form, if the succeeding parts shall be equal to that which has already appeared, a classical work on Swedish Medical Literature.

ART. VII.—*How to Work with the Microscope.* A Course of Lectures on Microscopical Manipulation and the Practical Application of the Microscope to different Branches of Investigation. Delivered during the Winter Session, 1856–7. By LIONEL S. BEALE, M.B., F.R.S., Licentiate of the Royal College of Physicians, Physician to King's College Hospital, &c.—*London*, 1857. pp. 124.

THE present generation of microscopic observers enjoys numerous advantages over its predecessors, not only in possessing more perfect tools to work with and more definite aims to accomplish, but also in being able easily to obtain that instruction in manipulation which has caused so much loss of time to those who had to acquire the knowledge for themselves empirically. The lectures of Dr. Beale are excellently adapted for the purpose to which they are devoted, of conveying that instruction; and as we have ourselves perused them with pleasure, we feel satisfied that they will prove useful and acceptable to all novices in microscopy. The directions are plain and intelligible, and the student who follows the successive steps here offered to him can scarcely fail to understand the uses and mechanism of the microscope. He will occasionally feel tantalized by a reference to specimens and drawings which the author has exhibited to his class in confirmation of his statements; but possibly a future edition may be rendered more attractive by the addition of illustrations; and we trust that in that case the author may feel assured of a sufficiently large sale to justify the pecuniary outlay necessary to secure their being carefully and elegantly executed.

Art. VIII. *Summary of New Publications.*

The most bulky volume that we have received during the past quarter is the first of a work by Henry Thomas Buckle, entitled 'History of Civilization in England.' It extends to 854 large octavo pages, which are entirely devoted (we do not exaggerate) to the general introduction. The task which the author proposes to himself is to determine the "connexion between human actions and physical laws," and by that means to fill up "a wide and dreary chasm" now existing between ordinary historians of the social events and the cultivators of physical science. We turn from Mr. Buckle to undertakings of humbler pretensions. 'The Waverley Journal,' conducted by Women, has recently entered into a new phase of its existence, since which it has come under our notice, and we can say of it that it gives promise of co-operating towards the "emancipation" of the female sex, in the best sense of the word. It is devoted to the consideration of all matters bearing upon the moral, intellectual, and social advancement of women. That the managers of the Journal have secured an efficient Editress is amply apparent from the vigorous, truthful, and well-written articles which have come under our notice. None can appreciate woman's help in the work of life better than the medical man; in this sense it is a pleasing duty to recommend the 'Waverley Journal' to the attention of our readers. 'The Dial Register,' a new daily political periodical, has just been started on the joint-stock principle, under Liberal colours, and comes under our favourable notice as an advocate of reform generally, and of sanitary improvements specially. Among this class of works we have to mention the volume of 'Experiences of a Civilian in Eastern Military Hospitals,' by Dr. Pincoffs, who, having had opportunities of investigating the military medical systems pursued in different countries, suggests numerous changes in our own; we hope to be able to revert again to his suggestions. Other popular works deserving special mention are, a charming Lecture by Dr. Radclyffe Hall 'On Work, Fresh Air, Exercise, essential to Happiness,' delivered at Torquay, with a view to promoting the Early Closing Movement; a volume by Dr. George Spencer Thomson 'On the Structure and Functions of the Eye;' a Lecture 'On the Principles of Moral Insanity,' by John Kitching; and a compilation by William Dalton, entitled 'A Key to the Adulteration of our Daily Food.' Medical reform finds advocates in the persons of Dr. Edwin Lee and Mr. J. S. Gamgee; Balneology has a new representative in Dr. Glover, who has published a very useful, as well as entertaining and instructive, work 'On Mineral Waters,' comprising very full accounts of the various watering-places in Great Britain and on the Continent; Dr. Stephen Ward ably and succinctly discusses 'The Medical Estimate of Life for Life Assurance.'

A considerable number of papers on Physiological and Pathological subjects are before us. To the former class belong the inquiries by Dr. John Davy regarding 'The Urinary Secretion of Fishes,' those of Mr. Paget into 'The Cause of the Rhythmical Motion of the Heart,' and an elaborate memoir 'On the Pelvic and Thoracic Members of Man and Mammifers,' by Professor Martins of Montpellier; among the latter we have to enumerate an essay by Dr. Boling of North America, 'On the Mechanism and Management of Parturition in the Shoulder Presentation;' a paper by Dr. Duncan 'On the Doctrine of the Duration of Labour,' one by Dr. Marcet 'On the Fatty Matters of Human Excrements in Disease,' another by Dr. Lindsay 'On the Influence upon the Lower Animals exerted by Cholera and other Epidemic Poisons;' Dr. Murchison has published a paper, exhibiting great research, 'On the Subject of Gastrocolic Fistula;' a paper 'On Exsection of the entire Os Calcis,' by Dr. Carnochan of New York, and a careful investigation into the Structure of the Cysticercus Cellulosæ of the Pig, by Mr. Rainey, conclude this list.

In addition to the work 'On the Microscope,' by Dr. Beale, spoken of among the Bibliographical Notices, we have to mention the first part of an illustrated work by the same author 'On the Clinical Uses of the Microscope;' a good-sized tome 'On Elementary Botany' has issued from the pen of Professor Henfrey, which will be noticed more at length in our January number, to which time we find ourselves compelled to postpone a review of Todd and Bowman's Physiology,' which we had hoped to present to our readers in the present number. A large work 'On Hygiene,' by Professor Œsterlen, well known to the subscribers of the Sydenham Society; an important document 'On Syphilisation,' and 'The Minutes of Proceedings of the Quarantine Convention,' held recently at Philadelphia,' have reached us respectively from Germany, Norway, and the United States. A fourth edition of Mr. Beasley's useful 'Druggists' General Receipt-book,' an essay by Dr. Lane 'On Hydropathy,' numerous surgical and philanthropic papers by Dr. Fabrizj of Nizza, whose qualities of head and heart will doubtless insure him a hearty welcome when he again visits this country; the Latin Harveian Oration by Dr. Copland, the Eighteenth Annual Report of the Registrar-General, and the Elevenh Report of the Commissioners in Lunacy, belong to the literary productions to which we would also desire to give a more prominent place than can be accorded to them in the list of Books Received.

PART THIRD.

Original Communications.

Art. I.

On the Formation of the Skeletons of Animals, and other Hard Structures formed in connexion with Living Tissues. By George Rainey, M.R.C.S., Lecturer and Demonstrator of Surgical and Microscopical Anatomy at St. Thomas's Hospital.

The observations about to be advanced upon the subject of the formation of the skeleton rest upon the fact, that by a proper employment of chemical and mechanical means, calcareous bodies can be produced artificially identical in their mode of formation with a multitude of natural products of a like composition, and so similar in their physical and optical characters as not to admit of being distinguished from them even by the aid of the microscope. Several of the artificial formations, in conjunction with similar natural products, have been submitted to the inspection of persons well practised in the use of the microscope, who have failed in distinguishing one kind from the other.

The following are some of the natural bodies which have been especially examined and compared with the artificial ones:—The minute globular calculi found in the urine of some quadrupeds, especially in that of the horse; the calcareous deposits found in the pineal body, and several other parts of the brain, also in several glandular structures; the globular calcareous particles composing the deepest stratum of the shells of crustaceous animals, as may be well seen, by a proper mode of preparation, in the shell of the crab, lobster, shrimp, &c.; the most recently-formed layers of the shells of mollusks, as the oyster, muscle, &c.; the otolithes of fishes, especially when in progress of development, or in those of a very small size, as in the young minnow, in which they are about the size of the largest artificial calculi. To these might be added innumerable others of a similar structure and composition.

All these structures—even those first mentioned—are classed by physiologists and histologists with organic products, being considered as made up of minute bodies endowed with a vital formative power, and called cytoblasts, nucleated cells, &c., according to the cell development theory of Schwann and Schleidon.

The chemical and mechanical means, above alluded to, necessary to form the artificial bodies are—first, the formation of a carbonate of lime, by the donble decomposition of a compound of lime and carbonate (sub-carbonate of the old Pharmacopœias) of potash or soda, each being previously dissolved in a separate portion of water containing, in solution, some viscid animal or vegetable substance, such as albumen or gum arabic; secondly, a density of the viscid medium in which the carbonate of lime is formed, about equal to that of the newly-formed carbonate itself; and thirdly, a state of perfect rest of the fluid in which the decomposition is going on for two or three weeks, or longer, according to the size and completeness of the calculi required.

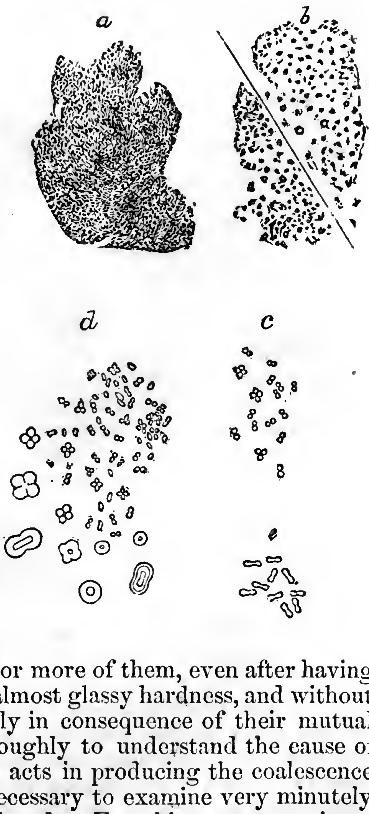
Now, I may observe that, although the fact is familiar to many, that a crystalline carbonate of lime is thus formed when the decomposition is effected in common water, yet I believe it has never been noticed that when the carbonate of lime is produced in such a medium as that above mentioned, in the place of being crystalline it is *globular*, and that, in this form, its molecules possess a most remarkable power of coalescing into large spherical transparent calculi, and also of intimately blending with all such substances as, in its nascent state, it may happen to be brought into contact with—as, for instance, the glass on which it is deposited.

This globular form of the carbonate of lime was first observed by me in 1849, and shown at that time to several of my friends; but I did not notice its other properties, and the extent to which it exists in organic products, until the year 1856.

Now as the various forms of this substance, as prepared artificially, can be shown to be identical with the corresponding forms of the same compound occurring in nature, a clear and comprehensive description of the best method of forming the artificial products cannot fail to be of interest, especially as it will furnish the best and only key to the successful investigation of the real nature and mode of formation of the natural ones. Besides, I may add, that the artificial calculi, when properly prepared, present the microscopist with a new class of polariscope objects, probably of all others the most beautiful. After making a great number of experiments, with a view to determine the process best adapted for furnishing these calculi of the largest size and in the shortest space of time, I found the following to be the best formula.

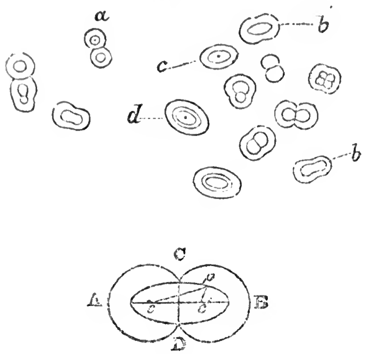
Dissolve a pound of gum arabic in as small a quantity of water as convenient, and, after straining off the solution, mix with it two ounces of carbonate of potash; then set aside the mixture for some days, in order that the carbonate of lime therein formed may subside. Afterwards, filter the supernatant alkaline solution of gum through cloth, and evaporate it by a very gentle heat to the consistency of very thick treacle. The carbonate of lime which is thus formed and deposited is in globular particles of different sizes, but not sufficiently transparent or spherical to be applicable to microscopic purposes. To obtain the more perfect forms of this globular carbonate of lime, put a given quantity of the above inspissated solution, with some more subcarbonate of potash dissolved in it—one ounce, for instance—into a two-ounce wide-mouthed bottle, and introduce into it two microscopic slides of the ordinary dimensions, so placed that their upper edges are resting one against the other, whilst their lower ones are separated as far as the width of the bottle will allow; then pour gradually, so as to disturb this solution as little as possible, into the same bottle a sufficient quantity of weak solution of gum arabic, perfectly clear, and of 1·050 specific gravity, entirely to fill it; afterwards, let these remain at perfect rest for three weeks or a month, or even longer, if the calculi are required to be of the largest size and perfectly transparent; and, lastly, remove the slides, wash them in repeated portions of water, and globular calculi will be found attached to their surface. Those on the under surface of each slide will be found to be the largest, most spherical, and to have the greatest transparency. In this process the lime is furnished by the decomposition of the malate and other soluble salts of lime contained in the weak solution, which are in about the proper proportion to produce the largest globules. The calculi are formed in the shortest time when the two solutions differ most in density, notwithstanding an aqueous solution of the alkali will not answer. It must be combined with gum at the time they are brought into contact, otherwise crystals, and not globules, of carbonate of lime will be produced. If the mucilage containing the alkaline carbonate, and that containing the salt of lime be of an equal density, they combine very slowly, and the globules are formed slowly; but their structure is very complete if only sufficient time be allowed for the thorough blending of the two solutions. I have found that two or three months will be necessary for that purpose.

FIG. 1.



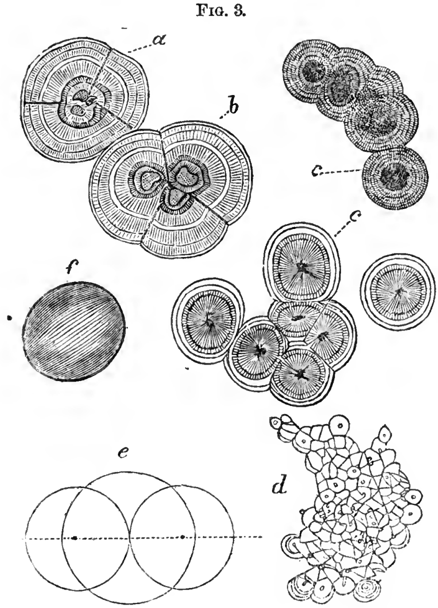
The most remarkable fact presenting itself in the formation of these spherical calcareous bodies is, that of the perfect coalescence of two or more of them, even after having attained $\frac{1}{125}$ of an inch in diameter, though of an almost glassy hardness, and without any external force acting upon them; but solely in consequence of their mutual attraction one for another. Now in order thoroughly to understand the cause of this singular fact, and the manner in which it acts in producing the coalescence of bodies of such a size and density, it will be necessary to examine very minutely every step of the process by which it is produced. For this purpose, minute portions of two such solutions, of unequal density, as those above described, must be brought in contact, on a piece of glass, under the microscope, and be examined by high magnifying powers whilst the carbonate of lime is being formed. The appearance which first presents itself is that of minute projections of one solution into the other, accompanied at the line of contact by a faint nebulosity, evidently arising from the particles of carbonate of lime there formed being then too minute to admit of being distinguished individually. (See Fig. 1, *a.*) In a short time—about an hour—portions of the nebulous part disappear, and are replaced by minute spherules, still too small to admit of accurate measurement. These are produced by the coalescence of the molecules which had before constituted the nebulosity. (See Fig. 1, *b.*) Next, these spherules, becoming fused together, form larger ones, which, in attaining an exactly spherical figure, pass through various intermediate forms, such as that of dumb-bells, caused by the contact of only two spherules, and ellipses of different degrees of eccentricity. (See Fig. 1, *c* and *d.*)

FIG. 2.



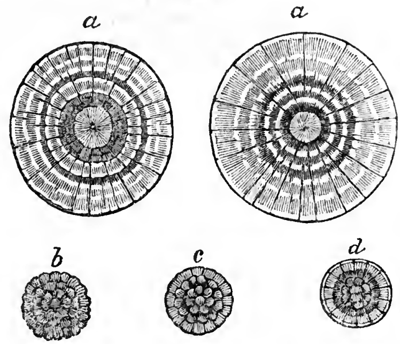
To examine the larger and more perfect forms of this compound, some deposit, taken from solutions which had stood for about a month, should be obtained, which, after being dried, must be put up in Canada balsam. Figs. 2, 3, and 4 contain representations of all these forms, which will be more particularly referred to when the manner in which they are severally produced, and the physical forces upon which their production depends, come under consideration. For this purpose the accompanying figures will serve, better than any verbal description, to convey an accurate idea of the appearances indicative of the different stages of coalescence; and as it will be more easy to refer to them than to written descriptions, I shall at once proceed to consider the cause of these appearances, and the manner in which this cause acts in producing them.

FIG. 3.



As every particle of matter, whatever may be its size, is under the influence of a force called gravity, to which the molecules of carbonate of lime, as produced in the manner already described, can form no exception, it will follow that the instant they are brought into existence they will commence arranging themselves in spherical figures, unless there should be some other force of an opposite kind acting upon them, either adequate entirely to overcome that of gravity, or sufficient only imperfectly to oppose its influence, in which case results of an intermediate kind would be produced, depending upon the relative powers of the opposing agencies. Now as it is a fact that the molecules of carbonate of lime, when formed in a solution of vegetable gum or albumen of the same specific gravity as the molecules themselves, do become arranged in spheres, the force acting upon them, and thus causing them to assume the spherical form—that is, to take up such a position one with respect to the other as to bring the greatest number of molecules into the smallest possible space—must either be universal attraction, or some other force capable of acting upon the

Fig. 4.



particles of matter precisely in the same manner. Now as the latter supposition is not likely to be true, the spherical form of the first set of particles of the carbonate of lime, formed under the circumstances detailed in the experiment, can be attributed only to gravity.

The formation of the smallest, or first set of spherules, that is, of those only just sufficiently large to present an appreciable form under the highest magnifying powers, being considered, it next remains to offer a few remarks on the manner in which these coalesce to produce larger ones. Now, as it has been demonstrated by Newton, that in a sphere the total attraction resulting from the particular attractions of all its component atoms is the same with respect to any body drawn towards it, as if all the attracting particles had been concentrated at the centre, these minute spherical particles, as so many gravitating points, will be drawn towards each other with a force varying inversely as the squares of the distances between their respective centres; hence, being contained in a medium of the same density in which external sources of attraction will be balanced, it is evident that they will by their mutual attraction alone readily form themselves into spherical masses. Now, as each of the spherical particles entering into the composition of these masses can only maintain its spherical form so long as all its component molecules are balanced between equal and opposite attracting forces, it must follow that when these spherules are brought into opposition with others of the same kind, as in the above conglomerations, the balance in each will be destroyed; and that the molecules, which were before at perfect rest, will now be thrown into a state of molecular agitation; and, as it is admitted, and that upon the best grounds, that the atoms composing all bodies, whatever may be their degree of hardness, are not in the condition of absolute contact, but are so circumstanced as to allow of a limited extent of motion among themselves, there will be no difficulty in comprehending how the same attractive force which had been at first separately exerted upon the molecules of individual spherules, so as to dispose them in spherical figures, will suffice, when exerted upon those of several spherules at the same time, to arrange them also in one aggregate spherical mass. Now as all the molecules of every component spherule had been before arranged in reference to its own centre, the position which these molecules will have to take up in the new sphere will differ from that which they before occupied in the old spherule; and as the sum of the spaces occupied by the component spherules must exceed that of the space which they will fill up when they are all incorporated into one sphere, it being the property of spheres to present a maximum of capacity with a minimum of superficies, it will follow as a natural consequence that each molecule, after leaving its component spherule, will have to pass over a certain space before it can obtain a fixed position in the sphere of which it is about to form a part. Hence, prior to the complete coalescence just mentioned, all the molecules of the component spherules must undergo a process of complete disintegration before they can attain that condition of perfect stasis which results from each molecule being balanced between equal and opposite attractive forces. See Fig. 3, *e*, which represents the sections of two calculi of equal size placed in apposition; also the section of one which would be produced by their union, whose proper situation and relative size is constructed in accordance with the fact of the capacities of spheres being as the cubes of their radii, and the correctness of the above deductions will be obvious.

The two molecules at the point of contact being between equal and opposite forces, will be as if not attracted at all by either sphere, but only by one another, and therefore being in this way detached each from its former sphere, will be in the condition necessary to form the centre of the new sphere; the molecules of the adjacent spheres in the vicinity of this point being only feebly attracted by one another, that attraction being as their distance from it, will be in a condition to admit of easy displacement by the molecules in the remoter hemispheres; which, having their attraction for one another less enfeebled, will be drawn together *en masse*, and thus the contents of the remotest portions of the two component spheres will be brought inwards into the outer portions of the space representing the section of the resultant sphere, whilst those situated nearer to their centres will be forced in opposite directions, some forwards into the fore part, and others backward into the back part of the same space; until all the molecules of the two spheres continuing thus to enter it, some moving towards its centre, and others from it, will entirely fill up this said space. Now as the molecules occupying the outer parts of the surface of the two component spheres will, from their position, be the last to reach the line indicating the surface of the aggregate sphere, over which they cannot go, and as those on the portion of the surface of these two spheres contained within the area of the circle intended to represent the section of the resultant sphere will be nearer than the other molecules to its surface, beyond which they cannot pass, it will follow that when the new sphere is completed, its superficies will be made up of a part of the molecules which had before entered into the composition of the superficies of the old spheres. And as the same reasoning will apply to the next and all the subsequent layers of particles, it will be obvious that the greater part of the molecules which had occupied any given position in relation to the centre of each of the component spheres will be similarly placed with respect to the centre of the aggregate sphere.

If the two component spherical calculi should be similarly laminated, as represented in Fig. 4, *a*, it will be apparent from the inspection of this figure that at the contiguous extremities of any two laminæ similarly situated in the two coalescing spheres, the molecules of each lamina will be under the same mechanical conditions—that is, they will be equally and oppositely attracted, just as the two molecules were which had united to form the centre, and therefore these will coalesce in the same manner. The same reasoning will apply to all the other molecules similarly situated in the two spheres, until both are blended together into one.

Some large calculi are formed by the coalescence of several small spherules of nearly the same size. The first stage in the formation of such calculi is a spherical conglomeration of these spherules, presenting a mulberry appearance (see Fig. 4, *b*, *c*, *d*), and looking very much like a form of corpuscle called by pathologists a "glomerulus." This form of calculus furnishes a good example of the process of coalescence, but still not so remarkable as the one just described. The first indication of this process is an indistinctness and want of definition of outline of all the component spherules, especially of those nearest to the surface. These afterwards lose every vestige of their original form, and become converted into an amorphous granular mass, of the form of the original conglomeration. Next, the disintegrated molecules nearest the surface coalescing, form a clear ring completely surrounding the amorphous matter occupying its interior. (See Fig. 4, *b*.) As the processes of disintegration and subsequent coalescence progress, the circumferential bright ring increases in width as the central amorphous part diminishes (see Fig. 4, *d*), until all the latter disappears and is replaced by a succession of bright concentric laminæ, extending from the circumference to the centre, as shown in Fig. 3. Now, the fact of the disintegration of the component spherules and the subsequent coalescence of their disintegrated molecules in bright rings, beginning at the circumference and terminating at the centre of one of these compound calculi, is exactly in accordance with the effect which gravity is well known to exert upon the particles of bodies presenting a spherical figure; this force varying as the distance from the circumference, where it has its maximum, to the centre, where it is at zero. Hence the molecular activity evinced by the more rapid separation and re-adjustment of the molecules occupying the superficies than near the centre is obviously the effect of that same cause which, in the first instance, brought the spherules into their globular form, and disposed them afterwards in one spherical mass.

The arrangement of the molecules, in such calculi, into concentric laminæ admits also of a similar explanation. Now, from what has just been observed, it will be seen that the size of one of these compound calculi will have been fixed by the coalescence of its peripheral molecules, these being the first to take up their final position in relation to its centre. But the molecules enclosed in this vitreous-looking shell, which is just the appearance it presents when put into Canada balsam, and viewed by the microscope, have not yet undergone this same arrangement: that is, been so disposed one with respect to one another as to occupy the smallest space they are capable of occupying; consequently after they have undergone the same process of coalescence as the peripheral ones, they will be brought into too small a compass entirely to fill up the space which in their amorphous condition they occupied. Hence, when their coalescence has been completed, an obvious interval must exist between them, and the case in which they are contained. Now, according to this supposition the coalescence is perfected at two separate periods, and therefore only one interval can be left. But such is not the case: the quantity of molecules which are at the same time in process of coalescence, that is, under the mechanical conditions necessary to bring and retain them in equilibrio, will probably be only very small, this quantity depending upon other influences besides gravity; one of these is active and passive inertia, an influence at one instant co-operating with all such mechanical obstacles as tended to keep the coalescing particles at rest, and at another opposing these same obstacles when they became hostile to their condition of motion; another is, variation of temperature. A state of perfect quiescence or the reverse, of the materials during the progress of the experiment, will also have considerable influence; consequently the precise number, distinctness, and degree of thickness of the laminæ composing these calculi, not being the result of a continuous process, but one which is thus interrupted by these and other influences over which we have no control, must always be attended with the greatest uncertainty. But there is another cause influencing the number of laminæ composing these calculi, which will be noticed hereafter.

Fig. 2 contains several spherical particles of different sizes, and in various stages of coalescence, from the dumb-bell figure to the perfect spherule. *a*, shows two small spherical particles just brought by the mutual attraction of one for the other into contact; *b*, two others which have sufficiently coalesced to acquire the form of a dumb-bell; *c*, two others, whose process of coalescence is further advanced. In both these particles, especially on the side where they are in contact, there is a difference in the structure of the central and the peripheral part, seen particularly distinctly when they are examined by polarized light, when these parts appear of different colours. During the coalescence of two such particles, the central portions appear to join before the peripheral ones. (See Fig. 2, *b*.) At a stage rather more advanced, the central portions of the two spherules have coalesced sufficiently to circumscribe a perfectly elliptical area, whilst the peripheral parts, retaining still more of their spherical form, present a depression all around their line of contact—that is, retain the dumb-bell form. This want of correspondence in shape of these two parts, shows that the central elliptical portion cannot have been the nucleus upon which the circumferential one was moulded. (See Fig. 2, *b* and *c*.)

The explanation of this fact seems sufficiently obvious. For a certain portion of the molecules in the remotest part of the remote hemispheres of the two coalescing spherules, being under the influence chiefly of the attractive force acting in the direction of the centre of each sphere, will retain their spherical figure; whilst certain of the other molecules being attracted equally and simultaneously by the two forces, acting in the direction of the centre of each spherule, will be placed under the mechanical conditions necessary to form them into an ellipse (see Fig. 2), whose axis major is the line A B, and axis minor C D, and whose foci are the points, c and c' the centres of the coalescing spheres. And the co-ordinates of the curve are the lines *c p* and *c' p*. Hence the figures made up of these several molecules, retaining in some parts the form of spheres, will refract the light as such, but in the central part that of the ellipse will transmit light according to the laws of transmission of light, by transparent media of this figure; as these transmit light differently, one can be easily distinguished from the other. After the coalescence of two of these spherules has advanced somewhat further, the spherical form of the remote hemispheres disappears, and the exterior outline of the two spherules, now completely fused into one calculus, is also elliptical, so that now the figure presents two ellipses, one within the other. (See Fig. 2, *c*.) The still further coalescence of these spherules is only attended with a gradual diminution of the eccentricity of these ellipses, which continue diminishing until their foci coincide, and the form of the calculus becomes accurately spherical, when, as was before observed, the molecules will all become quiescent, and gravity will have produced upon them its utmost effect. As there were before the figure attained the spherical form two ellipses, so now there will be two spheres, one contained within the other. The careful inspection of instances of coalescence like this, of which examples are not difficult to meet with, shows that the existence of rings or concentric laminæ in an organic or inorganic body is not necessarily the effect of successive depositions on its surface, as is generally believed by physiologists and pathologists.*

But some of the elliptical and spherical calculi as above formed, may be seen to have two or more internal divisions. This is the cause of lamination, to which allusion was before made. It may be observed, that the concentric divisions produced in this manner are more distinct, and more completely divide the calculi, in which they occur, into separate parts, than do the causes of lamination before mentioned. The coalescence of the particles of carbonate of lime does not necessarily result in the production of ellipses and spheres; it is only when the effect of gravity upon them is not interfered with by external mechanical causes, that such perfect forms are produced.

Fig. 3, shows spherical calculi of various sizes in different stages of coalescence; *d* are small ones upon one plane, so that they present a flat membranous-like appearance; *a* shows three calculi of about the same size, uniting into one, whose coalescence is nearly perfected, and the form of the resultant calculus almost spherical; *b* are three others very similarly placed with respect to one another in progress of coalescence, but not so far advanced; *c* shows several smaller calculi in progress of coalescence. The number of calculi composing any one calculus not completely formed, can always be determined by examining it by polarized light; the effect of this mode of examination is the formation of a cross upon the central part of each component calculus, which has not yet undergone disintegration and coalescence. This part, for reasons before stated, is the last to disappear. It remains frequently in an amorphous state long after the molecules in all the other parts have coalesced.

Besides the arrangement of the molecules into laminæ, as above described, the molecules are also disposed into exquisitely fine lines, of an imperfectly crystalline structure, extending as radii from the centre of a calculus to its circumference. These radiating lines are not equally visible in all calculi, and they do not exist in such as have not been able, from the action of some opposing mechanical cause, to acquire a perfectly globular form. They are particularly distinct in those calculi which have been formed under circumstances the most favourable for ensuring a perfectly spherical figure. This singular instance of the transformation of a form of carbonate of lime which, under other circumstances, seems to have no tendency to become crystalline, into radiating crystals, can be best seen by breaking up one of the larger kinds of calculi into fragments, and examining these fragments by polarized light.

The cause of this change of form of the carbonate of lime from the globular to the crystalline, appears to me to be deducible from a fact which has already been men-

* See Kölliker's Manual of Histology, p. 458.

tioned, but which now requires a more particular notice. It has been observed, that when the carbonate of lime is pure, that is, uncombined with vegetable gum or albumen, its molecules arrange themselves in straight lines, and thus form rectilinear figures or crystals; but that when combined with gum or albumen this power is either lost, or so much impaired that its molecules fall under the effective influence of universal attraction or gravity. Now, as it has been shown that the spherical arrangement of the molecules of the compound of carbonate of lime and gum is the necessary consequence of a principle of mutual attraction, the arrangement of the molecules of pure carbonate, which are just as much acted upon by gravity as those of the impure carbonate, in straight lines, may be inferred to be the effect of exactly an opposite principle; that is, one of mutual repulsion—the one having a tendency to bring the particles of matter into the smallest possible space, that is, into spheres; the other, a tendency to place them at the greatest distance apart, so far as its sphere of action extends with sufficient force to be uncontrolled by the influence of gravity, that is, to dispose them in straight lines. Probably this self-repulsive power does not operate with great force until the molecules composing substances capable of assuming the crystalline form are brought within very small distances. Hence, in the congelation of water, and some metals which have a crystalline form, expansion takes place the instant the globular arrangement of their molecules is changed into the rectilinear one.

To apply this reasoning, it is only necessary to show that the repulsive force which operates upon the molecules of the pure carbonate, causing them to assume the rectilinear arrangement, may, in the combination of this substance with gum, be so impaired as to be insufficient altogether to resist the effect of gravity when exerted under circumstances favourable for securing its full effect, but still not so much weakened as under circumstances where the effective force of gravity is but very trifling, or altogether null, to be incapable of producing an imperfectly crystalline form. That such is the case will be obvious on a little consideration of the mechanical conditions under which the molecules of carbonate of lime are placed in these calculi, in respect to the directions in which gravity is exerted upon them. For, as all the molecules composing one of these spherical calculi are attracted laterally by equal and opposite forces, the effect of gravity upon them in this direction will be the same as if they were not attracted at all, but being still effectively attracted in the directions of lines extending from the circumference to the centre, they will be drawn into straight lines or radii, and thus the force of gravity, in the place of opposing that of repulsion, or the force of crystallization, will, so far as position of molecules goes, have the same effect, and in this way conduce to a crystalline rather than to a globular disposition of their molecules. This explanation of the transformation of a substance, which under one set of mechanical conditions presented not the slightest appearance of being crystalline, into an imperfectly crystalline form, when these conditions became altered and made conducive to that effect, seems to me so completely to agree with the facts as made manifest by experiment, as to furnish conclusive evidence not only in favour of the correctness of the reasonings above employed, but also of the reality of the data upon which the reasonings are founded.

The forms of the carbonate of lime, pure and mixed, which have been described —namely, the spherical and crystalline, considered as the result of two forces, one attractive and universal in its operation, the other, repulsive and probably only partial in its action, are such as require the application of these forces in their maximum and minimum degrees; but it can be very easily shown by experiment that these forces can be so varied in their relative proportions as to produce between these extremes all the intermediate forms,—that is, examples can be shown, of which I have many in my possession, of the gradual rounding off of the angles of a rectilinear figure, and curving of its sides until it merges into the form of an oval or curvilinear one. The space allowed me for this communication will not permit of a detailed account of these experiments. This compound, which may be called the globular carbonate of lime, is not a mere mixture of gum with carbonate of lime, but it is one in which the atoms of these substances are molecularly combined. It is not in the slightest degree affected by prolonged boiling in water, but when heated to redness it becomes brown, and loses about 17 per cent. of its weight. In the form of the largest calculii, when rubbed between the fingers it gives the impression of fine sand. It is very hard, and does not readily break. The perfectly formed calculi, though of an accurately spherical figure, as can be seen when viewed by reflected light (see Fig. 3, *f*), appear almost flat when examined in Canada balsam by transmitted light. This fact is in perfect accordance with the explanation given of their mode of formation, according to which their density, and consequently their refractive power, would diminish from the circumference to the centre, and therefore the spherical aberration being by this construction increased, the transmitted light will of necessity be more dispersed than if it had been all of the same density. In this respect it would be exactly the opposite of the lens of the eyes of fishes. Besides the property which this substance possesses, of combining molecularly with particles of the same composition, it combines also with other solid substances with which it happens in its nascent state to be brought into contact. This is illustrated in a remarkable manner by the action which it has upon the surface of the glass slides on which it is formed, and allowed to remain for a sufficient time, when it becomes so intimately blended with the substance of the glass as to leave on the slide, after it has been washed with muriatic acid, impressions of the form of the globule or particle of carbonate which had been attached to its surface. These impressions are seen most distinctly by oblique light; they affect sufficiently the form and molecular condition of the glass at these parts, as to allow of casts being taken of them on films of collodion. Whether this intimate union is in part or not the result of chemical action, it is impossible to say, but that it is in a greater or less degree mechanical is obvious, from the change of form which the particles undergo after having been long attached to the glass—those which were oval or globular become more flattened; and on one side they become gradually thinned off until at this part they are so blended with the glass that no line of demarcation can be distinguished between the particle of carbonate and its surface. When the calculi formed in solution of gum are exposed to the action of weak hydrochloric acid, they rapidly dissolve with effervescence, and leave no residue; but when the calculi which have been formed in albumen are treated in the same manner, they leave a residue exactly of the form, and presenting the same markings as the original calculus, so long as they are wet. Showing that a portion of the albumen becomes solidified during the formation of these calculi, and blended molecularly with the particles of the carbonate of lime.

From a review of what has been stated concerning the globular form of the carbonate of lime, and the manner in which it is produced, it will be seen that the artificial process consists of two parts, one chemical and the other mechanical; and that the latter, in the most perfect calculi, admits of being subdivided into a stage of molecular coalescence, of molecular disintegration, and of such a re-arrangement of the molecules of particles, after their disintegration, as to lead to the formation of an imperfectly crystalline structure. In the mechanical part of the process, the two chief agents are such a degree of density as will prevent as much as possible all attractive forces from acting upon these bodies, excepting that exerted by their own molecules towards one another, and such an amount of motion as will favour the collocation of the floating particles into masses, and prevent them falling too rapidly to the bottom of the vessel. This object is effected by the process of diffusion going on between the two fluids from two causes, one in consequence of their difference of density, the other from their being in oppositely chemical conditions.

Having considered fully, but not, I believe, more so than necessary, all that belongs to the structure and formation of the artificial compound of carbonate of lime, it remains to examine the analogous forms of the same substance as they are found in nature, first, with respect to the perfect similarity of their structure; and secondly, in reference to the identity of the laws under which both the artificial and the natural bodies are formed.

The first of the natural products which I shall consider are the minute calculi found in the urine of the horse. As these structures are well known to microscopists, it will be unnecessary to describe them. It may suffice merely to say that calculi of a similar composition can be formed artificially so exactly like the natural ones as with difficulty to be distinguished from them by the microscope. The natural calculi also, if examined in a good specimen containing some of several different sizes, present appearances of coalescence in all respects the same as those of the artificial analogous forms.

As so close a resemblance between products entirely artificial, and therefore furnishing every possible facility for the experimental investigation of their mode of formation, and those considered to be of vital origin, being regarded by physiologists as the products of a low and obscure form of cell-life, cannot appear otherwise than anomalous, I shall offer a few remarks upon this apparent anomaly. Now, as the molecules of carbonate of lime entering into the composition of the natural calculi were, at the very same time that vitality was moulding them into spherical figures, as much under the influence of universal attraction, as were the molecules of the artificial calculi when undergoing the same process, it would follow, on the supposition of this being a vital process, that the vital principle in constructing the natural calculi would have first to oppose and overcome a physical principle which it has been shown is perfectly adequate to form exactly such a calculus without its aid, and then to employ in the place of this principle a series of supposed centres of force to produce precisely the same effect. As I cannot see in this or any other explanation founded upon such imaginary data anything approaching even to probability, I shall describe what appears to me to be a very simple and obvious mode of formation of these concretions. There can be no question but that the urine of the horse when recently secreted contains in solution some compound of lime, albuminous matter, and some salt of urea which is easy of decomposition, and when decomposed, furnishes carbonate of ammonia; hence, as soon as this decomposition of the urea or its salts commences, all the conditions necessary for the formation of the globular carbonate specified in the artificial process, are brought into existence. The carbonic acid set free from the decomposition of the carbonate of ammonia would combine with the lime to form a carbonate of lime, which meeting with the albumen of the albuminous matter whilst in its nascent state, would form with it the globular or coalescing compound in question, the particles of which being contained in a fluid medium, holding animal matter in solution, would coalesce to form globules of various sizes and degrees of perfectness, accordingly as all the other conditions may be more or less favourable for this process. There is one fact connected with this subject which has been regarded by physiologists and histologists as conclusive evidence in proof of the vital origin of these and all similar structures—which is, that of a soft material, "animal basis," being left, of the same form and appearance as the original body, after it has been decalcified by hydrochloric or other acids. Now, as it has been observed that precisely the same result follows a similar treatment of the artificial carbonate when precipitated in albumen, this evidence is valueless, being as much a character of the artificial as of the natural products. Hence, as it is certain that if all the conditions which have been mentioned as necessary for the formation of the artificial calculi exist in respect to the formation of those in the urine, similar calculi will be formed, and as there appears to be no just reason for assuming the absence of any one of these conditions, the above explanation of the mode of formation of these forms of natural calculi may be presumed to be correct.

The next structures requiring notice are those which have been mentioned in an early part of this paper as constituting the external skeleton or shells of crustaceous animals. These parts or organs are made up of alternate layers of hard and soft tissues very much blended together, especially in the more superficial regions of the shell. The hard part is chiefly composed of the different forms of the globular carbonate of lime, the form or character of this compound depending upon the part of the shell in which it is found. On the superficial surface—that is, that which is nearest the free surface of the shell—of the deepest membranous layer, the one lined with pigment corpuscles, the globular carbonate is in its molecular state, presenting the appearance of minute points or punctures dispersed more or less thickly over the membrane; but none exist on its deep surface. These molecular particles are too minute sensibly to polarize light, but their composition can be at once made apparent by weak hydrochloric acid. After thus acted upon, like the artificial analogous form, they leave a residue of soft material of the same figure. In the membranous layers next to this, these particles can be seen to have coalesced into the globular form, which in good specimens resemble in all respects so completely the artificial products in the same state of coalescence, that I have not met with any one who, on comparing the artificial and natural products, could tell the difference. By the polariscope, which furnishes the most rigid means of discriminating between such structures, no difference can be distinguished. (See Fig. 5, which is an accurate representation of a portion of the innermost layer of the shell of a very young lobster, about four inches in length, in which the globular particles, of all sizes, and in all stages of coalescence, can be seen.) The relative position of the molecular, globular, and laminated parts of the shell of the crab can be very well seen by grinding down vertical sections of the part of the shell which supports the nippers, taking great care not to remove, in the process of grinding, the globular or deeper layer, which being the least dense, is easily removed. Also a portion of the same shell may be ground horizontally from the outer surface of the shell, so as to leave the deepest layer of membrane. These sections had better be preserved in glycerine, as in this case drying them is not necessary as when they are to be kept in Canada balsam. Drying does not suit some specimens, either natural or artificial, which are to be examined by transmitted light, as the balsam does not displace all the air which in the process of drying enters the spaces between the minuter particles, and then these parts remain opaque. The globular form, besides composing the deepest layer of the shell, is, in shells of any considerable thickness, collected into vertical portions or columns, extending from the globular layer to the free surface of the shell; also accompanying these portions there are irregular passages extending from the pigment layer, which sometimes enters them; also to the free surface. These passages are best seen in vertical sections of decalcified shells. In some Crustacea these passages end on the surface in feathery processes. The other parts of the shell are made up of the same globular bodies after they have acquired their laminated character. Hence, from what has been stated, it appears that all the forms which the globular carbonate assumes in the artificially prepared compound, can be recognised in the hard part of the shells of this class of animals. It must be particularly observed, that in comparing the artificial with the natural products, the portions compared must be in the same molecular condition—that is, the molecular, the globular, and the laminated of the one must be severally compared with the corresponding conditions of the other, as there is but little resemblance in the appearance of different portions of this compound when in different states of mo-

Fig. 5.

lecular arrangements; but the natural does not differ in this respect more from the artificial, than the latter in its dissimilar molecular conditions differ one from another; for nothing can be more unlike in appearance than the clear spherules as they exist separately, and the globular body which they form by their coalescence after its molecules have undergone their final arrangement in laminæ, and radiating imperfectly crystalline fibres. The closest inspection of the one could never suggest the idea of its being made up of the molecules of the other.

Although all these forms of the globular carbonate may sometimes be found in one section of the shell of the crab or lobster, still this must not be expected. The thorough investigation of these bodies will require an extended examination of different crustaceans, and of different parts of them, and more especially of those wherein the operation of the physical principle is least opposed and interfered with by contiguous structures—that is, the more the conditions under which these bodies are formed in organized bodies resemble those of the artificial process, the more perfect the bodies will be. Young crustaceans are well adapted for this purpose, and those parts of them must be chosen where the calcification is in its incipient, as well as in its advanced states. The very young hermit crab is a good object, especially if the part where the calcareous and membranous portions join be examined; but in the examination of this animal and all others, as well as in the artificial products, one step cannot be successfully made in this investigation without the polariscope.

The small crustaceans having in their shells a large proportion of the softer constituents, and the calcareous parts not being so densely compacted, may be examined with advantage. The globular carbonate, in all its stages of coalescence, can be well seen, by a little patient examination, in the shells of the shrimp and prawn. In these it sometimes occurs in circular discs, containing in the centre globular particles—also in patches of a stellate form, assuming more of a crystalline arrangement than in the discs. Besides these, it can be found in all the ordinary forms occurring in the artificial specimens, as, for instance, in spherical bodies and coalescing particles, so exactly like the artificial ones as to leave no doubt whatever of the identity of the process to which both the natural and the artificial forms owe their origin. Such being the resemblance of the various parts of the shell, in this class of animals, with the corresponding forms obtained by the artificial process, it now remains to examine the chemical conditions under which the natural products are formed, and to compare them with those of the artificial process.

Now, as these animals cannot be supposed to create or form *de novo* the carbonate of lime contained in their shells, either this substance or its chemical elements must be looked for in the medium they inhabit; but as it seems scarcely possible that any solid particles of carbonate of lime formed in this medium could penetrate the substance of the shell and arrive at the external surface of the deepest layer of membrane—the part where the carbonate is first detected by the microscope—all its elements must of necessity be brought into this situation in a state of solution; and such can without any difficulty be shown to be the fact; as, on the one hand, in all crustaceans and molluscs, and without doubt all other animals having shells composed of carbonate of lime whose size is sufficient to admit of the application of the proper tests, carbonate of soda with albumen in solution can easily be detected on the surface of the animal next to the shell—(these are sufficiently abundant as at once to affect reddened litmus paper)—and the fluid from this situation deposits albumen on boiling; and on the other, there can be no difficulty in comprehending how the water, containing in solution different compounds of lime, can penetrate the passages of the shell above described, and diffusing itself through the entire shell, arrive at the superficial surface of the innermost membranous layer, and there, meeting with the sub-carbonate of soda and animal matter in solution, undergo the same decomposition with the same results as in the artificial process, or as was explained in describing the formation of the calculi found in the urine of the horse.

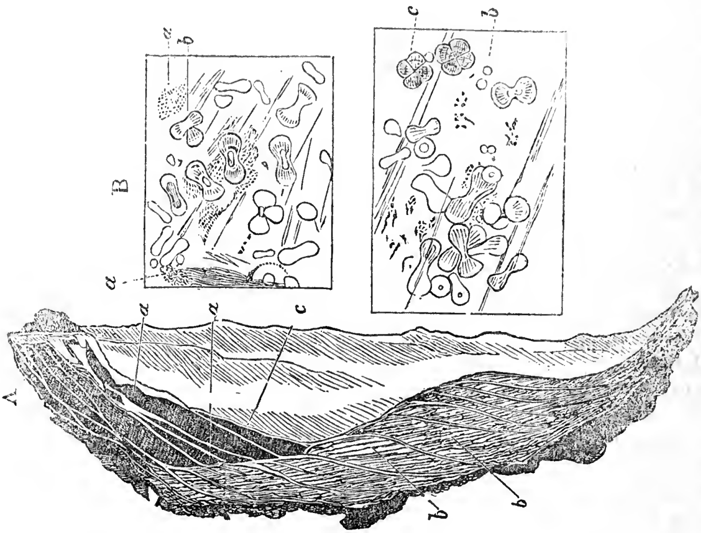
Now, if under such circumstances the different forms of the globular carbonate described in the shells of these crustaceans be the immediate result of an act of vitality, then will nature not only needlessly have brought together a set of conditions shown by experiment to be perfectly adequate to the same object, but she must also have been obliged to institute a separate act of interference between the salts of soda and lime, to prevent one decomposing the other. The production of an alkaline carbonate is a remarkable fact in the natural process, and results without doubt from the decomposition of some of the salts of potash or soda contained in the medium in which these animals live. Under what influence this decomposition is *immediately* effected is probably altogether unknown, and therefore it must remain classed with vital phenomena until a more advanced state of physiological science reveals the chemical, or perhaps more probably the electrical, machinery which directly causes it.

The animals having an external skeleton, which come next in order, are those comprised under the title of Mollusca. Of these I will describe such as are the most common, and in which the fact connected with the formation of their shells can be most easily verified by those who may wish to examine them for themselves. The shell of the muscle—and more especially of the oyster—furnishing all the necessary facts to be observed in this class of tissues, I shall confine my observations to them, taking it for granted that all the rest are formed after the same plan.

The shells of the muscle and oyster are laminated, like that of the crab or lobster, and grow in the same manner, by the successive additions of new layers to the internal surface of the layer last formed, so that each valve of a shell presents a cone, whose summit is the valve or layer first formed, and base, the layer which is formed last, the one in immediate contact with the surface of the mollusc. Each layer also consists of a membrane calcified, especially on its external surface. This arrangement is best demonstrated by putting an entire valve into hydrochloric acid, so feeble that the too sudden escape of the carbonic acid will not break the membrane into fragments, but raise it entire. The most external membrane of the shell of the muscle does not become calcified, nor does it receive an addition to its entire surface, but only grows at its free border. It is of a horny texture, and has distinctly cellular markings. But the internal membrane, as the muscle grows, receives an addition of a new membrane to the whole of its inner surface. This membrane is perfectly homogeneous, presenting no markings whatever, excepting close to the edge of the valve, where it becomes continuous with the external membrane. Nearly the same arrangement exists in the laminæ of the shell of the oyster, which, being less firmly adherent, are more favourably circumstanced for the examination of the process of calcification, the physical conditions favouring the deposit of globular carbonate in a globular form being less interfered with by the close proximity of the membranes on which it is deposited. Hence I shall confine my descriptions to the appearances demonstrative of the nature of this process as they have occurred to me in examining the shell of the oyster. I may also observe that the very largest oysters that can be obtained —those which have large cavities between their laminæ—are the best suited for this examination. I am not aware that the season of the year makes much difference in this respect, but season has doubtless some influence on the growth of these shells. The most successful examinations I have been able to make have been in the month of June. One way of exhibiting the structure of shells of this kind is by grinding vertical sections sufficiently thin to allow of being seen by transmitted light. This plan answers very well for showing their laminated character; but it gives no idea of their true structure, and much less of the manner in which they are formed. These can only be learned from the examination of extremely thin, partially-calcified laminæ, sufficiently transparent to allow of being examined by transmitted light without any previous manipulation which could injure their structure. The more incipient the calcification is, the more complete will be the spherical form of the coalescing particles. These particles are best seen on those layers which are situated over the inter-laminar cavities before mentioned, between them and the internal surface of the shell. In such a situation the coalescing globular particles, being on a membrane extended between two spaces, will be very little impeded in their development by the mechanical pressure of contiguous structures; and hence the globular calculi so situated will be under circumstances at least as favourable for their coalescence into perfectly spherical figures as in the very imperfect shells of the smaller crustaceas, and almost as much so as in the artificial process. I have in my possession specimens of these bodies in every state of coalescence taken from such situations, so exactly like the artificial forms as not to admit of being distinguished from them by the microscope, even with the aid of polarized light. Partially calcified membranes, like the above, are most easily obtained by allowing oysters of the largest size to die for want of water, and the branchiæ to become dried to the shell, as in this state they can be most easily distinguished from the surrounding perfectly-formed shell, and also allow of being removed in larger films at one time. Such specimens are not to be found in all oysters, but only occasionally, to meet with just this stage of calcification being an accidental circumstance. The globular form of the carbonate of lime can at any time be seen, though in an inferior degree of completeness, on the outer surface of the septa, situated between the inter-laminar cavities. These cavities being always filled with water containing carbonate of soda and animal matter, this globular deposit must have resulted from the decomposition of the salts of lime before existing in this water, and the precipitation of the carbonate of lime combined with albumen upon the surface of these calcareous septa.

On the very thin septa found in these situations, the exquisitely fine and perfectly homogeneous membrane on which the calcareous matter is deposited, can frequently be demonstrated without the aid of acid, more or less thickly covered with particles of globular carbonate in progress of transformation into plates of nacre; but I have never seen this membrane so folded as to decompose the rays of light, and thus produce a nacreous appearance, as has been supposed. I have never witnessed any such appearance where the calcareous matter has been completely removed, either mechanically or by an acid, which can only be determined with certainty by the polariscope: besides, the nacreous lustre of a piece of shell is not the least impaired by boiling it a long time in liquor potassæ, and but little so by heating it to redness, which must have been the case if produced by the delicate folds of what has been called the nacre membrane. Now from what has been stated concerning the chemical part of the process by which the carbonate of lime in the globular form is produced and deposited in the shells of crustaceans, much does not remain to be said concerning the same process in these molluscs. The entire surface of animals of this class in contact with the shell is moistened with a solution of carbonate of soda and animal matter, which can be easily demonstrated, as before observed, by the proper tests. A piece of reddened litmus paper, if allowed to remain in contact for a minute or two, with the part of a muscle or oyster next the shell, never fails to show the presence of an alkali. From the same surface the membrane is exuded, on which the carbonate of lime is formed and deposited. This membrane does not follow exactly the surface of the shell, in consequence of the form of the oyster becoming more flat as its size increases, but bridges over the more concave parts of the shell, so as to leave a space or spaces between it and these parts, which, when the membrane becomes calcified and blended with the parts of this layer with which it was before in immediate contact, remain as the inter-laminar spaces before mentioned. These layers begin to adhere first about the middle of the shell, and become connected last at their borders, and thus there is a free access of water containing the soluble salts of lime in solution to the outer surface of calcifying membrane, whilst, at the same time, the opposite surface being moistened by the alkaline solution furnished by the animal, the conditions necessary for the formation and deposition of the globular carbonate are the same as in the shells of crustaceans; and, as the optical characters of this natural product are the same as those of the artificial one, the physical agencies under which both are produced, may be inferred to be identical. Fig. 6 represents a vertical section of the shell of one of the largest kinds of oyster: *aa*, the inter-laminar spaces in which the alkaline solution was contained; *bb*, similar spaces, but filled up with amorphous carbonate of lime; *c*, septa between the

FIG. 6.



spaces containing the fluid; B, portions of membrane, taken from the part where it bridges over the deepest parts of the shell, just beginning to calcify, showing this process in its different stages; *aa*, the molecular state; *bb*, single spherules and dumb-bell-shaped particles: *c*, laminated calculi, in which, in the specimen, a cross can be seen by polarized light.

Although the primitive and secondary forms assumed by the globular carbonate are alike in the crustaceans and molluscs, yet their ultimate forms are different in both; but as this difference depends upon mechanical causes, it can be imitated to a great extent in the artificial process, and thus shown to be the result of physical agency. In the crustaceans the ultimate form of the globular carbonate is the same as that of the artificial calculi, which particularly requires for their formation the removal of all such external sources of attraction as are capable of disturbing that of their own molecules. Such are the spherical laminated calculi. But in the oyster, as without doubt in all other molluscs, the ultimate form is that of flattened plates of nacre. The form can be produced in the artificial process by merely allowing the globular deposit to remain at the bottom of the vessel in which it was precipitated, for several months. I have specimens taken from the shell of the oyster, in which this transformation can be seen in every possible stage, beginning with the form of regular spheres beautifully laminated, and going through every degree of flattening, the markings at the same time gradually disappearing, but last at the centre, until reduced to flat nacreous plates, generally with sharp irregular zig-zag edges. In this condition the plates show no tendency to coalesce. This form is produced conjointly by the pressure of the calcified membranes, between which the calculi are situated, and the attraction which these membranes exert on the calculi themselves. The latter is shown in the artificial process by the effect which the glass slide has in altering the form of the particles deposited upon its surface, and allowed to remain there a sufficiently long time. There is also another form called prismatic, which is situated chiefly about the edges of the shell. This is produced by the coalescence of molecules into globular bodies of different sizes, which occupying the softer parts of the shell, do not become pressed into plates, but which are still so crowded together as to be prevented from assuming the spherical figure. This form also has one analogous to it among the artificially prepared specimens. Thin sections of this form of natural carbonate, ground perfectly smooth and flat, present the appearance of compressed cells filled with air, having their convex sides looking upward—an appearance which, to the best of my knowledge, has never been correctly accounted for. It appears to me to arise from the fact of the rays by which this object is seen, not being those refracted by the flat ground surfaces, but by those molecules which are arranged in curves of which each piece is entirely made up, and of which the plane upper and lower surfaces of the section present arcs of different lengths. An appearance of the same deceptive character is presented by the siliceous cuticle of the common cane after having been boiled in nitric acid. This structure is made up of hexagonal pieces of silex, each having a flat upper and under surface, so joined together as to present one continuous plane, but each piece having within it a flask-shaped cavity; the surface appears, under the microscope, to consist of a multitude of papillar eminences, and in fact to be anything but flat. Besides these two forms of globular carbonate, there are in the more coloured membranous layers of the oyster regular rhomboidal crystals of carbonate of lime, containing sufficient of the albuminous constituent to leave, after the action of muriatic acid, a residue of the same form. These, in consequence of this residue, are considered by some physiologists to be organic cells.

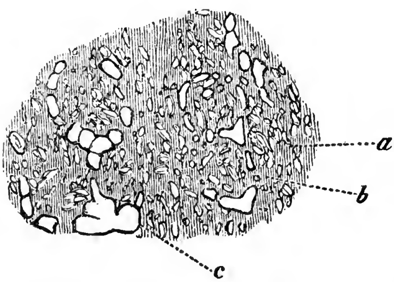
In shell structures the earthy matter is considered by those who have written upon the subject to be contained in the interior of cells, an idea which presupposes the existence of cells prior to that of the carbonate, which in no way agrees with the appearances presented by the microscope. The order in which the parts appear are, first, a membrane, generally without any marking whatever upon it, which can be taken for cells; secondly, very minute particles of carbonate combined with albumen, so as to leave a residue after being acted upon by muriatic acid; thirdly, the coalescence of these particles into larger globular portions, acted upon in the same way by acid as the smallest particles; so that if this albuminous residue is to be regarded as the membranous cell from which the earthy matter has been dissolved, the very smallest particles which are first deposited are as much cells as the larger globular portions; but it has been shown that precisely the same result can be produced artificially, therefore this residue cannot be regarded either as a true cell or as an infallible test of an organic formation; and lastly, the transformation of these globular portions into laminæ or plates, according to the mechanical conditions under which they are placed.

It may be observed that, in some instances where the globular carbonate is deposited near to a membrane having square markings, it seems to follow them so as to appear to be contained in square spaces or cells; but this is completely accidental, as in such cases it more frequently spreads irregularly, and without any limitation from such markings; besides, the membrane on which the carbonate is deposited, or rather formed, is in most instances perfectly homogeneous. Upon the whole I have never, in examining the structure of shells, seen anything to justify the idea of their being composed of organic cells filled with carbonate of lime. The amorphous carbonate which has been mentioned as occupying some of the inter-laminar spaces probably results from the mixture of successive portions of water which finds access to these spaces with the solution of the alkaline carbonate formed by the animal, the latter containing too small a proportion of albumen to form the globular compound. Occasionally in the thin plates of the shell of the oyster, branching dark lines are seen, resembling in their mode of ramification confervæ, which in the perfectly calcified shell are so blended with the surrounding parts as to be difficult of satisfactory examination; but in an imperfectly calcified layer they can be entirely detached in their growing condition from the shell, and shown to be in reality what they at first appeared. In these layers they are found on the surface, to which the water has access. The otolithes of fishes, and the calcareous concretions found in the pineal gland and other structures, might be shown to be formed in the same manner. But as I have not space for a detailed description of the formation of these structures, and as the concretions above mentioned contain more of the phosphate than carbonate of lime, I shall only make a few observations upon the mode of obtaining an artificial compound identical in its properties with that contained in these deposits, as well as in all normal structures of a similar composition—as, for instance, in bone, and upon the formation of the latter structure.

If pure phosphate of lime be formed and deposited under the same conditions as those mentioned in the process for obtaining the globular carbonate of lime, it does not lose its crystalline character, although its crystals congregate in globular masses; but if a small quantity of carbonate of lime be formed at the same time as the phosphate, these blend together and produce a compound resembling in its form and mechanical properties the globular carbonate of lime. The white of egg contains sufficient alkaline carbonate for that purpose; so that if phosphate of soda and muriate of lime dissolved in separate portions of white of egg be brought into contact and allowed to mix very gradually, a mixture of phosphate and carbonate of lime will result, of a globular form. This compound is not so homogeneous as that of the unmixed carbonate, but its optical and physical characters are very similar.

From this experiment it would appear that the carbonate of lime, found always in combination with the phosphate (as in bone, dentine, &c.), serves to prevent the crystallization of the phosphate, and to cause its particles to coalesce and to blend with other structures—a property which this carbonate has been shown, by its action on glass, to possess in a remarkable degree—and thus to bring the earthy component of bone under the same conditions necessary for the coalescence of its particle as have been shown to exist in respect to shells. To ascertain how far this inference agrees with microscopic appearances, I examined a very low form of bone, one intermediate in some respects between true bone and shell, namely, that to which the muscle moving the claw of the crab is attached. In the very young crab this bone can be seen to be formed by the coalescence of particles of carbonate of lime, exactly like those of the shell. The next bone examined, as particularly favourable for this purpose, was that of a young frog about an inch long. In this reptile the particles of earthy matter are much larger and more distinct than in birds or mammalians. Fig. 7 is a representation of a part taken from the sternum of this frog, preserved in glycerine; *a* is a piece of cartilage with nodulated portions of earthy matter formed upon it by the coalescence of previously existing particles, *b*. These nodulated portions still further coalescing form osseous rings, which, with others of a similar form and with the cartilaginous matrix, make up the substance of the bone. This cartilage, as the deposition and coalescence advance, assumes more the character of areolar tissue. The deposit is not formed in the cells of the cartilage, but on the intercellular substance, notwithstanding it does not so follow this substance that the circular areas enclosed by the osseous rings correspond in the least either in number, form, or arrangement with the cells of the cartilage on which they are formed. The circular spaces get smaller as the ossification advances, and they appear ultimately to become the so-called bone-cells or lacunæ. This transformation, or rather process of diminution of these spaces, is by no means difficult to determine, as the same field of the microscope in a good specimen will, at one view, show all the steps of the process. These parts as thus examined, present no canaliculi, which probably are only spaces between imperfectly coalesced portions of earthy matter widened and made more apparent in the dried bone by the process of desiccation and the presence of atmospheric air; the above is only one form of commencing ossification.

Fig. 7.



That which is called membranous ossification appears in principle to be the same. The canals of Havers, large at first, become gradually contracted in their calibre by alternate layers of membrane, corresponding to cartilage in their process, and bone more or less intimately blended together. Hence the appearance of the two structures described by Tomes and De Morgan, as seen in thin transverse sections. Probably the globular dentine described by anatomists is merely the form which the dentine assumes after the coalescence of its primitive particles, corresponding in this respect with the nodulated rings, described in the recently formed bone of the frog; and the dentine canals will perhaps prove to be merely longitudinal spaces, existing in dried sections of teeth, between portions of perfectly formed dentine arranged with different degrees of obliquity around the central pulp cavity. Passages of precisely the same character exist between the analogous portions of enamel, and which are also considered by some as distinct tubes. It is possible that the coalescence does in some instances proceed so far as to produce a circular space entirely surrounded by dentine; but this is probably only an exceptional circumstance, as it does not comport with the function of tubes generally, that a system of such organs intended to convey nutritious matter should either proceed from or terminate in a mere cellular interval, like the cavitas pulpæ dentis, destined to convey bloodvessels, nerves, and their connecting areolar tissue. As respects the medium by which the earthy components of bone are brought in a fluid state to the cartilage or membrane, where the ossification is going on, there to undergo such decomposition as shall result in the formation of the coalescing compound of lime, it may be observed that in one respect the bone, or osseous tendon, above alluded to, of the crab, is circumstanced the same as shell, its membranous covering being, like that of shell, moistened with a solution of carbonate of soda; and in another the same as ordinary bone, for the part on the opposite side of this covering not being, like that in shell, accessible to the fluid medium in which the animal lives, the soluble salts of lime cannot reach it—consequently these salts must be brought into the vicinity of this membrane by some other route. Now as the circulating fluid is intended to introduce into the body the substances of which it is composed, the route for the conveyance of the salts of lime may be inferred, in this instance, to be the bloodvessels, and the medium the fluid contained within them. Hence, considering the physiological resemblance, and similarity of the anatomical relations of these structures, there is no improbability in supposing that, as the soft parts contiguous to the dense structure do in the one possess the power of eliminating an alkaline carbonate, a like power should reside also in the corresponding parts of the other; and with respect to the formation and deposition of the earthy components of both these structures, the conditions are precisely the same, the animal to which each belongs being provided with a vascular system. A more complete examination of this question would require further experiments and chemical analyses.

Having, in the preceding observations, shown, as far as experiment and induction can show, that the molecules of what are called hard tissues owe their curvilinear disposition to the direct operation of physical force, it is only a fair inference that the molecules of the less dense ones, being for certainty affected by the same physical forces, and in the same manner, should owe their curvilinear arrangement to the same cause, and that the process of coalescence above described is as applicable to soft as to hard substances, whether they be animal, vegetable, or neither of these, provided they are brought under the conditions necessary for its operation. In the development of the hooklets of the cysticercus cellulosæ, there is undoubted evidence of coalescence; and as such I have described it in a communication made to the Royal Society in June, 1855, and published in the number of the 'Transactions' for July, 1857, though without a knowledge of the principal facts mentioned in this paper. Possibly some physiologists will consider that, in the explanations of molecular coalescence as applied to organic tissues, physical influence is made to occupy too prominent, and vitality too insignificant a station; but there are no grounds for such an objection, as these explanations simply rest upon the very obvious and natural inference that vitality, when employing as her agents material substances, makes also available at the same time those physical forces by which these same substances are directly influenced; and that, in the place of opposing these forces to produce effects which it has been shown that they of themselves are perfectly adequate to, she makes them subserve her own purposes. Besides these explanations are in no way inconsistent with the analogy of nature, as manifested by the tendency which all bodies have to assume a globular form when their component molecules are affected more by the mutual attraction they have one for another than by that of remote objects. But, on the contrary, the mental tracing of the molecules of coalescing spheres according to known laws, and the actual measurement of the distances between their centres, and the exact time occupied in their coalescence, will suggest to the mathematician the elements of new curves, and furnish data for the calculation of rates of velocity, which, considering the inconceivable minuteness of the moving atoms and the inappreciable tardiness of their motion even under the highest magnifying powers, will form a marvellous contrast with the magnitude and velocity of similar atoms when collected into telescopic masses, placed at almost infinite distances, and moving with incalculable speed, although in both extremes of existence they are guided by the same principle of intelligence, and governed by the same laws.*

Art. II.

On the Symptoms of Cancer of the Stomach. By William Brinton, M.D., Fellow of the Royal College of Physicians, Lecturer on Physiology in St. Thomas's Hospital, Physician to the Royal Free Hospital.

To those researches on the 'Pathology of Cancer of the Stomach,' which have already been laid before the readers of this Review, I am now permitted to add a brief description of the symptoms that reveal the same malady in the living subject.

The symptoms of cancer of the stomach are rendered peculiarly interesting by its well-known fatality and obscurity. And it might fairly be expected that any careful clinical study of these symptoms would bring to light some new details bearing on its diagnosis and treatment.

But the following essay has more specific claims to interest. The results of recent inquiry into the gastric ulcer demand a careful revision of almost all that has hitherto been written respecting gastric cancer. For while the differential diagnosis of the two diseases obviously implies an accurate knowledge of both, such inquiry has shown, that not only have the frequency and importance of the former disease been greatly undervalued, but its symptoms and appearances so imperfectly known, as to have been often confounded with those of the much rarer malady it can so closely resemble. In this respect I may claim for the following description greater accuracy than has been attainable by most authors on cancer of the stomach. I may add, that it incorporates the result of much original observation—especially in reference to the aid to diagnosis derivable from physical examination and microscopic research—and that its general statements are deduced from cases, verified by careful necropsy, and in number far exceeding any similar series hitherto collected.

The typical course of the malady might be sketched as follows:—

An elderly person, perhaps hitherto free from dyspepsia, begins to suffer from a

* As there may be some persons desirous to see specimens of the objects alluded to above, I have put into the hands of Messrs. Smith and Beck some specimens to be had at a cost not exceeding that at which they were prepared and put up.

capricious, and soon a diminished, appetite, which is by and by associated with occasional nausea, or even vomiting, and with a sense of uneasiness or distension in the stomach. His complexion, already pale and unwholesome, next acquires a muddy yellowish, or faint greenish, hue. His gastric symptoms now increase, often by a sudden and marked augmentation, which corresponds in many other cases to the period of their first appearance. Vomiting, if already present, becomes more frequent or urgent; local uneasiness deepens into pain; and both these symptoms are excited or increased by taking food. At a somewhat later period hæmorrhage generally occurs, usually but scanty in amount, and therefore depending to a great extent on casual circumstances for its detection. About this time a tumour often becomes perceptible near the middle of the epigastric region of the belly. As the local symptoms increase, the cachexia of the patient also augments, and is evidenced, not only by the colour already mentioned, but also by debility and emaciation, and at last by prostration, which ends in anasarca, delirium, and death.

The disease, however, often diverges from this course: the above symptoms being complicated, in various stages of their progress, by results more or less incidental to the locality of the cancer—such as ascites, jaundice, perforation, fistula, phlebitis—or by the addition of the various symptoms characteristic of consecutive cancerous deposit in other organs, especially in the liver and lungs. Most of these complications influence the termination of the malady, either as intercurrent or as concurrent causes of death. And in any case the disease generally marches to its termination with a continual increase of speed and severity, rarely receiving more than a temporary check, and ending in death about one year after it first declares itself.

In pursuance of the plan adopted in a previous essay,* I propose first to discuss each of the chief symptoms enumerated above, and then to notice some questions connected with their succession, combination, and duration. In doing so, it will be convenient here and there to add some pathological considerations, which either explain these symptoms, or are suggested by them. Theory in any wider sense it is my express object to avoid; not that I consider it useless or unimportant, but rather that I doubt whether it can be advantageously entered upon in the present state of our information, even had I both time and ability for such a task.

Loss of Appetite seems, on the whole, the most variable and the vaguest among the leading symptoms of gastric cancer. Out of 235 cases in which these symptoms are recorded with tolerable completeness, anorexia (as it is usually termed) was present 91, absent 24, times. In the remaining 120 it is not alluded to. But as, in many of these latter cases, the silence respecting it is ascribable to mere omission, it may be a question whether they ought not all to be divided into two groups, having a like ratio with the above (120=92+28), and then added to them (235=(91+92=)183+(24+28=) 52). Even such an estimate, however, would represent anorexia as much less frequent than it has been observed by Lebert,† whose careful analysis of 33 cases divides them all into two correlative numbers of 28 and 5. It may therefore be stated that this symptom is present in the majority of cases; or, more exactly, in a proportion somewhere between 78 and 85 per cent. of their total numbers.

But it is not in all these cases that the anorexia forms an early and marked symptom of the malady. On the contrary, its appearance is often deferred until a comparatively late period. And its intensity is still more frequently restricted to a degree which allows little stress to be laid upon it, apart from the circumstances which attend the disease. In rare instances, too, its variations amount to absolute intermissions. And in still rarer cases, the malady influences the appetite in a precisely opposite direction, augmenting instead of diminishing it to an unnatural extent.

The independent value of this symptom as an element of diagnosis is therefore considerably less than might be supposed from its numerical frequency. Its pre-

* On the Symptoms of Ulcer of the Stomach, in this Review, No xxxv.
† Traité pratique des Maladies Cancéreuses, p. 498. Paris, 1851.

sence is of little influence in this respect, unless it is early, as well as prominent, in the history of the case; unless it precedes by some time any violent or continuous pain or vomiting; and unless it is accompanied (rather than followed by) more or less cachexia and debility. On the other hand, where it exhibits these features, it combines with the other symptoms as a valuable piece of evidence, and is especially useful in assisting the differentiation of cancer and ulcer. For in the latter of these two maladies, loss of appetite is so rare, that we may almost deny its claim to be considered a symptom at all. True, it often happens that the subject of gastric ulcer evinces a great repugnance for food. But on close inquiry, it would almost always be found that this repugnance by no means depended on anorexia. The patient dare not eat, simply because experience has taught him the suffering (pain and vomiting) to which this act (as ordinarily performed) invariably gives rise. Indeed, except in the later stages of the malady, the cravings of his appetite are (for obvious reasons) rather increased than diminished; so that, every now and then, instinct prevails over judgment, and he eats a hearty meal. In cancer, on the contrary, anorexia often seems to be a specific result of the disease. As such, it appears to be effected through the same nervous channels which ordinarily mediate the sensations of hunger and satiety, and to commence during a very early stage of the deposit in the coats of the stomach, prior to all other local symptoms. It is, on the whole, best marked in the younger subjects of the disease, and in the softer varieties of cancerous deposit.

The sex of the patient appears to exercise no influence whatever on the presence or absence of anorexia in the history of the case. Two hundred examples show the ratio between male and female subjects to be precisely similar in both these categories.

Pain, the next symptom, is on the whole more frequent and specific than anorexia.

Out of 245 cases, this symptom is recorded as having been present in 189, absent in 16; in the remaining 40 it is not mentioned. Dividing this 40 into two numbers having the same ratio as 189 and 16, we get 36 and 3, which added to 189 and 16 respectively, will afford totals of 225 and 19. Hence it would seem that pain occurs in most cases; more exactly, in a proportion between $\frac{189}{245}$ and $\frac{225}{244}$, or $77\frac{1}{7}$th and $92\frac{1}{5}$th per cent., probably nearer the latter. In round numbers, cancer of the stomach is accompanied by pain in nearly 11 out of every 12 cases.

The sex of the patient seems little to affect the painfulness of the disorder. It is true that the cases in which pain has been absent show a larger proportion (4 in 5) of males than those in which it has been present (5 in 7). But the difference ($\frac{28-25}{35} = \frac{3}{35}$ or $\frac{1}{12}$) is small. And while neither of these two proportions deviates much from the average proportion of males (2 in 3) attacked by the disease, the greater deviation of the first is explicable by the smaller (and therefore less trustworthy) number of cases (only 14) on which it is based.

There is scarcely more reason to suppose that the painfulness of gastric cancer is materially influenced by the situation of the lesion in the stomach. In about 250 cases in which pain was a prominent symptom, the deposit occupied the various parts of the organ in their ordinary numerical ratio to each other. The cancers of the cardia and great curvature seem perhaps more perfectly painful (about $\frac{1}{6}$th and $\frac{2}{6}$ths respectively) than the average. But their numbers are small and unsafe. The pyloric (81 in number) are a trifle (about $\frac{1}{20}$th) less painful than the average of all situations indifferently.

The situation to which the pain is referred affords little deduction as to the exact site of the lesion, at any rate in its earlier stages. Thus a pyloric cancer may not only cause pain referable to the right hypochondrium, or to the epigastrium, but even to the umbilicus, the sternum, or the left hypochondrium. In cardiac cancer, again, the pain may not only be local, but may be referred to either hypochondrium, or even to the right shoulder. The cancer of the lesser curvature seems, however, to be connected with a peculiar reference of the pain to the inter-scapular region, as well as with a remarkable intensity of the pain in some cases. And any marked involvement of the posterior surface of the stomach often causes a pain, which ranges from the middle of the dorsal, to the lower part of the lumbar region.

But a careful inquiry into the import of this symptom soon resolves any mere numerical statements like the above, into their more trustworthy (even though less definite) constituents. The pain of gastric cancer is evidently no single and comparable element of its history in all cases—such as is, for example, the pain present in almost all instances of gastric ulcer—but is rather a variable (or even complex) symptom, producible by several circumstances, which impart to it a very different relationship to the malady, according as they are its exclusive or predominant causes in any given instance.

The pain most characteristic of the malady is usually of a lancinating character. Beginning at a comparatively early date, it rapidly assumes a marked severity, often becoming so intense in the course of but a few days, as to cause the patient an amount of suffering that throws every other symptom into the background. Its fluctuations scarcely ever amount to more than remissions: in other words, it rarely intermits, or ceases for any length of time. Unlike the pain of gastric ulcer, it is either little affected by the ingestion of food, or, if increased by eating, does not subside at the end of the act of gastric digestion, or after vomiting,—of which act it is often quite independent. As time goes on, such pain sometimes becomes less urgent, subsides, or even, in rare instances, disappears. More frequently, however, it changes its character; merging, in greater or less degree, into some of the following varieties.

In many cases, indeed, the pain is from the first devoid of any distinctly lancinating character: and is variously described by different patients as dull, slow, gnawing, or burning; as being attended by a sense of weight, oppression, tightness, distension, in the epigastrium; or by soreness or tenderness to pressure in this region.

These varieties of pain in cancer of the stomach appear due to the local circumstances of the disease: and, indeed, may often be traced to specific causes, such as go far to explain, not only their characters, but even the way in which they sometimes complicate and succeed each other in the history of a given case. The dull burning varieties of pain seem to belong rather to ulceration of the cancerous stomach than to the cancerous deposit itself; and as such, often closely simulate the pain of gastric ulcer. Thus this pain becomes increased by food, is referred (it may be) to an epigastric and a rachidian spot, and is relieved by the expulsion of the gastric contents. At the same time, the appearance of this kind of pain sometimes seems to relieve the more specifically cancerous pain, perhaps by the mechanical relief of tension which ulceration and hæmorrhage (its constant companions) together imply. Local tenderness or soreness are also referable to ulceration, though, where excessive, they allow a presumption of that adhesive inflammation so common in both cancer and ulcer of the stomach. A sensation of weight appears but rarely to correspond with a really heavy or bulky tumour. Tightness, oppression, and distension (often connected with weight) frequently belong to that constriction of the stomach which the cancerous deposit brings about: and when best marked, have repeatedly been observed in conjunction with a maximum of stenosis, with or without dilatation of the gastric cavity behind the seat of stricture.

Hence the circumstances of these varieties of pain assign them a comparatively subordinate relation to the malady. Its various stages imply events, which themselves conditionate various abnormal sensations, such as we must, in pathological strictness, eliminate from our inquiry into the pain produced by the cancerous deposit. Subtracting these incidental pains, we are left with a symptom which, though greatly reduced in mere numerical frequency, has a much more characteristic aspect, and therefore a more direct bearing on the nature and diagnosis of the disease.

Specific pain of this kind is probably not present in more than one-half the total number of gastric cancers. And it undoubtedly varies, not only within the limits which would ascribe it a lancinating character of greater or less intensity, but even in the fact of possessing this character at all. It may be dull, aching, or even burning; may even intermit or appear late, or vanish early in the history of the case.

Many would probably consider that such facts went far to show a subjective character of the pain;—that they exceeded, rather than fell short of, the evidence which some good authorities adduce for regarding as subjective the pain of malignant tumours in regions governed by the cerebro-spinal system of nerves.

It is because I believe this view to be not only an error, but a baneful one in its influence on pathology, that I venture to show how thoroughly unjustifiable are the assumptions on which it rests. Setting aside the important distinctions between the use of the word "subjective" in the physiology of innervation, and in that logic which at once is the culture and the implement of every educated man, such a view is placed in the dilemma of being either untrue in its facts, or, if we correct these, still more faulty in its deductions.

It is the very essence of pain *not* to be objective—in other words, not to be precise or accurate in the information it gives of the properties of any object. Even when brought about by the application of a stimulus to the peripheric organization of nerve, it gives little or no intelligence respecting the stimulus itself; so that, for example, a red-hot poker, and a few drops of liquid carbonic acid, would give rise to much the same kind of pain when applied to the palm of the hand. And when the stimulus by which the pain is produced impinges on the nerve at some point intervening between its periphery and its centre, the pain becomes even more subjective, gradually losing all accurate appreciation of the locality affected, and referring (for instance) an injury of the ulnar nerve at the elbow to its distribution in the fingers.

Hence to the statement—"that the pain of malignant tumours is subjective,"—it might fairly be answered, "True: but what pain is not so? On the other hand, if by 'subjective' you mean subjective in any other sense than the pain of a wound or a burn, or if you regard the uncertain and changeable relations between the malignant lesion and the pain it produces, as showing that there cannot possibly be the same physical chain of causation as that which obtains when a nerve is involved in such injuries, then you are implying a treatment, which centuries of pathological research may perhaps hereafter gradually build up, but which is at present a mere assertion. Not until we know immeasurably more of the structure and function of nerve, and of the exact relations of the cancerous deposit to the (hitherto undetected?) terminations of the nerves among which it lies, shall we be entitled to say that the adventitious mass does not provoke pain by a process precisely similar to that which is set up by a mechanical or chemical lesion or injury. In the mean time to remove pain into the region of the 'subjective,' is to withdraw it from those very researches by which alone its claims to this position can ever be substantiated."

But to regard any such theory as merely not proven, is to take a far more favourable view than is warranted by facts. The nature of the above varieties of pain, and the general accuracy with which they correspond to the various conditions mentioned, conclusively refute it. So also does the study of the specific pain of cancer. As in malignant tumours generally, the maximum and minimum of pain in cancer of the stomach can generally be traced to something more than a merely conjectural cause. Excruciating agony has two or three times been found coincident with a definite involvement of some large branches of the pneumogastric nerve in the cancerous deposit. And conversely, the presence of but little or no pain has far more frequently been concurrent with a general hardening of the whole organ, which has converted the stomach into a rigid inflexible tube, by a scanty but diffuse deposit, such as involves scarcely any displacement of its tissues. The somewhat greater pain of the disease in young subjects appears to be connected with a greater proneness to the softer and more rapidly developed forms of cancer, and with the greater local injury* which these usually involve. Indeed, allowing for the indirectness of the connexion between the stomach and the cerebro-spinal system,† the pain produced by gastric cancer seems to be rather less irregular than that produced by malignant tumours elsewhere.

Vomiting seems to be even more frequent than pain in the history of gastric cancer. Out of 256 cases, it is recorded as present in 198, absent in 28, and is unnoticed in the remaining 30. If we divide this 30 between the two other numbers in the ratio of these to each other (7 to 1), it gives us, in round numbers, 26 and 4, affording a total of 224 and 32 cases respectively. Hence vomiting is present in a proportion between ($\frac{198}{256}=\frac{3}{4}=$) $77\frac{1}{3}$ and ($\frac{224}{256}=\frac{7}{8}=$) $87\frac{1}{2}$ per cent., probably nearest the latter.

There is no sufficient ground for deducing any valid influence of sex on the presence or absence of this symptom. Apparently it is rather more frequent in the gastric cancer of the female, in whom the proportion of 87 per cent. witnessed in the male, rises to $89\frac{2}{3}$. Or, to put the fact more strikingly, the absence of vomiting throughout the malady is nearly one-third (1·3 to 1) more frequent in the male. But the numbers concerned are too small to justify any stress being laid on so limited a difference. Indeed, even if hereafter substantiated, it will probably turn out to be chiefly connected with the collateral circumstances of this symptom—for instance, with its occasional late appearance, allowing it now and then to be (as it were) anticipated by general exhaustion.

The locality occupied by the cancer appears to affect the occurrence of vomiting in a much more marked degree; although the numbers of cases I have collected only justify a conclusion in the case of the more frequent situations of the disease. Out of the 167 instances which specify the necessary details, vomiting was present in 146, absent in 21. The probability of the complete failure or absence of the symptom may therefore be represented by the fraction $\frac{21}{167}$ or $\frac{1}{8}$th in cancer of all parts of the stomach indifferently. In the greater curvature, the similar fraction would be $\frac{1}{5}$th; in the middle of the organ, $\frac{1}{4}$th; in the posterior wall, $\frac{1}{3}$rd—numbers too small to deserve reliance. In cancer of the whole organ the fraction amounts to $\frac{3}{11}$ths, or $\frac{}{3\cdot6}$ths; in the lesser curvature to $\frac{4}{17}$ths, or $\frac{1}{4\cdot25}$th; in the cardia to $\frac{3}{20}$ths or $\frac{1}{6\cdot6}$th; and in the pylorus to $\frac{9}{107}$ths, or $\frac{1}{12}$th. Since the numbers in the three latter localities together make up $\frac{5}{6}$ths of the whole,‡ their comparison is more trustworthy. They show (what indeed has been long conjectured) that cancer of the pylorus is more frequently attended with vomiting than cancer of any other part of the stomach, and suggest that this symptom is (at least in some cases) brought about chiefly by obstruction of the gastric cavity—that is, by obstruction such as any deposit of cancer around the narrow calibre of the pylorus would be especially likely to produce.

A further study of this symptom indicates that it may be conditionated by three or four different causes; the precise share of which in the vomiting of any particular case it is sometimes impossible to estimate.

Firstly, there seems to be a variety of vomiting (just as of pain) which is at any rate so far specific or inherent to the malady, as that it is producible by a scanty deposit in the coats of the organ, and is therefore observable at a very early period in the history of the disease. The frequency of this form of vomiting cannot be determined. But I should conjecture it to be not more than ten per cent. of the total number of cases; and little (if at all) greater in cancer of the stomach than in non-malignant deposits in or on this part of the digestive tube. It seems to be connected chiefly with a local irritation of the nerves distributed to the seat of the disease; and hence to vary chiefly with the abruptness of outline of the deposit, or with the displacement it inflicts.

* Under opposite circumstances, even enormous fungoid growths may be quite painless. Compare British Medical Journal, 1857, p. 493

† A connexion, the influence of which may be roughly estimated by comparing the pain of cancers without and within the chest.

‡ The Author, on the Pathology of Cancer of the Stomach, in this Review, No. xxxvii.

Next in the history of the disease, and of greater numerical frequency, comes the vomiting produced by obstruction or stenosis. It is affected by locality, as above mentioned. And in rare instances it is relieved or removed by the ulceration and removal of the obstructive cancerous mass.

A third variety of vomiting appears to form a still larger fraction of the above total frequency of its occurrence. That process of softening and ulceration which the cancerous deposit sooner or later undergoes, first removes the mucous membrane, and then lays bare a variable extent of the subjacent tissues, thus giving rise to an abnormal irritation which rarely fails to cause vomiting. In general, the symptom, once excited, recurs more or less frequently throughout the further course of the disease, a fact sufficiently explained by the continual increase of the lesion, and the influence of habit on the act. Occasionally, however, it subsides or disappears; sometimes, it would seem, from the low vitality or irritability of the softened or ulcerous mass which comes to adjoin the inner surface of the stomach; oftener (for a similar reason) from the access of general prostration.

Lastly, there is a less frequent (but more complex) class of cases, in which this symptom is apparently due to a failure of contraction in the muscular coat of the stomach, giving rise to a virtual obstruction which itself provokes vomiting. In rare instances, this interruption to peristalsis may be due to a nervous cause, such as would justify its being termed paralysis. More frequently it is traceable to a direct interference with the muscular structure, a large extent of which is removed by ulceration, or weakened by dilatation, or starved (and even destroyed) by interstitial deposit.*

Each of the above four varieties exhibits peculiarities such as often aid to distinguish it. The first, for example, generally forms the climax of a gradual increase of anorexia and nausea; and rarely shows any marked relation, either to the quality of the food, or to the period of digestion. The second offers a close resemblance to the vomiting seen in gastric ulcer; and, like it, is provoked by food—especially by large meals, and by hot, irritating, or proteinous articles of diet. As a rule, however, it is less constant, less intense, and therefore less dangerous, than the vomiting of gastric ulcer; and is often limited to the last few weeks of the patient's life. The third generally occurs at intervals of many hours—indeed, is usually a large (two pints to six pints) dark yeasty-looking vomit, the accumulation of many meals, and contains *sarcinæ*, *torulæ*, or both; while it is of course accompanied with the physical signs of dilatation of the stomach. The fourth can only be said to afford direct and independent evidence of its presence in those rare but authentic instances in which the patient has felt that the contracted stomach could neither receive more than a small quantity of food (all in excess of this amount being instantly vomited), nor pass onwards into the duodenum any appreciable fraction of it.

Hæmorrhage is a less frequent symptom than either of the preceding. Out of 244 cases of gastric cancer, a variable amount of hæmorrhage appears to have been present in about 103 instances—a number equivalent to a proportion of ($\frac{103}{244}$ or $\frac{3}{7}$) nearly $42\frac{1}{4}$ per cent.

In 18 of these instances, the hæmorrhage was sufficiently copious to be recognized as tolerably pure blood. The remaining 83 probably include a great many cases in which the blood had been more or less mixed with gastric juice, food, bile, or softened cancerous substance; and had undergone a variable change of colour, such as would impart to it a blackish, brownish, or "coffee-grounds" appearance.

But the frequency of hæmorrhage is doubtless much greater than the above statements suggest. Indeed, the pathology of cancer in general, together with the history of this particular malady, will quite entitle us to suppose that hæmorrhage in some degree rarely fails to occur, save in those instances in which the patient dies before the ulceration or softening of the deposit. But though, in all

* It is true that the evidence afforded by cases of gastric cancer rarely amounts to more than a presumption that the vomiting present is chiefly or exclusively caused by one or other of these forms of interference with peristalsis. But it must be remembered that the efficiency of every one of them is proved by its constituting the sole cause of his symptom in some other disease of the stomach.

other cases its occurrence is necessary or inevitable, its detection is contingent, being determined by a variety of collateral or even remote circumstances,—such as the quantity of blood poured out, the rapidity of its effusion, the casual coincidence of vomiting or purging, or the amount of attention habitually bestowed by the physician or patient upon the substances thus expelled. And hence there can be little doubt, both that a sedulous inquiry, aided if necessary by the microscope, would reveal this symptom in many cases in which it would otherwise remain unnoticed, and that the above numerical statement greatly underrates its true frequency.*

In respect to their nature or origin, such hæmorrhages may be divided into three varieties. First in the history of the malady comes the hæmorrhage of congestion, which often occurs at an early stage of the deposit, and which, though usually moderate in amount, is in rare instances so copious as to cause death. In these fatal cases, the accuracy of the term congestion is in some degree proved by the fact that a careful examination of the stomach, aided by injection of its vessels, conclusively shows that the hæmorrhage has proceeded from the minute vessels of the sub-mucous plexuses, or from the capillaries of the free gastric surface, and not from any of the trunks or branches of the larger arteries or veins. In one or two instances, the relation of the tumour to this congestion has been still more definitely indicated by a varicose enlargement of some of the small veins in its neighbourhood. Whether the passive or obstructive congestion which such a state suggests, is more or less frequent than an active congestion or afflux to the adventitious growth; and whether the latter state, in so far as it occurs, may not sometimes diminish, rather than aggravate, the former, are questions which cannot at present be answered. Thus much we may conclude respecting this variety of hæmorrhage, that while its very occurrence in gastric ulcer is in some degree hypothetical,† it appears to be a definite and not very infrequent form of bleeding in gastric cancer, and seems to be‡ often produced by a passive or physical engorgement of the vessels, itself the result of a mechanical obstruction brought about by the deposit.

The second variety of hæmorrhage is the result of the various lesions which the processes of ulceration and softening induce in the vessels of the cancerous mass. In conformity with this source, it generally occurs at a comparatively late period of the malady, and is by far the most frequent cause of bleeding witnessed in cancer of the stomach. The degeneration and destruction by which it is produced are often complicated by a process of fungation or out-growth, which increases the amount of blood generally effused by such hæmorrhage. As a rule, there can be no doubt that its amount is generally scanty—often sufficiently so to escape all detection. In rare instances, however, it is excessive, and from the circumstances that attend it, is always important (often even fatal) no matter how moderate its quantity.

The third variety of hæmorrhage is that produced by erosion of the larger vessels external to the stomach. Its production and comparative infrequency have already§ been alluded to. Unlike the two preceding varieties, hæmorrhage of this kind is probably but seldom overlooked. Hence, even in assuming that our figures are correct, and that it does not cause hæmorrhage in more than one out of 100 cases of gastric cancer, or in more than one of the 40 of this 100 which evince such a symptom, it is probable that we overstate its relative frequency. From obvious reasons, it generally occurs late in the disease, as measured both by the duration of time and by the succession of symptoms.

The characters of the blood expelled from the stomach of course vary with all those circumstances which affect the appearance of this fluid in the analogous

* In respect to some of these details, the hæmorrhage of gastric ulcer and gastric cancer have so much in common that I may avoid a good deal of repetition by referring the reader to my previous essay (On the Symptoms of Ulcer of the Stomach: British and Foreign Medico-Chirurgical Review, No. xxxv).

† Compare the author's Essay, loc. cit.

‡ Future observations can alone show whether villous cancer affecting the stomach is attended with that peculiar disposition to hæmorrhage of this kind which it seems to exhibit when occupying other mucous membranes.

§ On the Pathology of Cancer of the Stomach, in this Review, No. xxxviii, hæmorrhage that attends gastric ulcer. According as it is poured out by an artery or a vein; in gushes or by a slow oozing; in large or small quantity; and is expelled at once or only after undergoing more or less admixture and digestion—the blood detected in the patient's stool or vomit may be liquid or clotted; florid, dark, or black; pure, or so changed and combined with other contents of the alimentary canal as almost to defy recognition.

The relative frequency of its two means of exit I deduce from 49 cases, which specify this point; of these 49, in 31 blood was vomited; in 12, vomited and passed in the stools; in 6, found in the stools only. In 2 of these six cases, vomiting was absent; in 2 more the lesion, situated at the cardia, gave rise to an embarrassment of deglutition, which could scarcely be regarded as vomiting, and was very unlikely to return any quantity of blood from this part.

It is scarcely necessary to say that the so-called "coffee-grounds" vomiting, which was formerly supposed to be almost pathognomonic of cancer of the stomach, has really no such diagnostic value. It is shared by the gastric* ulcer. And its greater frequency in cancer may be attributed rather to the moderate amount in which hæmorrhage usually occurs in this latter disease, than to any more specific or significant fact. At any rate, this circumstance, and the comparatively greater frequency of vomiting on an empty stomach in cancer than in ulcer, seem quite to explain the disproportion observed.

In close connexion with hæmorrhage is another symptom, till now almost overlooked—namely, the detection of the characteristic cancerous cell-growth in the substances expelled from the stomach.

For obvious reasons, this sign is rarely to be found, except at such a date of the malady as would generally imply the presence of most, if not all of the preceding symptoms. Indeed, depending as it does rather upon an exfoliation or sloughing, than upon any mere slow dissolution of the cancerous mass, it is intimately associated with such a generation or decay of its substance as we have already seen can rarely occur without more or less hæmorrhage. And although it is, theoretically speaking, quite possible that sedulous research might, in rare cases, detect cancerous matter in the stools of a patient, practically it may, I think, be laid down, that there is scarcely any reasonable chance of its discovery save in those cases of gastric cancer where vomiting is not only present, but occurs upon an empty stomach, and with tolerable frequency.

The ordinary difficulties in the diagnosis of cancer under the microscope I need scarcely allude to, although these difficulties, always neither few nor small, are increased by the circumstances of its deposit in the stomach, especially by its form (almost always medullary at its exfoliating surface), and by the solvent effects of the gastric juice. For however the cancerous nature of such substances may be disguised by the equivocal size and shape of their cells, and by their separation and dissolution in the stomach, its recognition is practically much more impeded by the large and numerous impurities admixed with them here. The addition of blood, pus, bile, saliva, gastric juice, and the various and complex admixture of particles entering into the composition of the simplest foods and drinks, will often successfully oppose the most careful and repeated examination of these egesta.

The only practical rules I can offer, are the following: The fluids selected for examination should be such as are vomited tolerably free from any admixture of food. If much blood or bile be present, they should be diluted carefully by the addition of a moderate quantity of water. Thus attenuated, they should be searched (by decanting off the clear fluid) for any flocculi or larger masses they may contain. If the examination of these be at all delayed, syrup will form the best means of preserving them. To the practised eye, there is little danger of confounding a mass of cancer, even when already half dissolved, with shreds of œsophageal or gastric epithelium. But mere isolated cells or nuclei scarcely justify a decision.

* Melanosis is so extremely rare as a cause of this colour, that the diagnosis of its nature is identical in the two diseases. Compare op. cit.

On the whole, it is evident that this symptom, rarely visible until little doubt is left, often defying all detection, often yielding but equivocal evidence, can only now and then be of much value. And its usefulness is solely positive. In other words, its absence proves nothing. In both these respects, it stands in marked contrast with the analogous evidence afforded by the microscope in some other diseases, especially (for example) in suppurating pulmonary tubercle, where the distinctness of the tissue detected, and the comparative constancy of its presence, render it a ready and unmistakeable piece of positive evidence, and allow it (with proper precautions) some value as a negative.

The detection of a *tumour* constitutes a symptom, the precise frequency of which it is impossible to estimate.

Out of 237 cases which detail the leading symptoms, 128 specify the presence, 31 the absence of a tumour; in 78, neither presence nor absence is mentioned. If we divide these 78 into round numbers, having the ratio of 128 to 31, and add the 63 and 15 thus obtained to the original 128 and 31, we get 191 and 46. These numbers nearly correspond to the presence of a tumour in 2 out of 5 cases, or in 80 (80·59) per cent. On the other hand, if we assume that in all the cases in which this symptom is not mentioned, it was really absent, we should have a proportion of 128 in 237—about 8 in 15, or 54 per cent. only.

It might seem at first sight that the truth would necessarily lie about midway between these two extremes. In other words, it is fair to infer that many of the 78 were instances in which, after careful examination, no tumour could be detected, and hence that the real or hypothetical fractions of $\frac{128}{159}$ and $\frac{191}{237}$ respectively, or their corresponding proportion of 80 per cent., would be rather too high. It must, however, be recollected that the presence of a tumour often does not constitute a symptom in the ordinary sense, but rather a sign, which advances no active claims to the attention of either patient or physician, and is only discoverable by a careful search specially directed thereto. Indeed, it is notorious that the tactile and manipulative skill of different persons differs so much, that one detects what another cannot recognise, even after his attention has repeatedly been called to it. We may therefore suspect that even the 31 instances in which no tumour was detected include cases in which a more sedulous, or frequent, or skilful examination would have revealed it; and that, in many of the 78, no special inquiry had been directed to its presence. The observations of Lebert afford results quite in accordance with such a suspicion. He brings forward 36 cases, in 2 of which the tumour had been obscured by ascites, and in 30 of which (or about 80 per cent.) there was either dulness or swelling in the region of the diseased organ, leaving only 4 cases in which the symptom could be regarded as altogether absent. Hence an accurate and skilful examination would probably show that the estimate of 80 per cent. scarcely exaggerates the frequency with which this symptom is present in gastric cancer.

The sex of the patient does not seem to influence the frequency or distinctness of this symptom. Out of 96 cases in which it was present, 63 were males, 33 females—a proportion of the two sexes offering no perceptible difference from that (160* to 73) of the group of cases to which these 96 belonged.

The form of cancer scarcely seems to exert any more perceptible influence on the presence of a tumour. Out of 92 instances which specify the variety of cancer present, in 51 it was scirrhous, in 12 colloid, in 16 medullary, in 11 medullary complicating what was originally scirrhus, and in 2 colloid similarly deposited around scirrhus. Grouping the two latter numbers under the variety to which they originally (and essentially) belong, we obtain 64, 12, and 16 for scirrhous, colloid, and medullary respectively—numbers which, when compared with the proportionate share of these varieties (130, 17, and 32†) in gastric cancer generally, afford no contrast deserving notice. The slight excess of colloid (12 instead of 8½) is quite explicable by the small numbers concerned, and the vague use‡ often made

* Op. cit. p. 216.   † Op. cit. p. 221.   ‡ Op. cit. p. 226.

of this term. From obvious reasons, the tumour produced by scirrhus generally appears at a comparatively late period in the history of the malady.

The situation of the cancer in the stomach seems to exercise the influence we might expect on the appearance of a tumour. Whether we calculate this influence directly (by comparing the cases of its presence and absence) or indirectly (by comparing the former only with the general frequency of the symptom as deduced above), we obtain results which may be briefly summed up as follows. The presence of a tumour is favoured by the disease being situated in the middle of the stomach in its greater curvature, in the whole organ, and in the pylorus, its average frequency (assumed, for instance, to be 100) being increased to 300, 220, 160, 120, for these four situations respectively. On the other hand, it is opposed by the cancer being situated at the lesser curvature, the cardia,* and the posterior surface, the same average being respectively diminished to 95, 44, and something considerably less than 25. These facts are obviously explained by the relative proximity of the corresponding parts of the stomach to the yielding anterior wall of the belly, by contact with which alone any ordinary cancerous tumour of the organ is likely to be detected.

The situation of the tumour in the belly is variable to a degree which is but partially explained by any merely anatomical considerations. As might be expected, it is almost limited to the epigastric, umbilical, and hypochondriac regions; where it forms a projection of variable size, shape, and smoothness, ranging from a large, hard, irregular, nodulated mass, forming a projection which it is impossible to overlook, to a small, deep, elastic, and resisting substance, such as is often exceedingly difficult to recognize. Tumours of the great curvature are apt to project near the umbilicus; cancer of the whole stomach projects chiefly into the epigastrium, the upper part of which region (from equally obvious reasons) generally shows the swelling of a cancer situated in the lesser curvature. But the variableness in the situation of the tumour is best seen in the pyloric cases, which form so large a majority of the whole. In most of these, the swelling occupies the neighbourhood of the median line. Where it transgresses this rule, it selects the right hypochondrium more frequently than the left,—a preference which is referrible not only to the normal situation of the pylorus, but also to the frequency of adhesion between the diseased segment of the stomach and the contiguous liver. The vertical level of these pyloric tumours appears to be affected by the sex of the patient in an unexpected (though by no means inexplicable) degree. The horizontal line which separates the epigastric from the umbilical regions divides the locality usually occupied by such swellings into two pretty equal portions. Of these the upper (including the epigastric and hypochondriac regions) is, in the male, the seat of two-thirds of these tumours; the lower of one-third. The female subject of cancerous pylorus exactly reverses these proportions, two out of every three tumours occupying the umbilical regions. This curious contrast may be ascribed, partly to the naturally narrower limits of the female epigastrium, partly (and much more) to the constriction exercised by stays, two circumstances which concur to force the liver and stomach, together with any tumours which may be attached to them, to a lower level in the belly than they would otherwise occupy. The same circumstances appear to be traceable in the fact, that mobility of the tumour is much more frequent in the female—at least, mobility in such a degree as can allow the tumour to vary the region it occupies.

The detection of a tumour is sometimes opposed by great difficulties. The resistance offered by the supposed mass now and then leaves us in doubt, merging (as it does by various gradations) into a slight fulness to palpation, or dulness to percussion. Again, the tumour is often in absolute contact with the liver, and hence, if of moderate dimensions, may be so completely covered by this organ as to have its physical signs fused into those of the lobe of liver by which it is over-

* We may distinguish the effect of these situations by alluding to the circumstances through which they oppose the appearance of a tumour as including at least three elements—(*a*) remoteness from the surface, as in all these sites; (*b*) influence on perforation, and incidental death, as in the posterior one; (*c*) specific tendency to grow in another direction, as in the cardiac cancer, which generally shoots up the œsophagus.

lapped. Nay, more, a large proportion of these tumours adhere to the liver; while a still larger proportion are associated with a secondary cancerous deposit in the hepatic substance, such as often defies the greatest skill to distinguish from the swelling produced by the gastric cancer apposed to it. The adhesion and inflammation around an ordinary gastric ulcer can also give rise to a perceptible tumour, such as it is sometimes impossible to avoid suspecting as cancerous. Further, the tumour produced by a gastric cancer may be temporarily hidden by ascites, as well as by changes in the size and situation of the neighbouring intestines. And lastly, just as the cancerous deposit is often too scanty, and its situation too deep, to permit the appearance of this symptom at all, so the tumour produced by a cancerous stomach can* sometimes be closely imitated by cancer of the neighbouring mesentery or intestine. The gastric distension brought about by stenosis and dilatation behind it can, however, almost always be distinguished from a cancerous tumour. Even when (as is not uncommon) the two states are combined, the presence and degree of dilatation may be decided by a physical examination; the results of which, again, can be checked or verified by the characters of the vomiting present in such cases.

The above variations are well illustrated by the fluctuations in distinctness which such tumours sometimes exhibit in the course of a single case. The swelling distinguishable one day has disappeared on the next, and re-appears, it may be, a few days later. Occasionally, its subsidence is permanent: a fact which, if unexplained by any of the circumstances above mentioned, points to a loss of substance in the diseased mass, by softening, ulceration, or sloughing.

About eight per cent. of these tumours are pulsating—in other words, are so placed as to receive and transmit the impulse of the aorta. Of eight such pulsating tumours collected by me, all were pyloric, and the subjects of seven were of the male sex. Hence it would seem that in rather more than one out of every eight cases in which a cancer of the pylorus is betrayed by a tumour, that tumour pulsates, and that in the male sex this proportion is increased to one in six.

Mobility of the cancerous tumour seems to be rare, in that marked degree which permits it to occupy different abdominal regions. The three or four cases I have collected are all pyloric, female, and (at least originally) scirrhous.

Tenderness of the tumour is generally present in some slight degree. It is marked in about half the cases, but it is rarely great, save where the peritoneum is involved by the original or secondary deposit.

The *cachectic appearance* upon which so much stress is deservedly laid in the diagnosis of cancer, is scarcely a single symptom, but rather a congeries of symptoms, some of the slighter and less direct of which can neither be satisfactorily grouped with it, nor separated from it.

In respect to its frequency, a collection of 200 cases shows exactly 150, or 3 cases in 4, to have exhibited it in so marked a degree as to attract notice, and receive a due record in the history. This proportion is exactly alike for both sexes. An absence of this appearance is specifically mentioned in 5 of the above 200: 4 males and 1 female. Hence it is present in from 75 to 97½ per cent. of gastric cancers, probably in not less than 90 per cent.

As elements of such a cachectic aspect, we may fairly enumerate, not only the colour of the skin of the face and body, but its loss of moisture and elasticity, the diminution of the subcutaneous fat, the flabbiness (if I may use such a word) of the areolar tissue, the softness and smallness of the muscles. The latter symptoms are closely connected with emaciation and debility, and often with physical signs such as evince kindred characters of the pulse and heart.

Of all these, however, the altered facial colour is the most obvious and important, as well as the most constant. The more characteristic variety of this change is describable as imparting to the face a kind of muddy greenish pallor, or pale earthy hue, such as can scarcely be mistaken for any other colour. The chief

* As in a case of the author's, brought before the Pathological Society. Medical Times, 1857, vol. xiv. p. 202.

extremes (if we may so term them) of this symptomatic colour are jaundice on the one hand, and an anæmic whiteness on the other.

Jaundice appears to have occurred in about 14 of 260 cases, a proportion rather less than 5½ per cent. It may be conjectured that in about one half of these cases the jaundice was simply due to the hepatic disorganization produced by consecutive cancer of the liver.

The remainder were probably quite independent of any disease of this organ. The coloration of such jaundice appears to merge, by numerous gradations in different cases, into the ordinary hue of the cancerous cachexia. And (if we except those rare cases in which its symptoms and the date of its appearance refer it to a cause analogous to that of the jaundice which sometimes comes on in the later stages of fatal cardiac disease) it is difficult to avoid regarding it as little more than a variety of this cachexia. Such a view is especially confirmed by one or two important features it generally offers. The icteroid stain is rarely intense, indeed the conjunctiva is often but moderately yellow. The stools, again, however offensive, have neither the white colour nor the putrefying odor characteristic of true jaundice. And the skin and urine are rarely loaded with bile in anything like the usual degree.*

Pallor of skin is just as evidently a variety of the above colour in some cases, a complication of it in others. In other words, it often seems traceable to no other cause than the cachexia which it accompanies; oftener, however, it is due to some of those incidents of the malady which, occurring in other diseases, would bring about a similar result. Thus hemorrhage, suppuration, frequent vomiting, scanty ingestion of food, long confinement, and a host of circumstances (among which are recorded such rare contingencies as concurrent albuminuria) may all complicate and increase that pallor which is an element of the peculiar facial hue characteristic of gastric cancer.

With allusions equally hasty and imperfect, we must pass by the interesting subject of inquiry which the nature of this cachexia might afford us. Enough to say that, of all the provisional theories which suggest themselves, that which regards the original or pathognomonic cachexia as the result of the humoral disease which precedes and brings about the cancerous deposit, is, on the whole, the safest, as well as the most useful. According to this vague theory, there is a cachexia which, in many cases, can be verified prior to any of those local changes that afterwards rudely measure and express it. But besides this specific cachexia (the nature and date of which render it so important for pathology and diagnosis), there are a variety of conditions which closely resemble it, but bear to the total malady a relation of coincidence rather than of identity, or of effect rather than of cause. Often as these latter complicate and obscure the true cancerous cachexia, pathological accuracy requires their separation in the history of the malady, nay more, so far as practicable, in any given instance. Indeed it would be difficult to find a better illustration of the practical value of such accuracy than we may see in many cases of this malady. Contrary to what has been generally stated by authors, the cachectic aspect witnessed in gastric cancer is often imitated by mere ulcer of the stomach, sometimes so closely as really to defy distinction. But in all these cases the resemblance, however close, is easily explained. The circumstances of both diseases are such as to involve, in a large proportion of the more fully developed cases, a certain amount of cachexia—the joint product of ulceration, hæmorrhage, vomiting, pain, starvation. While in other instances a knowledge of this fact renders the symptom a valuable aid to diagnosis. Wherever cachexia precedes these circumstances, or is present in a degree utterly disproportionate to what their aggregate influence might lead us to expect, there it acquires the rank (*pro tanto*) of a leading (and almost pathognomonic) symptom.

Febrile symptoms seem to accompany gastric cancer much more frequently than

* A further discussion of this interesting point would lead me too far from my present subject. It may suffice to add, that without wishing to deny the analogy of this coloration to certain forms of jaundice, I am anxious to insist on its connection with the cancerous cachexia, and its distinctness from the commoner varieties of icterus.

is generally supposed. But I am not in a position to make any exact statement respecting the numerical frequency of their presence. As a rule, these symptoms rarely amount to definite hectic, but are generally limited to what is termed irritative or symptomatic fever. The tongue, often covered with a thickish white fur, which is especially distinct posteriorly, gleams through this covering, or appears at its edge of a deep red "raw" colour. The face is not unfrequently marked by injected red patches on the centre of each cheek, contrasting strongly with the pallid cachectic hue of the surrounding integuments. The urine is still more frequently of scanty amount and high specific gravity, loaded with urea and urates. The various forms of uric acid deposit are also anything but uncommon; indeed it is perhaps in this malady that we may find the best illustrations of that peculiar pink variety (or rather complication) of uric acid deposit, which has long been regarded as characteristic of constitutional malignant disease.

The import of this febrile reaction is probably very different in different cases. Ulceration, inflammation, peritonitis, suggest themselves as explanations, at least as frequently as the primary malady, or even the exhaustion it directly or indirectly produces. On the whole, however, the latter state seems to me the best clue to the nature of these symptoms in the majority of cases to which they are present. Especially is this explanation applicable where none of the above local complications appear to be prominent. In such instances the febrile reaction often seems strictly analogous to the feverish condition producible by starvation, exposure, or over-exertion, in persons otherwise healthy. And in consonance with this view, I think it will generally be found that its presence is a rough test of the progress of the malady—at any rate of its effect upon the constitution; and is therefore not only one element of a very unfavourable prognosis, but an indication for as much nourishment and support as the local circumstances of the malady will allow.

The *state of the bowels* in gastric cancer is another point on which I have no exact numerical details to offer. In most instances, however, either constipation or diarrhœa is specified as having been present during a considerable period of the malady. And in many the two states alternate with each other; constipation in the earlier months of the history being succeeded by diarrhœa towards the last few weeks of life.

Constipation, the more frequent of the two, appears to be due to the circumstances of the malady rather than to the malady itself, and in this respect is comparable with the constipation which so frequently accompanies gastric ulcer. Obstruction, vomiting, and pain, which prevent alike the distension and the movements of the intestinal canal, concur to form a ready explanation of the frequency and intensity of the symptom, which sometimes persists up to the last hour of life.

Diarrhœa seems mainly attributable to the direct irritation caused by the cancerous, purulent, or hæmorrhagic effusion which the gastric lesion introduces into the intestines. The noxious influences of these fluids are often aided by their decomposition or incipient putrefaction in their passage through the bowels. Ulceration of the cæcum, and of the neighbouring ileum and colon, is comparatively so rare, as to afford but a very exceptional explanation. The date of the occurrence of diarrhœa is generally consonant with the above view of its production; it is subsequent to ulceration, in other words is comparatively late in the history of the malady. It thus coincides with a period at which the patient's strength is generally so far exhausted as to be very susceptible of any further depressing influence. Hence it often indicates imminent danger, and is not unfrequently the proximate cause of death.

The symptoms which more specifically betray the approach of death, offer little that is peculiar to this disease. Anasarca is by no means uncommon, and is usually accompanied by ascites. A disproportionate effusion into the belly points rather to some local cause of the dropsy, such as pressure of the cancerous tumour on the *vena portæ*, and is hence less significant for prognosis. Jaundice, as before mentioned, appears sometimes to be a precursor of death, as is also a form of pulmonary effusion which I believe to be a mere passive congestion. Hiccough, again, is usually due to local causes acting on the diaphragm. The nervous phenomena that usher in the close of the malady are delirium, followed by coma.

The *duration* of the aggregate malady formed by the succession of all these symptoms may be calculated, from 198 cases, as averaging about $12\frac{1}{2}$ months. This average seems to differ little in the two sexes. Out of 142 cases, 107 male and 35 female, the average in the male was $12\frac{1}{3}$, in the female $11\frac{2}{7}$ months. This slight difference is rendered even less important by the smaller number of females referred to, especially in conjunction with the fact, that the deviations from the average are far more striking and less trustworthy, in the case of the longer durations than of the shorter. Indeed, as regards the longest or most chronic instances, it is curious to notice how few gradations exist between protraction to the close of the second year, and the one or two rare examples of a duration of six or seven years. Hence these extremely protracted cases are not improbably examples of dyspepsia ending in cancer, an opinion which may also be directly deduced from their symptoms.

Practically, then, we may deduce that the maximum duration of gastric cancer amounts to a period of about thirty-six months from the first appearance of the symptoms, and that very few cases survive the twenty-fourth month. The minimum is about one month. As might be inferred from the histological details before alluded to,* many of the most protracted cases are shown by necropsy to have a structure often designated "colloid" and "encephaloid," through originally scirrhus. The converse exception is much rarer, a large majority of the cancers rapidly fatal really belonging to these two intense forms of the malady.

But in noticing these facts, we must be careful not to overrate their significance. The commencement of the symptoms often has little connexion with the commencement of the disease. Pathologically, of course, it is not only absolutely impossible to fix the date of the deposit in any given instance, but probable that its material origin may be followed by all those varieties in the date of subsequent growth, which we can verify in external cancers. And even that aggregate of symptoms which, for diagnostic purposes, we may make the representative of the disease, must often give a very distorted view of the real duration of the case. Slight anorexia, cachexia, or emaciation would often elude the notice of the patient; or, perhaps, would rarely (in hospital practice) cause him to seek professional advice, such as might sometimes detect and distinguish the latent cause of these earlier symptoms. Hence, in most of the cases on which the above estimate is founded, the symptoms from which the commencement of the malady has been dated are those of active gastric disturbance in the shape of pain and vomiting.

This brings us to a point on which I am anxious to be explicit, in order not only to compensate the probable basis of the above estimate, but also to explain the deceptive resemblance between cancer and ulcer of the stomach—a resemblance which it is often of vital importance for the physician to unmask. In many cases of gastric cancer, the symptoms only date from the commencement of ulceration in the cancerous deposit: from a time, that is, when the incurable and malignant disease offers a close analogy, in many of its circumstances, to the curable ulcer; and an analogy which necessarily brings about the closest similarity in many of the local symptoms. As a rule, it seems that, whether we explain their access as due to this cause, or view them as independent of it, the more specific and frequent varieties of vomiting and pain, as well as the occurrence of hæmorrhage, begin at a period about midway in the history of the best marked cases. Hence, we may conjecture (1st) that the cancerous deposit itself generally precedes the symptoms by a period of at least some months; and (2nd) that the above estimate of the total duration of the symptoms will probably receive some extension from further clinical researches.

It may be interesting to combine the estimates we have come to respecting the frequency of the above symptoms separately, in order to determine their bearing on the diagnosis of the malady, or on the probabilities of any given case. Of the

* British and Foreign Medico-Chirurgical Review, No. xxxvii.

two estimates we have generally adduced for each symptom, we had better keep to the larger, both as a more direct and positive one, and as seeming to balance most exactly the two kinds of error—excess and deficiency—to which all such calculations are liable :—

	Anorexia.	Vomiting.	Pain.	Hæmorrhage.	Tumour.	Cachexia.
Present	91 ...	193 ...	189 ...	81 ...	128 ...	3 times.
In . . .	115 ...	221 ...	205 ...	202 ...	159 ...	4 cases.

Applying to these figures the simpler laws of probabilities, brings out the following statements.

The probabilities or "odds" are eleven to two against all six symptoms being simultaneously present in any given case. In other words, one or other of the six is absent in more than five out of six examples of the malady.

Supposing the least frequent—hæmorrhage—to be absent, the odds are still as many as eight to five against the concurrence of the remaining five.

It is only the absence of a second symptom—for example, cachexia—that equalizes the chances, so as to render exactly even the probabilities for and against the concurrence of the remaining four—pain, vomiting, anorexia, and tumour. In other words, the simultaneous presence of four of the above symptoms is about the average amount of evidence hitherto detected in cases of gastric cancer. The numerical frequency of anorexia and cachexia (70 to 75) is so nearly alike, that it scarcely matters which we include in this four; save that, on the whole, the presence or absence of cachexia is less mistakable than that of anorexia. Hence supposing, in any given instance, we find pain, vomiting, tumour, and cachexia are all present, we may feel that the diagnosis, even if uncertain, is as little so as, in the majority of cases, it is likely to be.

The chances of concurrence would of course increase as the number of symptoms decreased. Thus it is 23 to 12 that pain, vomiting, and tumour are all present; 52½ to 12 that pain is associated with vomiting.

A more practical hint is derived from an inquiry into the probabilities of a concurrent failure or absence of symptoms. Thus the odds are 2 to 1 against the simultaneous absence of hæmorrhage and cachexia; 2½ to 1 against the failure of hæmorrhage and anorexia; 40 to 1 against the absence of tumour and vomiting; 60 to 1 against the absence of vomiting and pain; 65 to 1 against the absence of tumour and pain; 225 to 1 against the absence of the more obvious and important symptoms of pain, vomiting, hæmorrhage, and tumour; and lastly, about 2560 to 1 against the absence of all the six symptoms tabulated above.

In alluding to such calculations, however, we must remember how large a variation of probabilities would often be brought about by a slight alteration in the original estimates. Supposing, for example, that we adopted the lower of the two estimates mentioned for most of the above symptoms, on the assumption that they were absent in every case in which they were not mentioned, the difference, we have seen, would not amount to much for each symptom separately. But its results would of course diverge far more widely; so that, for example, the odds would be about 55 to 1 (instead of 11 to 21) against all six symptoms concurring in any single case. The uncertainty which attaches to all calculations founded on limited numbers of cases may seem still more obvious. But in even as few as 200, this element of contingent error becomes comparatively small. Indeed the actual collation of these 200 cases singularly confirms the above calculations.

A similar, though less exact, check appears to be afforded by observation with respect to the general results of the preceding inquiry—which suggests, I think, rather a more secure diagnosis than many excellent authorities have hitherto thought practicable in this disease. This suggestion has certainly been confirmed by my own observations. In other words, I have rarely been unable to diagnose a cancer of the stomach, and have generally found that where its presence seemed long doubtful, there it was, in reality, absent.

But in whatever degree the facts above collected may aid diagnosis, by establishing conclusions such as no scantier series of observations could have justified, still it is right to add, that grounds for a definite opinion in any single case will sometimes be wanting. As already pointed out, in the phenomena of gastric cancer, time is everything. Hence the physician who examines a chronic case or (what is practically the same thing) sees the malady only in its early stage, must often remain in complete uncertainty respecting its nature, until the progress of the disease, increasing (in something like a geometrical progression) the number and distinctness of its symptoms, enables him to come to a decision. Indeed, it is hardly too much to say, that this peculiar mode of accretion of the symptoms may sometimes form a specific aid to our recognition of the disease.

My limits forbid any allusion to the diagnosis of the various complications of gastric cancer. As pathological incidents or events of the malady, they have already been mentioned elsewhere. Their symptoms are, on the whole, less distinct than those of the ordinary diseases which affect the organs they respectively occupy. Indeed, the late period of the gastric malady during which they usually occur, brings them into a coincidence with a degree of prostration such as obscures these symptoms, alike in respect to their own prominence, and to their reaction on the organism generally.

Art. III.

Annual Report of Cases admitted into the Medical Wards of St. George's Hospital during the Year ending December 31*st*, 1856. By G. Goddard Rogers, M.D., Medical Registrar.

Cases admitted during the year 1856.						Admitted during six years.	
Nature of Disease.	Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
1. Fevers:							
Continued fever	164	18	10·9	50	11	911	11·3
Remittent fever	..	..	..	..	..	1	?
Influenza	15	..	..	1	..	76	..
Asiatic cholera	1	..	..	..	..	141	46·3
2. Eruptive fevers:							
Measles	8	..	..	3	..	27	..
Scarlatina	13	..	..	7	..	79	12·6
Varioloid	1	..	..	..	..	17	..
Erysipelas	24	1	4·16	7	1	148	14·1
3. Intermittent fevers:							
Quotidian	6	..	..	..	..	28	..
Tertian	12	..	..	1	..	49	..
Quartan	1	..	..	..	..	6	..
Irregular	5	1	20·0	2	..	15	6·66
4. Rheumatism:							
Acute	101	5	3·96	38	4	433	3·92
Subacute and slight	94	..	..	21	..	608	?
Chronic	132	..	..	23	..	815	?
5. Gout	7	..	..	1	..	181	5·52
6. Rheumatic gout	33	3	?	6	2		
7. Poisoning:							
Irritant	1	..	..	..	..	11	18·1
Narcotic	8	..	..	..	..	15	6·66
Gaseous	..	..	..	..	..	2	..

Cases admitted during the year 1856.						Admitted during six years.	
Nature of Disease.	Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
7. Poisoning (continued):							
Syphilitic	5	..	..	3	..	56	..
Gonorrhœal	4	..	..	2	..		
Hydrophobia	..	..	..	..	..	..	..
8. Colica pictonum	3	..	..	1	..	40	..
9. Entozoa:							
Intestinal worms	7	..	..	2	...	29	?
Echinococcus hominis	..	..	..	..	..	2	100·0
10. Dropsy:							
Anasarca	108	35	32·4	102	35	618	34·7
Ascites	20	10	50·0	15	10	104	51·9
11. Hæmorrhages:							
Epistaxis	6	..	..	4	..	25	..
Hæmoptysis	13	3	23·07	11	3	110	21·9
Hæmatemesis	13	..	..	6	..	43	9·30
Hæmaturia	5	2	40·0	4	2	41	14·6
Intestinal	5	2	40·0	3	2	44	20·4
Uterine	14	2	14·2	8	2	49	4·08
12. Purpura	7	..	..	5	..	32	25·0
13. Scurvy	1	..	..	..	..	7	..
14. Anæmia	52	..	..	19	..	380	2·10
15. Chlorosis	14	..	..	4	..	112	..
16. Cachæmia	4	3	75·0	4	3	45	44·4
17. Tubercular diseases:							
Scrofula	13	2	15·3	8	1	68	17·6
Phthisis	130	41	31·5	41	22	794	33·5
Tabes mesenterica	..	..	..	..	..	..	..
Tubercles in brain	2	2	100·0	2	2	15	93·3
Accretions in peritoneum	2	2	100·0	2	2	24	87·5
18. Morbid growths:							
Hydatids	1	..	..	..	..	11	63·6
Encephaloid	12	10	83·4	9	8	60	73·3
Scirrhus	16	3	18·7	4	2	118	13·5
Colloid	1	1	100·0	1	1	2	50·0
Epithelial	1	1	100·0	1	1	2	100·0
Tumours of bone	..	..	..	..	..	2	..
19. Hysteria	55	1	?	4	1	318	..
20. Chorea	23	..	..	1	..	114	..
21. Delirium tremens	18	6	33·3	7	5	93	18·2
22. Tetanus	..	..	..	..	..	3	16·6
23. Diseases of brain and cord:							
Cephalitis	10	10	100·0	8	8	57	87·7
Chronic disease	7	2	28·5	4	2	52	23·07
Epilepsy	16	2	12·5	5	1	156	14·1
Apoplexy	9	7	77·7	3	3	40	60·0
Functional disturbance	33	1	3·03	5	1	230	2·60
Coma and convulsions	5	2	40·0	5	2	8	50·0
Insanity	6	..	..	4	..	10	..
Inflammation of cord	2	2	100·0	2	2	15	66·6
24. Paralysis:							
Hemiplegia	21	1	4·76	8	1	170	8·23
Paraplegia	10	2	20·0	3	2	109	11·0
General paralysis	1	1	100·0	1	1	4	25·0
Local paralysis	20	..	..	9	..	65	..
25. Neuralgia:							
Tic-douloureux	2	..	..	..	..	19	..

Cases admitted during the year 1856.						Admitted during six years.	
Nature of Disease.	Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
25. Neuralgia (continued):							
Sciatica	19	..	..	2	..	78	..
Hemicrania	1	..	..	1	..	10	..
Angina	..	..	..	..	..	2	..
26. Diseases of the heart:							
Pericarditis	19	4	21·05	19	4	92	34·7
Endocarditis	13	..	..	13	..	87	9·19
Hypertrophy	16	11	68·7	13	11	147	60·5
Dilatation	23	9	39·1	23	9	94	52·1
Valvular lesion	46	13	28·2	37	12	342	26·0
27. Diseases of bloodvessels:							
Aneurism	3	3	100·0	2	2	30	50·0
Phlebitis	9	1	11·1	5	1	37	29·7
28. Diseases of respiratory organs:							
Laryngitis	11	5	45·5	9	9	52	44·2
Tracheitis	3	1	33·3	..	..	7	28·5
Bronchitis	141	26	18·4	87	25	780	15·2
Emphysema	23	8	34·7	23	8	89	35·9
Asthma	4	..	..	3	..	11	..
Pneumonia	51	18	35·2	34	17	287	29·9
Pleurisy	48	18	37·5	29	16	314	33·1
Pertussis	2	1	?	1	1	8	25·0
Pneumothorax	2	1	50·0	2	1	7	71·4
29. Diseases of mouth and pharynx:							
Glossitis	1	..	..	1	..	2	..
Quinsey	2	..	..	1	..	55	..
Enlarged tonsils	9	..	..	7	..	30	..
Ulceration	11	..	..	9	..	37	5·40
Mumps	3	..	..	1	..	14	..
30. Diseases of stomach and œsophagus:							
Dyspepsia	52	..	..	16	..	371	..
Ulceration	8	2	25·0	8	2	53	92·3
Stricture of œsophagus	1	..	..	..	..	11	18·1
31. Diseases of intestinal canal:							
Obstruction	1	..	..	1	..	8	50·0
Constipation	36	..	..	8	..	254	..
Diarrhœa	39	3	7·69	16	3	219	7·07
Dysentery	6	1	16·6	1	1	27	37·0
Enteritis	..	..	..	..	..	1	..
Tympanitis	3	..	..	..	..	14	..
Ulceration	4	4	100·0	4	4	19	84·2
32. Peritonitis:							
Acute	27	10	37·03	12	8	132	40·1
Chronic	11	1	9·09	5	1	75	42·6
33. Diseases of liver and gall-bladder:							
Inflammation and congestion	15	2	13·3	6	2	51	21·5
Cirrhosis	10	8	80·0	10	8	68	76·4
Enlargement	9	2	22·2	8	2	65	26·1
Jaundice	17	2	11·7	12	2	96	25·0
Abscess	1	1	100·0	1	1	4	100·0
Gall-stones	4	1	25·0	4	1	6	16·6
34. Diseases of pancreas	..	..	..	..	..	..	..
35. Diseases of spleen:							
Enlargement	6	3	50·0	5	3	28	46·4
36. Diseases of urinary organs:							
Nephritis	9	3	33·3	7	3	38	21·0

Cases admitted during the year 1856.						Admitted during six years.	
Nature of Disease.	Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
36. Diseases of urinary organs (continued):							
Abscess	..	..	..	..	..	11	27·2
Renal calculus	2	1	50·0	1	1	2	50·0
Albuminuria	97	35	36·08	94	34	529	45·5
Cystitis	12	2	16·6	2	1	41	12·2
Diuresis	3	..	..	2	..	5	..
Ischuria	1	1	100·0	1	1	3	100·0
37. Diabetes	7	1	14·2	3	1	32	12·5
38. Diseases of the ovaries:							
Dropsy	11	1	9·09	2	1	34	23·5
Tumours	9	..	..	2	..	50	5·00
39. Diseases of the uterus:							
Amenorrhœa	24	1	?	13	1	89	..
Menorrhagia	6	..	..	3	..	75	..
Leucorrhœa	10	..	..	7	..	67	..
Tumours	10	1	10·0	3	1	39	12·8
Prolapsus	7	..	..	2	..	33	..
Ulceration	1	..	..	..	..	3	..
Congestion	5	..	..	..	..	18	..
Doubtful pelvic tumours	7	..	..	..	..	6	..
External organs	3	..	..	1	..	12	..
40. Diseases of bones and joints	7	1	14·2	6	1	54	22 2
41. Diseases of skin and cellular tissue:							
Erythema	15	1	?	7	1	78	..
Urticaria and roseola	1	..	..	1	..	18	..
Lichen and prurigo	1	..	..	..	..	15	..
Squamæ	8	..	..	4	..	55	..
Vesicular eruptions	9	..	..	6	..	83	..
Pustular eruptions	6	..	..	5	..	32	..
Pompholix and rupia	..	..	..	..	..	12	..
Abscess; cellular inflammation	16	2	12·5	12	2	68	33·8
42. Diseases of muscles	..	..	..	..	..	2	?
43. Anomalous and accidental cases	7	1	?	1	1	34	14·7

Remarks.

The present Report is drawn up in exactly the same manner with regard to the classification and nomenclature of diseases, as that of last year. At present I have not seen my way clear to suggest any marked alterations, although, doubtless, room exists for improvement. Amongst the divisions in which some modifications are desirable, may be mentioned 16, *cachœmia*, a vague term, to which considerable objections have been raised, but which has been found useful for including cases which otherwise could only come under the last heading of "anomalous;' and Section 36, in which a complete and more scientific arrangement of kidney diseases is sadly wanted. It would be no small gain if means could be devised whereby this Report should convey a more accurate idea of the mortality caused in the hospital by each individual disease. Hitherto it has been usual for a fatal complicated case to be entered *as fatal* under each head; as, for example, in acute rheumatism complicated with pericarditis, should death result, the fatal termination is recorded in Division 4 and 26, thus manifestly giving an erroneous idea of the results of the treatment of rheumatic fever, which in itself rarely, if ever, kills the patient. The only mode, as the system of registration now stands, is to indicate in the remarks on each division of disease the particular cases in which the accidental complication caused death. I now proceed to give a brief sketch of the more important facts observed in the above forty-three divisions, taking them in their numerical order.

1. *Fevers.*—Amongst the fatal complicated cases of continued fever, 2 had disease of the brain. In 1 of these the brain substance was studded with small abscesses—

2 cases were complicated with pneumonia.
1 „ „ „ „ pleurisy and gangrene of the lung.
1 „ „ „ „ hæmaturia and Bright's disease.
1 „ „ „ „ fistula in ano and acute rheumatism.
2 „ „ „ „ diarrhœa.
1 „ „ „ „ enormous enlargement of the spleen.
1 „ „ „ „ delirium tremens.
1 „ „ „ „ miscarriage.

In 18 fatal cases, extensive ulceration of the intestines was found in 9; in 1 the glands were greatly enlarged; 3 were free from ulceration; and 5 were not examined.

In the 39 complicated cases which terminated favourably, the complications are thus distributed:—

1 patient had scarlatina during convalescence.
4 „ bronchitis.
5 „ pneumonia.
4 „ rheumatism.
7 „ ulcerated throat or enlarged tonsils.
1 „ painter's colic.
1 „ facial palsy.
1 „ chorea.
1 „ phthisis.
5 „ some cutaneous disease.
1 „ peritonitis.
1 „ albuminuria.
1 „ parotitis.
1 „ inflamed inguinal glands.
3 „ diarrhœa.
1 „ anæmia.
1 „ large sloughing sores on back.

2. *Eruptive Fevers.*—Measles occurred in a case of erysipelas after admission. Another case was complicated with bronchitis, and another with functional disturbance of the brain.

In 3 cases of scarlatina, ulceration of the throat was the most serious complication.

1 attack succeeded continued fever (see Div. 1).
1 was followed by desquamative nephritis.
1 was complicated with acute rheumatism.
1 occurred in a patient who was excessively exhausted after confinement.

In the fatal case of erysipelas, hæmoptysis and intestinal hæmorrhage were the immediate cause of death. The post-mortem examination revealed cirrhosis of the liver and an enormous spleen.

Six other cases of erysipelas were thus complicated:—

1 with eczema.
1 „ anasarca.
1 „ epistaxis.
1 „ herpes.
1 „ rheumatism, valvular disease of the heart, and amenorrhœa.
1 „ measles (*vide supra*).

3. *Intermittent Fevers.*—In one case of tertian ague the spleen was greatly increased in size. In a patient with irregular ague who died, no examination of the body was allowed.

One case of ague was accompanied by bronchitis, and 1 by jaundice.

4. *Rheumatism.*—Under acute rheumatism 4 deaths are noted. In 1 the cause of the fatal result was obscure, and it was thought to be a case of poisoning by colchicum, from peculiar idiosyncrasy.

1 was complicated with gall-stones.
3 " " pericarditis.
1 " " pneumonia.

In 12 cases endocarditis was present. In 13 pericarditis was set up, whilst five were admitted with old standing valvular lesions.

Pleurisy, pneumonia, or bronchitis occurred in 7.

In a case of subacute rheumatism the acute form, accompanied by pericarditis, occurred soon after admission.

Amongst the cases of chronic rheumatism, 4 were found to have disease of the valvular apparatus of the heart.

5. *Gout.*—One case was associated with Bright's disease.

6. *Rheumatic Gout.*—Three deaths occurred:

1 was a case of jaundice and renal dropsy.
1 was a case of "*exhaustion*" (no autopsy allowed.)
1 was fatal from extensive bed-sores.

2 patients had disease of the heart; 1 had asthma.

7. *Poisoning.*—One case of poisoning by hydrocyanic acid ought to come under a separate heading, "Sedative."

Of the 8 cases of narcotic poisoning, 4 occurred in one day. One patient attempted self-destruction by taking sulphate of copper.

One case of syphilitic throat was accompanied by laryngitis, for which tracheotomy was necessary. The patient did well.

8. *Colica Pictonum.*—Cases of lead palsy are found under Div. 24, "Paralysis."

9. *Entozoa.*—

5 of these were cases of tapeworm.
1 was complicated with bronchitis.
1 " " " enlarged liver.

10. *Dropsy.*—There were 35 fatal cases of anasarca.

12 had disease of the heart.
22 " " kidneys.
19 " " lungs.
6 " " liver.
1 had encephaloid disease of the liver and spleen.
1 had dilatation of the pulmonary artery.

In 6 cases the cause of the dropsy was not ascertained.

The complicated cases of ascites are thus distributed;

5 had enlargement of the liver.
6 " cirrhosis of the liver.
1 " jaundice.
2 " encephaloid disease of the liver.
1 " epithelial cancer of the stomach.
1 " colloid cancer of stomach, intestines, and womb.
2 " disease of the heart and bronchitis.
1 " hæmatemesis.
1 " apoplexy.

11. *Hæmorrhages.*—Epistaxis occurred in 2 patients with Bright's disease, in a case of erysipelas, and in a case of jaundice.

Eight cases of hæmoptysis were connected with phthisis, and of these 2 proved fatal. The other death from hæmoptysis occurred in a patient with cirrhosis of the liver and hæmorrhage from the bowels.

Hæmatemesis was an accompaniment of some other disorder in 6 cases:

In 1 of hysteria.
,, anæmia.
,, purpura.
,, ulceration of the stomach.
,, malignant disease of the liver.
,, ascites and enlargement of the liver.

Three cases of hæmaturia were associated with dropsy, and in 2 of these Bright's disease existed. Two others were cases of bronchitis.

Intestinal hæmorrhage was complicated in 1 fatal case with hæmoptysis and erysipelas, in another with extensive ulceration of the rectum.

Two deaths are recorded under uterine hæmorrhage: 1 from acute peritonitis, the other from laryngitis and Bright's disease.

12. *Purpura.*—Five cases were complicated as under:

1 with dropsy and dilated heart.
,, hæmatemesis.
,, rheumatism.
,, obstruction of the bowels.
,, fever.

14. *Anœmia.*—

4 cases associated with rheumatism.
3 ,, ,, ,, some kind of hæmorrhage.
1 ,, ,, ,, scrofula.
1 ,, ,, ,, exhaustion after confinement, and afterwards with scarlatina, (see Div. 2. Eruptive Fevers.)

15. *Chlorosis.*—Four cases were complicated—2 with dyspepsia, 1 with phthisis, 1 with dropsy.

16. *Cachœmia.*—(See remarks at the head of this Report.) Three cases proved fatal.

1 was a case of pleuro-pneumonia and peritonitis.
,, ,, acute rheumatism and gall-stones.
,, ,, delirium tremens and pyæmia.

17. *Tubercular Diseases.*—Eight cases of phthisis were complicated with hæmoptysis, 17 with some other disorder of the respiratory organs, 6 with some disease of the heart, 1 with tubercular accretions in the peritoneum, 1 with tubercles in the brain, 6 with Bright's disease, 1 with encephaloid disease of the kidneys and liver.

18. *Morbid Growths.*—One case of hydatids in the liver came under notice.

In a fatal case of Bright's disease, epithelial cancer of the stomach was discovered.

There were 3 cases of encephaloid disease of the liver.
,, 1 ,, ,, ,, ,, ovaries.
,, 2 ,, ,, ,, ,, uterus.
,, 1 ,, ,, ,, ,, clitoris.
,, 1 ,, ,, ,, ,, brain.
,, 1 ,, ,, ,, ,, stomach.
,, 1 ,, ,, ,, ,, lungs.
,, 1 ,, ,, ,, ,, larynx.
,, 1 ,, ,, ,, ,, kidneys.

In 1 of these, cephalitis occurred, in another crural phlebitis; the rest of the complicated cases were associated with dropsy or phthisis.

Thirteen cases of scirrhus were limited to the uterus and vagina; in 2 cases the stomach, and in 1 the liver, were the seat of the disorder.

Colloid cancer occurred in a patient admitted with ascites.

19. *Hysteria.*—One patient died phthisical.

20. *Chorea.*—The number of cases admitted last year was above the average. Continued fever attacked 1 patient.

21. *Delirium Tremens.*—Six deaths are recorded:

1 from pyæmia.
,, bronchitis.
,, disease of the heart.
,, continued fever.
,, inflammation of the brain and general tuberculosis.

Whilst 1 was an uncomplicated case.

23. *Diseases of Brain and Cord.*—Three cases of cephalitis were dependent upon tubercles in the brain; 1 was associated with delirium tremens; 1 with encephaloid disease; 1 with apoplexy and Bright's disease; 1 with pneumonia; 1 with fever. In 2 the brain was filled with small abscesses.

Chronic disease of the brain was associated in 1 case with hemiplegia, in another with epilepsy.

Two cases were fatal:

1 from peritonitis and ulceration of the stomach.
,, continued fever.

Epilepsy was fatal in 2 cases:

In 1 of bronchitis.
,, pneumonia and desquamative nephritis.

Two other cases of epilepsy occurred in phthisical patients, another case was that of disease of the brain just alluded to, and a fourth was that of a patient admitted for some local paralysis. Under functional disturbance 1 death is recorded. The patient had dropsy, ulceration of the intestines, and *atrophy* of the liver. Three cases of coma were associated with disease of the kidney, 1 with pleurisy, and 1 with acute rheumatism.

Both cases of inflammation of the cord were accompanied by paraplegia. One had also cystitis.

24. *Paralysis.*—Of 8 complicated hemiplegic cases, 4 had disease of the kidney.

Two cases of paraplegia were accompanied by inflammation of the cord (see Div. 23.) The third was dependent upon caries of the spine.

A patient with general paralysis had also bronchitis and pneumonia.

25. *Neuralgia.*—One case of excruciating pain along the course of the ulnar nerve is not recorded in the above table.

26. *Diseases of the Heart.*—The connexion of heart disease with the rheumatic cases has been noticed in Div. 4.

In all the fatal cases of pericarditis, pleurisy was also found; in 1 an enormous mass of encephaloid matter existed in the chest.

Out of 9 fatal cases of dilatation of the heart,

4 had disease of the lungs,
5 ,, ,, ,, kidneys,

and dropsy co-existed in 4.

Valvular disease was accompanied by dropsy in 10 cases, 5 of which proved fatal; by disease of the lungs in 8 cases, 4 of which proved fatal; and by disease of the kidney in 4 cases, of which 3 died.

27. *Diseases of Bloodvessels.*—Death followed the rupture of an aneurism of a branch of the superior mesenteric artery.

In a fatal case of dropsy and pneumonia, great dilatation of the pulmonary artery was found.

A patient who died of Bright's disease had a large aneurismal pouch in the wall of the left ventricle of the heart.

Phlebitis was fatal in 5 cases:

In 1 of pneumonia.
,, cancer of uterus.
,, uterine hæmorrhage.
,, leucorrhœa.
,, diarrhœa.

28. *Diseases of Respiratory Organs.*—In 4 cases laryngitis was an accompaniment of phthisis. Three of the fatal cases had also disease of the kidney.

A child five years old, who had been suffering four days from croup, died soon after tracheotomy was performed.

A child eight years old, who had been three days ill, was operated upon two hours after admission, and recovered.

Another successful case of tracheotomy occurred in a child $4\frac{1}{2}$ years of age, who had been ill two days. The operation was performed nineteen hours after admission.

Dropsy accompanied 30 cases of bronchitis:

In 12 some affection of the heart was present.
In 18 „ „ „ kidneys was present.

Out of 23 cases of emphysema, 20 had bronchitis.

Asthma was associated,

in 1 case with diseased heart and bronchitis.
„ „ „ dropsy and albuminuria.
„ „ „ rheumatic gout.

Pleurisy and pneumonia co-existed in 6 cases.

Eight cases of pneumonia occurred in patients with continued fever.

Hooping-cough, with bronchitis and pneumonia, was fatal to a child seven years of age.

The fatal case of pneumo-thorax was associated with pleurisy and phthisis.

29. *Diseases of Mouth and Pharynx.*—Glossitis occurred in a female suffering from lepra.

Three cases of ulcerated throat belong to scarlatina (see Div. 2).

30. *Diseases of Stomach and Œsophagus.*—Three cases of ulceration of the stomach are marked as doubtful.

A patient with stricture of the œsophagus left abruptly.

31. *Diseases of the Intestinal Canal.*—Obstruction occurred in a man admitted with purpura.

A patient with diarrhœa died of pleurisy; the 2 other fatal cases of diarrhœa were associated with fever.

A patient with dysentery died from malignant ovarian disease.

The cases of ulceration of the intestine in fever or phthisis, are not recorded in this Division. In the 4 fatal cases, the rectum alone was ulcerated:—

1 was a case of profuse hæmorrhage.
2 were cases of acute peritonitis.
1 was a case of dropsy and atrophy of the liver.

32. *Peritonitis.*—The following are the 8 complicated fatal cases of acute peritonitis:—

3 of ulceration of stomach or intestines.
1 „ ischuria and cancer of uterus.
1 „ perforation of the bowels.
1 „ miscarriage.
1 „ ovarian dropsy (paracentesis).
1 „ pleuro-pneumonia.

Chronic peritonitis, in a fatal case, was associated with pneumonia and diabetes.

33. *Diseases of Liver and Gall-bladder.*

6 cases of cirrhosis connected with ascites.
2 „ „ „ „ general dropsy.
4 „ „ „ „ disease of the lungs.
2 „ „ „ „ disease of the kidneys.

Great enlargement of the liver occurred in a case of jaundice, which was fatal; and in a case of pleurisy, with malignant disease of the spleen, also fatal.

Jaundice was connected with gall-stones in 3 cases. The other death from gall-stones occurred in a woman admitted for acute rheumatism (see Div. 4).

A patient admitted with jaundice was found, some weeks later, to have diabetes also.

34. *Diseases of Spleen.*—In 3 cases which proved fatal from pleurisy, fever, and encephaloid disease respectively, the spleen was found to be more than double its natural size.

36. *Diseases of Urinary Organs.*—Nephritis was fatal—

In a case of epilepsy with bronchitis and pneumonia.
" dropsy and fatty heart.
" renal calculus and acute inflammation of knee-joint.

Albuminuria was associated—

In 13 cases	with disease of the		heart.
" 29	"	"	lungs.
" 6	"	phthisis.	
" 62	"	dropsy.	
" 2	"	malignant disease.	
" 1	"	diabetes.	

1 case of diuresis was complicated with anasarca, another with Bright's disease.

Ischuria was fatal in a case of peritonitis (see Div. 32).

37. *Diabetes.*—One patient died from pneumonia and peritonitis; 1 case was complicated with albuminuria, another with jaundice.

38. *Diseases of the Ovaries.*—A patient with ovarian dropsy, had also cancer of the vagina. One died of peritonitis after tapping had been performed (see Div. 32).

In the obstetric physician's ward there were 6 cases of pelvic tumours, the nature of which could not be clearly ascertained.

39. *Diseases of the Uterus.*—In a case of non-malignant tumour of the uterus, death occurred at a later period from encephaloid disease of the neighbouring parts.

One case of amenorrhœa was fatal from pleurisy and disease of the heart.

40. *Diseases of Bones and Joints.*—The following cases occurred in the medical wards:

2 of chronic synovitis.
1 of acute inflammation and suppuration of knee-joint.
2 of caries of the spine.
1 of scrofulous disease of hip-joint.
1 of syphilitic caries of the frontal bone.

41. *Diseases of Skin and Cellular Tissue.*—A fatal case of diffuse cellular inflammation was complicated with dropsy and bronchitis.

Another patient died from sloughing bed-sores.

43. *Anomalous and Accidental Cases.*

1 of spasm of glottis.
1 doubtful tumour in left hypochondrium.
1 case of otorrhœa.

And three or four cases of admission without necessity.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON MICROLOGY.

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Part I.—Physiological Micrology.

NERVOUS SYSTEM AND ORGANS OF SENSE.

Minute Anatomy of the Cerebellum and Spinal Cord. (By Bergmann of Rostock.)

The author* agrees with Bidder and Hanover as to the fact of the conical cells forming the covering of the spinal cord, passing at their smaller extremities into fibres which enter the grey substance, and there partly uniting themselves with other fibres, and partly passing into areolar tissue-cells. He adduces microscopical preparations which he has made, showing the passage of very fine fibres from the epithelium-like lining of the fourth ventricle of the toad, into the medulla. On transverse section of this ventricle, they are seen to form arches convex in an outward direction; but the author has not noticed any union with areolar tissue in their case. Bidder, however, thought that these fibres could not form a portion of any system of nerve-fibres. Bergmann says, that in one point he was at issue with Bidder. Although, he observes, the layer out of which the fibres proceed appears to be epithelial, traces even of the cilia not failing, yet isolated fibres existed not having the character of epithelial cylinders, but of spindle cells, which project from their nucleus-holding body a short process towards the ventricle, and a long one into the medulla. On looking carefully at these fibres as they lie near each other, they might be considered as limits of cells, and the nuclei as nuclei of these cells. These apparent cells are supported towards the medulla by a limitation, the fibres being united to each other by means of a delicate membrane, from which pass the tender projections. Possibly there may be between these fibres, cells, to which cilia belong. Bergmann proceeds to mention some appearances observed by him in the cerebellum of the newly-born cat when hardened by chromic acid. At the borders of very fine sections made perpendicular to the surface, a bright layer was observed between the grey substance and the pia mater, at one time more, at another less broad. This did not exist in all parts of the same thickness, but varied up to 0·007 and 0·008 Paris lines in thickness. This layer showed very fine lines drawn perpendicularly in such a way that in many places one might have thought it a non-nucleated cellular layer. It is in reality, however, only a delicate clear mass, penetrated by innumerable threads of great tenuity. These threads are the extreme branches of other and thinner ones which may be traced in the innermost layer of the grey substance. Here and there a nucleus was seen adherent, but it could not be made out whether or not it was intimately united to the fibre. Amidst isolated fibres were seen short branches at short angles, broken off. These had partly a direction towards the periphery, and partly also into the interior of the organ: a fact adverse to the idea that the fibres pertain to the ramifications of the ganglion corpuscles. The fibres in question probably form a network in the grey substance: the end of one of them isolated could in places be seen traced into the clear substance, and separating into finer fibrils, the structure calling to mind the relation of the radial fibres of the network where they are in contact with the limitary membrane; the similarity is greater from the fact that a very fine structureless lamella, differing from the pia mater, lies on the surface of the cerebellum, representing, as it were, the limitary membrane. How far the above-named fibres enter—for instance, between the ganglion corpuscles—is not known. The author also possesses preparations from other adult animals and man. In the case of the dog, similar fibres are seen projecting out of the grey substance; the transparent layer is entirely wanting, and possibly only belongs to histological development. The limitary membrane may become increased in thickness, and connected with the pia mater.

* Henle und Pfeuffer's Zeitschrift, Band viii. Heft 2, p. 360.

On the Nerves of the Intestinal Walls.—G. Meissner* looks upon the areolar tissue between the muscular and the mucous coats of the intestines as one of the parts most richly supplied by nerves in the entire body. These nerves, by numerous anastomoses, form a network, the finest twigs of which appear to penetrate the muscle. The primitive fibres for the most part, and perhaps entirely, belong to the kind without any double contour, and are beset by numerous nuclei. They form the finer and thicker branchlets, and are comprised in nucleus-holding sheaths, in numbers varying from five to twenty; whilst the finest branchlets only contain from two to three primitive fibres. The small intestines appear to be the most rich in nerves, but in the walls of the stomach the nerves are very sparing. In the walls of the intestine the number of ganglia in the nervous plexuses is immense, corresponding for the most part with the thickness of the nerve-branches in which they are found; and in the small intestines almost every nerve-branch leads to a ganglion. The largest observed by the author consisted of from thirty to fifty cells; but they ordinarily contained from five to ten, having the ordinary appearance of ganglion cells. In man, pigment granules were often to be seen as contents of the cells, but in the calf they were found to be quite clear and colourless. Many of the cells were bi-polar, and this was evident whenever a single cell was seen inserted in the course of a primitive fibre without a ganglion being formed. Such cells were generally spindle-shaped, and projected themselves at opposite poles into a fibre. Besides bi-polar cells, some were seen from which, either at one side or both poles, two fibres passed close to each other. The ganglia in the stomach are not relatively less than in the intestine. The ganglion cells in the stomach are larger than those of the intestinal walls. The author describes the best method of examining the nerves and ganglia of those parts, and speaks of the fresh intestine being sufficient, by the aid of acetic acid, for the examination; but inasmuch as this method is tedious, moderately concentrated pyroligneous acid affords the greatest help, which after some time makes the areolar tissue very transparent, leaving the nerves and ganglia unaltered. After dilute pyroligneous acid has acted for some days, acetic acid proves very serviceable.

On the Olfactory Mucous Membrane of Man.—By Professor Ecker.†—The author, speaking of the septum of the nostrils, says that the very vascular succulent Schneiderian membrane is obscurely separated from the other part of the mucous membrane, poor in vessels, and of a reddish yellow colour, on which, at the upper part of the septum the olfactory nerve extends itself. This olfactory region stretches downwards about 9‴ in width from the front backwards, and about $1\frac{1}{2}''$ in a horizontal direction. This spot is distinguished from neighbouring parts by being of a yellowish colour, less transparent, and somewhat thickened. Ecker considers this to deserve exclusively the designation of olfactory region, and calls it the "locus luteus." The undermost and most anterior part of the septal mucous membrane is covered by permanent epithelium, whilst from the line which unites the anterior free edge of the nasal bone with the anterior nasal part of the upper jaw, the ciliated form of epithelium extends upon the entire mucous membrane of the septum, with exception of the locus luteus. A definite direction of the ciliary movement could not be ascertained. Between the ciliary epithelium, measuring 0·090 millimetre in length, with clear long cilia or long peduncles, other cells were seen, whose relation to them could not be defined. They were of about the same length, but for the most part broader, often swelled out, and possessing very fine cilia. They varied at their extremities, being in some places diminished in size, and obviously closed; at other times open at their ends, cuplike. But seldom was any nucleus visible within. The cells of the locus luteus consist of both these kinds. They are elongated, and below pass into a long threadlike body, which for the most part is swollen out, and often forms bulgings out, in which the so-called compensation cells (*Ersatz-cellen*) exist. They are very delicate and perishable. This free end is not beset by ciliated epithelium, as the author seems formerly to have thought, but their free end contains numberless yellow pigment-granules, and it is to this colouring matter that the yellow appearance of this locus luteus is owing. The extremity of the threadlike projection bifurcates, and at the point of division oftentimes a finely granular swelling exists. Between these olfactory cells lie embedded the compensation cells. Immediately upon the surface of the mucous membrane, one comes to a layer of cells partly round, partly more irregular, and in places beset with projections, between which the root-threads of the olfactory cells sink. As regards the parietes of the nose, in the lowest and most anterior parts pavement epithelium is found, but its limits do not run parallel with that in the septum, for, proceeding from the anterior free margin of the nasal bone, it descends some lines behind the nasal part of the upper jaw, so that the anterior extremity of the lower turbinated bone, as well as the anterior part of the lower nasal cavity, are covered by this epithelium. The mucous membrane of the superior turbinated bone only in part is possessed of cilia; the spot which has none extends backwards from the covering of the nasal cavity

* Henle und Pfeuffer's Zeitschrift, Band viii. Heft 2, p. 264.
† Zeitschrift für wissenschaftlichen Zoologie, Band viii. p. 203: as quoted in Schmidt's Jahrbücher, No. 5, p. 167. 1857.

about 4''' wide, and, like the locus luteus, is coloured yellow, and contains the above-described olfactory cells; consequently it should also be considered as part of the olfactory region, which in the mammalia is much more extended, so much so, indeed, that the olfactory cells cover the entire non-ciliated olfactory region.

OSSEOUS AND CARTILAGINOUS SYSTEM.

Upon the Ossification of Primordial Cartilage. By A. Bauer.*—The author pursues two objects for investigation—1. The method of origin of the peculiar structure of bone-substance out of a structure so different as cartilage; and 2. The relation between the elements of cartilage and those of bone. He speaks of the origin of all bone not antecedently cartilaginous as referrible to the ossification of a blastema, which is generally looked upon as one of connective tissue, consisting of an indistinctly fibrillated basis, containing simple round cells, corresponding to the primary formative cells of areolar tissue, the future areolar tissue-corpuscles. Most probably, he continues, the ossification proceeds by the gradual formation of calcareous salts without any distinct limitation in the inter-cellular structure, the cells developing into dentate bone-corpuscles. The intervention of cartilage is not met with, nor any pre-existing cloudiness owing to calcareous deposit. Thus we have a direct ossification of areolar tissue. In the ossification of cartilage, however, the process is more complicated, consisting not of a mere change (*Umwandlung*) of substance, but of a total transformation (*Umgestaltung*) of structure, which renders the investigation of their histological changes so difficult. The best method of examining is to take fine sections, made in various directions through the edge of the calcification of the diaphysis of a fresh fœtal long-bone. We here see, as has been in part long known, that the cartilage cells, hitherto equally divided, assume a direction corresponding to the future bone-structure, and that in the cartilage of long-bones they become placed in rows, which are conspicuous, on transverse section, as roundish groups. The single cells increase in volume at the expense of the stroma; their contents become transparent, and show a large nucleus with nucleoli. In the walls of the cartilage cavities, or canals, a deposit takes place of dark granular earthy elements; and this calcareous deposit forms limits between cartilage and bone; but we as yet have not the microscopical elements of bone-substance. The cartilage cells are contained, yet unchanged, in the cartilage corpuscles, encrusted with lime, whose opaque character renders the observation of their metamorphosis difficult. In the place of a single cyst-like nucleus, more like it are found, which fill the mother-cell, and, after its dissolution, become free. It is this brood of cells, corresponding to the nucleus of the cartilage cell, which constitutes the contents of the calcified cartilage cavities, and also the starting-point of all following changes. The fact that in the ossification of cartilage an endogenous formation of cells takes place in the cartilage cells—a process which is to be distinguished from the increase of cartilage cells by division before ossification—has hitherto been thought by observers to be connected with the formation of the medullary elements of bone, having, according to them, no reference to the origin of bone-substance itself; whilst, in fact, a portion of the cell-formation is covered with vessels—fat cells, or marrow cells—surrounded by a layer of soft, striped, intervening substance, which lines the inner wall of the cartilage cavities. Of pure bone-structure, nothing was hitherto to be seen: but now it forms itself for the first time by direct ossification of the blastema, by change of its cells into bone corpuscles, and its intercellular substance into homogeneous non-granular osseous stroma.

Hence it is that the first bone-substance occurs in the form of a canal lining each calcified cartilage cavity, which becomes obvious on a transverse section as a circle surrounded by a simple row of bone cells. This bony cylinder thickens from within outwards by successive ossification, so that each cartilage canal is gradually more or less filled by a system of concentric long lamellæ. The quite uniform, concentrical arrangement of the lamellar substance of the tubules is obvious, as in the interior of each medullary canal a lamellar formation and successive ossification from the centre outwards occur. In this way the individual bone tubules of the newly-formed bone-substance must be, at first, still separated from the calcified bone-substance surrounding, and from each other. The calcified basis of the cartilage gradually passes into homogeneous bone-structure, whether the lime particles fuse into a homogeneous mass or are re-absorbed. After that, the granular lime-deposit disappears entirely, and along with it the organic structure which pertained to it; and the already-commenced resorption of the cartilage stroma proceeds further after the removal of the lime, in order to make room for the new bone-structure. Then the single laminated bony rings close in nearer together, until, after complete disappearance of the intervening cartilage layer, they touch each other. Hence it follows that the basis of the primordial cartilage has no part in the

* Müller's Archiv, No. 4, p. 347. 1857.

formation of bone-substance. This result from observation, according as it does with the chemical differences between bone-cartilage and the hyaline-cartilage substance, constitutes an objection to the theory of persistence of the latter in bone. One must therefore fall back for an explanation of the ossific process, either upon the theory of a chemical change, or that of a molecular displacement of the one substance by the other. The organic basis of bone is as little identical with the basis of the hyaline-cartilage anatomically as chemically. The last is incapable of a true ossification; its calcification is one indeed for the most part accompanying ossification, but is, in fact, a process differing from it. The bone-substance occurring in cartilage is newly formed in the cartilage cavities, nevertheless it does not obtain immediately as such, but there precedes it the formation of a blastema, consisting of simple cells and soft intercellular substance. This blastema accords in all relations with the ossified layer of the periosteum, and like it, must be looked upon as belonging to areolar tissue. Its ossification takes place through absorption of calcareous matter by the homogeneous intervening substance, and by the change of its cells into bone corpuscles. Also, in cartilage the ossification is preceded by the formation of areolar tissue. The part taken by the cartilage cells in the ossific process consists in their acting as mother cells of such cells as the cartilage corpuscles exhibit, surrounded by ossified areolar tissue and developed in a radial form. Never are the cells of the primordial cartilage changed, as such, into bone cells: they differ both in number, size, and disposition. In some places, such few bone corpuscles were seen inside the transparent contour of a cartilage cell, that it was evident that the production of other cells by them had been limited to but few. Pure bone-substance is limited to the circumference of the cartilage cells, whilst this itself is surrounded by calcified, or, in rachitic bones, by still hyaline-cartilage stroma. Such appearances give rise to the assumption that the bone corpuscles correspond to the nuclei of cartilage cells, or the cartilage cell thickened by an interior deposit. This view does not hold good in the ossification of fœtal cartilage. The ossifying process of the primordial cartilage has shown, on the one side, that the bone-substance, as it agrees chemically with the substance of the areolar tissue, so also, histologically, it must be referred to the elements of areolar tissue; and, on the other hand, the capability of a direct ossification must be refused to the tissue of hyaline-cartilage, because neither its basis nor its cells persist, as such, in the analogous elements of bone. The proposition that a bone-formation is, in like manner, possible out of cartilage as out of areolar tissue, by deposit of calcareous matter in its basis, is, according to the author, hereby confuted; and at the same time, in his opinion, the theory of the identity of cartilage with bone and areolar tissue, which was one of the most important supports of that proposition, removed.

EPITHELIAL SYSTEM.

On the Ciliated Epithelium and Ciliary Movement in the Generative Organs of Mammalia and Man.—O. Becker,* premising the necessity of examining the inferior animals as a guide to our researches in man, and adverting, as an example, to the fact that he had been led to find ciliary epithelium in a part of the human epididymis, owing to its discovery in the pig,—proceeds to advert to the condition and character of the epithelium in various portions of the human genital organs. He first considers the female organs, and then the male organs, after birth; and afterwards he proceeds to notice the existence of ciliated epithelium in the embryo. He then enlarges upon the general subject of ciliary movement in the ducts of the genital glands, and concludes by a consideration of this movement in closed cavities.

As regards the epithelium of the female organs after birth, Becker notices several differences. Whilst Kölliker described the pavement epithelium of the vagina as passing into the ciliated form in the cervix of the uterus, Henle and Gerlach seemed to have traced it towards the fundus; and in the dead bodies examined by Becker it was also found to be so. The average length of the cylindrical cells was 0·036 millimetre. In the Fallopian tubes and on the fimbriæ they were only about one-half this length. On the posterior surface of the fimbriæ, ciliary epithelium existed, in various transitional forms, passing into the pavement epithelium of the abdominal cavity. The author notices the observation of Bischoff,† that the inner genital organs after birth, until puberty, are wanting in ciliated epithelium; and also that of Valentin,‡ who describes it as wanting in young animals and in the woman at the catamenial periods, and, for the most part, during pregnancy. Becker says that this is only correct as regards the mucous membrane of the uterus. As regards the fimbriæ and the tubes, on the contrary, the epithelium in the newly-born mammals and human being is ciliated, and this is specially seen at the free borders of the fimbriæ, and at the uterine end of the tubes. In very young rabbits the movement of the cilia is most easily to be seen. Becker also, contrary to Bischoff, declares that the ciliated movement of the epithelium of the

* Untersuch. zu Naturlehre der Menschen und der Thiere, Band ii. Heft. 1, p. 71. 1857.
† Entwicklungsgeschichte der Säugethiere und des Menschen, p. 492. ‡ Lehrbuch der Physiologie, Band ii. p. 23.

Fallopian tubes and fimbriæ exists in the pregnant rabbit, and that it does not cease on the passage of the ovum. In the uterus of the newly-born child no ciliated epithelium exists, and this is also the case in the rabbit. At the time of puberty, however, ciliated epithelium is found in animals at the fundus of the uterus. The author then alludes to the observations of Kölliker, who found that during the catamenial periods and pregnancy, the time of periodic removal and reformation of epithelium, the ciliated form of epithelium alone was concerned; but asserts that, as far as he himself has examined the matter, ciliated epithelium was to be found at the top of the horns of the uterus in the rabbit that was pregnant. As regards the epithelial lining of the accessory ovaries (*Kanale der Nebeneierstöcke*), which Kölliker says are probably of a ciliated character, the author declares that it varies according to the stage of development of the ovary; but that in two newly-born children, and once in a woman, aged twenty-nine, who possessed a greatly-developed parovarium, the ciliary projections were easily seen. This was also the case in the parovarium of a rabbit fourteen days old.

Proceeding next in order to the male organs, Becker speaks of the ciliated epithelium of the epididymis. He had examined very many of the lower animals—such as the amorous sparrow, the swallow, the fowl, the goose, duck, pig, deer, horse, rabbit, cat, and dog—and in all he had found the vasa efferentia to be possessed of a simple ciliated epithelium. He notices, however, certain differences observable in several of these animals, especially as regards the course and direction of the vasa efferentia, which do not concern us here. He declares the presence of a double epithelium in the epididymis of all mammals; of which one, a simple ciliated epithelium with conical cells, covers the vasa efferentia; whilst the other, a complex laminated layer with cylindrical cells, beset, according to age and species, with unusually long cilia, occupies the entire tube of the epididymis as far as the vas deferens, and there, by means of a simple form of cylindrical epithelium, passes into the pavement variety. The epithelium of the canal of the epididymis at the time of birth and before puberty, consists of cells, of which the uppermost layer is hardly larger than the younger ones beneath. As the epididymis grows, these elongate and become laminated. The fibrous tunic of the seminal canals of the testis is so firmly united in the *rete* of Haller with the areolar tissue of the *corpus* of Highmore, that the canals almost appear to be without any special membrane. In the "coni vasculosi" there intervenes, between the fibrous layer and the structureless membrane, a circular layer of contractile fibre cells, which appears to be wanting at the summit of the cones. In man, as in most animals, the vascular cones possess a simple ciliated epithelium, whose cells are sharply contoured, being conical, with cilia from 0·008 to 0·010 millimetre long. It is very persistent, and is to be found in the newly-born subjects just as in the adult, and can often be expressed out of the seminal canals connected together so as to form hollow cylinders. Becker speaks of his finding this epithelium even in diseased parts, in which induration, along with obstruction of the excretory ducts, has taken place, and describes the cilia in one case of a testis destroyed for the most part by fibrous cancer, as distinctly seen in a state of motion.*

As to the canal of the epididymis, the epithelium is found to be laminated, the cells being quite cylindrical, perpendicularly placed, with strong but minutely contoured walls, very long, and with large nuclei. The head of the epididymis offers the longest cilia that can be seen in the human body. The epithelium here is remarkable for its frailty and liability to change, and its proneness to reproduce itself. At birth, as well in man as in animals, it is but slightly formed; and in young children, no cilia are to be found in the whole course of the canal. But about the time of puberty, the cells exhibit fluctuations in the size and length of the cilia, the contour becomes bolder, the contents granular and less transparent, the cell-membrane indented, crumpled, and in some cases folded together; but the character of the epithelium in the epididymis seems in a great degree to be dependent on the quantity of perfect seminal fluid therein collected; and this accords well with the completeness of the epithelium found in animals that are yet rutting. The cilia of the epithelium in the head of the epididymis are peculiar, as indicating a tendency to adhere to each other, thus giving the appearance as of a solid stem projecting from the interior of the cell, but not of cilia at the edge of the cell. Becker had never seen cilia on cells removed from the lower end, and had never missed them in the head of the epididymis. At the lower end he had observed epithelial cells of unusually large dimensions. In the vas deferens the epithelium was simply cylindrical, and in the upper third passed into the pavement form, which covers the vesiculæ seminales. The efferent ducts of the testis are not, however, the only places possessing ciliated epithelium. The so-called non-pedunculated hydatids of Morgagni, situated in the head of the epididymis, and described by Luschka as being generally, though not always, in connexion with the seminal canals of the part, are seen to possess ciliated epithelium as well as seminal threads, which two structures appear to bear a certain proportion to each other.

* For a beautiful instance, with illustrations, of the presence of ciliated as well as pavement epithelium in cysts within the testicle, see vol. vii. of the Pathological Society's Transactions, p. 241, as described by Mr. Athol Johnson. The specimen was removed from a child aged two years and three quarters, and was probably congenital.

The ciliated epithelium of the hydatids is always small and varied in form; at one time regularly cylindrical and slightly conical, at another time irregular and small. In like manner the pedunculated hydatids or cysts which generally exist where the vesicles of Gosselin are found, as remnants of fœtal structure, as also the parovarium and the "uterus masculinus," contain ciliated epithelium.

As regards the embryonal structures, the author ascertained that ciliated epithelium does not exist in the Wolffian bodies of the rabbit. Their existence at birth leads to the supposition that perhaps the Fallopian tubes and the head of the epididymis of the embryo might possess ciliated epithelium, but, although sought for in various embryos, none was found; nevertheless, their existence before birth seems certain.

The author then proceeds to demonstrate the best method of watching the movements of the ciliated epithelium, and describes those which he observed for the space of two hours, in a part of the testicle which had been removed, from a man aged forty-two, during life. He speaks of the ease with which the stream of fluid established by the cilia in a direction from the seminal gland, can be watched in certain animals, the walls of whose vascular cones are very transparent, and this the more so as seminal particles are carried along. He thinks that the stream in the vascular cones is not parallel with the long axis of the vessel, but observes a spiral direction.

As regards the ciliary movement in closed cavities, the author alludes to a case of cysts of the testicle related by Billroth in the 'Deutsche Klinik,' 1856, No. 10.

Upon the Epithelium of the Gall-Bladder, and also upon an Intermediate Metamorphosis of Fat.—Under this title R. Virchow has a paper of the following nature.* After alluding to his former mention of a peculiar appearance in the cylindrical epithelium of the gall-bladder—i.e., its gradual filling with finely granular fat, he draws a comparison between this and analogous processes in the intestinal epithelium cells, as also the changes in the fatty metamorphosis of cells. In general, but not invariably, the nucleus of the cells remains complete. Often also a collection of finely granular fat was observed in the net-like folds of the mucous membrane itself of the gall-bladder, and in one case in the sub-mucous tissue, anastomosing canals were seen, in which larger and smaller fat masses were seen. Virchow alludes to the observations of Gobley,† to the effect that only a very slight quantity of the neutral fat of the bile is found in the excrements, and hence most probably it is reabsorbed in the intestines. This absorption is thought by Virchow to occur in the gall-bladder. He observed the epithelium of this organ in man, the dog, and cat, and found it in all places to be comparable to the intestinal epithelium. In the dog the cylinders are very long, showing their free surface and a side view very well. On the free end of the cells a broad bright border, with radiating stripes, is to be seen (just as Kölliker has pictured of the intestinal epithelium), having, when quite fresh, a smooth margin. After a time, as Kölliker shows in the intestines, the margin becomes toothed, and the projections often have the lookof cilia, and other appearances arise —such as the lifting up of the cell membrane from the contents, as Kölliker and Remak had seen in the intestinal epithelium.

As regards the absorption of fat, first of all a finely granular fat entered, and later on fat in large glistening drops. Originally the fat exists at the uppermost parts of the cell, close under the homogeneous border, the deep parts lying free. It gradually passes deeper, until it extends and fills the entire cell, excepting where the nucleus exists. At this time, owing to the linear direction of the fat drops, they have somewhat the appearance of primitive muscular fibre undergoing fatty degeneration. Afterwards the fat ceases in the outermost parts of the cells, and finally it may only be seen at their bases. Along with the fatty infiltration, an infiltration of finely granular or finely crystallized brown or brownish-red pigment, occurs as well in the epithelium as in the superficial layers of the areolar tissue, easily attributable to a post-mortem deposition; but this is disproved by the examination of animals quite recently killed. In them the fatty infiltration of the epithelium coincides with the later periods of digestion, and the decrease of the fatty contents in the hepatic cells. It may be a question whether the fat in the cystic epithelium be derived from the bloodvessels of the gall-bladder, but the correspondence between this epithelium and that of the intestines, the gradual departure of the fat from the free side of the cells, are adverse to the supposition, and in the best examples of fatty liver, the cystic epithelium is free from the infiltration. Portions of the fat separated by the bile from the liver, become reabsorbed and mixed with the general current of the fluids. Thus the gall-bladder has other functions besides that of a mere receptacle, and other substances, as the pigment indicates, pass along with the fat.

* Virchow's Archiv, Band ii. Heft 6, p. 574. † Gazette Médicale de Paris, Sept. 1856.

Part II.—Pathological Micrology.

TUMOURS, MORBID GROWTHS, EXCRESCENCES, &C.

Gelatinous Growth at the Base of the Cranium, at the Clivus Blumenbachii. By Professor H. Luschka.*—The case was that of a man, aged twenty-six, at the base of whose skull, the dura mater, corresponding to the point of union between the bodies of the sphenoid and the occipital bones, was thickened by the presence of some large and small, flattened and lobulated outgrowths, of the colour and consistence of the transparent mucous polypi of the nose. These had obviously projected from the interior of the bones at the spheno-occipital synchondrosis. These structures were seen under the microscope to be very uneven as to their surface, the smaller projections being hemispherical, the larger ones partly pedunculated and partly not so. In the jelly-like basis no traces of fibrillation existed, but a large number of finely granular nuclei, without any arrangements, of the diameter of 0·008 millemetre, and also a number of very peculiar large-sized cells. These latter were for the most part rounded and elongated, their contents being homogeneous or clear, apparently not fluid, but of a slimy consistence. They were doubly contoured, their covering being of variable thickness, and at the thickest part they were the seat of from one to two nuclei, exactly like those prevailing in the basis of the new formation. Besides the above mentioned masses, a mass of the size of a barleycorn was found in the interior of the bone, and communicating with them. This was inclosed in a capsule, and of the colour and form of the so-called gelatinous nucleus of intervertebral cartilage. Under the microscope this mass was seen to consist of a partly homogeneous, partly fibrous basis, much fat, and large cells of 0·012 to 0·024 millimetre broad, with partly single and partly laminated walls; many were nucleated, and many so surrounded by a variously thick layer of molecular substance, as quite to obscure the entire cell. Here and there beautiful clear laminated forms were seen; these forms reminding one of the remnants of cartilage between the sacral vertebræ of man. The author was uncertain how far any relation could be traced between these cartilage remains and the gelatinous mass, and proposes a query as to the origin of the mass found in the bone.

After making a number of investigations on the subject, he found that at times the cartilaginous disc between the occiput and the sphenoid does not entirely disappear with the termination of growth—i.e., it is not replaced by bone-tissue; but that the centre of the upper limit of its substance breaks up into a fibrous mass beset by cells. This continues throughout life, or dwindles later on, and there is formed in its place a cavity containing red bone marrow; or it is occupied by a blastema, changing into bone-tissue, which sometimes grows and leads to the formation of exostoses at the "clivus" of Blumenbach; or, finally, it may form the foundation of soft productions, which, as in the case above described, break forth in the cranial cavity, and grow in various ways.

The author comments upon a tendency to the breaking up of the cartilage between the bodies of the cranial vertebræ, and the formation of cavities, as a sequel of typhus.

A Case of Neuroma within the Spinal Membranes. By Dr. Ludwig Benjamin.†—It was that of a man, aged sixty, who had suffered pain in the lower limbs for seven years, preceding paralysis of the bladder. The spinal arachnoid was thickened and injected, and at the lower part was the seat, here and there, of bony plates. A tumour was found almost of the size of an olive, having several nerve-branches surrounding it, but only outwardly adherent, and some passing directly into the mass without reappearing from it, and not at all connected with the cord itself. The tumour was of a brownish-white colour, and enclosed by a firm thin covering of elastic and areolar tissue, being a projection of the neurilemma from the nerves as they entered. On section, the growth was seen to be in some places resisting, granular, and of an ochreous colour; in other places it was soft and medulla-like, and filled with cysts varying in size up to that of a bean. The softest parts were seen by the microscope to contain pus-cells, united by a hyaline amorphous substance, in which acetic acid revealed from one to two small nuclei. The harder portions possessed a stroma coursing in various directions, partly of elastic and partly of nerve-like fibres, with numerous large, round, hard granular cells. Some elliptical caudate forms existed, as also corpora amylacea and granular fat. The cysts were of various sizes, chiefly small and thin, very fine arachnoid-like membrane, and contained clear, yellow amorphous gelatinous fluid. Some of the cysts communicated with their neighbours, and so formed a system of cavities. Many large vessels existed in the tumour, and the darker-coloured parts showed granules and rounded conglomerates of crystallized pigment, evidently arising from extravasation. On tracing the roots of the nerves before their entrance into the growth, where they appeared to be quite healthy, they were

* Virchow's Archiv, Band xi. Heft, 1, p. 8. 1857. † Ibid., Band xi. Heft 1, p. 87.

found to contain coloured pigment, with cells like the above-described, and fat; and on reaching the tumour the cells were seen in numbers to exist between their fibres, whose final course was not clear; the fine fibrils, however, of the stroma appeared to be united with them. The peculiar interest lay in the fact that the tumour was a true neuroma, in the alteration of the nerve substance itself, and in the presence of the cysts.

Growth of Connective Tissue from the Semilunar Valve of the Pulmonary Artery and Pedunculated Epithelial Cells. By H. Luschka.*—The author remarks upon the comparative prevalence of connective-tissue outgrowths at the origin of the crescentic aortic valve-flaps on the inner side, which are villous to the naked eye. He observes, however, that these formations are but very rarely met with on the pulmonary artery valve-flaps. In the case mentioned, no trace of any heart disease existed. The specimen was removed from the body of a woman, aged forty. Near the corpus Arantii of each flap on the free borders, a short pedunculated body, of the size of a small pea, existed, whose upper surface was very cleft and occupied by fine branching whitish threads. Microscopical examination showed that these terminated in a number of rounded laminæ, each of which possessed a darker portion, an axis, and a clearer peripheric border-like segment. Each axis appeared to be composed of fine elastic so-called nucleus-fibres, whose origin from cells was obvious in places; and on the addition of acetic acid, a small quantity of structureless connective tissue was seen between these fibres. This formed the chief part of the broad border-like material, and exhibited a very delicate whitish granular appearance. Embedded in this stroma were numerous connective-tissue corpuscles variously branched, some of which were very like bone corpuscles. Many projections of these cells formed a network, and most of them held a well-seen nucleus with a nucleolus. Occasional projections of these cells were elongated into peduncles stretching out beyond the level of the surface of the leaf-like formations, and ended in a structure corresponding to an epithelial cell, being a cell-like nucleus-holding body, generally of a roundish shape. The author had formerly described, among the ordinary epithelium of the crescentic folds, cells elongated, thread-like, and projecting more or less far. The above-described called to the author's mind the ciliated conical epithelial cells which he found lining the canal of the spinal cord of the horse, and which were supplied with these pedicles.

SECRETING GLANDS.

The Lachrymal Glands, Hypertrophy of. By Mr. Savory,† Surgeon to the Royal General Dispensary.—The lachrymal gland was found after death to be developed into a tumour, three inches long, and two inches in breadth and depth, filling the orbit, and imbedding the globe of the eye, which was greatly sunken and flaccid. The mass of the tumour was uniform in structure throughout, soft, somewhat elastic, easily torn with the needle, the separated parts readily breaking up, mingling with water on section, and yielding a thick white opaque fluid by scraping. The microscope showed scarcely anything else but nuclei and clusters of lachrymal gland-cells, which were very easily broken up by manipulation. They were remarkably uniform in size and shape, and scarcely a trace of areolar tissue could be found amongst them. The tumour was removed after death from the body of a man, aged seventy-eight, who had suffered from it between two and three years, but without any constitutional taint or general ill-health. It was so large that shortly before death the contents of the orbit protruded in a mass like a small egg, the entire globe of the eye being concealed by it, excepting a small part of the cornea.

Liver, Cyst within, containing Ciliated Epithelium. By Dr. N. Friedrich.‡—The case was that of a man, aged twenty-eight, who died of pulmonary and laryngeal phthisis. The liver was dwindled, anæmic, the centre of the lobule being of a dark brown colour, owing to brownish-yellow pigment-granules in the centre of almost each hepatic cell. The intervening stroma was very thickened, containing most varied cell-forms with large projections and sharp contour. A white-looking projection from the surface near the suspensory ligament was seen, which, when cut into, was seen to consist of a cyst larger than a hazel nut, whose walls were formed of thick greyish-white membrane, connected with the hepatic substance by a spongy mass of areolar tissue. The contents of the cyst were a very tenacious yellowish-grey, almost gelatinous mass, which could be removed entire from the cyst wall.

The microscope showed the walls of the cyst to contain a network of elastic tissue and areolar tissue-corpuscles; also many bloodvessels, and a delicate, rather varicose system of canals, to be looked on as lymphatic vessels. The inner surface of the sac showed large num-

* Virchow's Archiv, Band ii. Heft 6, p. 567.
† Medical Times and Gazette, Feb. 21, 1857.
‡ Virchow's Archiv, Band xi. Heft 5, p. 466.

bers of roundish cells rather smaller than pus corpuscles, with nuclei; and towards the interior of the cyst these elements presented transitions into perfect ciliated cells, just like those found on the bronchial mucous membrane. These ciliated cells also contained yellow and yellow-brown pigments, but no fatty degeneration of them was seen.

The contents of the cyst, when acted on by acetic acid, coagulated in a striped direction, contained bodies which, by the presence of cilia, single, and still *in situ*, as well as by their conical form, were recognised as allied ciliated epithelium; they also contained oval and irregular, slightly angular bodies, which appeared to be the nuclei of epithelium, gradually removed from the inner surface of the sac, and mixed with the fluid, around which the cell-membrane, after diffusion of the contents, had collapsed so closely, that only a simple band with cilia remained, until at last the cilia also became destroyed, through each membrane being uplifted from the nucleus, on addition of water. The author here alludes to similar changes which he had seen in the bronchial epithelium of an old woman affected by chronic bronchitis. But besides the above elements in the cyst contents, a number, not large, of large round bodies existed, varying as high as 0·1 millimetre in diameter, with granular contents, clearing on the addition of acetic acid, and also a granular detritus, owing probably to the destruction of these latter cells.

The above-described cyst is considered by the observer to be most probably a dilated gall-duct, its walls agreeing thereto, as also from the fact that a branch of the vena portæ lay directly in contact. It might be a question whether it was congenital, or whether after birth it had originated owing to some local cause, and if so, the change in the character of the epithelium would be remarkable. As regards the congenital supposition, it is worthy of notice, that according to Leydig,* in some animals partly only during fœtal life, in part during the entire life, the gall-ducts possess ciliated epithelium, so it might be that in the fœtal life of higher animals similar epithelium might be found in the gall-ducts. Friedrich found, in the gall-bladder, and the large gall-ducts, of the three-and-a-half months' embryo of the ox, cylindrical epithelial cells, which had on their upper portion partly conical appendices and partly broad ridges, which latter had the appearance of being united cilia, or being about to divide itself into such. In other fœtuses he failed to find them, but he did so in a human fœtus of three or four months.

Virchow appends an observation to this paper, in which he says that Luschka had communicated to him a case lately, in which he had found papillary growths covered with ciliated epithelium in an ovarian cyst. A case had also been communicated by Virchow to the Obstetric Society, in which ciliated epithelium and nerve-tissue, &c., were found in the ovary. Virchow says he has moreover observed similar striped ridges on the epithelium of the gall-duct and bladder in the adult man.

NERVOUS SYSTEM.

Nerves in Degenerated Tissues. By F. Marfels.†—The author, for the purpose of observing the condition of nerves in degenerated parts, examined four cases, to ascertain the condition of the vagus nerve in pulmonary consumption and marasmus. He found that the fatty degeneration did not affect the nerves immediately, but that the development of cells preceded it. These cells are nucleus-holding, of the size of small colourless blood-corpuscles, which lie in the midst of the fibres, chiefly unaccompanied by any granular deposit. Potash, æther, and iodine effect no change in them. On one occasion, in examining portions of the ischiatic nerve, he found these cells situated inside the axis cylinders, thus establishing a correspondence with the discovery in some of the lower animals of the partly granulated and non-nucleated, and partly-transparent, clear, and nucleated cells in nerves. The author failed to observe nuclei in the sheath of the primitive nerve-fibres in the man; on the contrary, however, he found them in animals where he thinks he saw the sheaths occupied by fibres. On two occasions he observed the escape of nerve contents from the sheath, when at the same time the above-mentioned cells existed.‡

* Lehrbuch der Histologie. 1857. † Virchow's Archiv, Band xi. Heft 2, p. 200.

‡ We are compelled for want of space to postpone the remainder of the Report.

HALF-YEARLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, AND HYGIENE.

By Benjamin W. Richardson, M.D., L.R.C.P.

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I. Toxicology.

The celebrated toxicological cause of the past six months has been the Glasgow poisoning case. This instance of arsenical poisoning has been so fully discussed elsewhere, we may say everywhere, that we do not feel bound to more than mention it. We pass, therefore, to the reports of cases and papers given in foreign literature, the number of which is so great that we have difficulty in the space allowed to select those of scientific interest.

Poisoning by Arseniuretted Hydrogen.—Poisoning by the inhalation of arseniuretted hydrogen gas, although of comparatively rare occurrence, has occasionally resulted from accidents in chemical manipulation. In the case of Gehlen, a German chemist, the inhalation of a small quantity of this gas proved fatal on the ninth day. Another instance has been published by Dr. O'Reilly, of Dublin. A gentleman, for the sake of experiment, wished to respire one hundred and fifty cubic inches of hydrogen gas. Unfortunately the sulphuric acid he used in its preparation was largely contaminated with arsenic. His death took place on the sixth day. It was supposed that a quantity of arsenic equal to about twelve grains of arsenious acid was inhaled. Dr. Mouat has lately published a most interesting case of the same kind of poisoning, which, however, arrived at a favourable termination.

During the summer of 1851, whilst Professor Robertson of Calcutta, was exhibiting to his class, the application of Marsh's test, a pupil incautiously opened a window in front of the Professor, who at the time was standing with his back to an open door; the current of air produced had the effect of directing the gas, which was at that time being abundantly and rapidly generated, directly towards the lecturer. He soon became aware of a sense of burning and constriction in his throat, and was compelled abruptly to leave his class-room. Dr. Mouat visited him next morning, about sixteen hours after the accident, and found him labouring under the following symptoms:—Intense acrid burning sensation from the pharynx to the lower extremity of the alimentary canal, excessive irritability of the stomach, vomiting, first of the previously-taken food, then of the bile, ultimately of dark coffee-ground-looking matter, consisting of broken-down blood corpuscles and desquamated epithelium of the stomach and lower portion of the œsophagus, obstinate constipation of the bowels. There was severe deep-seated pain in the lumbar region; he voided between three and four pints of bloody urine, which on being analysed exhibited minute traces of arsenic. Symptoms of great constitutional disturbance were present; considerable fever; full, hard, frequent, incompressible pulse, dry, hot, unperspiring skin, intense restlessness, anxiety, and general uneasiness, a pale, anxious countenance, and considerable prostration of the vital powers. On the third day the bowels not having been opened, and a good deal of tenderness in the left iliac region, with a sense of weight and dragging in the fundament being present, the administration of a dose of castor-oil and laudanum produced a copious clay-coloured evacuation, with a tubular membranous-looking slough, somewhat ragged in appearance, and about four inches in length. This consisted of a portion of the lining membrane of the rectum, with a large amount of fibrinous exudation. For four days, under the exhibition of castor oil, small patches of fibrinous exudation continued to be discharged, the stools being scanty and deficient in bile. On the seventh day symptoms of acute hepatitis presented themselves. In addition to topical bleeding, small doses of calomel and soda were frequently administered, his system showing no symptoms of mercurial influence, to which when in health he was morbidly susceptible. In eighteen hours he was jaundiced. The mercury was discontinued, and diuretics and diluents substituted. On the twenty-second day, although he was considerably reduced, all distressing symptoms had disappeared. Dr. Mouat seems inclined to connect the attack of hepatic inflammation with the cessation of the hæmorrhoidal discharge which had taken place from the date of the accident, and of which the patient never experienced a return. The treatment adopted consisted in the first instance of leeches to the epigastrium, the administration of Murray's fluid magnesia, with ice and iced drinks. Subsequently, the abstraction of blood by cupping from the loins, the free exhibition of diluents, castor oil, opium, &c.

Dr. Mouat believes that the inhalation of minute quantities of arseniuretted hydrogen in a well ventilated room is not injurious to health. He is of opinion that this gas is disengaged from the surfaces of bodies injected with arsenious acid. Flies and insects are rapidly destroyed on alighting on them, yet persons employed in the dissection of subjects so injected, never exhibit any signs of arsenical poisoning.—*Indian Annals of Medical Science*, April, 1856, pp. 657–660.

Inflammation and Ulceration of the Sound Skin, produced by the application of a strong Arsenial Solution.—Dr. W. N. Brown, of Melrose, has recorded the case of a farm servant who was affected with inflammation of the skin of the lower part of the abdomen, the penis, scrotum, and upper part of the thighs, running on in some places to ulceration, consequent on exposure for two hours to the action of a solution of white arsenic. He had been engaged in washing sheep in a bath composed of white arsenic dissolved in boiling water, and his trousers had become saturated with the drippings from the sheep. The skin was nowhere broken. He was engaged in the work for nearly two hours, and on going home had immediately changed his clothes. In the evening he complained of pain and smarting, and the following morning the skin was red and inflamed. He had severe burning pain, and considerable constitutional derangement. It was a fortnight before he could return to work. The solution consisted of two pounds of arsenic, and a considerable quantity of soft soap to about fifty gallons of boiling water.—*Edinburgh Medical Journal*, August, 1857, pp. 148, 149.

Toxicological Effects of Carbonic Acid.—M. Wanner has communicated to the Academy of Sciences a notice of some observations made with the hope of arriving at a just conclusion as to the effects of inhalation of carbonic gas. After killing three guinea-pigs by inhalations of carbonic acid, the author examined under the microscope a small portion of the pulmonary tissue, the mucous membrane, and the subcutaneous cellular tissue. In these different specimens the capillaries were distended, of a blackish slate-colour; the blood was of the same hue. M Wanner asks if in these three cases death were not due to the same cause—that is to say, to the action of carbonic acid, which, brought into relation with the blood, as has been remarked by M. Lehmann, crystallizes the hæmatosine. He concludes from his experiment:—1st. That the heart's action ceases as soon as the movement of the blood is arrested, either in the pulmonic or systemic capillaries. 2nd. That what is called asphyxia is nothing more than a more or less complete arrest of the progress of the blood by an agent which, combining with one of its constituents, renders it no longer in a condition to undergo movement. It is not, therefore, the want of air which causes death in asphyxia, but rather the non-expulsion of carbonic acid from the lungs.—*Archives Générales*, April, 1857, p. 242.

On the Physiological and Toxicological Properties of Woorara.—M. Pelikan has communicated to the Academy of Sciences the results of his experiments on woorara poison. With regard to its physiological effects, the author arrives at the same results as M. Cl. Bernard. As to its toxicological action, M. Pelikan finds that an aqueous solution introduced into the stomach by an elastic tube produces poisonous effects, but more slowly and less energetically. This cannot be explained on the supposition that woorara contains a certain quantity of serpent-poison, for it is characteristic of nearly all the narcotic poisons which are easily absorbable. Curarine possesses all the active properties of woorara. Five centigrammes of the alkaloid introduced under the skin of a rabbit caused death, with all the symptoms of poisoning by woorara. When woorara is absorbed in a sufficient dose to produce death, there can be no question as to the antidote. Strychnia can provoke its peculiar symptoms only in the case where the dose of woorara has been insufficient, and *vice versâ*. Solution of woorara precipitated by tannin loses its effect in an ordinary dose, but in powder mixed with powdered tannin, and introduced into a wound, it preserves its poisonous action. The action of the poison is not destroyed by iodine dissolved in iodide of potassium, neither in the case of the two solutions mixed, evaporated, and the residue introduced into the subcutaneous tissue.—*Archives Générales*, April, 1857, p. 504.

Poisoning by "Meeta Bish."—A case of attempted suicide by a vegetable poison termed "Meeta Bish," is recorded by Mr. Baillie, of the Bengal medical service. The symptoms resembled those arising from hellebore, and some vegetable substance found in the chudder of the patient was stated by Dr. Falconer to belong to the species Veratrum. The man affirmed that he had mixed this with some other poison procured from a native vendor, and had taken them together.

The detailed symptoms were as follows:—When Mr. Baillie first saw the patient—a young man, aged about twenty, he supposed him to be in the collapse stage of the cholera. He was constantly purged and incessantly vomiting; the skin was cold and clammy; that of the fingers shrivelled, and the nails of a bluish hue. The vomiting, however, differed from that of cholera in not being free, and the ejected matter consisted of a thick tenacious mucus, with which the lips and mouth were covered, and which the patient seemed, as it were, to champ.

The pulse was hardly perceptible, the centre of the tongue was covered with a white, dry fur, its tip and edges were red. There was a sense of constriction of the fauces, with some tenderness over the epigastrium, and intense thirst, the gratification of which was followed by immediate retching. The countenance presented an odd expression, the eyelids were nearly closed, and he appeared to peep through the lashes. He stated that during the first two hours after taking the poison he perceived no uneasiness. The principal treatment consisted in the repeated application of sinapisms to the epigastrium and calves, together with hot bottles to the hands and feet, and in the administration of ammonia, in the form of liq. ammoniæ, gtt. xv., in water, every half hour, and subsequently in effervescing draughts of the sesquicarbonate with lemon juice. He gradually recovered, and was discharged at the end of a week.—*Indian Annals*, Oct., 1856, pp. 298–9.

Symptoms of Poisoning produced by Ergot.—The following case occurred to M. Trastour. A woman of lymphatic temperament, and very fat, had profuse uterine hæmorrhage after delivery of a still-born fœtus at eight months. Three *grammes* (forty-five grains) of ergot were given in six doses in the course of an hour; cold applications were also made to the abdomen and thighs. The hæmorrhage was checked and the uterus contracted, but in a few hours there were severe symptoms of disturbance of the circulation. The patient rolled constantly from one side to the other; her face was pale, her lips a little blue. The pulse in the radial and brachial arteries was imperceptible; the heart-beat was regular, not increased in frequency, but weak. The patient complained of prickling sensations, and of cold in her hands and feet; she said that she no longer felt her limbs. The extremities were really cold to the touch; the skin and the nails had a blue tint, both in the hands and feet; intellect was clear. These symptoms were combated by the alternate administration of broth and wine, then by some spoonfuls of opiate syrup given every two hours. All the alarming symptoms had disappeared in three days; convalescence was tedious, but perfect recovery took place.—*Journal de la Section de Médecine de la Loire-Inférieure*, and *Gazette Médicale de Paris*, July 25th, 1857.

Poisoning by the root of the Hyoscyamus Niger.—The root of henbane has some resemblance to that of the parsnep; and poisoning with it may the more easily occur, if it is used before or after the completion of its growth, when it is very fleshy, has a sweet taste, and contains a large amount of narcotic principle. At the beginning of May, 1855, the henbane in Sweden had not begun to put forth leaves; and at this time the following cases of poisoning by its use came under the notice of Hr. Sondahl:—

Late in the evening of May 6th he was called to a day labourer's family, who at midday had partaken of a quantity of soup made from some roots taken from their garden. The woman of the house, aged thirty-nine, had taken about a pint of the soup, and had eaten some of the root; her son, aged four years and a half, had drank about the same quantity of soup, but had eaten none of the root; and an old widow, aged sixty-nine, who resided in the house, had taken an entire soup-plateful both of the liquid and solid constituents of the soup. In about half an hour the woman of the house was seized with giddiness, a feeling of weight over the eyes, and headache. She soon afterwards felt weak in her legs, and the people with her in the room seemed to assume grotesque appearances. The old woman had also the same symptoms. The latter fell in attempting to go to her bed, but was able to rise and reach the bed. In a short time the boy began to complain of being unwell; he was very restless, and joined the women in laughter. This state of ridiculous excitement lasted in each of the patients about half an hour, and was succeeded by noisy and then by quiet delirium. In the old woman and the boy the symptoms appeared to increase; in the woman of the house some amount of intelligence returned, so that she was able to send for her husband.

Hr. Sondahl found the old woman sitting in a corner of the room, muttering to herself, and rocking her body to and fro; now catching at the air or at some imaginary appearance; then pulling about the bed-clothes, and answering questions either not at all or incorrectly. The pupils were widely dilated and immovable. The patient complained of intolerance of light, and shaded her eyes with her hands—not by closing the lids. In all three, especially in the boy, the eyes, which were rather staring, appeared larger than usual. The cutaneous sensibility was not diminished in the old woman; the motor power also, with the exception of the weakness of the legs and the constant rocking of the body, seemed unaltered, and there were no twitchings or other signs of convulsions. The circulation was somewhat quickened; the pulse was 100, tolerably full, but weak and equal. The respiratory and abdominal organs presented nothing unusual; but she afterwards said that her mouth felt thick, and that she had a disagreeable taste in it.

The boy was throwing himself about in bed, was incessantly screaming, and occasionally rubbed his hot head with both hands. At intervals he had twitchings in his arms and legs; the fingers were alternately extended and contracted. He momentarily looked about restlessly with staring eyes. The pupil of the left eye was much dilated, and was insensible to the influence of light, but there was no photophobia; in the right eye the iris was adherent to the cornea in consequence of previous inflammation. The state of the cutaneous sensibility could not be ascertained with certainty. The respiration was unequal, stertorous, generally with simultaneous twitchings of the limbs. The pulse was not hard, but frequent (144), partly in consequence of the violent movements. Nothing abnormal was heard in the digestive organs.

The woman of the house was walking about in her room, but somewhat unsteadily. Generally she answered questions rationally, but sometimes was a little confused. She complained of violent headache, especially over the eyes. She sometimes saw stars and sparks before her eyes, and had also peculiar illusions. All white objects appeared to her surrounded by rings or borders, in which yellow predominated. If she looked into a cup the edges appeared yellow, but the interior seemed as if there were small animals moving in it. The pupils were much dilated, but they contracted under the influence of light, though more slowly than usual. She complained of some noise in the ears, and had no inclination to sleep, nor had either of the other patients. The cutaneous sensibility was unchanged, and the gait was insecure, but the patient had perfect command over her limbs. The pulse was from eighty to ninety. The patient complained of a loathsome bitter taste in her mouth, but could swallow well. Her lips, tongue, and mouth appeared thick. She felt no thirst, nor any burning, or pain, in her mouth.

The old woman vomited after taking five grains of sulphate of copper, followed in ten minutes by ten grains of sulphate of zinc. The vomited matters contained half-digested portions of the root. When the vomiting had ceased, she took a teaspoonful of an aqueous solution of tannin, and half a cup of strong coffee. The boy, after a dose of four grains of sulphate of copper, vomited a quantity of fluid matter, but none of the root. He afterwards had some spoonfuls of solution of tannin, and a large quantity of strong coffee, and cold lotion was applied to his head. The woman of the house took coffee alone.

During the hour and a-half that Herr Sondahl remained with the patients, their state had so far improved that the screaming and tossing about were less violent. On the following morning the old woman was found asleep; she was easily awakened, and answered questions rationally, but soon fell asleep again. The boy had not slept, but had been tranquil, and said he was quite well. At times he still had slight twitchings, especially in the legs, and looked rather confused. The woman of the house had not slept, and had vomited copiously after drinking coffee. She complained only of slight pain in the head; the pupils were moveable, but still somewhat dilated; she at intervals had *muscæ volitantes*, and objects appeared to be strongly illuminated, and to have coloured edges. At midday all the patients were quite convalescent.

Herr Sondahl compares these cases with some others related by Schilizzi, and also with one described in the eighth volume of 'Hygiea,' which ended fatally. The principal symptoms in all were the same.—*Hygiea*, Band xvii., and *Schmidt's Jahrbücher*, Jahrgang, 1857, No. 7.

Colica Pictonum produced by the White-lead Treatment of a Severe Scald.—Dr. G. A. Kunkler relates the case of an Irish servant girl who severely scalded the fore-arm and hand. Extensive vesication followed. The blisters were punctured, and common white paint, of the consistence of cream, was freely applied with a camel-hair brush, the part was covered with cotton and a roller applied over the whole. This dressing was repeated on the following day. On the third day she exhibited unmistakable signs of colica saturnina—acute abdominal pain, retraction of the umbilicus, constipation, and slight discoloration of the gums. The symptoms yielded to opium and purgatives, and the linseed oil and lime-water dressing was substituted for the lead, under which treatment the burn got well. Dr. Kunkler states that he has freely used the white paint in a number of other cases, some of them of great severity, without meeting with a bad result. The Editor believes this to be the only well-authenticated case of colic resulting from the application of white lead to burns or scalds.—*North American Medico-Chirurgical Review*, July, 1857, p. 605.

Snake Bite.—Dr. Shortt records a case of recovery from snake bite, in which the administration of arsenic and ammonia appeared productive of good effect. The species of snake by which the injury was inflicted was not known. Abdool Rhymon, aged twenty-five, an active hale young man, in climbing a mahaddee, or toddy palm tree, for the purpose of cutting charrak for his elephant, was bitten on the right knee, at the middle and inner side of the patella, by a snake, which he says was twice as thick as his index finger. About twenty minutes after the occurrence he felt giddy, became deaf, and was unable to speak distinctly. His tongue felt thick and adherent to the roof of the mouth, and he staggered in walking. Dr. Shortt saw him thirty minutes after he had been bitten. He complained of coldness, and a sense of oppression at the chest. He had an anxious look, was tremulous, the skin was rough and contracted, but of the natural temperature and moisture; the eyes were congested, the pupils contracted to the size of a pin's head, and unaffected by light. The pulse was small and irregular. There was vomiting of a greenish coloured liquid. The principal treatment adopted consisted in the administration at short intervals of three draughts, each containing one drachm of liq. ammoniæ, one ounce of brandy, and twenty minims of laudanum; the application of a sinapism to the epigastrium, and of liq. ammoniæ to the wound, the latter being frequently repeated. After the third draught, although previously relieved, he complained of returning dyspnœa and oppression; he was restless, his countenance was suffused and his pulse 120, irregular. A mustard emetic was ordered, and after its action a draught, containing two drachms of liq. potass. arsenitis, which was to be repeated in half an hour. Brandy was also freely administered. The patient made a favourable recovery, and was discharged four days after the accident. Altogether he took eight ounces of brandy, three drachms of liq. ammoniæ, and two grains of arsenious acid. The brandy and ammonia appeared of use, but the mustard emetic and liq. potass. arsenitis seemed to relieve the oppressive symptoms at once.—*Indian Annals*, October, 1856, pp. 299-301.

Mortality from Snake-Bites in the Province of Sind. From Official Records, with a special Report on the Snake Season of 1854. By C. J. F. Imlach, M.D., Assistant-Surgeon.—In consequence of the great mortality occurring from snake-bites throughout the province of Sind, a letter, dated June 6th, 1854, was addressed to Government by the Commissioner in that province. This document stated that although during the cold months one case per month was the average mortality from this cause, a total of seventy-four deaths had been returned on the verdict of formal inquests during the whole period. In one week fourteen such verdicts had been reported. This great mortality led to a suspicion that deaths from other causes had been assigned to this agency, and it was supposed that the excuse of a snake-bite might be often invented to conceal the murder of a woman from motives of jealousy—a crime very common in Sind. The following facts negatived this supposition. The cases of snake-bites were not in the same proportion throughout the year. During the cold months, when the reptiles are torpid, few deaths occur; but in the autumn, when the young are produced, and especially after heavy rains, when they are driven from their retreats, deaths are most numerous. The number of male sufferers to that of females is just in proportion to their employment as outdoor labourers. The amount of mortality was much greater in low districts covered by grass and jungle, especially when the snakes were driven by inundation from their underground hiding-places to the spots where cattle and herdsmen congregate, the number of deaths rising and falling with the waters of the inundation. Careful police inquiries confirm the reported cases.

To remedy the evil, the Commissioner suggests that proper directions and remedies should be issued to the local authorities; that a reward should be offered for the destruction of venomous snakes; that a prize of 500 rupees should be adjudged to the best treatise in English on the poisonous snakes, on the mode of action of the poison, together with a code of practical instruction on the treatment to be pursued; and one of 250 rupees for the best translation of the code of instruction into Sindee or Persian. These suggestions have been accepted by the Government, with the addition recommended by the Medical Board, that the prize treatise should contain an account of the symptoms exhibited during life by the poisoned, with a description of the post mortem appearances.

Dr. Imlach's paper includes reports from the deputy collectors of several districts, together with extracts from his own report addressed to the superintending surgeon, Sind, on the snake season of 1854. The following are the principal points of interest:—

a. With regard to the varieties of poisonous snakes in Sind, correct information appears to be wanting. Mr. Young, deputy-collector at Sehwan, states, on the authority of a jogee or snake charmer, that the number of species is smaller than is generally supposed, and that, as is commonly affirmed by naturalists, the snakes with long tapering tails are all harmless. The kardar of Sehwan describes forty-three species, most of them poisonous. The deputy collector of Jerruck, on information communicated by the kardars, enumerates eleven species of poisonous snakes, giving a short description of each. Dr. Imlach has met with only four:—the black cobra, the kuppur (scytab byzonata), the loondee (blind snake), and the ghorel.

The cobra averages from two feet six to over three feet in length, the adult is jet black, the younger specimen of a leaden blue colour. All exhibit the white spectacle mark on the inner side of the hood. The bite of the cobra is very poisonous, and the venom is not exhausted by two insertions of its fangs. Dr. Imlach relates the case of a jogee who nearly lost his life by the bite of one of these reptiles, which had previously fairly bitten a dog twice. On introducing a dog to a cobra they each appear to act on the defensive, the snake does not bite until irritated, and the dog only seizes it as a last act of self-defence, catching it by the head, as if by instinct, shaking it as a terrier does a rat, and chewing it until he has rendered the cobra powerless. The cobra cannot withstand the poison of the kuppur. In its society the cobra appears as it were fascinated; on being bitten it shows no signs of pain, but in a few minutes its head and crest sink down, and it dies in less than half an hour.

The kuppur, which Dr. Imlach takes to be the scytab byzonata, and to differ only in colour from the West Indian snake of that name, is the most deadly reptile in Sind. The greatest number of deaths are produced by the bite of this species. Its average length is 15 inches, and it measures from 2 to 2¼ inches round the thickest part of the body. The head is covered with small scales of a flat, irregular, hexagonal shape; the neck is small in comparison with the head and body; the tail fines off abruptly, barely exceeds an inch and a half in length, and is very small in circumference. The expression of the kuppur is most ferocious, the eye brilliant, and however long he may remain unmolested, his crest will be found erect, and his head fixed in the direction of the person watching him, ready for the defensive. The forehead of the kuppur exhibits a cross in white and brown, the ground mark of the body is of a dull muddy grey, and the sides of the body are marked with black and white double crescents, the concavity of each being towards the abdomen. A fowl bitten by this snake dies in three-quarters of a minute, exhibiting symptoms similar to those observed in man, the mucous membrane is powerfully acted on, the head droops, the legs give way, and it rolls on its side in the last agony.

The blind or double-hooded snake is considered to be harmless. (The poison is not so dangerous.—Report of Deputy Collector of Jerruck.)

The ghorel is exceedingly venomous; it has the head covered with large plates. The specimens procured were under two feet in length, exhibiting white and black linear marks along the body on a greyish yellow ground.

β. Snakes principally frequent swampy districts, low-lying lands covered with grass, the banks of old canals, and rocky ground in the vicinity of flooded land.

γ. From the beginning of May to the latter end of October may be considered the snake season; during this time the snakes wander about, and the greater number of accidents occur.

δ. The parts bitten are usually the hands or lower extremities; no species of snake in Sind appears to possess the power of leaping to any height.

ε. The report furnishes but little new information with regard to the symptoms produced by this class of animal poisons. It would appear that the phenomena following the bite of the different varieties of snake differ more in degree than in kind. At first there is a great heat of body and headache; this is succeeded in two or three hours by drowsiness, which merges into complete insensibility. In all cases the patient is insensible before death. Swelling supervenes in from half-an-hour to three hours from the accident. The part bitten becomes black, or bleeds; the latter is considered by the natives a favourable sign. In some cases blood issues from the mouth, ears, and pores of the skin. Purging is considered favourable. The whole body in many cases changes in colour, becoming of a reddish, blackish, or yellowish hue.

ζ. With regard to the average of fatal cases, as compared with recoveries, out of 48 cases reported in the Jerruck Deputy Collectorate, 20 were fatal. This proportion, however, is probably much too high, owing to neglect in making returns of those cases which terminated favourably. Of 306 cases collected by Dr. Imlach, 63 were fatal, or 20·58 per cent.; 262 of these cases were males, and of these 53 died, or a per centage of 20.21. Of the 44 females bitten, 10 died, or 22·72 per cent.

η. *Remedies and their Effects*—Most of the native remedies are either perfectly inert, or have a purgative or stimulant effect. As specimens of the first class, we may note charms, the application of blood-stone to the wound, wrapping the person in fresh goat-skin, the application of the skin of the tail of a white cock to the bite, &c. Of the purgatives, the fruit of the peloo tree in large quantities of ghee appears to be the favourite. Black pepper, onions, and tooree-seed are also given internally, and assafœtida is administered in the form of snuff. The actual cautery is occasionally applied; sometimes also the patient is treated by a kind of water-cure, being wrapped in wet clothes. Of European remedies, ammonia is the one on which chief reliance seems placed, and very favourable results from its use may be extracted from Dr. Imlach's tables, for of 45 cases treated with ammonia only 4 proved fatal. —*Transactions of the Medical and Physical Society of Bombay for the years* 1855 *and* 1856, pp. 80–131.

Poisoning by Creasote.—Mr. Batho, surgeon to the 26th Regiment Native Infantry, records a case of poisoning from an over-dose of creasote. A stout young man swallowed half an ounce of the drug. The symptoms presented were complete insensibility, small and rapid pulse, quick and laboured respiration, attended with puffing of the cheeks and violent working of the alæ nasi, foaming from the mouth (exhaling a strong odour of creasote), a warm skin, flushing of the face, slight injection of the conjunctiva. There was neither vomiting, purging, nor convulsions. On the following day there was sympathetic fever, with pungent heat of the mouth, fauces, and œsophagus; no tenderness at the epigastrium, but little thirst, a foul tongue; and the mucous membrane of the mouth and fauces presented patches of erythematous redness. On the 3rd day there was a sense of burning in the chest, and difficulty of deglutition. These symptoms gradually subsided, and he was discharged on the fifth day. The principal points in the treatment were the repeated injection and withdrawal by the stomach-pump of albumen in large quantity, until the odour of creasote was no longer perceptible, and the combating of the inflammatory symptoms by leeching the throat, general bleeding, and the administration of demulcents, castor-oil in the form of enema, and a turpentine epithem to the thorax.—*Transactions of the Medical and Physical Society of Bombay, for the years* 1855–56. Appendix, p. 19.

II. Hygiene.

Notes on the Cape of Good Hope, its Climate, &c. &c.—Mr. Martin (Bengal Medical Service) comes to the conclusion that "with reference at any rate to the troops, the Cape of Good Hope is one of the most healthy of the British Colonies; and that, compared with even a European climate, it may be considered as favourable for British soldiers. The average mortality among the troops serving in the colony, during six successive periods, when they were not exposed to the risks and fatalities of active military service, only amounted to something less than 12 per 1000;" the rate of mortality of troops serving in Great Britain being about 15 per 1000. The Caffre war, with its attendant deprivation, exposure, and excitement, has entailed very serious losses, apart from the direct casualties of combat, which, however, cannot be put to the account of climate. The author observes that the amount of moisture in the atmosphere is never very great, the rain which falls is in amount inconsiderable, so that the fevers of all types are rarely met with; while the great and sudden transitions from heat to cold, and cold to heat, induce an unusual frequency of rheumatic affections, with their complications and consequences of heart disease. Accordingly, the diseases that predominate at the Cape are diseases of the heart, rheumatic affections, and functional disorders of the stomach. Head affections are not common. Chest affections are more so; in children, mild forms of bronchitis are not unfrequent; in adults, if pneumonia or any of the more acute affections of the lungs come on, they arise from neglect or extraordinary exposure. Diseases of the bowels are stated not to be common, but bilious cholera, diarrhœa, and dysentery prevail in the autumn. After some desultory observations about children's diseases, the author remarks that leprosy is not uncommon, in the form attended by loss of the joints, and intractable ulcerations; and then says, "There is a complaint peculiar to the Cape climate, called Zinkins, which is most painful and prostrating for the time, but not dangerous. It seems to be a severe form of influenza, but I do not think it occurs as an epidemic: it is characterized by painful swellings about the jaws, face, head, and joints, great lassitude, and nervous derangement." For the children of European origin, the climate seems to act like a hothouse, causing a rapid and precocious development; to Indian invalids, who often resort to the Cape as a sanatorium, it is apt to prove too relaxing; while the sudden changes of heat and cold, dryness and moisture, render it peculiarly trying to those who are endowed with weak lungs. "The cases which seem to have a chance of doing well at the Cape, are those in which the nervous system has become debilitated by residence in a tropical climate, and has induced the various forms of indigestion, disorder of the functions of the liver, bowels, &c."—*Madras Annals of Medical Science*, Oct. 1856.

Quarantine.—The minutes of the proceedings of the Quarantine Convention held at Philadelphia by invitation of the Philadelphia Board of Health, in May, 1857, is an interesting document. The Convention agreed, That the system of quarantine regulation should be revised. That the diseases small-pox, typhus, yellow-fever, and cholera may be introduced into a community by foul vessels and cargo and diseased crews, but that such diseases cannot become epidemic unless there exist in the community circumstances favourable for their development, independent of importation. That all parts of a vessel should be ventilated during a voyage, and that no vessel arriving between the 1st of May and the 1st of November should be admitted into port until her hold is ventilated and her bilge-water removed. That all portions of the cargo of a vessel capable of communicating a disease should be removed and purified. That all persons labouring under a disease should be removed immediately, and comfortably accommodated. That these provisions should be intrusted to a single officer, who should be a qualified medical man, and one competent and firm in the discharge of his duties. That a thorough examination should be made of all emigrants on their arrival, and that if they are not protected against the small-pox, they should be vaccinated. Lastly, the Convention recommends that there should be attached to quarantine establishments stations for meteorological observations and vaccination, and that records of these should be published for the public benefit.

Transmission of the Virus of Grease from the Horse to Man.—Drs. Maunoury and Pichot have published an interesting series of experiments tending to prove the identity of grease and cowpox. This doctrine, which was always maintained by Jenner, has received confirmation from the observations of Loy, Godine, and others.

The following is a summary of the facts related by Drs. Maunoury and Pichot. François Bathélemy B——, aged twenty-eight, of lymphatic temperament, a farrier, presented himself to Dr. Pichot on the 5th of March, 1856. He had not been vaccinated. On the backs of his hands, which were red and swollen, were several confluent opaline pustules, depressed in their centre, having all the appearance of vaccine pustules of the eighth or ninth day. The inflammation with which the pustules were surrounded had appeared on the second, the pustules themselves preceded the inflammation some days. This man had not been in contact with any cow, but on the 11th of February he had shod a horse suffering from grease. There existed at the time numerous cracks about his hands. The disease from which the horse was suffering was certified by a qualified veterinarian. On the 11th of April, circular depressed cicatrices occupied the situation of the pustules. Various inoculations were practised with the liquid taken from the pustules presented by B——, with the effect of reproducing the same disease. The most perfect set of experiments were made by M. Maunoury, who transmitted the virus through four sets of cases. The following are the results of his observations:—

1. That virus obtained from the hands of the farrier B——, and inoculated on the arm of an infant, produced a full pustule, having all the characters of a vaccine pustule—form, evolution, termination.
2. That lymph taken from this pustule and inoculated on the arm of three persons, has produced identical pustules, which are truly vaccine.
3. That the transmission of the virus by successive generations, has not diminished the intensity of the force of the poison. One of the set of cases presented large pustules, depressed in the centre, and filled with matter; each pustule served for several inoculations, and the charging of several sets of glasses.
4. That from these facts it is evident that the virus taken from the pustules of the farrier was identical with the vaccine.—*Archives Générales de Médecine*, April, 1857, pp. 365–398.

III. Miscellanea.

Death by Hanging.—Dr. E. Leudet publishes a case of asphyxia by hanging, in which death was the result. Examination revealed fracture of the two great cornua of the thyroid cartilage, tearing of the thyro-hyoidean membrane, and incomplete rupture of the epiglottis.

An English sailor was occupied in the rigging of a ship, when a spar broke under his weight, and in falling he was arrested by a rope, which held him suspended by the neck. When he was taken down he had not lost consciousness. He was carried, about an hour after the accident, to the Hôtel Dieu, at Rouen. The face was cyanosed, the conjunctivæ much injected, there was considerable dyspnœa, a full, strong, and frequent pulse. The rope had left a large bluish-red furrow round the neck, there was tenderness on pressure over the anterior part of the larynx, the voice was feeble but very distinct, respiratory murmur feeble, expectoration sanguinolent. He died at one o'clock the next morning. On examination thirty-two hours after death, the integuments of the whole body, but principally of the face, were found cyanosed. There was considerable ecchymosis and swelling about the neck, the two sterno-cleido-mastoid muscles were ruptured transversely, the right almost completely, the left through half its thickness. The great vessels and nerves had escaped. In front of the thyroid cartilage was a small cavity bounded by cellular tissue and the ruptured muscles, which communicated with the cavity of the larynx through a rent in the thyro-hyoid membrane. The body of the os hyoides was sound, its right greater cornu loosened. The two great cornua of the thyroid cartilage were fractured near their base, one was almost detached. There was an oblique crack on the front of the thyroid cartilage, also a transverse fissure on the posterior surface of the fibro-cartilage of the epiglottis.—*Archives Générales de Médecine*, April, 1857, pp. 479–481.

Fatal Results from the Sting of a Bee.—Dr. N. Nivison records a case in which death was apparently produced by the sting of a bee. A farmer in good health, aged about fifty, was stung by a bee at the side of the neck, on the 8th of August, 1856. He had been used to such accidents, which were always followed by considerable swelling and local inflammation.

On this occasion, though severe pain was momentarily felt, no swelling or discoloration followed. He removed the sting with his fingers. Two hours after he began to experience unpleasant sensations, and a degree of nausea, which was followed by free vomiting. Four hours after the accident, when Dr. Nivison saw him, he complained of nausea, was vomiting occasionally, and his breathing was somewhat oppressed and sighing. There was no trace of local irritation in the part stung. Dr. Nivison supposed that the poison had been speedily absorbed, or had entered directly the circulation. The next day the vomiting had continued, and diarrhœa was added. The countenance was shrunken, wan, and anxious. The pulse had lost much of its force and volume, but was natural in frequency. The tongue was covered with a dirty white fur, pale, soft, and flabby. These symptoms continued with but little relief from remedies; sleeplessness, and great restlessness, and jactitation were present during the whole of the attack. The patient sank, and died six days from the date of the sting. The remedies prescribed were sinapisms to the epigastrium, the free use of brandy and opiates, mercury in the form of calomel and hydr. cum cretâ, and quinine. The attack differed from any of the gastro-intestinal diseases peculiar to the season of the year, and Dr. Nivison's impressions were strongly in favour of the belief that it resulted from the cause specified. He thinks that had the danger been anticipated, by the free and early exhibition of powerful stimulants and anodynes, the result might have been averted.—*New York Journal of Medicine*, May, 1857, pp. 339–341.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.

Fellow of the Royal C llege of Physicians, Lecturer on Materia Medica, and Physician to St. Mary's Hospital.

I. *Embolic Apoplexy from Detachment of Fibrinous Coagula in Aneurism of the Carotid.* By Dr. Fr. Esmarch, Director of the Surgical Clinique at Kiel. (Archiv für Pathol. Anat. und Physiologie, Band xi. Heft 5.)

Captain C. H., from Sweden, consulted a medical friend of Professor Esmarch, concerning an attack of angina tonsillaris, and at the same time drew his attention to a tumour of the *left* side of his neck which had formed suddenly three years previously, without appreciable causes, and had now attained the size of a hen's egg. It occupied the upper triangle of the neck, was slightly diminished by pressure, and communicated a distinct thrill to the touch. It was at once diagnosed as an aneurism of the common carotid. On repeating the examination a few days later, and exerting pressure upon the tumour for the purpose of reducing it, the patient suddenly fell back with symptoms of apoplexy. He was at once bled and conveyed to the hospital, where he was placed under the care of Dr. Esmarch, on the 8th of May, 1855.

The patient was well built and robust; in a state of coma, from which he could only be roused momentarily; the pulse was moderately full, heart normal. The whole *right* side of the face was paralysed, the right cheek was distended in expiration; there were spasmodic movements in the facial muscles of the left side. The pupils reacted to the stimulus of light. The tongue, which was much furred, pointed to the left. Respiration was stertorous. The thoracic muscles and diaphragm acted well, but only the left abdominal muscles moved in respiration. Both right extremities were completely paralysed. Deglutition, defæcation, and micturition were normal. The tumour pulsated isochronously with the carotids, but presented no murmurs.

Professor Esmarch diagnosed the detachment of fibrin from the aneurismal sac, and a consequent obliteration of the left cerebral carotid. Ice was applied to the head, sinapisms to the legs, and an enema with vinegar was administered. Some improvement ensued; the paralytic symptoms diminished, and the patient was able to converse with a countryman; a relapse, however, followed, and on the 11th of May profound coma ensued; the pulse was very quick, the skin cool, the complexion livid, the right pupil was somewhat drawn out transversely, but both pupils continued to react to the light; both lips were distended by expiration; urine was passed involuntarily. The liver increased, the pulse became too quick to be counted, respiration slow, and accompanied by screams in inspiration; and death ensued at midnight.

The cadaveric examination was made the day after by Professor Weber.

The aneurism had a spindle-shaped form, and commenced about four centimetres above its issue from the aorta; the external and internal carotids quitted the upper end of the aneurism, preserving their normal size. The internal jugular vein was pushed outwards, the vagus lay between the vein and the aneurism and was unaltered, but the descending branch of the hypoglossus was adherent to the tumour, and much altered in appearance. The whole internal surface of the carotid from its origin was in a state of atheromatous degeneration, and contained enormous chalky formations; both below and above, a portion of the inner coat of the artery formed a projecting ridge in the aneurismal sac. The sac was partly lined with a smooth red membrane, partly with more or less firmly-attached, ragged, fibrinous coagula; much loose fibrin, irregularly interwoven, was also in the sac; a firm coagulum was drawn out of the internal carotid, which tore off from its continuation within the carotic foramen. There was no coagulum in the external carotid. Within the cranium there was found considerable hyperæmia of the vessels of the pia mater, a moderate effusion of serum under the arachnoid; the whole middle portion of the left hemisphere, including a part of the corpus callosum, was converted into a pulp of a greyish-yellow colour. Beneath the aquæductus Sylvii, in the mesial line of the pons Varolii, was a perfectly recent extravasation of blood of the size of a bean; a smaller one, a centimetre in front of the former, and in the vicinity several small capillary extravasations. Normal cerebral tissue could not be discovered in the softened portion; it consisted of granular matter and short fragments of broken-up fibres, with capillaries containing shrivelled corpuscles. In the extravasations at the pons, the blood corpuscles were unaltered. The cerebral carotid, the arteria fossa Sylvii, and the arteria ophthalmica, were completely blocked up with coagula of a dark-brown colour, inclosing numerous red and greyish-white plugs, which evidently were derived from the aneurism. Their identity was proved by the microscope. The thoracic viscera presented no marked disorganization, except that the ascending aorta exhibited extensive atheroma; the same was the case with most of the large arteries.

Professor Esmarch, in his concluding observations, dwells upon the danger of much manipulation of aneurismal tumours, as being liable to give rise to such consequences as those above described. He particularly discusses Mr. Fergusson's mode of treating aneurism of the subclavia, recently brought before the Medico-Chirurgical Society, which consists in forcing the coagula contained in the tumour into the axillary and brachial arteries.

II. *On Œdema Glottidis.* By Professor Pitha. (Prager Vierteljahrsschrift, Jahrgang xiv., 1857. Band liv.)

In an interesting article of considerable extent, on the whole subject of tumefaction of the glottis (which applies rather to the epiglottis and the ary-epiglottic folds than the true glottis), we find the following instance of acute œdema, assumed to have been brought on in a healthy person by an article of diet.

Madame Grabinger, a robust ruddy woman, aged thirty, on a hot summer's day felt poorly and faint immediately after her dinner, which she had enjoyed in perfect health. She lay down on the sofa and went to sleep quietly. After about half an hour the children observed that their mother breathed laboriously and noisily, so that they roused her. She awoke with difficulty; her face was distorted, pale, and swollen; the voice hoarse; the respiration became more laborious; the patient complained in broken words of constriction of the throat, and a sense of suffocation. A few minutes later, Professor Pitha found the patient wringing her hands in a state of most distressing dyspnœa. The features pale and frightfully swelled, and the whole neck alarmingly swollen. The voice had entirely disappeared, and the patient implored assistance in the most painful manner, constantly pointing to the larynx. The Professor could obtain no clue from the children, but on looking round the room he perceived a plate with a few strawberries; on pointing to them interrogatively, the patient nodded violently, evidently gratified by his suspicion. He at once prescribed an emetic of tartrate of antimony, which put an end to the symptoms as speedily as they had supervened. After the first spoonful copious vomiting ensued, accompanied by a discharge of masses of mucus, upon which respiration at once became easy, and voice and speech returned. The tumefaction of the face and the other symptoms gradually disappeared, and on the following morning the patient was quite recovered. Professor Pitha analyses the symptoms, and while he admits that the presence of œdema was not absolutely proved, he maintains that all rational signs of the affection showed it to be there.

III. *A Case of Perforation of the Interventricular Septum.* By Dr. Oulmont. (L'Union Médicale, No. 95.)

A needlewoman, aged sixty-nine, was admitted into the Lariboisière Hospital on March

16th, 1857. She had for several years been subject to dyspnœa on going up stairs and walking fast, but had suffered from palpitations only for the last two months; on admission the palpitations were severe. The patient lost her breath on the slightest movement; there was cough without constant expectoration. There had been œdema of the legs since the palpitation; the appetite was poor, and she had diarrhœa for some days previous to admission. The complexion was purple, especially the cheeks, the nose, and the lips; the patches of colour were seen to be made up of dilated vessels; the tips of the toes and fingers were of the same hue. There was no enlargement in the region of the heart; the impulse was of medium intensity. There was a distinct frémissement cataire throughout the precordial region; a very strong rasping bruit replaced the first sound of the heart, occupying even the entire interval; the second sound was dull and indistinct. The abnormal sound was audible throughout the entire precordial region, but was most intense at the base. The pulse was feeble, small, compressible, and not intermitting. No marked change took place, and the patient died on the 10th of April.

The parts that were cyanosed during life remained so after death. The pericardium contained from sixty to eighty grammes of a reddish serum; the surfaces were intact. The anterior surface of heart showed to the left of the interventricular fissure a round projection, three centimetres in diameter, offering to the touch the sensation of fluctuation. An incision carried through it entered the left ventricle, which was full of black clots. On removing these, a species of entonnoir was perceived on the side of the interventricular septum, at the bottom of which was an opening one centimetre broad. On opening the right ventricle, the fact of a large communication existing between the two ventricles was confirmed; it occupied the most anterior part of the septum, near its upper third. At the left orifice there were a few warty excrescences. The parietes of the left ventricle varied much in thickness; at the left edge of the heart it was fifteen millimetres, on a level with the point which was thinned it was only three millimetres in thickness. The other cavities of the heart presented no abnormity of any importance; the arch of the aorta contained a cretaceous patch. Except fatty degeneration and congestion of the liver, and hypostatic congestion of the lungs, the other organs exhibited no abnormities.

IV. *Case of Cirrhosis of the Liver and Bronzed Skin.* By CHARLES FRICKE, M.D. (North American Medico-Chirurgical Review, July, 1857.)

An Irish laborer, aged twenty-five, was imprisoned in Baltimore, in April, 1856, at which time he appeared in good health, except that the skin of his hands was in a hypertrophic condition. On the 1st of September following, Dr. Fricke found him failing in strength, suffering from nausea, tympanitic distension of the epigastrium, more or less headache, and constipation, but without fever. He improved after a few days' rest, but at the end of the month was attacked with jaundice, and oppression in the region of the liver. The symptoms abated under treatment, and recurred from time to time. In December, the jaundice was universal, without perceptible enlargement of the liver or derangement of the digestion. "As the case progressed, the principal phenomena exhibited were dropsy, hæmorrhage, black urine, and bronzed skin." The fæces were hard, rounded, white balls for the last six months before death. "The bronzed skin, the most interesting phenomenon, and the one to which I wish to direct attention more particularly, was first remarked in January. At the commencement it was not well defined, nor more decided than is often observed in cases of cirrhosed liver; but after a time the tinge became so deep as to attract decided attention. It was limited to the forehead, face, and neck, and almost as deep in hue as is found in the livers of remittent fever patients. One fact is worthy of notice. The demarcation between this colour and that of the rest of the body was not clearly defined; but they merged into each other by degrees. He died April 29th, 1857, after a convulsion." The abdominal cavity was found to contain about a gallon of serous fluid, and the intestine was filled with dark liquid blood. The kidneys were congested and somewhat large, but otherwise healthy; the supra-renal capsules of normal size, hue, and consistence, presenting no alteration whatever. The liver, one-third smaller than usual, was of a bronze hue externally, with an irregular nodulated surface. The cut surface was pea green, and examined by the microscope, showed much hypertrophied fibrous tissue, and cells filled with "the same" colouring matter.

V. *A Knitting-needle found in the Liver.* By Dr. LANGWAGEN. (Archiv für Physiologische Heilkunde, 1857, Erstes und Zweites Heft.)

The following case occurred in the hospital of St. George, in Leipsic, under the care of Dr. H. Clarus:—

Mrs. R. O., aged forty-six, married, was brought to the hospital on the 2nd of September, 1856, in a state of maniacal delirium; she had never been previously ill. The excitement and sleeplessness continued; she took no food, gradually lost strength, and wasted away; diarrhœa ensued, and she died on the 29th September.

Post-mortem.—The dura mater thickened, adherent to the pia mater at several points; about three ounces of serum escaped on opening it. The superficial veins were much congested; the cerebral tissue presented a reddish hue on section, the ventricles contained about half an ounce of clear serum, the choroid plexuses were beset with cysts. The lungs œdematous, the lower lobe of the right lung presented hypostatic congestion. The liver was generally normal, but the left lobe was a little atrophied. At the point of juncture of the left and right lobes there was a white callous cicatrix, which followed the longitudinal direction of the furrow intervening between the two lobes. The cicatricial tissue was three inches and a quarter long and two lines broad, very dense, and contained two inches of a knitting-needle, which was so closely invested by the tissue that it was difficult to separate them. The needle, after separation, was found to be much rusted, and only at one point round and smooth. Almost immediately beneath the needle was a large but healthy vein. Neither on the abdominal surface, nor in the stomach, nor in the liver itself, was any trace to be found of a former injury. The friends of the patient were unable to give any information on the point.

VI. *On Indian Fevers.* By J. B. Scriven, Esq., First Assistant Surgeon, Presidency General Hospital. (Indian Annals of Medical Science, April, 1857.)

It is commonly taught that in India, and in tropical regions, typhus and typhoid fevers are unknown. In a valuable paper on the subject of the fevers occurring in India, Mr. Scriven disproves the correctness of the doctrine, by giving several cases and post-mortems. As the question is one of importance, we proceed to give a brief summary of each of the cases reported.

1. A. B., European, had bilious purging on November 17th, 1851, attributed to drinking beer, and afterwards playing at cricket. It continued to the 19th, when there was slight irritative fever, much weakness, and undefined pain in the head; 20th: diarrhœa during the night, motions watery and bilious, much weaker, pulse weak and quick, tongue white and irritable at the edges; 21st, 5 A.M.: pulse frequent and irritable, considerable heat of head, general heat of surface, pupils slightly dilated, considerable restlessness, tongue dry and furred. Delirium supervened, the diarrhœa continued, and death ensued on the 22d. Much meningeal congestion, subarachnoid and interventricular effusion, softening of cerebellum, lungs congested, great injection of stomach and duodenum. Patches of ulceration in Peyer's glands, particularly in lower portion of intestine, involving extensively the upper surface of ileo-cæcal valve. The mucous membrane of the large intestine was injected, but not ulcerated.

2. B. C., aged eighteen, European, January 12th, 1853: tenderness in right iliac fossa, purging, tongue clean. Skin rather hot, slight headache, improvement. June 15th: more fever, tongue furred, tendency to delirium, no eruption, abdominal tenderness. Tongue became dry and brown, he vomited everything, symptoms continued, death on eighth day. Ilium much injected, solitary and Peyer's glands much raised, white; the latter had lost their characteristic pits and ridges; the disease less marked in approaching jejunum; mesenteric glands enlarged, spleen and lungs congested.

3. C. D., European, aged twenty-one, July 2d, 1853: Post-mortem only given, but symptoms stated to have resembled those of No. 1. Slight traces of peritonitis, great vascularity of mucous membranes of stomach. "The first one or two of Peyer's patches showed no disease; further on, a few white spots, like solitary glands enlarged, were seen on the surface of the patches; lower down, the agminated glands became more distinctly diseased, being surrounded by inflamed mucous membrane, which here and there assumed a greyish colour. Still further down, they appeared very considerably raised, of a dirty white colour, having protuberances on their surface, and little pits scattered over them." In the lower half of the ilium the most elevated points of the agminated glands showed signs of mortification. There were many ulcers, with partially separated sloughs, on the upper surface of the ileo-cæcal valve, where, as well as on the under surface, they caused an appearance of transverse ridges. There were some ulcers in the colon; spleen gorged. Nothing of consequence in other organs.

4. E. F., August 5th, 1853: Ill twelve days, violent delirium, rose spots, no diarrhœa, a doubtful case; nothing found after death but engorgement of liver, spleen, and encephalon, with slight elevation of the lower, solitary, and agminated glands.

The last three cases were all taken ill within about forty-two days, and belonged to the same regiment. Mr. Scriven, while attending them, suffered from slight febrile symptoms, lassitude, headache, loss of appetite, and diarrhœa, which he attributes to the same poison.

5. G. H., spleen engorged, other organs healthy, excepting ilium; 3½ inches from cæcum showed solitary and Peyer's glands raised, hard, and surrounded by vascular zone; this condition increased downward; irregular ulceration about two feet from cæcum, at the highest point of the enlarged glands, of a greenish colour. Three of Peyer's patches ulcerated over their whole surface, and partly covered with a greenish slough. A few ulcers in the cæcum.

6. J. K., aged seventeen, French sailor, May 6th, 1856, had been sick fifteen days with headache, vertigo, and pain in belly (diarrhœa); pulse 120, thirst, furred tongue; on admission slight tenderness in iliac fossæ. On the 8th he became delirious, the delirium continued for above two days; rose-spots appeared on the 10th; the bowels became relaxed, and the tongue black and dry. On the 16th there was decided improvement, no fresh spots had appeared, and there was no delirium. The treatment consisted mainly in the exhibition of quina, opiates, and port wine; with slight variations convalescence went on favourably, and the patient entirely recovered. On June 4th he is reported perfectly well; with an enormous appetite, the bowels open once in twenty-four hours, and stools not perfectly solid.

7. L. M., aged seventeen, sailor, January 5th, 1857; has had diarrhœa six days, little vomiting, no appetite, headache, tongue white, thirst. The tongue became brown; the abdomen full, hard, and tympanitic. Intellect confused; urine albuminous, sordes on lips, pupils dilated. On the 9th the countenance was vacant, there was great prostration, pulse 160, slight tenderness in right hypogastrium, drowsy and stupid. After this, gradual recovery for a few days, when erysipelas and death supervened. The small intestine exhibited "the first two or three Peyer's patches (proceeding from above downwards) healthy, the next three or four were congested and raised, of a dusky purple colour. Below this the mucous membrane itself was inflamed, and Peyer's patches ulcerated. The solitary glands were likewise inflamed, and many of them ulcerated. The ulcers involved the whole of the surfaces of the patches, there was no slough present upon any of them, the ulcers were of a purple colour." The corresponding mesenteric glands were enlarged, and deep purple. The kidneys had numerous white granules scattered through the cortical substance; the lungs showed the remains of pleurisy and "lobular pneumonia;" the other organs were healthy.

In addition to the above cases, the author adverts to three cases of "typhus with bowel complication," which occurred in the practice of Mr. Payne, the details of which are not, however, given.

Mr. Scriven terminates his paper by brief remarks on the differences between the malarious, the ephemeral, the sun, and typhoid fevers of India, and on the treatment of the last-mentioned form. Upon this he remarks, that having no antidote, as for malarious disorder, "we must be content to sustain the patient's strength by wine, stimulants, and tonics, soothing the irritability of the intestines by chalk mixture with opium, and fomentations and poultices to the belly, quieting delirium by small doses of morphia, and treating local inflammations when they arise, though as a rule, not by depletion."

VII. *Puerperal Fever and Erysipelas.* By J. Levergood, M.D. (The North American Medico-Chirurgical Review, July, 1857.)

Although the relation between puerperal fever and erysipelas is an established fact, it is useful to accumulate evidence, and remind practitioners of the danger incurred by losing sight of it.

There was no puerperal fever at Wrightsville, in Pennsylvania, where the writer practised, nor in its neighbourhood, when, in 1853, Dr. B. C. Lloyd was called to attend a man whose entire left arm was affected with phlegmonous erysipelas. While this patient was under treatment, Dr. Lloyd was summoned to Mrs. D., in labour with her third child. "This lady lived more than a mile from town, in what is known as York Valley, renowned for its beautiful farms, fertile soil, and wealthy inhabitants, and proverbial for its healthfulness." There was no epidemic in the locality, and the lady was in excellent health. The labour was normal and easy. On the third day puerperal fever set in, and death ensued on the fifth. A second female, who lived a mile from the former, was attended by Dr. Lloyd in her sixth accouchement about the same time. The labour was normal, and the patient healthy: fatal puerperal fever ensued on the third day. The third case occurred in a farmer's wife who lived five miles from Wrightsville, and was safely delivered of twins. This succession of cases of puerperal fever, limited to Dr. Lloyd's practice, satisfied him that they were due to contagion, of which he was the bearer from the erysipelatous patient. He accordingly ceased attending upon the man, and no further cases of puerperal fever occurred.

VIII. *Report on a Memoir entitled, 'On Gangrene of the Extremities in Typhoid Fever, by Dr. Bourgeois.'* By Dr. Behier. (L'Union Médicale, June 13–16, 1857.)

Messrs. Barth, Hervieux, and Behier were commissioned to report upon Dr. Bourgeois' paper, which was presented to the Société Médicale des Hôpitaux, and was based upon two cases of typhoid fever followed by gangrene. The first was a young woman of sixteen, generally enjoying good health, who, after an indisposition of about a fortnight's duration, which was regarded by the author as slight typhoid fever, was seized with sudden pain in the right leg. The limb became cold, upon which the pain ceased. There was no general tumefaction, nor any along the vessels. The limb first became grey, and then of a coppery red, with purple spots. A circular boundary formed at the upper third of the calf: the limb separated, and the patient recovered. The second case occurred in a child, aged eleven years, who, while typhoid fever occurred in her village, was seized with symptoms of typhoid. During an apparent convalescence, sudden pain supervened in both legs, without any tumefaction. The limbs became cold and discoloured, as in the first case, and a line of demarcation formed; but erysipelas supervened, and the patient died. In neither case was a post-mortem examination made—a circumstance much regretted by the commission. The latter regard the symptoms preceding the gangrene as erroneously attributed to typhoid fever, and only a part of the disease which led to the gangrene. They observe that all cases of acute gangrene which are recorded by writers were preceded by such symptoms as those mentioned by Dr. Bourgeois, and then minutely enter into the literature of gangrene dependent upon arterial obliteration.

They sum up their general conclusions on this point as follows:

Arterial obliteration is the most frequent cause of spontaneous gangrene of an entire member.

It may be complicated with venous obliteration, in which case the gangrene is accompanied by œdema of the member.

When arterial obliteration exists alone, the gangrene presents the dry form.

Arteritis is one of the most frequent causes, if not the only one, hitherto proved to give ri se to obliteration of the arteries.

Arteritis induces general symptoms, which vary much, but are generally analogous to typhoid fever. These symptoms, however, have never yet presented a sufficiently definite character to justify the assumption that spontaneous gangrene may thus be developed in the course of genuine typhoid fever.

If Messrs. Barth, Hervieux, and Behier do us the honour to peruse the 'British and Foreign Medico-Chirurgical Review,'* we would specially direct their attention to the recent article, On Senile Gangrene, by Mr. Lee, which may possibly induce them in a measure to modify their views in regard to the cause of such gangrene depending upon arteritis. The views of Mr. Lee find a strong corroboration in the above two cases.

IX. *On the Determination of the Amount of Albumen contained in the Urine, Serum of the Blood, and Exudations, by means of the Polarizing Apparatus of Ventzke-Soleil.* By Dr. F. Hoppe. (Archiv für Pathol. Anat., &c. Band xi. Heft 6.)

The quantitative determination of cane sugar by the rotation of the plane of polarized light in an acknowledged fact in technical chemistry: the circular polarization of albuminous fluids is stated by the author to present but little more difficulty. Bouchardat and Becquerel were the first to attempt the calculation of the amount of albumen contained in the animal fluids by this method. The apparatus employed by Dr. Hoppe is a modification of Soleil's polariscope by Ventzke, intended specially for the rapid measurement of the amount of sugar contained in diabetic urine. The first question—whether the rotation of the plane of polarization bears a definite ratio to the amount of albumen contained in a liquid?—is answered absolutely in the affirmative. The second question was, as to the relation of the rotation produced by a solution of albumen to that of a solution of sugar, both solutions being of equal strength? The answer afforded by the investigations of the author is, that the albumen contained in a fluid rotates the plane of the polarized light almost as much to the left as a similar per-centage of grape sugar rotates it to the right. Accordingly, a scale attached to Ventzke's apparatus, which enables the observer to read off the weight of grape sugar contained in 100 cubic centimetres of fluid, also allows a direct determination of the weight of albumen.

* July, 1857, p. 162.

If fluids that contain albumen, and whose rotation has been determined, are allowed to stand for several days, they become opaque, and the rotation is diminished: in many albuminous urines the former rotation, as well as the clearness, may be restored by the addition of a drop of acetic acid. This could not be done with serum; but the author has never observed an alteration in the rotation of an albuminous solution within from two to three days. An allowance must be made in this mode of determining the amount of albumen in a liquid where much soda is present, as it was invariably found that under these circumstances too high a result was obtained.

Dr. Hoppe sums up his conclusions thus: 1. The method proposed is easily executed with albuminous solutions which are not too dark in colour. 2. It possesses no greater sources of error than those presented by the chemical determination. 3. Turbid fluids may be rendered clear by the addition of small quantities of soda or acetic acid without impairing the result; but if the soda be added in excess it will be too high. 4. The rotation with the albumen is nearly as much to the left as with sugar it is to the right, so that a scale may be constructed from which, without further calculation, the amount of albumen or grape sugar contained in 100 cubic centimetres of liquid may be at once read off.

X. *Hæmophilia.* By Dr. Magnus Huss. (Archives Générales de Médecine. Août, 1857.)

Hæmophilia, a term which is scarcely naturalized among ourselves, was first employed by Grandidier to designate that peculiar tendency to hæmorrhage which characterizes some individuals and families. The following case, which is reported and commented upon at length by the well-known Professor Huss, of Stockholm, is almost unique of its kind.

Maria K., a servant, aged twenty-three, a country girl, the child of labouring people who had always enjoyed sound health, and exhibited no hæmorrhagic tendency nor other hereditary taint, was admitted into the Seraphim Hospital of Stockholm, January 16th, 1851, of good complexion, healthy looking, and well built. Catamenia appeared without abnormal features at the age of fifteen; had never been ill, excepting convulsive fits in her infancy; there had been no remarkable tendency to hæmorrhage, but whenever she had met with an injury the parts had cicatrized as in any other person. She went into service when nineteen years old, and was maltreated on the 4th August, 1850. She was severely buffeted, and struck about the head, in consequence of which she was much excited and seized with convulsions, during which she screamed and struck her head against the surrounding objects; she was insensible for about half an hour. On recovering herself she found that she had bled profusely from the head, without being able to find any lesion of continuity. She continued in a torpid state during the succeeding three days; she merely remembers that the hæmorrhage persisted, and that blood also flowed from the eyes, the left ear, and that she vomited blood. She continued very feeble after this time, the hæmorrhages recurring almost daily. They ceased after two months, when she recovered her health for a fortnight; without any cause, the hæmorrhage then returned one night from the cranium, and at the same time she vomited coagulated blood. The hæmorrhage from the cranium continued for a week, then stopped, and the patient was well for two months. After that, the hæmorrhage recurred every eight or fourteen days from the cranium, eyelids, and left ear. When examined after her admission, no traces of a present or previous solution of continuity could be traced on the cranium; there was no trace of injury to the bones. The hæmorrhagic attacks continued to occur on the slightest emotion, but without special symptoms, except on one occasion, when, at the commencement of copious hæmatemesis, the patient was seized with violent delirium, then lost consciousness, and remained for eight days in a state of profound torpor. She then woke up, and the two left extremities were slightly paralyzed, and their sensibility somewhat blunted. After the lapse of three weeks these symptoms had entirely disappeared.

In March, 1852, she is described as being anæmic, pale, somewhat emaciated, and depressed, but the functions otherwise in normal condition; the kidneys, liver, sexual and other organs apparently healthy; emotion so directly influenced the occurrence of the hæmorrhage, that the patient by entering into a dispute could generally produce it at will. When it took place, she felt so fatigued as to be forced to lie down; the hæmorrhage occurred on each side of the coronal suture, on a space three centimetres by ten. The blood was seen to sweat out at the roots of the hairs, first forming a red point, which gradually augmented to a drop which coalesced with others; if wiped off, the surface would soon be again covered with blood; its colour was that of arterial blood. Examined by a lens, no lesion could be detected, but the blood could be seen issuing from the hair follicle; the root of the hair was not diseased. The hæmorrhage lasted from a few hours to two or three days; the skin of the bleeding part was not tumefied, but slightly tender, and its temperature elevated. The attack was generally preceded for one, two, or three days, by a sense of weight on the head and vertigo, with a feeling of heat and pulsation at the spot immediately before the bleeding occurred. Hæmatemesis and bleeding at the left ear were only occasional concomitants of the attacks; but the former was always accompanied by stupor, alternating with delirium.

The treatment, which consisted in the administration of tonics and astringents of all kinds, with nutritious diet, only caused temporary arrests of the hæmorrhage. Strange to say, that in spite of the peculiar tendency to hæmorrhage from certain parts, there was no predisposition to it from others, for contusions and solutions of continuity were not followed by unusual hæmorrhage. On the application of local astringents to the head, such violent symptoms of cerebral congestion were manifested as to render venesection necessary.

All treatment failing, the patient was sent home in the same state in which she was admitted. The details of this interesting case are followed by an analysis of the theories applicable to the hæmorrhagic tendency, and Professor Huss arrives at the conclusion that it is due to spasmodic contraction of the veins passing from the capillary network of the parts, in which this form of hæmorrhage was manifested.

QUARTERLY REPORT ON SURGERY.

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

I. *On the Treatment of Club Feet.* By Dr. Brown. (Boston Journal, vol. lvi. pp. 89–94.)

In this paper, Dr. Brown lays down some practical rules derived from eighteen years' attention to this branch of practice. He observes, that although the division of tendons, fasciæ, &c.—the contraction of which keeps the foot in its abnormal position—is a requisite preliminary, it constitutes but a small portion of the cure. The subsequent treatment is difficult and tedious, requiring, upon an average, two or three months' daily attention and manipulation. This treatment must be attended to by the surgeon personally; and the author has always had cause for regret when he has trusted it to another, finding that all his experience and daily attention are none too much for the cure of a bad foot. It is a great error to content oneself with dividing the tendons, and then handing the patient over to the care of his friends and the machinist, it being very doubtful whether a cure was ever so effected. Many of the cases that have come under Dr. Brown's care had been already operated upon by other surgeons, and so left.

When a patient is old enough, and has used his foot much, Dr. Brown first advises rest. He then divides all the tendons and fasciæ that keep the foot in its false position, employing for the purpose a very small tenotome, its cutting portion being less than half an inch in length, while its shank, 1¼ or 1½ inch long, is very small and round. When the cutting edge is too long, the slight sawing movement during the section of the tendons may cause unnecessary division of the skin, leaving the external orifice too large, and notched. Only a drop or two of blood issues from the punctures, which are always healed next day. Court-plaster or small cotton compresses are applied to the punctures, a bandage is passed around the foot and ankle, and the apparatus is then applied—the author preferring its immediate application, owing to the support it affords, but taking care not to strap it too tightly. The patient is seen again in the course of the day and next day, and on the third day the apparatus is removed; and the foot, having been washed either in water or bay-rum (which keeps the skin healthy), is replaced. Great care is to be taken that the apparatus cause no irritation of the skin; and if excoriation does arise, the bay-rum should be used, and an apparatus having a different bearing employed.

The first object Dr. Brown has in view is to bring the foot out on a line with the leg, and turn in the ankle. This done, and the foot remaining in its new position without artificial aid, he next attempts to bring it up towards the leg, bringing down the heel—i. e., to give flexion and extension to the ankle-joint, which is all-important in effecting a cure, no one being able to walk well without this. This is a point that has not been sufficiently attended to. We all make an acute angle between the foot and leg every step we take, particularly in going up-hill or up-stairs. If the foot be brought only to a right angle, in place of an acute one, it will, although seemingly cured, inevitably turn in in walking, and will revert to its original mal-position. In order to save time, the author formerly endeavoured to fulfil all these indications simultaneously; but finding that the combined efforts counteracted each other, he has for some time past first sought to turn the foot out in a line with the leg, the ankle turning in at the same time. The apparatus for this purpose is almost straight, extending from above the knee, on the outside of the leg, to the extremity of the foot, bearing on the malleolus as a fulcrum, and having a branch at right angles for keeping the heel steady. The foot is strapped to the part of the apparatus which runs parallel to it, and this is moved by a socket turned by a key, so that it may be graduated at will. The next indication—that of giving free motion to the ankle-joint—is effected by another apparatus having three sockets with keys, allowing the foot to be placed in any direction, or different parts of it in opposite directions. Drs. Little and Guerin have both expressed their approval of this apparatus. Scarpa's shoe, as improved by Little and modified by Adams, may also be used; and it gives great relief to occasionally change the apparatus. The patient should be seen daily, and every other day we should remove the apparatus and manipulate the foot. A great variety of apparatus is required for the different forms of club-foot; and these deformities sometimes so run into each other that it is difficult to say which is the most prominent feature of the deviation.

Paralysis of some of the muscles adds to the difficulty of treatment, and necessitates more complicated apparatus. It is very generally connected with irritation from dentition. Although the paralysed muscles not infrequently recover their tone and strength, still their antagonists, in the mean time, become contracted and shortened, and the foot is drawn to one side. In such cases it is best to divide the shortened muscles, and to keep the foot in its normal position during a few weeks, more or less. When it can be easily so maintained, it may be placed in a boot which is laced round the ankle, and has a spring running outside the leg to the calf, where it is strapped. A pad gently bears upon the malleolus, so as to keep it in place; and there is a joint, corresponding to the ankle-joint, with a check, so as to allow the foot to form an acute angle with the leg in walking, but not permitting it to fall below a right angle when it is raised to make a step. In paralysis of the flexors, the anterior part of the foot is apt to fall and drag, which produces much inconvenience in walking. According to Dr. Brown's experience, the foot can thus be better kept in position than when springs are placed on each side of the leg. Where there is but one, the inclination of the part may be regulated by bending the spring in or out, just above the external malleolus, which cannot be easily done when there is a spring on each side, the two requiring to be separated, and each bent by itself. Though called springs, they are made of iron slightly tempered, so that they can be bent by a strong hand; yet they are stiff enough to maintain their position when bent, and at the same time to sustain the foot and leg in their relative situations.

II. *On Hypertrophy of the Salivary Glands of the Palatine Region.* By M. Rouyer. (Moniteur des Hôpitaux, 1857. Nos. 2–4.)

M. Rouyer bases this paper upon five cases observed under M. Nélaton, and which he gives in detail. He refers to others related by authors who did not suspect their true pathological nature.

The salivary glands situated within the parietes of the buccal cavity vary in appearance according to the regions they are observed in. We have now to do with only those of the palatine arch, and especially of the velum palati. They are abundant where they line the osseous portion of the arch, particularly at its lateral parts, near the alveoli. In the velum they form a thick plane covered by the muscles. The glands vary from ½ to 2 millimetres in size, the largest being found at the adherent border of the velum, on each side of and near to the mesial line, where they form a layer 7 or 8 millimetres in thickness. Their orifices are here distinctly visible, even at a distance. In the remainder of the velum the glands form a plane thicker than the muscular one, and extending to its free edge, where they almost exclusively constitute the uvula. The glandular is separated from the muscular plane by an aponeurotic expansion extending from the palatine muscles.

The hypertrophy of these glands is oftenest met with in young persons of robust health, and in the 11 cases here collected, 5 of the subjects were females, and 6 males. The increase in size takes place very slowly, and may scarcely attract attention for years, and then proceed rapidly. In some of the cases related, the tumours had acquired the size of half an egg. They have always commenced in the velum palati, although observation will probably show that the same enlargements prevail in other parts of the buccal regions. A very important character of these tumours is, that they contract no adhesions to surrounding parts, but become encysted and are easily enucleated. The mucous membrane covering them may be either normal or slightly vascular, and is not adherent. They are seen in well-defined relief under this, having a lobular surface, and being elastic and resistent to the touch. They are painless, and the patient applies for relief on account of the functional disturbances their increase in size gives rise to. They may have already reached a considerable size before they have attracted the patient's attention, the alteration of the voice, which is the first symptom, being attributed to other causes. Their slow growth, painless character, and well-defined form, distinguish them from cancerous or syphilitic growths. The operation necessary for their removal is very simple, consisting in making a longitudinal incision, and detaching the tumour from the mucous membrane by means of a spatula, or the fingers. In one case, in which the ablation was imperfect, M. Nélaton applied the actual cautery with success. Relapse does not follow the operation. Microscopic examination of the tumours made by M. Robin clearly proved their glandular nature, and in several instances they were found to contain calcareous concretions.

III. *On Ligature of the Common Carotid Artery.* By Dr. James R. Wood. (New York Journal of Medicine, New Series, vol. iii. pp. 9–64.)

The author, surgeon to the Bellevue Hospital, New York, intending to publish the cases of this operation that have occurred to himself, first sought among his medical friends of New York for unpublished cases. The result is the publication of a considerable body of facts upon the subject; there being forty-eight cases reported upon, including the author's nine cases, these last being detailed at some length.

Besides these, we have some interesting remarks from the pen of Dr. Valentine Mott, upon forty-four cases of ligature of the common carotid. The cases occurred in his own practice. Some of these we will first transcribe. In the five instances in which Dr. Mott has tied both carotids in the same patient, no secondary hæmorrhage occurred; the interval between the two operations varying from two months to one year. In one case, however—a desperate one, in which there was great suffering—the second carotid was tied at the end of fifteen minutes. A few hours after, coma came on, which was followed by stupor, and death ensued within forty-eight hours after the operation. In two instances, the operation was performed at a very early age (in one at three months) for aneurism by anastomosis, and recovery took place in both. In two instances the artery was tied upon the distal principle for aneurism of the innominata. In one of these, occurring in a man of about sixty, bleeding followed the separation of the ligature, and the patient sank after two or three days. In the other (a man between forty and fifty) the ligature came away on the 13th day, and the patient dying a twelvemonth afterwards, it was found that the aneurism of the innominata had contracted to a hard and very compact spherical mass.

The following is Dr. Wood's summary of the particulars of the cases he has collected. The common carotid was ligatured for the following causes:—(1) *hæmorrhage* in 9 cases, with 6 recoveries and 3 deaths; (2) *malignant disease of head or face* in 17 cases, of which 4 resulted in the apparent cure of the original disease, 10 were decidedly benefited (the growth of the tumour being arrested for a while), 2 died, and in 1 the result not noted; (3) *aneurism by anastomosis* in 10 cases, of which 4 were cured, 1 died, and 5 were benefited; (4) *aneurism of the branches of the carotid* in 4 cases, all of which recovered; (5) *epilepsy* in 2 cases, both benefited, but not cured; (6) *operations for tumours* in 7 cases, all recovered. Secondary hæmorrhage occurred in 5 instances, slightly in 2, and severely in 3, all recovering, the hæmorrhage being controlled by pressure. The date of the separation of the ligature was noted in 24 cases. The maximum period was 31, the minimum 9, and the average 15 days.

IV. *Case of Luxation of the Os Coccygis.* By Dr. Roeser. (Froriep's Notizen, 1857, Band ii. No. 10.)

Dr. Roeser observes, that in the few cases on record in which this bone has been dislocated the luxation has been outwards or inwards, whereas in the present case it was lateral. A large corpulent woman, thirty-six years of age, while suddenly descending from a table on which she was standing, fell in a riding position across the back of a low wooden chair. Severe pain in the coccygeal region, which was much aggravated by attempting to sit, was the consequence, but she managed to get about for some hours. At last it became so severe that she was obliged to take to her bed, where she found she could neither move nor turn. After she had passed the night in this way the author saw her, and found her with so changed a countenance, and such an immovability and partial stiffness of the body, as to give very much the appearance of approaching tetanus. Besides the severe pain in the coccygeal region, she complained of a painful, tense, dragging sensation, extending from below upwards towards the nape, stretching along the arms to the fingers, which were numbed. The slightest voluntary movement of the body, or of the head, to one side or the other, and still less sitting up in bed, could not be borne. The head was confused, and the intellect somewhat clouded. There was no unnatural sensation in the lower extremities, and the urine and fæces were passed naturally. Upon examination, a small swelling was felt on the left side of the fissure of the buttocks, which proved to be the coccyx torn away from the sacrum, and carried towards the descending ramus of the left ischium. The end of the sacrum whence it had been displaced could be plainly felt. When a finger was introduced into the rectum, the exact nature of the displacement was still better ascertained; and when firm pressure was made downwards and to the right against the displaced bone, it suddenly resumed its normal position. The patient declared she immediately felt quite another being, the confusion of the head and painful sensation along the spine and arms disappearing. At the end of the fifth day, no inconvenience beyond a slight burning pain near the sacrum remained.

V. *A Case of Union by the First Intention after Lithotomy.* By Professor Reyer, of Cairo. (Wien Medicin. Wochenschrift, 1857. No. 24.)

The subject of this case, when he applied at the Klinik at Cairo, seemed about sixty years of age, and was of firm, though spare and emaciated habit. He had already, ten years since, undergone lithotomy; and on examination, a cicatrix, about two lines in breadth and one inch in length, was observed along the raphe *in perinæo.* The patient stated that this wound had healed in fourteen days. During the last two years the symptoms of stone had again become urgent, and the patient sought the performance of a new operation. On sounding him, which caused great suffering, a rough, convex stone of the middle size was detected. The urine was strongly alkaline, and deposited pus-cells, ammonio-magnesian phosphates, some blood, and epithelium cells, and a great number of the ova of the *distoma hæmatobium.** A median incision was made along the old cicatrix, the prostate being but slightly incised. Its tissue was somewhat lacerated on introducing the forceps, the operation being however completed within four minutes. There was only slight bleeding; and a grain of opium was given in order to insure constipation, the passage of stools after the operation being a frequent cause of secondary hæmorrhage. Cold applications were made to the perineal and pubic regions; and articles of food and drink were interdicted, the latter in order to render the urine as sparse as possible. Very little urine passed through the wound, and on the second day it was discharged by the urethra, and so continued to be. From the third day broths were given, and on the fifth day the wound was carefully examined. Adhesion was complete throughout, although a slight projection of one of the edges left a strip of unhealed surface, but this, four days afterwards, had cicatrized. The patient left the hospital quite well twelve days after the operation. The urine, however, still exhibited the phosphates and the *distomata.* The stone removed was 4·6 centimetres long by 3·2 broad, and consisted of a small nucleus of oxalate of lime and uric acid, covered by a thin layer of oxalate of lime, the remainder being made up of phosphates.

Among seventy-three operations performed by the author at Cairo, he has only met with one other case of immediate union; but although he is not aware of any such case in European practice, all practitioners in Egypt have met with similar ones.

VI. *On Fistula Lachrymalis.* By M. Ancelet. (Gazette des Hôpitaux, 1857, No. 69.)

M. Ancelet observes, that although the production of lachrymal tumour and consequent fistula is often primarily due to chronic inflammation affecting the nasal canal and Schneiderian membrane, yet in a considerable number of cases the obstruction of the canal is only consecutive to the development of the tumour due to other causes. Thus, in two cases recently occurring to himself, blepharitis, leading to obstruction of the lachrymal points and ducts, evidently preceded the obstruction of the canal. The treatment of such cases must commence with that of the blepharitis. For the purpose of rendering the obstructing mucosities of the canal more fluid, the author objects to the use of Anel's syringe, which dilates the fistula and destroys cicatrization; he prefers the application of simple moistened compresses, and the employment of nasal fumigations. For the purpose of applying compression to the dilated sac—a mode of treatment formerly employed, but of late gone out of use, owing to the difficulty of application—M. Ancelet employs collodion. He has resorted to it in two cases. In one, complete success followed in ten days, the collodion being renewed every day. In the other, the treatment had to be suspended after amelioration had been produced, in consequence of a renewal of the inflammation, attributed to the incautiousness of the patient.

* For a curious account by Prof. Reyer of the prevalence of this parasite in Egypt, see British and Foreign Medico-Chirurgical Review, vol. xviii. p. 258.

VII. *A Revision of the Doctrine of Dislocation.* By Professor Roser. (Archiv für Physical Heilk., 1857, p. 42.)

Professor Roser lays down the following maxims respecting dislocation, as illustrated by dislocations of the femur, derived from observations, inquiries, and experiments continued over many years.

1. The essential obstacle to the reduction of a dislocation almost always consists, not in muscular contraction, but in the narrowness of the aperture in the capsular ligament; 2. The difficulty of reduction generally arises only from its not being at once possible to ascertain the direction and position of the luxated head in relation to the rent in the capsule. When these are found, the bone is either reduced of itself by muscular action, or a slight impulse, rotatory movement, &c., causes the head to slide in; 3. Much depends not only upon the size but upon the position and direction of the rents in the capsule; 4. The head of a dislocated bone is not, as a general rule, found in the primary position which was given to it by the force applied; but, under the influence of the flexion, adduction, &c., of the dislocated limb, immediately following, it assumes a secondary position; 5. The reduction-process must have for object, first, to restore the dislocated bone to its primary position, and thence to effect its return; 6. The classification of the dislocations of every joint must be based upon the essential anatomico-mechanical points; and the symptomatic classification adopted by Malgaigne is as unscientific as it is impracticable; 7. The most common dislocation of the head of the femur is downwards, when it passes beneath the tendon of the *obturator internus;* 8. This dislocation is produced by violent flexion of the hip-joint, during slight rotation of the thigh inwards; 9. The dislocation downwards becomes in general converted during extension (with rotation inwards) into dislocation backwards, *luxatio ischiadica;* 10. By adduction it may be converted into a *luxatio obturatoria;* 11. For the reduction of all these cases in which the hole in the capsule exists below, strong flexion appears to be necessary, in order to bring the head of the bone into its primary position, opposite the rupture of the capsule; 12. The dislocation on to the *foramen ovale* appears to be a mere variation of the dislocation downwards; 13. In the *luxatio obturatoria* the head of the bone is placed beneath and behind the *obturator externus;* 14. Malgaigne's "perineal luxation" is only a slight modification of the *luxatio obturatoria;* 15. The dislocation forwards, between the *fossa ileo-pectinea* and the *psoas*, is produced by excessive extension with rotation outwards. A secondary displacement results from slight flexion. The reduction is brought about by extension and rotation inwards; 16. Luxation behind the *pectineus* is to be regarded as a mere modification of the *luxatio ileo-pectinea.* It is not to be considered as a *luxatio obturatoria;* 17. The extremely rare luxation on the outer side of the anterior inferior spine must be regarded as a modification of the *luxatio iliaca*, produced by violent rotation outwards; 18. The *luxutio iliaca* is the rarest of all the forms of dislocation, although it is commonly regarded as the most frequent. The majority of those cases that have been so termed, and a considerable portion of those cases so denominated by Malgaigne, are, in fact, examples of *luxatio ischiadica;* 19. The *luxatio iliaca* essentially arises from rotation inwards, with adduction and flexion of the limb. Its reduction is based upon rotation outwards.

VIII. *On the Prevention of the Ill-Consequences of Operations.* By Professor Deroubaix (Presse Médicale Belge, 1857. Nos. 17–24.)

Professor Deroubaix, Surgeon to the St. Jean, Brussels, terminates a series of papers upon this subject with the following summary:—

1. If it is the duty of a surgeon to seek, by the improvement of his operative procedures, to obviate the immediate accidents of operations, the endeavour to discover the means of prevention of the secondary accidents, which are far more dangerous, is still more imperiously demanded at his hands. 2. Could the great surgical operations be rendered less dangerous in themselves, the intervention of surgery would be much more clearly and more frequently indicated in cases where the practitioner now often is obliged to remain a passive spectator of disorders which infallibly prove fatal. 3. The danger of great operations is not due to the size of the surfaces concerned, but to the number and volume of the veins divided. 4. When the division of veins proves mischievous, it does so by giving rise, through a mechanism the nature of which it is not always easy to appreciate, to the production of purulent infection, one of the most fearful consequences of traumatic lesions. The great danger and extreme frequency of this complication justify the efforts made for its prevention or removal. 5. There are two directions, both perhaps equally good, by following which we may succeed in rendering pyæmia of much less frequent occurrence. The first of these consists in im-

proving and rendering less uncertain the process of healing by the first intention; and the second in so modifying the divided surfaces as to convert them into a lesion of continuity of far less dangerous character. 6. Metallic caustics, at least in the immense majority of cases, do not give rise to purulent infection; but they are not applicable to certain operations—as, e.g., amputations. 7. It is rational, then, when seeking for substitutive or modificatory means for the prevention of pyæmia, to resort to such as most resemble caustics in their mode of action, and yet are exempt from the disadvantages of these therapeutical agents. 8. The tincture of iodine would seem to possess properties enabling it to fulfil these indications, seeing the deep-seated modification it impresses on the tissues, and the plastic effects it gives rise to. It does not act upon the ligatures, and therefore does not give rise to the danger of secondary hæmorrhage. When it is applied to bleeding surfaces after an operation, it induces a general hyposthenic effect of short duration, and a local hyposthenic effect, which imparts peculiar characteristics to the granulations and cicatrization. 9. The most remarkable results of this hyposthenization are, the much less indolence of the wound, the slight amount of suppuration, the notable diminution of the general reaction, and the maintenance of a condition approaching that of health. These phenomena offer no impediment to rapid cicatrization. 10. The discharge from the surface of the wound is considerably diminished as a consequence of the application of the tincture; but this does not prevent arterial hæmorrhage, or the loss of blood from the large veins. 11. The putridity of the wound becomes evidently diminished; and when the tincture is applied to the divided extremities of the veins, these become corrugated and narrowed, and then agglutinated. If phlebitis arises, it is obliterative and adhesive, not suppurating. 12. The application of the tincture to the sawn surface of the bones does not lead to necrosis. 13. The tincture imparts no preservative power against pyæmia when an open venous orifice, through which pus may be easily, so to say, mechanically introduced, exists at any point of the surface. 14. In ordinary cases, even the tincture is no certain preventative of purulent infection. When, after it has been applied, we find the vicinity of the wound remaining very painful, we should suspect a commencement of phlebitis, and the course of the pain should be carefully inquired into. 15. It should be remarked that as the general hyposthenization which results from the application of the tincture exhibits itself in symptoms, comparable to a certain point to those produced by chloroform, prudence is required in the simultaneous or successive employment of the two substances. Perhaps this is the principal defect in the iodine. 16. The injection of the tincture into the veins is immediately fatal. It induces an entirely peculiar coagulation of the blood, incapable of being confounded with any other pathological or spontaneous coagulation. 17. Nevertheless this medicinal substance cannot, when applied to a bleeding surface, be carried in substance into the torrent of the circulation, unless, indeed, venous orifices be maintained open by adhesions. It is absorbed in the state of an alkaline iodide, and may be found in such a state of combination in the blood and urine. The amount ordinarily absorbed exerts no ill effect upon the economy.

IX. *On the Treatment of Hæmorrhoids.* By M. Van Holsbeek. (Presse Médicale Belge, 1857. No. 22.)

Dr. Van Holsbeek recommends the following formula as of remarkable efficacy in the various forms of hæmorrhoids, provided that these are uncomplicated:—℞ Sulphuri loti, sacchari canarini, of each ℥j; ext. strychn. vux. vom., gr. vj. To be mixed with a sufficient quantity of tragacanth so as to form twenty-four lozenges. Of these two are to be taken the first day, the number being increased by one every day, until six are taken daily. The patient is then to keep at that number during four days, when he is to diminish it gradually until only two are again taken daily. If a radical cure is not by this time effected, he must follow the same course again. The amendment is, however, usually so rapid that the treatment at farthest lasts a week.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)

LETTSOMIAN LECTURER ON MIDWIFERY, ETC., ETC.

I. Physiology and Pathology of the Unimpregnated State.

1. *Periodical Dropsy of the Ovary.* By Dr. Magnus Huss, of Stockholm. (Archives Générales de Médecine, July, 1857.)

2. *A Case of Retro-Uterine Hematocele, with Rupture into the Peritoneal Sac, and Recovery.* By Dr. B. Breslau, of Munich. (Monatsschr. f. Geburtsk., June, 1857.)
3. *The Constitution of Women, as illustrated by Abdominal Cellulitis, or Inflammation of the Cellular Membrane of the Abdomen and Pelvis.* By Charles Bell, M.D. (Edinburgh Medical Journal, 1856 and 1857.)

1. The case of periodical dropsy of the ovary, detailed by Professor Huss, is of great interest in its bearing on the pathology of this organ. Miss C. B., aged thirty-nine, was sent, in the summer of 1855, to be treated at Stockholm. She had begun to menstruate at sixteen; this function had been continued without interruption or pain. She had enjoyed good health, and had led an active life. The first indication of disease appeared in the beginning of 1825, when she began to suffer dysuria. This state lasted only two days. A month later the same symptom returned for two or three days; the urine during these days was less in quantity, and thicker. In other respects Miss C. B. felt well. From that time similar attacks returned regularly once a month—about midway between two menstrual periods. During Dec., 1852, whilst suffering an attack of dysuria, the patient perceived for the first time an abdominal tumour; this, she said, was of the size of an apple, and was seated in the middle of the hypogastric region, directly above the pubes; it was circumscribed, and was not painful on pressure. The tumour diminished before the setting in of menstruation, and at the end the patient ceased to perceive it. Since then, the tumour returned regularly at the same period—that is, in the mid-interval of the catameniæ, and diminished before and during menstruation, disappearing at the end. Each time the tumour returned, it was a little larger, tending to bear more towards the right side. Its growth was always attended by pain and stretching in the region occupied by it. Since it attained bulk enough to reach the umbilicus, filling the whole right iliac region and the hypogastric region, it has never disappeared entirely. The patient declares she has observed that the more abundant the menses, the less was the subsequent development of the tumour, and *vice versa.* Latterly the menses have been less abundant. Miss C. B. has never suffered from metrorrhagia or leucorrhœa. The increase of the swelling has always been attended by more or less difficulty in micturition. The breasts have never undergone any sympathetic influence, only they have gradually diminished in size.

During the last year the patient has suffered from gastric disorder. Seen in June, 1855, by Dr. Huss. The uterus was healthy; it was a little depressed, the fundus thrown considerably backwards. The vagina was distended on the right side by an elastic swelling, evidently due to the tumour which was felt outside. This tumour, perfectly smooth, had the consistency of a bladder filled with liquid. When menstruation appeared on the 1st of July, the tumour had almost completely disappeared. Iodide of potassium was given during the month. No swelling in the hypogastrium took place. During the second week in August the tumour became the seat of sharp pains, with a sensation of great stretching. The volume of the tumour increased rapidly. A blister was placed on the right side, and proto-ioduret of mercury was given internally, so as to produce a slight stomatitis. During its administration the swelling subsided much more rapidly than usual.

The patient returned to the country. The same phenomena continued. She came under observation again at Stockholm in May, 1856. During her absence the periodicity had continued regularly, with the difference that the diminution of the ovary had been less and less. An exploratory puncture of the tumour was made by the vagina. Several ounces of thick jelly-like liquid escaped. On the next occasion of the growth of the tumour, paracentesis was performed. Two pints of a thick fluid, yellow, like honey, quickly setting in jelly, escaped. There appeared to be three distinct cysts, one portion only being completely emptied. No benefit followed the operation. The same phenomena continued to recur. The further progress of the case is not recorded.

Dr. Huss, in his discussion of the case, refers to other observations in illustration of his opinion that this was a case of periodical dropsy of the ovary. The only case, he says, he has known which bore any resemblance in the periodical inflammatory swelling of the ovary, was one observed by himself eight years ago.

Case II.—A girl, aged eighteen, robust and healthy in appearance, began to menstruate at fifteen. The catameniæ had from their origin been regular and abundant. For the last year she had experienced at each period a painful tension, augmenting each time, with a feeling of swelling in the left iliac region. During the last two months she has, three or four days before the appearance of the catamenia, felt a painful weight and fulness in the pelvis. The left iliac region swelled, and became extremely painful to the touch, and the patient says she felt a tumour there. This tumour remained during the two or three first days of menstruation, when the pain abated, afterwards the swelling was dissipated. When examined in the interval between two periods, nothing abnormal was discovered. Examined again at the advent of menstruation, the left iliac region was found more prominent than the right; percussion gave a dull sound; a tumour, well circumscribed, the size of a hen's egg, was felt by external palpation. Examined by rectum, this tumour was extremely painful, firm, as if stretched, and smooth of surface. During these days the patient had a fever of an inflammatory character. Examined four days after the cessation of menstruation, a tumour the size of a walnut was felt by rectum, softer and less painful to touch, which some days later disappeared. This was a *periodical ovaritis.*

[This latter case is not a rare one. It is an example of ovarian irritation passing beyond the physiological boundary of periodical congestion into inflammation. It is indeed illustrative of the first case, but is different in character.]

2. Dr. Breslau's case of retro-uterine hæmatocele. N. F., aged twenty-eight, had enjoyed good health till the age of twenty, and had menstruated regularly from sixteen. In her twentieth year she suffered, in consequence of a severe emotion, from convulsions, and a disposition to cramps in the hands, especially at the menstrual epochs. Hallucinations of taste and smell were often present. In her twenty-fourth year she had her first child; labour natural. The nervous symptoms persisted. The periods lasted three to four days, and returned regularly every four weeks. Towards the end of 1856, the patient was oppressed with pains in the back, but her general health was tolerable until the 21st of January of that year, when, an obstinate form of diarrhœa during a thaw prevailing in Munich, she suddenly fell ill with an intense intestinal catarrh. Recovered from this, pains returned in the hypogastric and sacral regions. Constitutional irritation followed, and a tumour the size of a hen's egg was felt a little to the left and above the symphysis. The Douglasian space was filled by a tense elastic swelling. Some days later the menses set in. The retro-uterine swelling increased, with the addition of pain in micturition and defæcation. Two days later a sudden sharp pain occurred in the depth of the pelvis, and spread over the abdomen. On examination, no trace of the swelling could now be felt. It appeared clear that the sac containing the retro-uterine hæmatocele had burst into the peritoneal cavity. The patient gradually recovered.

3. Dr. Charles Bell's papers on Pelvic Cellulitis form a valuable contribution to the pathology of this affection. After a careful historical survey of the subject, the author gives his own experience. He objects to the opinion entertained by some, that the disease is of a chronic character. He insists that the pelvic cellulitis, when proceeding to suppuration, is an acute disease; and that the inflammation frequently involves surrounding structures. He calls attention to the facts, that it is a disease to which all females are liable, both in the single and married state, from the age of puberty until an advanced period of life; that it is more commonly met with in early life, and immediately after parturition, especially in primiparæ. It appears most usually between the third and tenth day after parturition, corresponding in this respect with the other important inflammatory puerperal diseases, from which it is very difficult to distinguish it in its commencement. Dr. Bell's observations on the etiology are important. In the married state, he says, it seems in general to be occasioned by exposure to cold soon after parturition; but if we were to inquire minutely into the circumstances of the case, we should probably find that this cause is more apparent than real, and that there is strong reason to believe that the disease is the result of pressure, arising from constipation, or from the womb itself, during pregnancy. This opinion is fully supported by the numerous cases preceded or attended by constipation, as well as by the fact that cellulitis is more common in the primipara, in whom the muscles of the abdomen have a less tendency to yield. But the effect of constipation is also seen in the single state. In the unmarried state, however, cellulitis is in general more distinctly the consequence of mechanical injuries arising from bruises, falls, blows, and operations on the uterine organs, especially the hazardous and often rashly-performed operation of slitting open the os and cervix uteri.

Diagnosis.—In consequence of the incidental or contingent diseases, but more particularly peritonitis, its symptoms are at first usually complicated and obscure; as the disease advances, certain symptoms assume a more prominent and permanent character, and may almost be considered as pathognomonic. It is necessary in many cases to examine both by rectum and vagina. Sometimes, from the thickness of the parietes of the abdomen, and the deep situation of the inflammation, the pain from common pressure is scarcely perceptible; in such cases M. Broussais advises lateral pressure. Early in its progress, cellulitis often resembles an aggravated form of after-pains. The spasms may at first extend over the whole abdomen, and gradually concentrate, until at last they are fixed in one place, generally in the iliac fossa of either side. The lochia are not always suppressed, nor the milk permanently interrupted. It is not always preceded by rigors, nor do they occur early, unless the peritoneum sympathises extensively in the inflammatory action; but they invariably come at a more advanced period, when they indicate the formation of pus. They return more or less frequently, and are followed by feverishness and profuse perspirations, which come especially at night. Foul tongue, rapid pulse, occasionally constipation, but frequently diarrhœa, apparently arising from the irritation produced by the tumour. Great difficulty in passing urine, which seems to arise from a displacement of the bladder. As the disease advances, the limb of the affected side becomes stiff, and so bent that the patient cannot stand straight, or walk without great pain. Although the tumour is not invariably situated in the iliac fossa, it is most frequently met with there; but it always conveys a peculiar brawny hardness to the touch, and it is often long of being dispersed after the inflammatory action has been overcome. It is a striking characteristic that it is liable to have sudden and alarming exacerbations, attended by increase of pain and fever, indicating those occasional attacks of peritonitis which, if not attended to, may prove fatal. If the pus is not artificially evacuated, it almost invariably makes an outlet for itself, generally into the bowels or vagina; it may burst into the peritoneum, which is always fatal; more rarely into the uterus; it occasionally perforates the parietes of the abdomen at the iliac fossa, but sometimes near the umbilicus. [The Reporter has recently seen a case, under Dr. McClintock, at the Dublin Lying-in Hospital, in which the abscess has burst in this latter unusual place.] When it bursts into the bladder it is less fatal than when into the peritoneum.

Treatment.—Dr. Bell thinks general bleeding rarely necessary. Leeches are useful, applied repeatedly; a full dose of calomel, and antimonial powder, and Dover's powders—i.e., five grains of each, followed by a dose of castor-oil; then small doses of calomel and opium, or, what he has found most beneficial, small doses of calomel and James's powder—one-fourth to one-sixth of a grain of each—minutely triturated with white sugar, to be taken every two hours until pain and fever are subdued. Blisters and narcotics are useful. For the hard tumours which generally remain, the best application is the iodine or iodide of potassium ointment, and the occasional application of a blister. Several cases are detailed which deserve perusal.

II. Fœtal Physiology.

On the Anatomy of the Fœtal Circulating Organs. By Dr. C. Langer. (Zeitschr. der k. k. Gesellssch. d. Aerzte zu Wien, May and June, 1857.)

Dr. C. Langer has made some useful observations on the fœtal organs of circulation, especially on the involution of the right side of the heart and ductus arteriosus, after birth. It is known that in intra-uterine life the thickness of the two ventricles is about the same. Owing to the thoroughfare of the ductus arteriosus, and the placental circulation, the right ventricle has as much work to perform as the left. But after birth, simultaneously with the involution of the duct of Botallius, the relative labour of the left ventricle increasing and that of the right decreasing, the walls of the right ventricle undergo a comparative thinning. On the third day, no great difference in the thickness of the walls of the two ventricles is to be detected; on the fifth day it is, however, perceptible, and between the ninth and fourteenth days it is so marked that complete involution can no longer be doubted. The structure of the ductus arteriosus has been described as similar to that of the aorta and the pulmonary artery; but Dr. Langer finds a marked difference not only in the mature fœtus, but in the seven-month fœtus also. Sections of the aorta show the three layers of the walls plainly developed. The elastic layer is plainly defined. But a cross-section of the ductus arteriosus of five and seven-month fœtuses, shows the three layers but very imperfectly defined; and *developed elastic fibres are not present.* In mature still-born children there is wanting, in the walls of the ductus arteriosus, a compact developed layer of elastic fibrous tissue; only fine fibres are seen. The walls of the duct are stronger than those of the aorta and pulmonary artery. Instead of a developed elastic tissue, the duct exhibits a tissue which can be referred to the connective tissue. In longitudinal sections of the duct, including parts of the aorta, it may be seen that the thick elastic fibres of the tunica media of the aorta diminish as they approach the duct, and quite disappear near the orifice. The calibre of the canal in new-born children is about that of a branch of the pulmonary artery. The difference of tissue causes, near the aorta and pulmonary artery, a greater yieldingness of the walls; for this reason, injected preparations are not to be trusted as giving estimates of capacity, since the force of the injection commonly causes aneurismal dilatations. During the first three days no alteration of the duct is perceptible. On the ninth day the thickening of the walls has obviously advanced; the inner surface is more uneven. The canal is hardly passable by a large needle in the middle; but the nearer the sections are made to the aorta and the pulmonary artery, the larger is the bore. From the fourteenth day the middle stricture increases, especially towards the pulmonary artery. It is remarkable that injections pass with difficulty along the arterial canals, even in children some days old. This may be owing to the dilatability of the walls, and the tendency of the inner membrane to be thrown up into valvular folds.

That it is not to the altered mechanical circulation-conditions of extra-uterine life alone that the closure of the ductus arteriosus is owing, is proved by the original peculiar structural relations of the duct as compared with the aorta and the pulmonary artery. Through these the way for the later involution is prepared.

III. Physiology and Pathology of Gestation, &c.

1. *Extra-uterine Gestation, with Rupture of the Cyst, and happy Result.* By Dr. Bertrand, of Schlangenbad. (Monatsschr. f. Geburtsk. May, 1857.)
2. *On the Presence of Sugar in the Urine of Pregnant, Parturient, and Puerpera Women.* By Dr. Theodor Kirsten, of Leipzig. (Monatsschr. f. Geburtsk. June, 1857.)

1. On the morning of the 4th April, 1853, Dr. Bertrand was called to a married woman, aged twenty-nine. She had complained of acute pain in the side. When seen, her bloodless aspect struck the observer; her pulse could hardly be counted; the abdomen was swelled, and yielded a dull sound on percussion in the right inguinal region and over the pubes. This part was very painful to touch. Frequent vomiting; great thirst. It was now learned that the patient had been eight weeks pregnant, and that blood (but very little) had escaped by the genital passage. Examination revealed that the vaginal portion of the uterus was conical, as in primiparæ. The examining finger was soiled with blood and mucus. One grain of opium was given every two or three hours, ice-water and acetic ether. Peritonitis followed. But she gradually recovered; the anæmia resulting from great extravasation of blood lasting some time.

2. The researches of Blot on the presence of sugar in the urine of pregnant, parturient, and puerperal women, suggested to Dr. Kirsten the expediency of independent inquiries with a view to the verification of Blot's results. M. Blot arrived at the conclusion,* that the presence of sugar in the urine of women under these circumstances was a physiological phenomenon; and that its disappearance was the result of an intercurrent pathological condition. Dr. Kirsten observes, that if this conclusion were true, we should possess in the disappearance of the sugar, a tolerably sure measure of the condition of a puerperal woman, since this would indicate a commencing pathological disturbance, whilst its return would indicate reconvalescence. Dr. Kirsten examined the sugar-relations in two women. His observations do not altogether accord with those of M. Blot: they rather point to the reverse condition—namely, that sugar is present in greatest quantity in the urine of puerperal women when the milk-secretion—whether through a pathological process, or the weaning of the child—is arrested. Thus he observed in several puerperal women whose children had died, that on the second, third, or fourth day after the death the sugar appeared in greatest plenty. After this time, the quantity fell in the same degree as the milk diminished; but in four cases it could be demonstrated twelve days later. In three cases in which the patients were seriously ill in the puerperal state, and in whom the milk-secretion was almost null, the sugar was found in greatest quantity. One of these last women had suffered from common œdema during pregnancy. The examination of her urine revealed copious albumen, which diminished with the œdema, without disappearing altogether. Towards the end of pregnancy traces of sugar became apparent. She was delivered easily of a badly-nourished child. Repeated attacks of peritonitis followed. The milk secretion was very scanty, and the milk very thin: sugar was present in the urine in abundance. In the second patient, who suffered from peritonitis, followed by pyæmia, Dr. Kirsten was able to detect sugar up to the day before her death, this substance having been present in great quantity at the beginning of her illness. The third case was quite similar. On the other hand he was rarely able to discover more than mere traces of sugar in the most healthy women, who had well-nourished children and a superabundance of milk.

It hence appears that glycosuria belongs rather to pathology than to physiology. The key, Dr. Kirsten thinks, is to be found in the most recent researches of Bernard. This physiologist has shown that the formation of sugar in the liver is especially apparent when the abdominal circulation is increased, and the temperature rises. The biliary matter chiefly turns into sugar at a temperature of 40° Cent. No time was more favourable for this transformation than gestation, when the abdominal circulation and temperature are always raised to the necessary point for sugar-formation: whence we ought always to expect glycosuria in pregnant women. But as this is not the case, we are obliged to conclude that the greater quantity of sugar produced at this period is wanted for the nourishment of the child, so that it cannot be excreted. It would be interesting, with a view to the verification of this hypothesis, to examine the urine of women whose children may die, as quickly as possible after their death.

* See Midwifery Report; British and Foreign Medico-Chirurgical Review, April, 1857.

During the puerperal week the abdominal circulation is lessened, and the afflux of blood takes place towards the periphery, as is evidenced by the milk-secretion and sweats. This condition would not be favourable for the formation of sugar.

IV. Labour.

1. *Case of Unconscious Delivery.* By George Smith, M.D. (Indian Annals of Medical Science. April, 1857.)
2. *Retarded Deliveries, probably Caused by Psychical Influences.* By Robert Annan. (Edinburgh Medical Journal, Feb. 1857.)
3. *On a New Obstetric Instrument.* By Dr. Ludwig Contato. (Wochenbl. der k. k. Gesellssch. d. Aerzte zu Wien. March, 1857.)
4. *On Tympanitis Uteri.* By Dr. Alois Valenta. (Wochenbl. d. k. k. Gesellsch. d. Aerzte zu Wien. Feb. 1857.
5. *On Tympanitis Uteri.* By Dr. McClintock. (Communicated to the Reporter. Sept. 1857.)

1. Dr. Smith's case of unconscious delivery is interesting both in a physiological and medico-legal point of view.

Mrs. S.—— was daily expecting her confinement. On the 24th April, 1856, I was sent for suddenly, and on reaching the house, found the child born, and lying under the bed-clothes, close to the body of its mother. The cord was entire, and the placenta within the vagina. The delivery had taken place suddenly. During the night preceding her delivery, this lady had felt quite well. She rose several times to attend to her sick child. About half-past five, A.M., she walked from her house to the bungalow in which she was confined. When she reached the bungalow, she lay down upon the cot, and experienced a *very slight* sensation, as if her bowels were about to be relieved—a feeling as if something had touched her body followed, and caused her to ask the ayah to lift the bed-clothes, when, to the surprise and alarm of both, the child was found entirely extruded. Mrs. S—— was awake, yet so little was her notice attracted by her feelings, that the delivery took place unconsciously. This was her second child. The first was born with the usual pains, after a labour extending over six hours. The child—a female—was a little undersized, but not much.

2. Mr. Annan's cases of retarded delivery serve to illustrate the vexed question of the duration of gestation. A woman after passing the danger of premature labour, suffered shipwreck, went through excessive physical exertion, and exposure, and mental anxiety, in attempting to save a child; she nevertheless had no pains, and was delivered 332 days after the cessation of the last menstruation. Mr. A—— next relates two cases of retarded delivery following psychical exaltation. In one, a woman aged thirty-four, mother of several children, expected, according to her reckoning, labour on the 5th June, 1851; at the end of the seventh month, having attended the exhibition of a mesmerist, she returned home so unwell and excited that she had to seek medical advice, especially dreading premature labour. There were no pains, and in a few days she had recovered. The period of her delivery passed over, and on the 20th July, quite six weeks later, she was delivered of a child weighing ten pounds four ounces, by the forceps. The size of the placenta corresponded to that of the child. Her former children had not much exceeded seven pounds in weight. In the second case, a woman aged forty-four was delivered in the beginning of October, 1840, whilst she had had her last menstrual period at the end of December. Shortly before she had heard of the severe labour of an acquaintance, and had been much excited in consequence. Here also uterine contractions first appeared long after the calculated time—namely, on the 20th November; and the woman was delivered on the following day of a dead child, with the forceps. It weighed nine pounds eight ounces. Another case is related, in which no obvious cause of retardation was present. A woman, aged twenty-six, who had aborted three times, which was also threatened in her last pregnancy, was delivered of a child weighing ten pounds eleven ounces, on the 15th February, 1857. Her last period was observed on the 1st April, 1856, so that about 327 days had expired. [The Reporter has also observed cases where excessive weight of the child corresponded with apparent protraction of gestation.]

3. The new instrument devised by Dr. Contato is said to be adapted to decapitating the fœtus *in utero*. The inventor calls it the Decapitator. It is difficult to give a perfect idea of its construction without the aid of figures. The general principle, however, may be said to be the same as that of the guillotine. It consists of two parts. One part terminates in a curved half-ring, the inner or concave border of which is grooved, for the reception of a knife, which, fixed to the other half of the instrument, is made to slide along the stem of the first half. The objections to the instrument seem to be, 1st, to discover the cases in which

the operation of decapitation is desirable; 2nd, to apply the instrument. The inventor gives no practical illustration to prove the working capacity of the instrument.

4. Dr. Valenta's case exhibits one form of a rare affection—tympanitis of the uterus. A woman, aged forty-three, who had borne twelve children, was brought to the Obstetric Clinic at Vienna, on the 15th October, 1856, from a country district distant two hours' journey. Labour had set in on the 13th. The midwife, not feeling any presenting part, sent for the surgeon, who ruptured the membranes; upon this the left hand came down. On the 14th, attempts were made to extract the child by the sharp hook; this failing, another surgeon being called, replaced the arm, and prescribed ergot. On the following day she was taken to Vienna. The uterus was then found unusually but equally distended, so that it was impossible to discern any part of the child; the fundus reached to the zyphoid process, and was very painful; percussion in the circumference of the uterus gave a remarkably clear-sounding tympanitic resonance. The child's left hand, bare of epithelium, was felt in the vagina. No sign of rupture was discovered. Great febrile irritation, shivering, pulse 160, bilious vomiting. Exploration of the position of the child revealed the cause of the distension of the womb and the resonance, for suddenly a rush of stinking gas escaped from the uterus, followed by loosening of the distension. Delivery by turning was effected. It was found that the brain had escaped through the opening made previously by the perforator.

The patient sank on the following day under symptoms of acute peritonitis. Autopsy revealed adhesions of the right Fallopian tube and ovary to uterus and psoas muscle; the left tube distended with gas; the uterus itself very flabby, easily moveable, its anterior surface pale-red and smooth, its posterior surface bluish-red and covered with flocculent deposits; the substance of the uterus in the highest degree lacerable; the inner surface covered by a loose, knotty, delicate, dirty-brown, very foul-smelling mass.

The conclusion of Dr. Valenta was that the tympanitis was due to the evolution of putrid gas from the decomposition of the child, the gas being prevented from escaping by the closure of the mouth of the womb by the child. [It is probable that early delivery by turning on the 13th would have obviated this process, and averted the fatal result.—Reporter.]

5. The case of tympanitis uteri, with which the Reporter has been favoured by Mr. McClintock, is especially illustrative in juxtaposition with the preceding. The case occurred in the Dublin Lying-in Hospital, during the present year. A primipara passed nine hours in the second stage of labour. At 8 P.M. the os was mostly dilated; the membranes broken; pulse 112; tongue getting furred. The symptoms indicated the desirability of completing delivery. At 9 A.M. the irritation had increased; pulse 120; complains of constant pains in uterus; head partially in pelvis; no tumour on head; fœtal heart inaudible. The entire uterine tumour anteriorly is *quite resonant* on percussion. When finger was passed up between head and pubes, a discharge of fœtid air came away from the uterus. Craniotomy; no blood came from head. Rigor at 6·30 P.M.: and another on the second day; tongue white, dry, rough; belly tympanitic; no abdominal pain or tenderness. Opium, quinine, and wine administered. Sloughing of vagina followed—the result partly of pressure during protracted labour, and subsequent poisoning of the blood from absorption of foul air. The child was putrid.

It seems a necessary condition for the production of tympanitis uteri, that the child should be dead and the membranes broken, so that air may enter the uterus, and give rise to the ordinary putrefactive process.

THERAPEUTICAL RECORD.

Guano in Obstinate Skin Diseases.—Récamier (Gazette Médicale de Paris, No. 4, 1857), in the latter portion of his life, prescribed baths of guano in the treatment of some skin diseases; and a Belgian practitioner, M. Van der Abeele, has recorded six cases where this plan has been adopted with success. Three were cases of ecthyma and three of eczema. The baths employed contained 500 grammes of guano in solution.

Valerianate of Atropia.—M. Michéa (L'Union Médicale, January, 1857), who has for many years devoted great attention to the therapeutical virtues of the Solanaceæ, publishes a case in which the valerianate of atropia proved to be more efficacious in asthma than stramonium, and some other previous remedies. He employs the valerianate in the form of pills, as being most convenient and most certain, and the dose is from half a milligramme to two milligrammes. In asthma, he advises the discontinuance of the remedy, and its renewed use alternately, every week in persons under twenty-five years of age, and every fortnight in persons above that age.

Nitrate of Silver in Ozæna.—The remedy proposed by M. Galligioli (Raccoglitore Medico di Fano, 1857) is the fused nitrate of silver, but employed in a different manner to that adopted by others. He prefers making a pommade by mixing nitrate of silver with hog's lard, and introducing it into the nasal fossæ.

Strychnia and the Woorara Poison.—Dr. Vulpian (L'Union Médicale, Jan., 1857) has made some experiments on animals with these poisons, in consequence of a suggestion by Dr. Thibeaud that they are antagonistic in their actions, and might therefore be employed as antidotes to each other. The result, however, is that Dr. Vulpian does not consider woorara an antidote to strychnia, and that it is equally inefficacious, and may be mischievous, in the treatment of tetanus.

Poisoning by Caustic Ammonia.—Efficacy of Chlorate of Potash.—Dr. Fonssagrioes, of Cherbourg (L'Union Médicale, January, 1857), relates a case in which a sailor swallowed a large quantity of caustic ammonia with a suicidal intention. Vinegar and water were administered in abundance, but an œdematous and inflammatory condition of the glottis was induced, requiring the application of leeches; and afterwards an enormous quantity of limpid mucus was discharged from the mouth. The chlorate of potash was employed to alleviate this symptom, in the dose of two grammes a day, and in five days the salivation was almost entirely suspended.

Hydrocotyle Asiatica.—M. Fournier (Bulletin Général de Thérapeutique, March, 1857), in analysing this plant, has found that one hundred parts afford fifteen of ash. These ashes consist of a soluble portion and of a part insoluble in water. The soluble salts are the chlorides, iodides, and sulphates of magnesium, sodium and potassium. The carbonates are almost completely wanting. The author prefers the hydro-alcoholic extract, prepared in a vacuum, as the best form for medical use.

Tannin-draught in Chronic Bronchitis.—Dr. Berthel (Bulletin Général de Thérapeutique, March, 1857) recommends the following draught in bronchitis of long duration:—20 centigrammes of tannin, 5 centigrammes of belladonna, 15 centigrammes of hemlock, 90 grammes of infusion of senna, 50 grammes of fennel water, and 50 grammes of syrup of mallow. A tablespoonful to be taken every two hours.

Achillea Millefolium as an Emmenagogue.—Dr. Ronzier Joly (Bulletin Général de Thérapeutique, March, 1857) relates some cases which proved the efficacy of this plant as an emmenagogue. He was first induced by its powers in restoring the menstrual flux by the case of a young girl of eighteen, who had tried the usual means of cure without effect, and in whom the administration of a strong infusion of the millefolium induced the menses in half an hour. Upon inquiry, Dr. Joly ascertained that, in a village some miles distant from his residence, the women were in the constant habit of using the plant for the same purpose.

Balsam of Copaiba in Psoriasis.—M. Hardy (Bulletin Général de Thérapeutique, March, 1857), Physician of the Hôpital St. Louis, commences the treatment of psoriasis by giving about three grammes of balsam of copaiba, then, during the treatment, the dose is raised to four and six grammes: the copaiba is administered in the morning, fasting, and in the interval of meals. This treatment is continued for a certain time—at least a month, and sometimes more. But M. Hardy seldom confines his treatment to the employment of copaiba, although he has employed it alone, successfully, in certain cases. In general he associates it with local measures.

Bran-Bread in Diabetes.—Mr. Camplin (Medical Times, May 2, 1857) gives the following directions for the preparation of bran-bread. Take a sufficient quantity of wheat bran, boil it in two successive waters for ten minutes, each time straining it through a sieve, then wash it well with cold water, on the sieve, until the water runs off perfectly clear; squeeze the bran in a cloth as dry as you can, then spread it thinly on a dish, and place it in a slow oven: if put in at night, let it remain until morning, when, if perfectly dry and crisp, it will be fit for grinding. The bran thus prepared must be ground in a fine mill, and sifted through a wire sieve of sufficient fineness to require the use of a brush to pass it through: that which does not pass through at first must be ground and sifted again, until the whole is soft and fine. Take of this bran powder, 3 ounces Troy; 3 fresh eggs: $1\frac{1}{2}$ ounce of butter; rather less than half-a-pint of milk; mix the eggs with part of the milk, and warm the butter with the other portion: then stir the whole well together, adding a little nutmeg and ginger, or any other agreeable spice. Immediately before putting in the oven, stir in first 35 grains of sesquicarbonate of soda, and then 3 drachms of dilute hydrochloric acid. The loaf thus prepared should be baked in a basin (previously well buttered) for about an hour, or rather more. Biscuits may be prepared as above, omitting the soda and hydrochloric acid, and part of the milk, and making them of proper consistence for moulding into shape.

MEDICAL INTELLIGENCE.

Mr. Dempster on the Causes of Marsh Poison in India.

(To the Editor of the British and Foreign Medico-Chirurgical Review.)

Sir,—In the October number of the "Medico-Chirurgical Review" for 1855, you did me the favour to notice the report of a committee (of which I was the sole medical member) on certain sanitary questions connected with canals and canal irrigation in India, and you concluded that notice with the following remarks:—

"We cannot conclude this notice of the valuable inquiry carried out by Mr. Dempster and Major (now Colonel) Baker, without expressing a hope that we may again hear from them, and that the above summary of the results arrived at by those gentlemen may excite others to institute similar investigations."

In answer to this invitation, I now beg to lay before you some very interesting and important additional facts relating to the subject, which have recently been brought to my knowledge, and which I hope will be found worthy to be communicated to the profession in England and the colonies, through the medium of your journal.

In autumn last, a fever of an unusually virulent character committed great havoc in a "Zilla" or district about twelve miles distant from the city of Allyghur, in the upper provinces of the Bengal Presidency. It was of much importance to ascertain, if possible, what was the real nature of this disease. Was it an epidemic, arising from occult atmospheric or other influences which can neither be foreseen nor counteracted by any means yet at our disposal? Or was it a pure endemic, depending on local, and perhaps easily remediable, causes? These were obviously questions of much practical import to the inhabitants of the district under consideration.

The fever was said by some to be contagious, but mention was also made of a "nulla" (the Korrum) having overflowed its banks, and it was determined to apply my spleen test for malarious fever. The examinations were commenced nine miles from the "nulla," and carefully continued up to it. It was found that there had been an unusual amount of fever all over the district, and several cases of diseased spleen were brought up for advice. For the test purposes, however, these were not counted, but the *by-standers examined just as they came to hand.* Till a village was reached three miles from the "nulla," and to some extent influenced by its inundation, no case of spleen was found by an examination so conducted. In this village, however, 60 per cent. had spleen.

On reaching the "nulla" this rate kept up, and in one village (Chandous) which had suffered most from fever, 80 per cent. of the remaining inhabitants were found afflicted with diseased spleen! This at once indicated a severe form of intermittent fever, and many other circumstances combined to confirm this belief—viz.:

1. Three escape channels from the New Ganges canal enter the "nulla" above the village of Chandous, and fill it to *overflowing*, even in the cold or dry season, for a distance of some ten miles. During the periodical rainy season this prevented the water from finding an escape, and the country was consequently greatly flooded.
2. The villages *above* Chandous suffered to no unusual extent from fever.
3. The villages *on* the "nulla" or inundations formed by it, have suffered out of all proportion to the rest of the country.
4. At Chandous itself, where the canal water enters, 3000 people out of a population of 3600 have been carried off. The mortality was greatly assisted by the flood entering a "Jheel" or shallow lake, and thus increasing the malarious surface to a great extent.
5. The side or sides *nearest* the "nulla" of other large villages opposite a less extent of inundation, suffered most. The centres and other sides seem to have been protected. This suffering of particular sides of villages was distinctly seen from the tops of the "zemindar's" houses, as on those sides the houses of the villages were observed to be in ruins.
6. Smaller villages close by were overwhelmed by the malaria—that is, were, with here and there an exception, a mass of roofless houses.
7. At about ten miles below Chandous the bed of the "nulla" is larger, or the flood had expended itself in spreading right and left, and there was no unusual fever.

8. In the district generally people died in from five to fifteen days from fever last year. At the village three miles from the "nulla" (where 60 per cent. of spleen disease was found) they died in three days. *On* the "nulla" (where the percentage of spleen disease among the survivors amounted to eighty) they died in twenty-four hours during the ague or cold stage of the fever.

Some other points of interest were observed, but enough has been stated to prove the accuracy of the test I proposed, and its practical utility in leading at once to the right conclusions in investigations of this nature, in certain provinces of our Indian dominions.

In my appendix to the Canal Sanitary Report I showed why the large military station of Kurnaul had been abandoned.

For a great many years a canal had flowed in the immediate vicinity of that large cantonment, and yet, according to Colonel Tulloch's tables, it ranked in point of salubrity for European troops second or third among all the military stations of the Bengal Presidency. This canal (the Delhi) was an old Mahomedan's work, and only re-opened and improved by us. It occupies the bed of a large "nulla'—the natural drainage channel of that part of the country—and follows its most tortuous windings. So long as the *highest* level of water in this canal was *below* the lowest ground in the neighbourhood of the cantonment, it still performed its office of a natural drain; but when—with the most humane and benevolent intentions—the level of the water in the canal was raised so high that it could deliver water *on* the low grounds opposite Kurnaul, for the purposes of irrigation, it is obvious that no more drainage could escape by that channel. No other existed; and when the periodical rains came, all the low ground in the vicinity of the cantonment became an extensive swamp, and the troops, European and native, were universally prostrated by fevers of the most virulent type.

A remedy—an expensive one, it is true—might have been applied; but Kurnaul was no longer the important military position it was once esteemed, and Lord Ellenborough abolished it altogether as a cantonment for European troops.

Precisely the same thing has now occurred in the case above related. The Great Ganges Canal is itself constructed on proper principles. It passes along the "*Watershed line*" of the country through which it flows, and can never interfere with its natural drainage, whatever amount of water it may carry down to fertilize the land. But it appears it lately became necessary to expose a part of the bottom of this new canal, and this was effected by turning three escape channels into the "Kurram nulla," by which it was filled to *overflowing*, and no other drain for the country being provided, all the low ground in the neighbourhood was swamped during and after the periodical rainy season.

The fatal results of this mistake have been fully detailed above. The whole of this subject has an important bearing on all questions relating to the medical topography of a great part of the Bengal Presidency.

In conclusion, I would respectfully urge on the Indian Government the necessity of strictly enforcing the limits of the sanitary zones recommended by our committee, and of prohibiting canal irrigation within five miles of all large military stations occupied by European troops. If the principles on which these zones were defined be correct, their strict observance becomes of greater importance than ever, now that so large and permanent an addition is about to be made to our European troops in Bengal.

I remain, yours faithfully,

T. E. Dempster, Bengal Medical Service.

August 31st, 1857.

An American Opinion of Dr. Fell.

In the "American Medical Monthly" for July, 1857, we read some remarks upon the Fell treatment of cancer, of which it may not be *mal-à-propos* to present a portion to our readers:—

"Quite a commotion has been produced in the London Medical world by J. Weldon Fell, M.D., who has asserted that he could remove cancerous disease without using the knife. As we understand the matter, by some *hocus-pocus* he succeeded in persuading the authorities of the Middlesex Hospital, London, to assign him certain patients to be subjected to his treatment. The 'Lancet' immediately came down on the whole arrangement, as violating by its secrecy the very principles of professional ethics, and deprecated the position taken by the surgeons of the institution. For this, or for some other reason, Dr. Fell has published a book on Cancer, and its treatment, in which he gives the formulæ which he uses, and thus pricks his own gas-bag, which must at once collapse and shrink to flatness itself. Will our readers be so kind as to pause a moment, and if absolutely necessary, take something to strengthen them before they read that Dr. Fell's remedy is——*puccoon!!*—that is to say, in English, *Bloodroot*, in botany, *Sanguinaria Canadensis.* Hear what Dr. Fell says: 'Many remedial agents were tried without producing the desired effect, and all efforts to cure the disease were for a long time unsuccessful, and apparently hopeless, *until I heard of a root used by the North American Indians on the shores of Lake Superior*, which the Indian traders told me was used by them with success in these affections. . . . . And no doubt some poor squaw, suffering from this dreadful disease, was the first who applied it, after having tried all the simple herbarium of the uneducated savage without success, and then in despair applied the bruised bloody pulp of the white flowering puccoon.' This is the old dodge of 'Indian doctors,' which with us in America is at once understood, deceiving none but the most ignorant. In sooth, if all the wonderful remedies ascribed to the Lake Superior Indians had been discovered by them, it would be abundant evidence that they were a most industrious set of men in the study of remedies. We remember to have read of an expedition for determining the boundaries of one of our Southern States, which on emerging from the untenanted forest, found an Indian settlement, where the women used puccoon to colour the areola about the nipple as an addition to their beauty. The number of similar spots on the shirt bosoms of the party the next day was noted as remarkable. Can Dr. Fell's theory have sprung from this?"

The paper concludes with some reflections upon our gullibility, which a certain feeling of self-love prevents our transferring to these pages.

BOOKS RECEIVED FOR REVIEW.

A Treatise on the Cure of Stammering. By J. Hunt, Ph.D. Third Edition. 1857.

On the Urinary Secretion of Fishes. By John Davy, M.D. Trans. Roy. Soc. Edin. Vol. XXI. Edinburgh, 1857.

General Board of Health. Papers relating to the History and Practice of Vaccination. Presented to both Houses of Parliament. London, 1857.

The Structure and Functions of the Eye, illustrative of the Power, Wisdom, and Goodness of God. By George Spencer Thomson, M.D. London, 1857.

The Principles of Moral Insanity. A Lecture by John Kitching. York, 1857. (Reprint.)

The Asylum Journal of Medical Science. Edited by J. C. Bucknill, M.D. July, 1857.

The Baths of Germany, France, and Switzerland. By Edwin Lee, M.D. Third Edition. London, 1857.

Essay on the Mechanism and Management of Parturition in the Shoulder Presentation. By W. M. Boling, M.D. Montgomery, Ala.

The Medical Profession in Great Britain and Ireland. By Edwin Lee, M.D. London, 1857.

The Retrospect of Medicine. Edited by W. Braithwaite. January—June, 1857.

The Liverpool Medico-Chirurgical Journal. No. 2. 1857.

The Half-Yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D., and C. B. Radcliffe, M.D. Jan.—June, 1857.

The Assurance Magazine. No. 28.

The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M.D. July, 1857.

Riflessioni sopra alcuni Punti relativi alla Rinoplastia. Di Paolo Fabrizj, D.M. Nizza, 1856.

Résumé des Leçons de Médecine Opératione Acoustique. Par Paul Fabrizj, D.M. 1856.

Sulle Forbici Chirurgichi. De Paolo Fabrizj, D.M. Valletta, 1857.

Lettera del Prof. Antonio Scarpa, diretta al Dr. P. Fabrizj, sopra la Perforazione della Membrana del Tympano.

Intorno ad un Viaggio Fatto a pro degli Interni Indigesti della Corsica. Da Dr. Paolo Fabrizj. Nizza, 1856.

Report on the Formation and General Management of Renkioi Hospital. Addressed to the Right Hon. the Secretary of State for War. By E. A. Parkes, M.D.

An Elementary Course of Botany. By Arthur Henfrey, F.R.S. London, 1857.

History of Civilization in England. By H. T. Buckle. Vol. I. London, 1857.

Medical Reform a Social Question. Two Letters. By Joseph Sampson Gamgee, Staff-Surgeon, &c. London, 1857.

Professorship of Materia Medica in the Queen's College, Belfast. Testimonials in favour of Henry M'Cormac, M.D. Belfast, 1857.

Thirtieth Annual Report of the Directors of the James Murray's Royal Asylum for Lunatics, near Perth. Jan., 1857.

Testimonials in favour of Charles Wilson, M.D.

Waverley Journal, conducted by Women. Vol. IV. No. 37. July 11, 1857.

The Dial Register. No. 1. July, 1857.

Annual Report of the Royal Edinburgh Asylum for 1856.

On the Hæmantlion, an Instrument to facilitate the Examination of Blood in Disease. By Horace Dobell, M.D. (From the Journal of Arts.)

How to Work with the Microscope. By Lionel S. Beale, M.D. London, 1857.

The Use of the Microscope in Clinical Medicine. Illustrated. By Lionel S. Beale, M.D. London, 1857.

The Doctrine of the Duration of Labour. By J. Matthew Duncan, M.D. (Reprint.)

The Druggists' General Receipt Book. By Henry Beasley. London, 1857.

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