dispelled by the most heartrending proofs when the man has disordered his wife, and killed his child. Mercury for both parents is now the only cure for their lamentable condition, and it is a certain one. Mercury given to the man when first diseased would, I firmly believe, have prevented this terrible calamity; and I would now humbly suggest to all who undertake the treatment of venereal disease that, if they have a certainty that their patients will remain celibates all their lives, they may heal up their sores, and dispel their eruptions and sore throats in any manner they like; but that they have no right to expose the pure, innocent, high-minded females of society to contamination by marrying men treated without mercury. I am quite aware that this doctrine is at variance with that entertained by many able men at the present day, and may be unpalatable to some of my hearers; but I trust, Mr. President, that you and the other members of this great Association will give me credit for sincerity in thus declaring the result of a long experience, and advocating a line of treatment which that experience compels me to believe is most essential for the well-being and protection of society. To investigate and arrive at truth in medicine, is one of the cherished objects of the British Medical Association; and, honoured as I have been by the request to deliver this address, I feel that it would be an unworthy requital of your kindness if I were to shrink from promulgating well-grounded opinions because other and abler men entertain the contrary. Truth in medicine can be established only by experience; in the necessarily hurried manner that time permits I have endeavoured to communicate my own; others, with equal opportunity for observation, must have encountered cases resembling those upon which my convictions are founded; and I earnestly hope that their communications will furnish such an accumulation of evidence as will enable the profession to arrive at sound conclusions.

THERAPEUTICAL MEMORANDA.

[Under this head, we shall publish from time to time, as materials accumulate, short records of remarkable cases in practice which are sufficiently rare, interesting, or instructive, to deserve record, but do not call for lengthened statement or comment. Brevity and point should be the valuable characteristics of cases forwarded for this column.]

ON THE USE OF STRYCHNIA IN CERTAIN FORMS OF EPILEPSY.

By WALTER TYRRELL, ESQ., Great Malvern.

IN a disease like Epilepsy, of which the pathology is very obscure, it is satisfactory when we can say that we have established any definite fact, more especially when that fact is one bearing upon treatment. Now that bromide of potassium has a decided effect in relieving certain forms of epilepsy is one such fact; and I wish to point out in this note what I think will be established, on trial, as another fact equally worthy of notice; viz., that other varieties of epilepsy may be cured by strychnia. I do not mean to say that I can point to this or that case, and say it will be cured by strychnia; in our present state of knowledge we are unable to do this with any remedy; but I think I can point to a large class of cases in which the administration of strychnia will be followed in the majority of cases by the most beneficial results. There is a common form of epilepsy in which anæmia and defective nervous control are prominent symptoms. The majority of these patients are women, and with them the disease is apt to commence at or about the time of the first menstruation; and the attacks show a disposition to occur more frequently and with more severity near the catamenial epoch. In the intervals of menstruation, the disease often shows itself irregularly in the form of *petit mal*. In such cases, and in children in whom epilepsy, often commencing with some of the varieties of *petit mal*, gradually passes into the more severe form, this plan of treatment by strychnia is very successful; and there are few cases which are not benefited by a perseverance in its use.

My experience would lead me to believe that large doses are unnecessary; for, although I have carried the dose as high as one-third of a grain twice daily, with benefit, I am now in favour of employing smaller amounts, given more frequently. I have been using this remedy now for nearly ten years, and I can certainly show some valuable results. I am now preparing to tabulate the whole of my cases for publication in a collected form.

I much wish that some of my medical brethren would give the remedy a fair trial in some of the cases I have mentioned, or in any cases in which bromide of potassium has failed to relieve. To any wishing to give it a trial, I shall be happy to give any further details.

ADDRESS IN SURGERY

BY

THOMAS NUNNELEY, ESQ., F.R.C.S.,

Surgeon to the General Infirmary at Leeds.

MR. PRESIDENT AND GENTLEMEN,—Long years ago, when I first became a member of this Association—then in its infancy, and numbering only a few hundred country members, being, as its name implied, a Provincial Association—it was thought no small honour to be the person selected to read the annual Address in Medicine or Surgery. Now, when not only its title, but its constitution, is changed, and it is in fact, as it is in name, a British Medical Association, counting its members by as many thousands as in those times it did by hundreds, and embracing not only the *élite* of the provinces, but many of the worthiest in each metropolis of the three divisions of the kingdom, many of the best men in our own colonies, and honoured by the fellowship of numbers of the most distinguished men in Europe and America; when, year after year, the Professors of most of our oldest and largest Universities, who, by themselves and their predecessors in their respective chairs, have made the study of our profession an honour to the British name, and a credit to human nature, have appeared in the office I am now called upon to fulfil, the honour is indeed great. But with that how greatly is the responsibility increased! so much, indeed, that, had I not first been named by my professional brethren amongst whom I live, and had not the selection been afterwards confirmed by our governing Council, I should hardly have presumed to venture upon the office. Even now, in so doing, I must throw myself upon your favourable indulgence, and appeal to that kindly feeling which, I trust, the friendship engendered by our meetings through so many years, will allow me to crave, not in vain, at your hands. That you appreciate my zeal in the cultivation of our noblest of professions, I feel assured; that you will forgive and look favourably upon my present shortcomings, I presume to hope.

Last year, the learned Linacre Professor of Anatomy in Oxford gave, as you well remember, an elaborate and interesting dissertation upon some most important questions in physiology—especially those in which modern science has made most progress—and showed the Association how great is the aid to be gained by the extended application of physical science to the unravelling of physiological problems. To have even attempted this a few years ago would have been not merely useless and impossible, but by many would have been denounced as presumptuous, irrational, and profane. How the advanced ideas laid before you by the learned Oxford Professors, Drs. Acland and Rolleston, were supported, and even carried still onwards, by the profound reasoning, the extended learning, the mathematical calculations, and the chemical knowledge of the Dublin Professor, I need not remind you; for no one who had the opportunity of hearing the address of Dr. Haughton can fail, even now, to remember how wit and wisdom were conjoined, and to have been deeply impressed with the fact that, if medical men are to be capable of efficiently dealing with physiological and pathological questions, and the correct treatment of disease, they must be something more than mere anatomists and routine administrators of drugs; that they must dive deeply into and quaff freely of that extended scientific knowledge to the importance of which all our seats of learning have at last become alive, and which they are making such praiseworthy exertions to afford facilities for enabling their alumni to acquire.

Gentlemen, had I merely consulted the bent of my own feelings, I might have ventured, at however humble a distance, to follow in the same direction; for such inquiries have ever been amongst those which, from early life, have had attractive charms for me. I have ever regarded an extended acquaintance with the operations of nature as the true path by which the laws of life, whether of man, animal, or plant; the production, increase, maturity, decadence; the calling into existence and the passing into decay; the maintenance in health and vigour; the lapse into weakness and decay; the advance or degeneration of the species; the restoration and improvement; the descent into feebleness and imbecility of both mind and body in the individual—are to be learned and understood. I have ever felt that, in proportion to our knowledge of Nature, so will be our power, both for good and for evil, over her; and that, as man is but one manifestation of Nature, the better we

are acquainted with the laws of matter, so shall we, whose especial province it is to deal with matter in its noblest and most complex forms, be better enabled to perform our duty. Let us but know the properties and the innermost laws which govern what we call inert matter; and, depend upon it, ascending from the simplest forms of organic material through a more and more complex combination of atoms until we arrive at man, with his varied mental and corporeal powers and weaknesses, so shall we understand the laws by which he is created, maintained in health, cast down by disease and decay, and finally returned to that dust whence he sprang. So also shall we be better able not merely to ward off those changes which constitute disease, but we shall be enabled to apply our means, whether it be by the application of hygienic laws or the administration of drugs—not as at present, for the most part empirically, but with a knowledge of their specific effects, so that they shall act directly and certainly, accomplishing the purpose for which we employ them, even as now the chemist in his laboratory manipulates his agents; and thus we shall raise our profession to the dignity and precision of a true science. Chimerical as it may perhaps appear to those who are not familiar with the progress of the collateral sciences, those who know what has been done and is being done by modern scientific investigations, will readily admit there are not wanting preliminary indications which, like the rising cloud, now no bigger than a man's hand, presage that such an era may ere long burst upon the world in full power, and bring with it changes as great and beneficial as did that little watery vapour, and far more universal. To mention no other fact; look what Marshall Hall's discovery of reflex nervous action, not more than thirty years old, following as it did rapidly upon those of Bell and Magendie, has done for physiology. Why may we not reasonably look forward to like advances in therapeutics and practical medicine? We have men amongst us this day who have done much, and who will certainly accomplish more. Why may not those great general laws which govern and regulate the universal phenomena of gravitation, light, heat, electricity, magnetism, galvanism, chemical and vital action, by which the face of all Nature, not of this earth and its inhabitants alone, is changed and metamorphosed, be proved to be mere modifications of one all-pervading principle, which alike controls and explains the creation and distribution of distant worlds, and the origin and action of microscopic cells, and regulates the stupendous results of their various modifications and influences? When we consider how little was known of these subjects a century ago, and what is now understood and done with them, who shall presume to say how soon and how much more knowledge may not be obtained?

To-day, however, such is not my duty. While this lofty aim is ever to be kept in view as the goal which it is the duty of all of us—especially of those who are entering the medical profession, and of its younger members, who have leisure for the task—to strive to reach, we must not forget our present condition. We who are older, have suffering humanity to relieve, health to restore, and disease to mitigate, with such means as we best can employ. As last year physiology was the theme, I have this year assigned to me, what is to us and our fellow-citizens, as practical men, of at least as much immediate importance, the duty of addressing you on Practical Surgery, and pointing out, as far as may be in the limits of an address, the improvements and alterations which in recent times have taken place in it, both as a science and as an art.

This, considering the busy place in which we are assembled, can hardly be thought otherwise than appropriate. The splendid hospital which the liberality of the town and neighbourhood has raised, is a practical proof how pressing and important, in our every-day life, Surgery is. It must never be forgotten, that the cultivation of both science and art is essential to the constitution of a good surgeon. While he who is really a dextrous and exquisite artist, or a mere "practical surgeon", may, if he has not a competent knowledge of the scientific principles which should guide and regulate his art, be a most dangerous man, and really a bad surgeon; so, on the other hand, a man who has a profound knowledge of his profession as a science, may, if his hand have never acquired or have forgotten its cunning, or if his mind be so constituted that he cannot apply his knowledge to the case before him, be utterly unworthy to be trusted with the manipulation of diseased parts. Though it is undoubtedly true, that the more thoroughly the principles of surgery are understood, the less the practice of it as an operative art will be required, I apprehend the most Utopian dreamer will never figure to himself the day when operative surgery will be otherwise than a necessary blessing to mankind. So long as man is a machine, controlling and controlled by resisting matter, so long will he be liable to the laws of matter, and be subject to accident and disease which require manipulatory interference. Hence the great importance of all improvements, not only in our means of diagnosis, but in the practical treatment of disease. In this respect, our profession has no reason to be ashamed. Great as has been the advance in the various branches of scientific

knowledge, I venture to say that in no single one has it been greater than in our own. Let those who are acquainted with the history of our profession—those who have adopted the excellent method of devoting some of their leisure to the study of the works of the many great men who have lived and worked in bygone days—declare the progress and improvements within even comparatively modern times. Nay, let those of only this generation, men of my own age, recollect what has been done within their own day, and they will not blush; for the progress made is unsurpassed, and such as, were it set forth in full, would much astonish those who are merely *au courant* with the professional opinions of the present day. It is my duty to lay some of these improvements before you; and here the very richness of the material is a source of embarrassment. Of all it is impossible to speak even in passing words. Where volumes would hardly suffice to indicate their value, I must, for the most part, be content merely to glance.

I recollect the time when the *stethoscope* was almost an unknown instrument, and well remember the hesitation there was in arriving at an opinion as to its value as a means of diagnosis; yet it has done far more for mankind than many a battle fought and won, which has ennobled and made famous its general throughout the length and breadth of the land. Forty years ago, the *microscope* was little more than a toy—as often, from its imperfections, its aberrations of colour and sphericity, and its feeble defining powers, leading to error as to truth. Now it has become almost perfect, and its most skilful and ardent manipulators are those of our profession; while its employment, not only for histological investigations, but in the daily diagnosis of disease and its treatment, has become almost as essential to the scientific practitioner as is the telescope to the astronomer. The *ophthalmoscope* is of so recent introduction, that it may safely be said that the majority of medical men have not mastered the difficulties of its application; yet it has already revolutionised the diagnosis and treatment of several of the most serious affections of the eye, and rendered our knowledge of much that before was confused, uncertain, and impossible to understand, clear and precise. So the *laryngoscope* has enabled us to illuminate the larynx and trachea, which until now were closed, dark, and unapproachable to any of our senses; and thus not only can we see diseases which heretofore could hardly be guessed at, but, by operation and treatment, cure them. Though the *endoscope* has as yet not accomplished all that its votaries have said in its favour, and in the hands of the practical surgeon has, I believe, so far failed to be of very much use in illuminating the deeper cavities of the body, it is doubtless the forerunner of improved instruments, which will enable us to light up more than one of the hollow organs, and to see what is there going on. Nay, those are not wanting amongst us who anticipate the day as not very distant when we may be able to read as in a book the darkest recesses of our bodies; and I believe he who denies that this shall ever come to pass will be a bolder and a rasher man than he who believes in its realisation. Then, again, the *sphygmograph*, as a help to precision in estimating the true value of the variations in the pulse and the measure of the heart's action, in a more exact manner than digital touch alone can accomplish, is an application of physiological and mechanical skill which is worthy of great praise. Nor must the employment of delicate *thermometers* for ascertaining calorific variations, under different abnormal conditions, be passed over without mention, as in many cases tending to help diagnosis and prognosis.

THROMBOSIS and EMBOLISM, as now understood, are comparatively recent terms, by which are indicated conditions the most serious and dangerous, often suddenly fatal, which heretofore were utterly incomprehensible; and, though the study of these conditions may not, so far, have done all that could be wished in enabling us to cure them when once actually existing in a severe degree, it has taught us to understand the nature of the affection, and the cause of the great danger of it; to know what not to do, which formerly was, in ignorance, often done injuriously; and to do much to avoid the occurrence of the affection—even, if the attack be of lesser dimensions, and the obstructing plug be seated in the smaller vessels, or in those whose patency is not essential to the immediate maintenance of life, to greatly assist nature in effecting a cure.

The term PYÆMIA is of modern introduction; and, though much still remains to be solved respecting the true cause of the origin of the dreaded condition which it indicates, the very recognition of its existence shows a great advance towards precision of diagnosis, and throws a beam of light into that chaos of confusion which, within our own times, enveloped the true cause of that fatal result which so often follows accidents and operations. Though much still remains to be elucidated, what is known is an important onward step towards that scientific precision which eventually will enable us to reduce the fatality following surgical injuries, whether inflicted by accident or by operation, to the least possible proportion.

At no very distant date, DROPSY was most commonly, in the medical mind, as it still is in the non-medical, an entity, a positive affection, to be treated as a distinct disease; whereas now even the youngest student knows that it is very rarely, if ever, otherwise than a consequence of disease in organs often seated far away from the dropsical part; and that to cure these organs is to remove the dropsy, while merely to busy ourselves with the removal of the effused fluid, and neglect the abnormal condition which gives rise to it, is truly to let the patient die untreated. Though some distinction was recognised between two most dissimilar conditions giving rise to a like state of abdominal distension, and having, on a cursory examination, many of the same symptoms depending upon the effused fluid, the pathological nature of ascites and ovarian disease, totally differing as they do, was hardly comprehended—the one most commonly a mere consequence of disease; the other a serious local malady, but little, if at all, under the control of drugs, but amenable to direct surgical interference. Until recently, the poor sufferer from ovarian disease was doomed to almost helpless distress, dragging a miserable, comfortless, and, for the most part, short existence, after the disease had once been developed; the temporary relief obtained by tapping being by no means so dangerless as many imagine; for I find, in looking over the record of operations in our infirmary—and there is no reason for believing that the ratio is materially different from that of other hospitals—that, of the last thirty-eight cases of paracentesis for ovarian disease, ten ended fatally. Now, as the result of modern surgery, ovarian disease is a curable one. If, in any of the great achievements whereby mankind has been benefited, Great Britain is entitled to a foremost place, most assuredly she may assert her claim to it in this; for her surgeons may call the operation their own. So bold was the conception, so surrounded with difficulties and dangers in the diagnosis and the execution, was the proceeding, that no wonder the world stood aghast and incredulous at the proposition, or that those who were the pioneers were denounced and subjected to obloquy for their temerity. That at first, from many causes, deaths were frequent, was unavoidable; that ovariectomy will always be one of the most important and serious operations to which a human being can be subjected, is certain; but it has now fairly won its place as equally legitimate as any other of the great operations in surgery. By it, hundreds of women in this country and in America have been restored to all the blessings of health and all the functions of life; so that the operation is now practised wherever surgery is cultivated. Even in France, Germany, Italy, and other countries which have been slow in adopting it, ovariectomy is now not very unfrequently performed. The dangers of it are constantly decreasing; for, with continued experience, what cases are fitted for operation, and what are not, is better determined; the kind of operative proceeding and all its surroundings, and the proper after-treatment, are better understood. I believe it would be no difficult task to prove that, in experienced hands, ovariectomy is a less dangerous operation than amputation of the upper thigh. Less than thirty years ago, a man who proposed ovariectomy was denounced by more than one of those who were then princes in medicine and surgery, as a dangerous lunatic. Now, in this country, it is of almost daily performance.

From time immemorial, STRICTURE OF THE URETHRA has been a terrible curse to the unfortunate sufferer from it, and—shall I add?—an opprobrium to surgery. Now, if this be not entirely removed, it is certainly immensely mitigated. For generations, one device for the relief of stricture has followed another; various forms and many materials have been employed, in instruments and chemicals, for passing along the urethra; and operations without number have been invented for enlarging its constricted and unyielding calibre; but, at the best, these contrivances have only been effectual in the least severe forms of the malady; and not unfrequently, even in these, the improvement has only been temporary; while, in the more severe and distressing cases, tedious and painful curative attempts have too frequently resulted in failure. Many of the suggestions for destroying the strictured part by escharotics, or for dividing the thickened membrane, whether from within or from without the urethra, have not been dangerless to life, and have so frequently failed in effecting a cure, that it is no exaggeration to say no practical surgeon has been satisfied with the result which he has obtained by the use of them. Now, thanks to the simple, innocuous, and, comparatively speaking, painless operation, and the ingenious instruments devised by Mr. Holt, in the great majority of cases, even those of the worst description, a speedy and permanent cure may be obtained. Wherever a passage exists along the urethra, no matter how small, and a bougie, no matter how slender, can be got along it into the bladder, I believe that, with few exceptions, the complaint can be quickly cured. I have now done the operation so many times, without, in any one instance, any dangerous or serious symptoms supervening, that I can endorse all that Mr. Holt, in the last edition of his book, has said of it. In my opinion,

surgeons have only to give it that fair trial which it deserves, to secure for it very general adoption. Were it employed in an early stage of the complaint, I believe that those dreadful and yet not very unfrequent cases, where the perinæum is riddled with false passages, and the structures burrowed with hard gristly sinuses, would no longer be seen.

During the last few years, great activity has been exhibited in OPHTHALMIC SURGERY. Our greatly improved means of diagnosis in affections of the retina, choroid, vitreous humour, and crystalline lens, since the introduction of the ophthalmoscope, is an unalloyed good, *when the diagnosis is founded upon sufficiently practical skill in the management of the instrument, and an accurate acquaintance with the precise appearances presented by the various tissues of the interior of the eyeball in a normal, as well as in a morbid condition.* To obtain this, however, in passing, I may venture to say, I suspect to require a more patient investigation than is always given; and hence, as with the microscope in the hands of the inexperienced, the instrument has not very unfrequently led to error, rather than to truth, from supposing all that appears to be seen, exists in the eye; whereas it may be merely an optical delusion, or a confounding of the really natural appearance of the normal structures with those which result from diseased changes in them. That the method suggested by O'Ferrall, of removing the entire globe of the eye when it has suffered destructive change, or when it continues in an irritable condition after mechanical injury or inflammatory action, so that its presence excites sympathetic mischief in the other one, leaving its muscles and fibrous capsule as a moveable nucleus, is a great improvement over the old plan of merely removing the anterior half of the ball, all those who have frequently practised both will readily admit; for not only is the operation far less severe, but it is far more successful in its results. I must, however, presume to express a doubt whether operative dexterity and bold manipulation have not in some instances taken the place of true surgical knowledge and acumen, and not unfrequently led to consequences the reverse of beneficial to the patient, in some of the many operations now so readily resorted to upon the interior tissues of the eyeball. I doubt not that occasional success has rewarded the operator when it might not have been achieved by the more cautious treatment of the older surgeons; but I fear, not unfrequently harm, rather than benefit, has ensued when operations have been practised upon eyes so changed by disease as to afford little probability of restoration to healthy function. Of all the tissues of the eye with which an undue liberty has been taken, almost as though it had been uselessly placed in the organ, is, I think, the iris, which, if it could complain, would, I suspect, with some justice do so. Its physiological value seems to have been unduly ignored; and, in more than one kind of operation, it is now ruthlessly, and, I fear, sometimes unnecessarily, destroyed. Thirty or forty years ago, our best and most successful ophthalmic surgeons rarely touched it, if they could avoid it. Now it would appear as though it could not be too frequently excised or torn away, whether it be healthy or diseased. Experience must determine which of the two proceedings is the best; but I must own to my judgment—perhaps some of my hearers may be disposed to exclaim, “Your prejudice”—inclining me rather towards the earlier than the more modern proceeding.*

Operations for the closure of RECTO-VAGINAL and VESICO-VAGINAL FISTULÆ I need do no more than mention; for, as is well known, when successful, as they so often are, by rescuing the miserable sufferer from these horrible complaints, and from a life of forced seclusion and misery, in which existence is a burden to themselves and a nuisance to all about them, and restoring them to the enjoyment of companionship and activity, they are a benefit to humanity, and a triumph of modern surgical skill.

Though the introduction of LITHOTRITY is not of so very recent a date, many of those now present were in active practice before it was known, and it is only within the last few years that improvements in instruments, and the method of using them, can be said to have placed it upon a firm basis, and given it that hold upon surgeons and the public which it now possesses. I am not about to enter upon a critical examination of its advantages, as compared with those of lithotomy, otherwise than to say that, while unquestionably it is in many cases an operation to be preferred to lithotomy, I am not altogether convinced it is so infinitely superior, that in every doubtful case it should be selected, or that the mode which has sometimes been adopted in comparing the results of the two methods, is so correct and impartial, as to give an exact and reliable comparison of the true value of them. In selected cases, it may be perfectly certain there are very few of us who, were we ourselves the

* Let me not be understood as decrying operations for detaching adhesions of the iris when they will not otherwise yield to treatment, or operations for artificial pupil when necessary, or even excision of a portion of the iris when it has been lacerated or bruised in cataract operations, but rather to the frequent removal of large portions of the iris in extraction of the lens, and in glaucoma, when the objects to be achieved can be equally well attained without such destruction of important tissue.

subjects of operation, 'would not prefer lithotripsy to lithotomy; yet I believe it to be true that in many cases the latter is the better operation of the two; and, taking everything into consideration, in comparing the results of them it will be found the ratio of recoveries after them approaches nearer than the advocates of lithotripsy have sometimes striven to show. Those cases which are selected for lithotripsy and do well would, so far as my experience goes, almost invariably do as well with lithotomy. In looking over the records of the two operations in our infirmary for the last sixteen years, I find that out of 111 lithotomy operations there were 97 recoveries and 14 deaths; while out of 25 lithotripsy cases, 21 recovered and 4 died, showing a decided percentage in favour of lithotomy. Upon many points these records are not sufficiently explicit to allow of a very accurate judgment being formed, otherwise than it is clear that the patients subjected to lithotripsy were in at least as favourable a condition for recovery as were those who underwent the cutting operation. That this high percentage of deaths after lithotripsy is decidedly in excess of what it ought to be, according to other and larger returns, is certain; and were, all the details known, it might be possible to reconcile and explain the very great discrepancy between it and those of several excellent surgeons, particularly Sir H. Thompson, which are much more favourable. Still the fact remains that, as a rule, the cases which are submitted to lithotripsy are in a condition far more favourable for recovery than those which are lithotomised, independent of the operations themselves, and two most important conclusions may be asserted without fear of contradiction: 1. That many persons having large and hard stones, who could not be relieved by lithotripsy, do recover after lithotomy; 2. That many of those who die after lithotomy do so directly or indirectly solely in consequence of the great size of the stone. The size of the stone has, in my experience, more to do with the success or the failure of the operation, than any other single thing. Age has great influence, but the size of the stone has more; and yet it is precisely these unfavourable cases which are subjected to lithotomy, and not to lithotripsy. With a small stone and a healthy young subject all experience proves lithotomy to be an operation of very little danger—a fact which ought always to be borne in mind when canvassing the respective value of the two methods.

THE REMOVAL OF THE ENTIRE TONGUE is altogether a modern surgical operation. Though for time out of mind greater or less portions of the tongue have been removed by cutting instruments, escharotics, actual cautery or ligatures, the importance of the organ in deglutition and articulation, the difficulty of reaching its base, and especially the fear of not being able to arrest the hæmorrhage, owing to the depth of the wound, the size of the arteries, and their near origin from the carotids, have not unreasonably deterred attempts at more than partial amputation of it. I believe it was Mr. Syme who first suggested an operation for its entire removal, and performed it in the presence of many members of the Association, when its meeting was held in Edinburgh. Unfortunately that patient, as well as a second, died a few days after the operation; and a solemn warning was published by Mr. Syme, who declared that the operation was so serious, that farther attempts were not justifiable, as no one could recover from it. Subsequently, I believe, in a third case, Mr. Syme was, by a like proceeding, rewarded with success; and Mr. Fiddes in Jamaica, and Dr. G. Buchanan in Glasgow, have also succeeded by the plan laid down by Mr. Syme. However, believing that the severity of the operation depended far more upon the method of proceeding than upon the mere removal of the tongue itself, I devised what I hoped would prove to be a less formidable one, and which experience has proved to be so. Up to the present time I have removed the entire tongue 19 times, and Dr. Fenwick* of Montreal has done the same operation once, without any untoward symptom following in a single instance. In most cases, the patient has not required any after-treatment, being able to sit up the following day, and in ten days to be considered well. In the majority of operations, not a drachm of blood has been lost. In two cases only has there been any hæmorrhage, and in those not more than half an ounce of blood was lost. In one, a point of hot wire, and in the other, a ligature, at once arrested the bleeding. The little constitutional disturbance which follows this operation is surprising; indeed, in the majority of cases, there is none. Now I do not for one moment assert or believe that this operation will permanently eradicate cancer of the tongue, or prevent its recurrence, any more than the removal of the disease by the scalpel in any other part of the body will secure immunity for the future; but this I do declare, that, by

affording an easy and safe method of getting freely beyond the disease, and demonstrating how very little inconvenience in articulation and deglutition is caused by the ablation of the entire organ, it will encourage much earlier resort to the operation than would otherwise be thought proper; and thus, so far as operation can do, cure the patient. The importance of the subject, and the little opportunity which there has yet been of making the method known, will, I trust, be considered a sufficient excuse for my introducing a matter in which I am personally interested.

Ever since the time of Tagliacotius, and more especially since John Hunter's immortal work on *Inflammation*, made surgeons understand the *rationale* of the union of parts newly brought together, PLASTIC OPERATIONS, as they are now commonly called, have been practised more or less frequently, and with more or less success; but I doubt if even now their value and importance is sufficiently appreciated, or the enormous benefit which can be effected by them, when every other plan of treatment fails, in those dreadful contracted cicatrices which result from burns, is adequately recognised. Every surgeon must know of such cases. I have reason for believing that in the Leeds Infirmary such cases have been, and still are, more frequently and successfully treated by operation than they are in some places; and, perhaps, more commonly than some surgeons are aware of.

In tabulating the records of operations done in the Leeds Infirmary during the past sixteen years, I find one hundred and seventy-two cases of plastic surgery entered. Of these, none were on the lower extremity, forty were on the upper; of which thirty-two were cured, six relieved, and two failed. These were all contractions of the axilla, whereby the arm was tied down to the side; of the bend of the elbow, the fore-arm being greatly contracted on the arm; or of the wrist, palm of the hand, or fingers. Many of the cases were extreme and complicated, more than one contraction existing; fifty were of the neck and face, whereof forty-one are entered as cured, seven as relieved, one no better, and one as dead: many of these, also, were extreme, requiring more than one operation. Several were operated upon by the late Mr. Teale, and supplied the data for his papers on Plastic Surgery. Two cases were of the chest, both being relieved. Staphyloraphy was performed four times, two of the cases being cured, and two greatly improved. In one, a rhinoplastic operation improved the nose. The remaining seventy-six cases were either of the cheeks, eyelids, or lips, the majority being single or double hare-lip operations. It is, however, of cicatrices following deep burns that I wish more especially to speak, as I believe every other kind of operation than the transplanting of normal skin, to be not unfrequently worse than useless. That occasionally some benefit may follow the stretching of the tender and inelastic new cutis, and the hard, rigid, fibrous bands under it; the dissecting the skin from the subjacent tissues, the simple division of it, the entire removal of it, and allowing the gap so caused to granulate and cicatrise afresh when on the stretch; or the attempt to form new cutaneous tissue by metallic setons, and various other contrivances, I am not prepared to deny, after what has been stated by surgeons to occur; but in my experience it has not been so, and I cannot but suspect that, if we had a fair account after the lapse of two years of most of those cases which at first appeared to have been benefited, we should find the condition of the parts very much less favourable than at first they promised to be, for almost invariably contraction in the new tissue gradually goes on until a hard, unyielding cicatrix is again formed, as bad as it ever was. John Hunter long ago wisely remarked that Nature was more chary of forming new skin, as though it were more difficult for her to do it, than almost any other tissue in the body.* When she does form new skin, after the entire substance of the dermis and subcutaneous tissue has been destroyed, it is, as every surgeon knows, a very poor apology for the original structure. The only effectual method for obtaining a cure, I believe to be the substitution of a portion of the neighbouring sound skin for the cicatrix. That the attempt is not altogether free from risk is certain; for, if the flap should slough, as it may do, the patient may be rendered worse than he was before. But in every operation there is some risk: the deformity to be remedied is often so distressing and serious, the cure in many cases is so complete, and the proportion of failures to cures is so small, that I am strongly impressed with the propriety of operating in every case where a satisfactory flap of sound skin can be obtained. The operation is so important, and frequently so tedious, that it is not to be undertaken without due consideration of all the proceedings to be adopted. Some of those which are necessary to success I will shortly call attention to in the JOURNAL.

Every surgeon knows but too well that FRACTURES OF THE SPINE are

* See a report of the operation in the *Canadian Medical Journal* for December 1868; see also the *Lancet* for November 14th, 1868, where Mr. Sampson Gamgee relates a case in which he removed the tongue by a different proceeding—Rignoli's; terrible hæmorrhage occurred at the time of operation, and also subsequently, the patient dying of exhaustion and sloughing of the parts. During the last week, Dr. Bell of Ottawa, Canada, has favoured me with a visit, and informed me of a second successful case which he and Dr. Fenwick have just had. ‡

* "Skinning is a process in which Nature is always a great economist, and without a single exception."—Hunter's Works by Palmer, vol. iii, p. 501.

not only amongst the most fatal, if not the most fatal, injuries we have to do with; but also that perhaps they are those for which surgery has hitherto done the least. Though we occasionally hear of a patient who is supposed to have suffered from a fracture in some part of the spinal column recovering, this exception to the usual course is so very rare, as only to prove the truth of the rule; and in some of the alleged instances it is permissible to inquire whether the symptoms may not rather have resulted from concussion of the cord, effusion of a small quantity of blood in its sheath, twisting of its ligaments, or some similar condition, than from fracture of, and compression by, displaced portions of the bones. I do not mean to question the possibility of recovery taking place after fracture of the spine, even though nothing be done; indeed, there is sufficient evidence to prove that it has; but that it is to be looked for in any given case, is more than any surgeon would anticipate. So hopeless are these injuries, that Sir Astley Cooper more than once declared, if one patient out of one hundred cases can be saved by operative interference, it is one saved from death.

I believe Mr. Henry Cline was the first who actually trephined the spine, though, as Dr. McDonnell in his two excellent pamphlets has pointed out, he was not the first to discuss the propriety of its being done.* The suggestion was first made by Dr. James long before,† and also by Louis; but I cannot find from the passage in James' *Dictionary* that he had either seen or knew of the operation having been done; or, from the report in the *Archives Générales*, that Louis did more, in the case of a fractured spine by a musket-ball, than introduce his finger into the wound whence a ball had been extracted, and take away some loose fragments of bone, though both of them perfectly understood the *rationale* of the operation; and Louis actually discussed with M. Duplessis the propriety of making an aperture when there was not one.‡

Though Mr. Cline's patient died, the case served to direct much attention to the operation; and indeed to divide surgeons into two parties as to the propriety of the proceeding. Opposite opinions were expressed with very great bitterness, into which personal feeling largely entered. Sir A. Cooper expressed himself strongly in favour of the operation,§ in which he was supported by several excellent practical surgeons, such as Benjamin Bell, Tyrrell, South, and others. (To these names may now be added that of one of the most distinguished living authorities on the structure and functions of the medulla spinalis, Dr. Brown-Séquard.) Sir C. Bell, John Bell, and Alexander Shaw, on theoretical grounds alone, denounced it in such unmeasured terms as to show that at least as much feeling and prejudice as surgical knowledge controlled their judgment.|| Though the operation of trephining the

* *A Case of Fracture of the Spine*, in which the operation of trephining was performed, with observations, by R. McDonnell, M.D., F.R.S., etc., 1865; *On the Operation of Trephining in Case of Fracture of the Spine*, by R. McDonnell, M.D., F.R.S., 1866.

† "If the spinal marrow is wounded, death follows inevitably. Though, as it may seem cruel not to attempt the relief of one under the unhappy circumstances, the surgeon should lay the injured part bare by the knife, and elevate the fragments, which press upon the medulla, in a proper manner; or, when they are quite loose, extract them in a proper manner; then let him cleanse the wound thoroughly, and apply balsamic medicines, using the napkin and scapulary. He must continue this until the wound heal or the patient dies" (*James' Medicinal Dictionary*. London: 1745. Vol. ii. *Fractures—Fracture of the Vertebrae*.)

‡ *Archives Générales de Médecine*, 1836, tome ii, p. 421.

§ "Mr. Henry Cline was the only person who took a scientific view of the accident. He considered it to be similar to fracture with depression of the cranium, and to require that the pressure should be removed; and as the cases had proved so uniformly fatal, he thought himself justified in stepping out of the usual course, with the hope of preserving life. He made an incision upon the depressed bone, as the patient was lying on his breast, raised the muscles covering the spinal arch, applied a small trephine to the arch, and cut it through on each side, so as to remove the spinous process and the arch of the bone, which pressed upon the spinal marrow. The only case in which he tried it did not succeed; and, unfortunately, he did not live to bring his opinion to the test of experience to warrant a decided judgment being formed. He was blamed for making this trial. I am not sure he would have been ultimately successful; but in a case otherwise without hope, I am certain such an attempt was laudable."

"I beg the reader to observe this operation is not mine; that I have expressed some doubt of its ultimate success, but I wish the trial to be made as the only means of deciding positively on its utility; and if it saves a life in one hundred, it is more than I have yet seen accomplished by surgery" (Cooper, on *Fractures and Dislocations*, 4to, p. 516).

Referring to the above passage in Cooper's work, Sir C. Bell says: "The man must be already dead whose condition is not made worse by such an operation as this. What sort of schooling must he have had who does not believe that a man would be the worse for having a bone dug out from around the spinal marrow?" (*Observations on Injuries of the Spine*, by Charles Bell, 1824, p. 22).

"Such are the symptoms, and such frequently the manner, of our patient's death; and, notwithstanding the bloody operation described in books of making incisions, finding the fractured or luxated bone, and drawing it out by the spine or splinters, there is nothing practicable; and these very ignorant directions, given upon the highest authorities, are dangerous to none but boys. The cutting into the fractured vertebra is a dream." (John Bell, *Principles of Surgery*, 4to, vol. i, p. 626).

|| Mr. Le Gros Clark, in his *Lectures on Surgery*, just delivered at the Royal College of Surgeons of England, expresses an opinion decidedly adverse to the operation.

spine has occasionally been done in this country, in France, and Germany, and still more frequently in America, the statement will probably be regarded as near the truth that, from one cause or the other, the great majority of surgeons have never entertained a sufficiently favourable opinion of the operation to induce them to perform it. For many years it seems to have been almost forgotten. Yet I cannot but think this neglect has resulted more from submission to habit, to traditional authority, and a want of a due consideration of all the facts connected with the subject, than from a full and careful consideration of them. Doubtless the almost uniform fatality after fracture of the spinal column has greatly tended towards inclining surgeons to regard operative interference as useless.

Dr. McDonnell, in the first of the two pamphlets to which I have referred, has collected all the cases he could find recorded where trephining of the spine had then been performed. These are twenty-six in number, in seven of which, he says, life was preserved for some time. Since then, Dr. McDonnell has operated upon a man whose life was certainly prolonged by it, and in whom, for a time, marked improvement took place. I have operated upon four men, three of whom died, the injuries, as shown by *post mortem* examination, being such as not to allow of recovery taking place; but the other man, who most assuredly, judging from all the cases I have seen of fractured spine, must shortly have died, recovered, and lived for upwards of two and a half years after the operation, which was done August 1st, 1866. He continued in excellent health: though weak and partially paralysed in the legs, he had full power in the upper extremities. His condition at the time of the operation was most unfavourable; and after it he was most unfortunate, for he had a very severe attack of hospital gangrene, which was then prevalent in the Infirmary; this caused very extensive sloughing over the sacrum and where the wound had been made. Dr. Gordon of Dublin has had an equally successful case.* Dr. Brown-Séquard informs me of a case he saw in 1867, in Western New York State, where Dr. Potter had, with most satisfactory results, operated. Though the spinal cord had been completely crushed, life was saved in an enjoyable condition. In May of this year I operated upon a third case, and again in June upon a fourth. Thus out of 33 cases of fracture in various parts of the spinal column, where trephining has been resorted to, life has been permanently saved in at least three of them, and considerably prolonged in several others; while, in some who died, marked improvement immediately followed the operation. Moreover, in no one of these successful cases was the condition of the patient favourable. In all of them the accident had occurred some time before the operation was performed, so that pressure on the spinal marrow had been prolonged, and much mischief thereby done. In Dr. McDonnell's case, the injury was inflicted five weeks before the operation was performed. In my patient, who recovered, as long a time had elapsed. In my fourth case, the operation was done on the tenth day after the injury; while, in Dr. Gordon's, two months had actually intervened between the fall which occasioned the paralysis and the operation. I am aware this very length of time may, by those who are opposed to the operation, be adduced as an argument against it, and as favouring the assertion that these patients recovered in spite of the operation, rather than in consequence of it. However, before arriving at such a conclusion, I would venture to advise those who are inclined to use it, to make themselves acquainted with the details of the cases, when I shall be much surprised if they should continue to do so. If so, I should merely reply that if such a mode of arguing were allowed, it would render all observation useless. It would be a mere assertion, contradicted by every analogous fact. If Sir A. Cooper's experience of these cases, when left to themselves, be true (and that it is so I believe all who have seen many such cases will coincide in), then I would declare the success which has attended the operations lately done, is as satisfactory evidence of the propriety of them, in suitable cases, as is that for the propriety of trephining the skull when fracture and depression of it have happened.

The three great arguments which may be adduced against the operation are:

1. The difficulty in diagnosing between those cases in which the operation may be useful, and those in which it cannot possibly afford relief, owing either to the extent of the fractured bones, the impossibility of removing the pressure, or of remedying the mischief which has already been inflicted upon the medulla spinalis or to other organs, as in my fourth case, where a good deal of blood had been effused about both, and within one, of the kidneys. That this is a serious question is undoubtedly true; but it is almost equally so in fracture of the cranium and many other dangerous injuries involving important operations. If we are to be deterred from operating in every case which is not free from doubt, we must abandon many operations which all agree ought

to be undertaken. It is a good argument for putting us on our guard, and for inducing us to carefully watch and minutely investigate every symptom; but it is not so for prohibiting operative interference, provided this be based upon sound anatomical and pathological knowledge. I believe careful attention and extended experience will enable us, in fracture of the spine, to determine with as much accuracy upon what cases are fitted for trephining and what are not, as it has done in fractures of the skull, and in selecting such cases of ovarian disease as may be submitted to operation, and such as ought to be let alone.

2. The position of the fractured bone often renders it impossible to reach it. It is said, with some truth, that commonly it is by the body or bodies of one or more vertebrae being broken and displaced, that pressure on the front of the medullary cord is caused; and, as this cannot be removed, the mischief will not be relieved by merely taking away the arches and spinous processes behind it. No doubt this is correct in many cases, but it is not so in all, and possibly not so commonly as is supposed. It is not unfrequently happens when the fracture is caused by direct violence, as in this district it often is (and likely enough in others), applied to the part itself, as by the falling of stone or earth in quarries and coal-pits, when the man is in a bent position, that the arches are broken and depressed upon the cord, causing pressure of it, precisely as a piece of depressed skull presses upon the brain.* Now, why the elevation of the bone should not relieve the pressure and its effects in the one, as it is expected to do in the other, I am at a loss to conceive. Such also, I presume, will be the effect where the pressure is caused by effused blood, rather than by depressed bone. In the cranium, if blood be effused under the dura mater, even though we might not think of dividing the membrane, we should not hesitate to elevate the depressed portion of bone lying over the effused blood, in the reasonable hope of relieving the pressure, and so in the spine. If the blood be external to the membrane and immediately under the bones, the pressure would certainly be more effectually removed in the spine than in the cranium. It must also be remembered that in many instances it happens, even when the fractured and displaced portion is the body of one of the vertebrae, that the offending piece of bone may be small and not greatly displaced, so that, by removing the counter-pressure caused by the arches, which, perhaps, may also be displaced, the narrowing of the canal, though mainly caused by the anterior pressure, may be effectually removed. As the cord does not completely fill the bony canal, is not tightly strung in it, and is not inflexible, there is no reason to suppose that it cannot recover from a slight displacement in any direction, or that it will not accommodate itself to its altered position, provided the calibre of the canal be not materially lessened, and the cord itself have not been injured. The brain recovers from temporary pressure, and I see no reason why the spinal cord may not equally do so. Indeed, we have ample proof that it does, in the frequent recoveries after not only lateral, but the most excessive and acute angular curvatures of the vertebrae. When disease in the bones has ceased, we find the functions of the distorted cord are as perfectly performed as they are in the straightest vertebral columns. The degree of displacement in such cases of extensive caries of the vertebrae is often far greater than can occur in recoverable cases of displacement by injury.

3. Destructive inflammation of the medulla and its membranes set up by the operation, has been declared to be an almost necessary consequence of trephining the spinal bones, and has doubtless deterred many surgeons from venturing upon it. Upon this Sir Charles Bell and Mr. Shaw were particularly emphatic. Yet experience has proved that to a great extent the fear is imaginary; for, if the operations which have been performed have not been sufficient to remove all other objections to the operation, they certainly have been sufficient to prove this one has been very greatly exaggerated. It must not be overlooked that, owing to well known anatomical arrangements, the spinal cord is far more favourably placed than is the brain. The dura mater in the cranium, owing to its intimate connection with the bones, is very much more likely to suffer when fracture occurs than it is in the vertebral column, where it is not in close apposition with the bones; and I need not remark upon what every surgeon so well knows, that upon its integrity

* Even complete separation of the bodies of the vertebrae does not necessarily destroy the cord. I have recently had a very interesting case which proves this. A man on the roof of a building nearly forty feet high slipped, and fell transversely upon some iron palisading. Two of the spikes penetrated the long muscles of the back, fracturing the transverse processes of two lumbar vertebrae, and the floating ribs on each side, passed into the abdominal cavity and wounded the liver and other viscera. Yet the poor fellow retained complete sensation and motion in both lower extremities, and during the night actually got out of bed and walked round it. After death, the fibro-cartilaginous body between the second and third lumbar vertebrae was found to be completely divided, and the spinal and transverse processes separated, the only connecting structures between the vertebrae, being the ligaments. This specimen, with three others, taken from the fatal cases where trephining had been performed, are exhibited in the museum. In more than one instance, it will be seen that though the bodies of the vertebrae have been separated from each other, there has been no displacement of them.

depends the condition of the arachnoid and pia mater. If it be opened, they most probably will become inflamed and suppuration follow; if it be not injured, their normal condition may be maintained, and the brain and cord substance escape inflammatory mischief. In my three cases where death followed, careful examination showed no trace of inflammatory action about the cord or its membrane.

It has been asserted as a reason why the operation should not be performed, that it is a very difficult and painful one; that the wound would not heal, and that, even though the cutaneous and muscular one should do so, the bones cannot be reproduced, and thus the spine will be left too weak to support the head and shoulders. These are imaginary fears which have been proved to be altogether groundless. That the operation requires care is perfectly true, but so do most other important operations in surgery. It is neither so delicate, nor involves so much anatomical knowledge as many which are constantly undertaken. It is not a very painful one; the parts which are divided are not particularly sensitive, and even if they were so, there is, in the majority of cases, no reason why an anæsthetic should not be given. Whenever the fracture is below the middle dorsal region, so that respiration and the heart's action are not interfered with, there appears no reason why such may not be administered. Chloroform was given in Dr. McDonnell's and Dr. Gordon's cases, as it was fully in three of mine, and partially in the fourth. The same argument would forbid nearly every operation. In all the cases which have recovered, the wounds have healed up well and quickly, and in the three which have permanently recovered, no weakness of the spine has been complained of, or is at all apparent. The man upon whom Dr. Gordon operated is following a mechanical occupation, and he upon whom I operated could easily have done so, as his arms and head were perfectly supported, and moved; indeed his sister, with whom he lived, declared he was "wonderfully strong in his upper parts;" while the man in New York State, operated on by Dr. Potter, enjoys life. Moreover, there is great reason to believe that breaches to a moderate extent in the vertebral arches may be filled, if not with ossific deposit, at least with strong fibrous tissue. This supposition is strongly supported by experimental inquiry upon animals; for Dr. Brown-Séquard has been good enough to show me preparations, which he has taken the trouble to bring from Paris, of the spinal columns of cats and dogs, where, during life, he removed some of the vertebral arches and spinous processes, and after death found the spaces partially filled with deposit of bony material. The want of ossific deposit after trephining the cranium is not regarded as an argument against the operation where a portion of the skull has been depressed. Other arguments *pro* and *contra* could easily be brought forward, were I writing a treatise upon the subject, which I am not, and for which this is neither the time nor place; but I hope I have said sufficient to induce surgeons to consider the subject impartially, and to give the operation a fair trial. That success will be frequent, I do not pretend to anticipate; the nature of the injury does not allow this to be hoped for; the fatality after similar operations upon the cranium alone would not justify this, but if one case in twenty should be successful, as I think may fairly be anticipated, it would still be great gain; for Sir A. Cooper declared that if one in the hundred recovered, it would be the one snatched from death. So far, out of thirty-three operations, permanent success has been obtained in three cases, and prolongation of life in several others.

ABNORMAL CONDITIONS OF JOINTS and their treatment form no small or unimportant part of a surgeon's work. From time immemorial they have done so; and there is no reason to think they will not do so in the future. Disease in the articulations has probably been the cause of important surgical interference almost as frequently as disease in all other parts of the body put together. Probably more limbs have been amputated in consequence of disease in, or accidents to, the joints, than from all other causes conjoined. Hence, whatever improves the surgery of the articulations, is an immense gain. Not to dwell upon the gradual but immense advance in the surgery of rest, which for fifty years has been steadily growing, and which really was the true cause of much of the success of Scott's treatment, though he himself was not fully aware of it, I think I may unhesitatingly claim your attention to two of the greatest advances in surgery—the reduction of dislocated limbs by manipulation, and the excision of such joints as are hopelessly diseased, instead of amputation of the whole limb. These two great improvements could hardly have been made prior to our own day; for until anæsthetics were discovered, manipulatory reduction of dislocated limbs could only have been practised in very exceptional instances, and excision of joints would not have been frequent.

Here I would venture for one moment to digress, to point out how the unity of all parts of our profession is demonstrated, and the absolute necessity there is for those who pursue one branch to be educated in all. Nature has constituted vitality one; and we who are the hand-maids of Nature, helping her in her distress, putting her in the right

when she is wrong, and assisting and encouraging her when she is right, must deal with her in her entirety if we would be good medical men. To understand one condition, and to ward off the other, wherever the disease which causes it may be seated, we must know all, and discard as unphilosophical and dangerous errors, mere specialities. Anaesthetics, when first introduced, were regarded as a blessing, by the saving of pain; but none of us then fully recognised how greatly they would extend our surgical power, by enabling us to accomplish so much more than had ever been done before. True it is that dislocated bones have been put into their sockets from time to time by rude movements; but such attempts have been conducted on no rule: in the great majority of cases they altogether failed, and when they did succeed, it was more frequently by brute force or chance than by skill.

In the whole round of surgery, I know of nothing more perfect than the reduction of a dislocated limb by manipulation, as it has been not inaptly called. To those who have only seen a dislocated hip, for instance, replaced by the common method of extension and counter-extension with pulleys, and for the first time witness a few painless and comparatively gentle movements made with the dislocated thigh by the unassisted surgeon, the process very likely not occupying a minute, and find the limb restored to its normal condition, the proceeding must appear almost like magic. To Dr. Reed of Rochester, U.S., we are, I believe, indebted for first directing attention to this plan, which he pointed out as applicable to two of the four varieties of hip-joint dislocation. The method is, however, capable of a far more extended application than Dr. Reed appears to have been aware of. In our Infirmary, we have reduced not only every form of dislocation the hip is liable to, but those of the shoulder also. I have practised it in a man seventy-three years old, and I have assisted my colleague, Mr. T. P. Teale, to do so in a child of only two years. My own cases of success number upwards of twenty, and each of my colleagues has had several. Many cases have been reported by Mr. Birkett in Guy's; Mr. Hutchinson in the London Hospital; and by other surgeons in hospital and private practice, so that the general value of the method may be regarded as fairly established. I may mention that its successful performance mainly depends upon attention to two things—I, our anatomical knowledge, enabling us to place the bones and muscles in the most advantageous position; and, 2, on bringing the muscles into a proper condition, in which they shall have neither too much nor too little power of action, for either state will prevent success. If their action be too great, their resistance cannot be overcome; on the other hand, if it be entirely suspended, the head of the bone will not be drawn into or maintained into its natural cavity. Hence the anaesthetic should only be carried far enough to suspend volition and spasmodic action, leaving some little power of perception and contraction. When in this semi-passive condition, the limb should be firmly seized, put into gentle rotary motion in such a direction as our knowledge of the attachment of its muscles tells us, when they act, will cause them to draw the head of the bone towards its socket, and then, by a sudden and more forcible action, they are roused into quick contraction, by which the bone is partly thrown and partly pulled into its socket. To every dislocation of the ball and socket-joints, this simple method is applicable; to the hinge-joints, it is not equally so.

Though excision of the knee, shoulder, and elbow-joints, was introduced in the latter part of the last century, the operation was so little practised, and took so little hold that, as a practical operation, EXCISION OF JOINTS may fairly be claimed as of recent date. Mr. White* of Manchester appears to have the honour of being the surgeon, in 1768, who first successfully excised the shoulder-joint, and, on theoretical grounds, to have suggested excision of the hip-joint, though he never performed it. It was, so far as I know, Mr. Park of Liverpool, who first performed excision of the knee-joint, which he did in July 1781, on a seaman who, after a long and dangerous illness, recovered so completely that he was reported to be able to perform the duties of a common sailor. In a second operation, the patient died. These two cases appear to have constituted Mr. Park's experience. Soon after the publication of the successful case, the two Moreaus, father and son, in France, excised, with more or less success, not only the knee-joint, but the shoulder also. A few excisions were done in France and Germany. The results, however, do not appear to have been very satisfactory, and for many years the plan seems to have sunk into oblivion. In 1827, Mr. Crampton of Dublin wrote in favour of the operation, after which a joint was now and then excised; but still the feeling against the operation was so strong, that it was regarded as almost forbidden, especially of the knee. Mr. Syme† published a work on Excision of Joints, in which he spoke in favour of excision of the shoulder and elbow-

joints, but forbade excision of the wrist and hip, and spoke doubtfully of the knee-joint.* During the last thirty years, and especially the last five, excision of joints has come more and more into vogue; and at the present day many surgeons practise and advocate it. Mr. Jones, Mr. Price, Sir W. Fergusson, Professor Humphry, and others, may be named as advocates of it. Still, however, in many hospitals, it is not frequently performed; and many surgeons look upon excision—except, perhaps, of the elbow—with considerable distrust, and choose amputation as the preferable operation, believing the danger to the life of the patient to be less; and thus agree with Samuel Cooper, who, in his *Dictionary of Surgery* (6th ed.), says, after reviewing the operation: "I see no reason for preferring excision to amputation. No doubt, more limbs might be saved by the practice than by that of amputation; but more lives would be lost."

One of the latest English writers on the subject, who even proclaims himself an advocate for excision, says: "I believe excision of the knee to be a more severe operation than amputation, more immediately dangerous to life, and requiring a longer time for convalescence."† Dr. W. Mac Cormac of Belfast, in a recent article, where he compares the results of excision of the knee with those of amputation of the thigh (speaking, however, it should be stated, more from the cases which he has tabulated from the practice of other surgeons, than from his own experience), declares excision of the knee to be at least twice as dangerous an operation as amputation of the thigh.‡ On the other hand, in addition to the surgeons whose names I have above mentioned as in favour of the operation, must be placed that of Mr. Butcher of Dublin, who speaks in the most decided terms of the advantages and comparative safety of excision of the knee. Out of seven cases upon which he has operated, six made good recoveries; only one proved fatal.

I think I am hardly speaking too decidedly when I say that nearly all surgeons who have practised excision of joints agree that, whatever be its merits so far as the lower extremity is concerned, there can be no doubt of its advantages in the upper. I have now performed excision of joints so many times with success, and with so few failures, that, were I to speak only from my own experience, I should, in suitable cases, give an almost unqualified opinion in favour of excision of nearly every joint in the body, as compared with the corresponding amputation necessary to get rid of the disease; while I believe my colleagues, from their experience, would hardly be disposed to speak quite so strongly. I have successfully excised the shoulder-joint three times. The elbow-joint I have removed eight times. Five of the cases recovered with useful arms. Two of the cases died, one being a woman who, when nearly well, was carried off by erysipelas, then prevalent in the infirmary; the other was a man with compound comminuted fracture of the joint, caused by a fall from a great height with bales of wool; these fell upon him, and crushed his body: peritonitis was set up, and, I believe, was the cause of death. The remaining case was one of partial excision, which in the end required excision of the whole joint, and then did well. Twice I have removed the wrist-joint according to Lister's excellent method. One case did exceedingly well; the other required amputation, the metacarpal bones being found extensively involved. The hip-joint I have excised six times, with only one death, and that from an attack of diphtheria seizing the boy three days after the operation. One operation, where long continued suppuration of the pelvis had previously existed in a bad subject, was followed by great suppuration and very slow healing of the wound. The other four patients recovered quickly and well, with hardly any lameness, a high-heeled shoe being all that is required. I have excised the knee eight times, with uniform success. Some of the patients walk with no other lameness than a stiff knee. In two of these cases, the condition is quite equal to that reported of Park's sailor. One man gains his living by selling coals, a sack of which he commonly carries on his back. Excision of the various bones of the foot, one, two, or three being removed, where awhile ago amputation above the ankle would have been adopted, I have done and seen done very frequently, with uniform success, so far as immediate recovery is concerned, and, in the great majority of cases, with a permanently useful limb; but, to ensure this, the greatest care

* The feeling against excision of the knee-joint was doubtless increased by the ill success of those who practised it; for not only were many deaths caused, but long and tedious recoveries often only resulting in misshapen and useless limbs, so that no one appears to have realised anything like the signal success reported by Park; and thus suspicion was raised in many minds that the extraordinary usefulness of the limb in that case had been overstated. The unfavourable feeling towards the operation was certainly not lessened by the fact that in more than one instance where excision of the knee-joint had been done in children who recovered, the growth of the limb was arrested, and thus, as they grew up, one leg increased with the body, while the other remained that of a child.

† Holmes's *Surgical Diseases of Children*. 8vo. London.

‡ "Observations on Amputation of the Thigh and on the Merits of that Operation, as compared with Excision of the Knee." By Wm. Mac Cormac, A.M., M.D. *Dublin Quarterly Journal of Medical Science*, August 1868.

* White's *Cases in Surgery*, 1770.

† *A Treatise on Excision of Diseased Joints*. By James Syme. 8vo. Edinburgh: 1831.

must be taken to remove every affected bone. If this be not done, the probability is great that, in the end, amputation will be necessary. From not being sufficiently careful on this point, in some cases, Syme's or Chopart's amputation has been required before the patient could be said to be well.

Thus, then, I think I am justified in declaring that, in my experience, excision of joints has been most satisfactory, not merely as regards the immediate effect of the operation, but in the after-condition of the patients. Their power of locomotion, when the lower extremity is concerned, and the retention of the hand and arm, with all their varied movements, in full, as compared with their maimed and comparatively helpless condition when amputation has been performed, even though supplied with the best apparatus the skilled mechanician can furnish, contrast most favourably. The records of our Infirmary show that, in the last sixteen years, sixty-six excisions of joints have been performed. Of these, five died; one was relieved; in two, amputation was subsequently done; fifty-eight recovered. Of the sixty-six, thirty-eight were excisions in the upper extremity, and twenty-eight in the lower. Six of the upper extremity were primary, for accidents. Of these, five recovered, the average age being 22½ years; the man who died was 43 years old. Of the thirty-two pathological excisions of the upper extremity, thirty recovered. One, aged 30, was greatly benefited temporarily; amputation became necessary, which also relieved for a time, and was followed by death from phthisis. One, aged 55, died from exhaustion after erysipelas; while the average age of the thirty who recovered was 17½ years. Of the lower extremity, only five were primary excisions for injury, being all of the tarsal bones; four of them recovered, the average age being 37; one died, aged 34 years. The pathological excisions were twenty-three, of which nineteen recovered, the average age being 17 years; two were relieved, of the average of 22½ years; two died, of the average age of 19 years; and a boy, aged 13, died of diphtheria, after excision of the hip-joint; the other, aged 25, after excision of the knee. These statistics do not, I think, contrast unfavourably with those of corresponding amputations.

The success of joint-excisions, I am convinced, very greatly depends upon the selection of suitable cases for the operation, the manner in which this is performed, and the after-management of the case. If the disease be principally in the synovial membranes, ligaments, or cartilages, or if the bones be involved only to a moderate extent, and that owing to the disease having extended from the first named structures to them, the whole disease may be extirpated without the loss of much bone, and that which is left will probably be healthy. On the other hand, if the bones be the parts which have been primarily diseased, or are extensively so, it is doubtful if the whole of the parts involved can be removed; so that the mischief may still go on; sinuses leading down to carious bone are formed; these give rise to pain, inability to move, and exit to a large discharge of pus, by which the patient is worn out, or so much bone requires removal (as I have seen done), that the medullary canal is laid open and involved in the suppuration, by which pyæmia, in all probability, will be induced, and the unhappy patient soon carried off; or, should he struggle through, the limb will be rendered useless, from great shortening or want of firm union, if the excessive suffering does not necessitate amputation. If the subject be young, and the epiphyses be removed, there will be great danger, if not certainty, that the development of the limb will be arrested, as has happened not unfrequently. A very striking and interesting illustration of this result has fallen under my own observation in a girl fifteen years of age, whose knee had been excised when she was six years old, by which the growth of the thigh, as well as of the leg and foot, had been arrested; so that, when she was presented to me, she had one limb that of a well-grown girl of fifteen, and the other that of a child six years old. Moreover, the union was very loose and imperfect, and at so bad an angle, that the limb was an useless burden. I re-excised the parts, and got good bony union, with the limb perfectly straight.

A second most important consideration towards obtaining success is, I believe, not to make the wound in the soft parts larger than necessary. This is a point too often not attended to, and incisions far larger than necessary to allow of the bones being removed are often made. Now, I consider that any division of parts larger than is necessary for the section and removal of the diseased bone, without any rough handling, is a direct evil unnecessarily inflicted. Such large wounds lead to greater hæmorrhage, to greater danger of pyæmia, and to greater drain upon the system; for, as they invariably suppurate, the larger the wound, the greater is the suppuration; and thus, at the best, the recovery is thereby rendered more prolonged.

The after-treatment of the limb is of the utmost importance; though I cannot help thinking too great stress has been laid upon it, in every case at once getting the limb into an immovably fixed and often dily extended position, and that very often a different one from what,

possibly for many months, it has been retained in. The knee is the only joint I care immediately to put into an extended and fixed position, and even that much less rigidly than is said to be necessary.* A few days before the operation, I have a piece of strong firm sole-leather, extending from the trochanter to the heel, and broad enough to encircle three-fourths of the limb, well soaked in water, and accurately moulded to the sound leg, upon which it is allowed to dry. Upon this splint, immediately the wound has been closed by iron sutures and wet lint, I lay the operated one, with a little padding of cotton-wool, which protects the limb and absorbs the discharge. Two sand-bags, placed on each side of the thigh and leg, are sufficient to keep the whole limb steady. This I find to be far more comfortable to the patient, and to keep the limb in a better position than the wooden box of Jones, Price, Butcher, and others, which, I think, has a tendency to tilt the end of the femur forwards, and the tibia backwards, and so to assist in union taking place at an angle, to which there is already a tendency. After excision of the hip and the elbow, I do not confine the limb in any apparatus. There is very little disposition to move it. After excision of the elbow, I commonly simply lay the limb in a semiflexed position upon a pillow. Children, after excision of the hip, I allow to lie in the same bent position in which they have almost invariably lain before the operation. After two or three weeks, or when the wound has nearly healed, I slowly bring the limb down by means of graduated weights suspended by the ankle. Union, whether by bone or ligament, does not take place before this time; and in the meanwhile the patient rests much more comfortably in the position to which he has been accustomed, the wound heals much more readily than it would do in the constrained one of full extension, and strength is more quickly gained.

It is yet a moot question whether, in excision of the knee-joint, the patella, supposing it not to be implicated in the disease, should be left, or it should in all cases be removed. Mr. Butcher, Mr. Holmes, and some others, are very decided in their belief that in every case it should be taken away. To determine this question, I have not removed it in three cases; in two, where the cartilaginous surface was diseased, I have removed the whole inner half of it by a longitudinal section; and in the remaining case I have taken it away altogether. Indeed, in one case, which had originated in a fall upon the knee, the patella itself had been the chief seat of the disease, and had in a great measure disappeared. My mind is not fully made up on the question; but I think, when the patella has been altogether taken away, the recovery has been somewhat more speedy, and the limb has been more slightly afterwards. But, on the other hand, where the patella has been left, I think that the ankylosis has been firmer and the limb stronger, though the patella forms an ugly protuberance. Provided the epiphyses are not removed, I see no objection to excision, and much in favour of it, in children. Youths and young adults are the most favourable cases. After thirty-five years of age, each year puts the patient in a less favourable position for the operation; though even then, all other things being favourable, I would not hesitate to perform excision. I cannot but hope and believe, as cases are well selected, and due care taken in the operation and after-treatment, the plan will grow more and more into favour, as its success becomes demonstrated; and the long delay, which is now allowed before the operation is performed, during which the pain and discharge enfeeble the patient, will be considerably shortened; by which, though possibly some patients may be subjected to excision of a joint who might have recovered by ankylosis, I am by no means sure even such will not have been gainers by the operation; while I am confident many others will be saved a vast amount of misery and confinement, and be restored to health and activity, who otherwise would not be.

I need not remind this assembly, so learned in all that relates to professional lore, by what slow steps our forefathers progressed in attaining that knowledge and practice by which BLEEDING FROM A WOUNDED ARTERY is now, in ordinary circumstances, so easily arrested; but the mere student of the present day practice alone, who, seeing a divided or open artery simply tied by a thread, and the hæmorrhage at once cease, likely enough considers the process so easy as almost to have been intuitively adopted even in the earliest times, should refer to works of a former day, when he will find that, ever since surgery has had an existence, down to the penultimate generation, the fear and danger of loss of blood from an open blood-vessel have been a matter of supreme difficulty, and directly or indirectly, by operations which have not been undertaken from a fear of fatal hæmorrhage, or which have been done in order to avoid this catastrophe, have, perhaps, inflicted more misery, distress, and loss of life upon mankind than anything else in surgery. He will then find how limbs were burnt off, and how tumours were sawn away by hot chains; how constantly actual and potential cauteries were in liberal employment in every surgical operation; and how one never-

* Butcher, *On Excision of the Knee-joint*.

failing styptic after another was invented, lauded, and abandoned, merely from this difficulty. "It is really this accident of hæmorrhage," says John Bell, "that has retarded our profession for ages; for the ancients, ignorant of the ways of stopping hæmorrhages, did not venture to cut out the most trivial tumour, or they did so with fear and uncertainty. They performed these operations slowly and imperfectly, with burning irons or ligatures, which we now perform rapidly and safely with the knife. If they ventured to amputate a member, it was only by cutting, after it was gangrened, among the putrid flesh. They merely separated parts that were already dead and bloodless; so great was their abhorrence of blood."* It was not until the middle of the fifteenth century that Ambrose Paré thought of tying an open artery; being then, as he says, "inspired by God with this good thought for the good of mankind and the improvement and honour of surgery"—a thought too simple and grand to be easily adopted, and so it met with such opposition as, with other good things done by him, to lead to his being prosecuted by the Paris College of Physicians. The invention was badly received and slowly adopted. It was not until a century later that Petit invented a rude form of tourniquet; and not until the present century the single ligature, as now employed, was commonly used. Indeed, prior to the time of John Hunter and the most important and conclusive experiments of Jones, supplemented as they were by the observations of Scarpa, Freer, Hodgson, and others, the principle of the ligature could not have been understood. Until then, the fear was lest the artery should be cut through too soon; and every possible means was adopted to prevent that action which we now know to be essential to success. Though in this country the works and practice of these surgeons had caused the multiple ligature to be abandoned, for many years later it had not been entirely given up on the continent; for I once saw a very celebrated French surgeon, in an operation for popliteal aneurism, place a double ligature, with a considerable space between the threads, upon the femoral artery—followed, as might be expected, and as commonly happened, by the death of the patient. Even in England, so late as 1814, we find that excellent surgeon, Hey *primus*, writing: "I have been accustomed, in amputations, to tie the femoral artery twice, leaving a small space between the ligatures; and this method has been constantly used in the Leeds Infirmary since its establishment."†

The ligature, as employed for many years past, is so simple, so easy of application, and in the majority of cases so satisfactory, that many surgeons are still quite contented with it; yet that it has grave defects is undeniable. Its immediate action in arresting the flow of blood is certain, and is probably as satisfactory as anything that can be suggested; but that its after-effects are not unfrequently injurious and productive of mischief, no one can deny. For the permanent sealing of a blood-vessel, when it has been tied by a ligature, two dissimilar actions are required. The adhesive inflammation is essential for the closing of the vessel; and the ulcerative, that the thread may be liberated. Now this latter action may take place too quickly, before the adhesive has sealed the vessel, or sloughing may follow, and that, which not very aptly is called secondary hæmorrhage, may occur. Besides this, the mere presence of the ligature must necessarily prevent the entire closure of the wound by primary union; and suppuration, to a greater or less extent, must ensue. Suppuration, having once been set up, may, and often does, cause much mischief. Thus, without endorsing all that has been lately written against the introduction of fibrous material into the living body, every surgeon must admit that there are valid reasons against the presence of any foreign body. Whatever it may be composed of, it must *per se* be more or less an evil. Accordingly, a means for obtaining a certain and secure closure of a bleeding artery, in which no foreign substance shall remain, or, if remaining, shall be innocuous, has been a grand desideratum with all practical surgeons. Cold, exposure to the air, styptics of various kinds, and cauteries, actual and potential, are all useful when the bleeding vessel is of small size; but, as every surgeon knows, are not to be depended upon when the vessels are of more than a moderate calibre.

M. Thierry, reflecting upon the fact that it commonly happens that arteries of a large size, even the femoral, do not bleed when the limb has been torn or twisted off, ingeniously conceived that, if in all cases the divided arteries could be brought into the same condition as they are found in these cases, a like condition of closure would occur. Many experiments were made by him, MM. Carron du Villard, Maunoir, and Amussat in France, Lieben and others in Germany and elsewhere, upon the blood-vessels of living and dead animals of various kinds, as dogs, donkeys, horses, and smaller creatures, for the most part with such success as to justify the application of the plan of isolating blood-vessels,

drawing them out of their sheaths, and so twisting their coats that these shall be torn through, to operations upon the human subject. Accordingly, many surgeons upon the continent tried torsion in a considerable number of cases, in many instances with success; but in many others, where the vessel was beyond a moderate size, the hæmorrhage was not permanently arrested. In England, Mr. Aston Key (with whom, at that time, I had the advantage of being on terms of the closest intimacy) was perhaps the one who was most sanguine in his expectations, and who gave the method the fairest trial; but he soon abandoned it. In France, where the plan took its rise and was more practised than elsewhere, the advantage or not of torsion was very warmly debated. While some surgeons reported great success, others of the most undoubted skill and reliability, as Delpech of Montpellier, were unfortunate in their results; and in the Hôpital St. Louis in Paris, out of six amputations where the blood-vessels were twisted, the torsion failed in five, and only succeeded in one case. Dupuytren, who was requested by the Institute to report upon the proceeding, made many experiments at the Hôtel Dieu, and arrived at the definite conclusion, "that in man, though torsion may be applied with security to arteries of a small size, it cannot be trusted to without imprudence in those which are at all voluminous . . . that a great many failures must be placed by the side of successes; that, according to the observations of many practitioners of experience and incontestable skill, sometimes owing to inflammation and abundant suppuration, extending along the sheaths of the vessels in consequence of their torsion; sometimes from its failing to arrest the hæmorrhage; sometimes, owing to many circumstances, it being impracticable; so that, after many attempts, it has been found necessary to have recourse to the ligature. So far as union by the first intention is concerned, favourable as at first sight it appears to be, torsion has not proved to possess any marked advantage over the ligature."* Though this conclusion could not then have been known to Mr. Key, he independently arrived at a similar estimate of torsion, as it is evident most other surgeons did. Since then, except for small vessels, where it continued to be successfully employed, torsion has been wholly abandoned for those of larger calibre, until the last three years, when attention has again been much directed so it. In December 1867, I saw in the Edinburgh Infirmary a case where Mr. Syme had successfully used torsion to the popliteal artery. The interesting papers of Professor Humphry and Mr. Bryant on Torsion will be in the recollection of all those who were present in the Surgical Section at our meeting in Oxford last year. I am not aware that the recent revival of torsion in the living human body, or the experiments made on animals, have added anything to the knowledge or conclusions of those older eminent surgeons I have referred to, who practised and abandoned it nearly forty years ago. I apprehend I am not far wrong in stating that the opinion of most of the few surgeons of the present day who have been induced to practise it, is much the same as that I have quoted from Dupuytren. For small vessels, it may well enough be employed; but, for myself, I must say I could not twist a large artery and lie comfortably in bed the next night, lest, while I slept, the elastic artery should untwist itself, and my patient bleed until he slept never to wake again. Besides which, the after anxiety as to suppuration along the vascular sheath, and its consequences, is not to be ignored.†

To our learned associate, Sir J. Simpson—to whom the world is so much indebted, that had he been half as successful in destroying and making miserable human life in warfare, as he has been in saving suffering, and conferring blessings upon distressed humanity, would have been amongst those whom the State delights to enrich and ennoble—our profession owes a most ingenious method of arresting bleeding from divided blood-vessels without the use of ligatures. He suggested the plan of closing the open vessel by pressing it against bone by a long steel pin carried through the soft tissues on each side of it, or by carrying the pin under the vessel, and twisting a loop of wire over its two ends in such a way as to allow of its being easily removed when desired without disturbing the parts. So much has of late been said for and against acupressure, that I need do no more than refer those who desire to know all that can be said in favour of acupressure to the work of the two Aberdeen surgeons, Pirrie and Keith,‡ who, at the hospital there, have devised modifications in the method of using the compressing power, and have followed out the plan with an enthusiasm and perseverance that only those who were fully impressed with the great value they believed it to possess could have done, and which they be-

* *Leçons Orales*, vol. iv, p. 212; Velpeau's *Nouveaux Eléments de Médecine Opératoire*. Art. "Torsion."

† See an interesting paper in the *Lancet*, April 17th, 1869, by Dr. Ogston of Aberdeen, "On the Comparative Strength of Arteries secured by the Methods of Ligature, Acupressure, and Torsion."

‡ *A Practical Treatise on Acupressure*. By W. Pirrie and W. Keith. 8vo. 1867.

* *The Principles of Surgery*, vol. i, p. 142. By John Bell, Surgeon. 4to. Edinburgh: 1801.

† Hey's *Practical Observations on Surgery*. Third edition, 1814. The old Leeds Infirmary was opened in the year 1766.

lieve their success fully justifies. On the other hand, many most excellent surgeons, whose skill, candour, and experience are beyond all question, have never felt themselves justified in having recourse to acupressure; while many of those who have tried it, from one cause or another, have ceased to employ it. For myself, I must candidly confess I have never felt sanguine that it possessed all the advantages its advocates claimed for it; nor could I shut my eyes to its disadvantages. True it is, that the presence of a fibrous material is avoided; which, however, can be easily got rid of by the use of a metallic thread, as I have frequently done, and have twice successfully secured the femoral artery by an iron wire in thigh-amputations. But there is substituted for the ligatures the presence of the needles and the compressing wires, by which necessarily, according as one or other method is employed, more or less of the soft tissues are subjected to as great a pressure as is sufficient to close the arteries, be they ever so numerous. This, to my mind, is equivalent in mischief, and, though different in kind, yet possibly as great as the presence of the fibrous material in the wound. Moreover, the difficulty of effectually closing the open mouths of large arteries, when much retracted and deeply imbedded in soft yielding tissues of considerable thickness, is so great that I cannot believe it to be in all cases possible, without the employment of very considerable force. Besides which, the tension of a stump often varies much on the day following an amputation from what it was at the time of the operation; so that the force which might then be just sufficient to press together the sides of the cut vessels, may, within less than twenty-four hours, amount to a strangulating pressure, or the limb may have so shrunk in size that what was due pressure then may the next day be too little for security. Thus, as I said with torsion, so I say with acupressure—and, if reports as to hæmorrhage are to be believed, with equal justice—I could not feel satisfied as to the safety of my patient, had I left a recently divided large artery only secured by the quarter or half twist of a needle. Though I know the femoral artery has several times been thus closed, the practical question is, not whether such can be effected on some occasions, but whether it can be done as an every-day working operation, and our patient be left as secure from hæmorrhage as he is by any other applicable means, or, if the plan possesses any other such great advantages over them as shall justify the exposure to an increased risk of bleeding. I think it does not. I have purposely visited more than one hospital where acupressure has been in vogue; and in our own Infirmary I have carefully watched many cases in which it has been employed with the full intention of adopting it, had I found it possessed advantages over ligatures; but attentive observation of these cases, and a comparison of them with those of my own in the hospital at the same time in which it was not used, have shown that the latter recovered in every respect as well as where it had been used; while I find my colleagues, who at first eagerly adopted the plan, have quietly abandoned it, which, had marked success attended its use, they certainly would not have done. For upwards of eighteen months, I believe, acupressure has not been used in the Leeds Infirmary.

Long ago tying a vessel with fine silk, and cutting the ends close off, was tried; but the irritation, and consequent suppuration set up, led to the abandonment of the plan. I have frequently substituted annealed iron wire, which can be made as flexible as silk or hempen thread for fibrous material; but I have found a new difficulty. So tolerant is the animal economy of the presence of this metal that, in many cases, only the adhesive inflammation has been set up, and the ligatures have not been separated for many weeks. Though, doubtless, if the ends were cut close off and the wound closed, in many cases, the wire would be encased in lymph and lie quiet, or, in time, be absorbed, still this quiescence cannot always be secured. Suppuration may be set up, and hæmorrhage occur when not expected. Hunter was well acquainted with this tolerance of the presence of metals, and also with the fact, that foreign bodies which are deeply imbedded are not so liable to excite inflammation as when they are near to the surface.* Now, as most of the arteries which are tied are not very far removed from the skin, this tendency to suppuration may exist when they are so tied, and thus lessen the chance of the compressing material remaining quiet.

How far the method of soaking a fibrous material in carbolic acid,

* "This circumstance, of the deeper seated parts not so readily taking on the suppurative inflammation as those which are superficial, is shown in cases where extraneous bodies irritate any parts; for we find that extraneous bodies are in general capable of producing inflammation; but, if these extraneous bodies are deeply seated, they remain for years without doing more than producing the adhesive inflammation, which means they are enclosed in a cyst, and only give some uneasiness . . . but if the same body was nearer the skin it would produce suppuration. . . . It is probable that these cases of pins, etc., owe their want of power in producing suppuration not entirely to situation, but in some degree to the nature of the substance, metals, perhaps, not having the power of irritation beyond the adhesive, for when the adhesive has taken place the part appears to be satisfied."—Hunter's Works, by Palmer, vol. iii, p. 287.

as suggested by Professor Lister, and the still more recent one of employing catgut so treated, with the ends cut close off, will accomplish all that he anticipates, time alone will show. If I might presume to venture on an opinion, it would not be one of very confident success. That a ligature composed of animal tissue may occasionally undergo absorption without exciting suppuration or ulcerative action is very possible; but whether other, and quite as important, practical difficulties may not accompany its use is, I suspect, altogether unproved.

Doubtless, these and like considerations have presented themselves to many minds, and have led to many suggestions. The idea of employing moveable forceps, of varying length and strength, according to the size and depth of the vessels, occurred to me, as it did to other surgeons; but whether they contrived such forceps, and actually used them, I am unable to say. I have now closed the arteries with such forceps in so many operations of magnitude, that I can speak with confidence as to their efficacy and security, and also, I believe, of their advantage. Though I think it better to place the forceps upon the bare arterial coats, yet, in many cases, where the vessel has been surrounded by tendinous fibres, or where it has been retracted and could not be withdrawn from the surrounding muscular fibres, I have not hesitated to include some of this tissue in the blades, and no hæmorrhage has occurred. Upon the larger vessels, as the femoral, I allow the forceps to remain four days, though I believe they might safely be withdrawn at an earlier period. Upon the brachial and tibials, two or three days is sufficient. The forceps should be long enough to project from the wound, to allow of their easy removal, and yet not to be in the way; they are brought out between the flaps, which are closed in the ordinary manner; they do not interfere with any kind of dressing which may be used; and, as the pressure is obtained by a cross-spring just external to the wound, this is not interfered with by their removal. In no one instance has suppuration been set up in their track; nor have I seen an instance of pyæmia follow their use. They must be made of good steel, and may be electro-plated or not. They completely meet the objection raised against the use of fibrous material—they do not excite ulceration, they press no tissue but the artery itself; and, by greatly lessening the duration of the presence of a foreign body in the wound, they meet that objection, so far as at present can be with safety effected.*

There remains but one other subject to which I propose to allude. It is, however, an important one. I own to approaching it with some hesitation and reluctance. Hitherto what I have glanced at has afforded me pleasure and satisfaction. There has been shown progress towards that scientific precision which we all so much desire to attain. In what I am about to speak of, I have to encounter that which I believe to be a professional error, founded on false facts, and supported by plausible assumptions, rather than by accurate observation and true deductions. I refer to the recent—shall I venture to say fashionable?—method of treating wounds by what has been called "antiseptic treatment," by the advocates of which, it is to be feared, the sound physiological and pathological doctrines and practice of the last generation of British surgeons are unheeded, and in danger of being temporarily forgotten for what seem to me unsupported fancies, which have little other existence than what is found in the imagination of those who believe in them. Had the works of John Hunter been now so generally studied as they once were, and the doctrines which he enunciated been carefully kept in mind, this fallacy could hardly have obtained the temporary currency it has done.† "THE ANTISEPTIC TREATMENT OF WOUNDS" ignores those truths which formed the life-long labour of our great physiologist to establish, and for which the world has been taught to regard him as not the least of its benefactors. The theory and reasoning by which the antiseptic treatment of wounds is supported, appear to overlook facts open to all the world, and to disregard observations familiar to every person, through all ages, from the earliest period to the present day. If the antiseptic theory be true, no wound ought ever to have healed until carbolic acid, or some like substance, had been discovered and applied. A simple abrasion ought to have been regarded as a death-warrant, without hope of reprieve; a burn or a scald could not have been recovered from; and vesications, of which all physicians, for thousands of years, have known full well the value, by affording an extensive nidus for deadly monads to revel in, should only have de-

* "On a New Mode of Closing Blood-vessels by Moveable Forceps." By the Writer. *British Medical Journal*, 1867, vol. ii, p. 310.

† In the first suitable case of aneurism, I intend to cut down upon the artery, and place upon it a pair of forceps so as to close its calibre for a short time, and then remove them without disturbing the wound. This plan I hope to find more effectual and less severe than pressure, either by the fingers or the tourniquet.

‡ See the *Treatises on the Blood, Inflammation, and Gun-shot Wounds*, by Hunter, *passim*. Also James and Thompson on *Inflammation*; Travers on *Constitutional Irritation*; and the works of Sir A. Cooper, Abernethy, Dupuytren, Velpeau, and other surgical writers.

stroyed the patient by pyæmia; and a simple issue or seton, which used to be regarded, and probably ere long will again be so esteemed, as a valuable outlet for morbid material, would simply be an easy inlet for septic organisms, and infallible death to the sufferer. Again, a parturient creature, human or animal, ought never to have survived to have nourished her offspring; for if, to guard against the ingress of these organisms, it be necessary to keep an abscess covered with a carbolic screen, while it is pricked in the dark with a bistoury dipped in carbolic acid, guided by surgical fingers similarly imbrued, and to use a probe smeared with the like, when introduced into a small sinus, so that those ever present, ever active germs, which must surreptitiously dart in, may at once be killed as they do so,* how much more surely must they find ready entrance and tempting food by the way, through the dilated vagina, into the patulous uterus, with its rugose surface, as it were, expressly prepared and ready for them to generate and multiply in, and cause immediate putrefaction and inevitable destruction to the female? How could the obstetric hand avoid carrying them in shoals, without number, into the vagina? No account is taken of the constitution of the patient, his habits of life, his strength or his weakness, the condition of his digestive organs, the state of his blood, his temperament, diathesis, hereditary disposition, age or sex, his state of mind, the nature of the parts injured, the extent of the mischief inflicted, the manner in which it has been caused by sharp or by blunt instruments, the force employed, the presence or not of foreign substances, the effusion of blood into the parts, the possibility or not of bringing them together, the prevalent diseases, the condition of the air, temperature, moisture, climate, electrical and other physical and moral circumstances by which he is surrounded; nor of his food, clothing, and lodgment, which up to now have been considered as having a not unimportant influence over the progress of a wound towards healing, whether it should grow together by the first intention, heal by the effusion of lymph, the second intention, or should suppurate kindly and granulate healthily; or whether ill-conditioned pus should be formed, irritative fever be set up, absorption of pus take place, and death by pyæmia follow. Surgical science and medical knowledge are reduced to the one plain rule of, *in full faith*—for that is as essential as the acid itself—plentifully imbruing the part with carbolic acid; when, to a true disciple, all wounds, whatsoever and wheresoever they may be, and whatever tissues or cavities be implicated, become as nothing, or, still better, the *débris* may, likely enough, so it is asserted, become valuable pabulum for the growth of new normal tissues.†

Ever since Hunter proved the vitality of the blood, and pointed out the circumstances under which this could be maintained, the process by which wounds heal by the first intention, or without suppuration, has been thought to be well understood, as also the means which should be adopted to secure this result. Suppuration, *per se*, is not an unhealthy action, nor is pus itself always an injurious substance; but when the process can be prevented by union by the first intention, so much the better for the patient; for, wherever pus or effused blood exist, there is more or less danger of their becoming decomposed, absorption taking place, and the system being poisoned by them; or, it may be, of surgical fever supervening, and, through the constitutional affection, of the wound taking on unhealthy action. That this happens by no means very rarely, even when there is no external opening, I think every surgeon must know full well. That a cavity containing broken-down grumous blood, a large abscess filled with ill-conditioned pus, or a suppurating joint, without an external aperture or with a very small one, and giving rise to dangerous constitutional mischief, will not unfrequently, on being laid freely open, so as to afford ready outlet to their contents, and expose their cavities to the atmosphere, immediately assume a healthy condition, while the constitutional amendment is as marked and contemporaneous with the local improvement, is an undoubted fact. Occurrences of this kind are easily explained by the chemical theory of decomposition of animal matters, but how they can be explained on the germ-theory of putrefaction, when, in the one case, there has been no exposure to the external air, and in the other, a free exposure to it is immediately beneficial, I am at a loss to comprehend. In the three former cases, germs cannot possibly have induced the mischief, inasmuch as there have been no openings by which they could have entered, supposing them actually to have been present in the assumed myriads in the atmosphere—for, as yet, no one has ventured to assert they can penetrate the unbroken

skin; and, in the latter, if any had entered by the small and imperfect aperture, an immensely increased number ought to have found ingress by the greatly enlarged opening. To expose any of the normally protected membranes, even the dermis itself, to the air, has always been known to be prejudicial; and to keep a raw or suppurating surface protected from it has as long been known to be important for obtaining a speedy cure. It is a method which Nature constantly employs in the scabbing over of grazed or wounded parts, and it is a method which surgeons, time out of mind, have practised. It is, however, not the simple exposure of such parts to the atmosphere which causes the mischief, as, on the germ-theory of septic putrefaction, it should be; for keeping them in an irritable condition is quite as often the cause. An irritable sore, causing constitutional fever while the part is exercised or in a depending position, will, other circumstances being precisely the same, on being kept quiet in a horizontal position, often at once heal, and the fever cease. A raw or wounded surface, if ever so protected from the atmosphere by a stimulating resinous plaster, will not unfrequently soon become inflamed and suppurating; whereas, if covered with a simple, bland, albuminous, or mucilaginous substance, it will at once cease to be so. Now the former resinous material would repel or destroy the germs, while the latter should tempt their presence and nourish them. Yet, with that, ill-conditioned septic suppuration takes place; with these, it does not. A piece of common adhesive plaster, friction with a turpentine liniment, a scratch with a rusty nail or a fish-bone, or even a common pin, a leech-bite, or the sting of a bee, will, in some persons, cause as much suppurative mischief, absorption of pus, sloughing of tissues, purulent deposits, septic poisoning, and the death of the person, as will any compound fracture, dissecting wound, inoculation with inflammatory serous effusion, puerperal fever, idiopathic cellular erysipelas, or even the bite of a venomous snake, in others, or, perhaps, the same person, at a different time and under different circumstances; the symptoms, like the result, being identical in all these forms, however originating. These effects may be constitutional, but they cannot be germ-poisoning. Some of them may even be the direct effects of an actual poison inserted at the time the wound has been made; but what reason exists for imagining that septic germs are introduced by the sting of a wasp or the bite of a cobra, rather than by the prick of a needle or the tooth of a healthy animal? In the former, the presence of a virulent poison is well known to exist. In the latter, if there be germs, we might expect them to be carried into the wound. Yet the formation of pus is the exception and not the rule. That septic effects take place in the animal body by the decomposition of either the fluids or the solids themselves, or by the exposure of them to the air, is, and for long has been, believed in as a medical doctrine, which also has had the almost, if not altogether, unanimous support of chemists. Upon it much medical and surgical reasoning and practice for generations have been based, until the present day, as those who refer to the works of the great practitioners of the last and early part of the present century well know. Hence the constant employment of septic and antiseptic terms and remedies, as applied to the constitutional condition, and not to extrinsic circumstances as now.

John Hunter taught, and Sir A. Cooper and many other surgeons practised it, that, when a wound can be sealed with its own blood, provided the quantity be not too great, so that it decomposes and acts as a foreign substance, and yet enough to desiccate and form a cement which excludes the air, no better can be found. I very often successfully employ it in compound fractures. Liston and many others adopted isinglass, which for generations before as goldbeaters' skin, court-plaster, or white of egg and other gelatinous and albuminous forms, have been in popular use. More recently, Dr. Richardson has suggested a compound of tannic acid and collodion, which he calls styptic collod, and declares superior to every other. All these appear to act in a like manner, by affording an unirritating cover to the raw surface. Possibly, if there be much blood effused, the styptic collod, by the tannic acid which it contains securing the certain coagulation of this, may possess some merit over the simple dried blood itself.

For some years past, there has been a growing tendency to attribute various diseases in man to microscopic organic bodies; and, since the proof that some of the diseased conditions in grasses, tubers, plants, and trees, may be traced to such sources, this belief has grown stronger. Analogy has shewn that some human cutaneous affections may have their origin in this way; and, as it appears certain that some of these minute bodies are active agents in assisting, or, at least, are always present in, diastatic action, such as fermentation, it has been thought not improbable some of the widely spreading epidemic diseases—zymotic, as it has become the fashion to call them—may have their origin in the atmospheric diffusion of various species of organisms. This hypothesis, which, as yet, is a mere speculation, very insufficiently

* See Lister *passim*. BRITISH MEDICAL JOURNAL and *Lancet*.

† "And now, before speaking of some cases treated with carbolic acid on the antiseptic system, I wish to direct your attention to an experiment illustrating the germ-theory of putrefaction. It is upon this theory the antiseptic treatment is based, and I venture to say that, without a belief in the truth of that theory, no man can be thoroughly successful in the treatment."—Lister, in BRITISH MEDICAL JOURNAL, July 18th, 1868.

supported by facts, has had an undue prominence given to it by some of the more active public hygienists, and, not very remotely, has biased men's minds towards the adoption of the germ-theory of surgical diseases resulting from wounds.

The germ-theory of putrefaction appears to rest upon the assumptions—

1. That the air is filled with minute living organisms, which are so numerous that they freely pervade every atom of it:
2. That these organisms are the sole and true cause of decomposition, and of the putrefaction of every thing and everywhere:
3. That these minute creatures are as active as they are ubiquitous, continually going to and fro, seeking whom they may devour, so that upon whatever raw surface they are allowed to settle or to enter, putrefaction and destruction follow; where they are excluded, no supuration takes place, or if it do occur (which, indeed, it is impossible to deny) it becomes innocuous:
4. That the true remedy against them is carbolic acid; for, though other things may be useful against them, none are so effectual as this, which not only has the power of destroying them, but is able to convert dead and foreign matters into material for the production of healthy normal tissues. Dead bones, which heretofore commonly required to be taken away mechanically, are said, under its influence, to become absorbed and quickly to disappear; even fibrous material being afterwards unheard of.

As to the existence of these living organisms upon which the antiseptic treatment is based, I cannot enter at length. It would be altogether beyond the scope of the present address to enter fully upon the disputed question, whether homogenesis—generation from parents—or heterogenesis—spontaneous generation—is the true mode of the creation and multiplication of sperms. That question must be determined by each member for himself consulting the many works and papers recently published on the subject, which will be found principally in the *Comptes Rendus* of the French Academy, and more especially the writings of M. Pasteur on the former side, and M. Pouchet on the latter.* It is to M. Pasteur and his disciples, the Panspermists, who also maintain the doctrine that all decomposition and putrefaction are physiological phenomena, depending upon living germs derived from the atmosphere, that the modern antiseptic school of surgeons look for the rationale of their treatment of wounds. Now, though many of the experiments of Pasteur and his followers are very ingenious and, to some scientific persons, satisfactory, on the other hand, Pouchet and many other most reliable observers have altogether failed to realise the same results when repeating them, and, in many instances, have obtained exactly the opposite. They have also instituted other experiments and observations, which appear to completely negative those of Pasteur. Amongst the most recent and able of these, is our well known associate, Professor Hughes Bennett, who, in an admirable lecture delivered at the Edinburgh College of Surgeons, in January 1868, has given the result of his elaborate and careful experiments and reasonings upon them, in which he altogether denies the correctness of Pasteur's conclusions;† and, I think, proves that some fatal fallacy

pervades those of Professor Lister, made with urine placed in jars fitted with small contorted tubes, which he supposed to be sufficient to exclude the ingress of germs. Satisfactory as these experiments of Mr. Lister appear to have been to some of our associates, I confess they never carried conviction to my mind, even before their fallacy was demonstrated. We may then, I think, at the very least, say the presence of these germs is not proved; and probably, with safety, we may go much further, and deny their existence in the number and universality maintained by Pasteur and Lister. That not unfrequently some minute organisms, vegetable as well as animal, may be wafted in the air, and float about, as microscopic particles of organic and inorganic matter do, is certain; but this is a very different thing from what is meant by the Panspermists. It is, moreover, greatly to be doubted if these low organisms are of such varied genera and species as many suppose. It is far more probable that many which are so considered are but different stages of the same monad. I believe, when such bodies are formed in putrescent substances, they are commonly the result, and not the cause of the putrefaction (the *sarcina ventriculi* vomited with the fermenting fluid in diseased conditions of the pylorus, for instance); that they are formed by the interchange of the elements of the nidus in which they are placed, or with those of the surrounding air; and that the change in it is not caused by them: in other words, that the chemical theory of decomposition, as of material out of the body, so of effused fluids and secretions in the human being, is correct. That these changes result from, and are greatly controlled by, the vital conditions of our bodies, as was maintained by Hunter, and believed in by the ablest men before and since his time, chemists as well as physiologists and pathologists, is certainly true.

That suppuration does occur, even when the antiseptic treatment has been managed and manipulated by the most reverent worshippers of the hypothesis, is undoubtedly true; but then, it is said, the pus becomes innocuous because of the treatment. Now this is simply to beg the question, and is a mere assertion which altogether ignores the millions of cases, throughout all ages and under all circumstances, where supuration has taken place in every variety of wound with as little ill effect as it has ever done in the best selected cases where the antiseptic treatment has been controlled by the chiefs of the hypothesis. Such instances must occur to the mind of every surgeon now present. There is no one who cannot call to recollection cases of marvellous recovery from such shocking mutilations as, according to all surgical rules, ought to have been followed by the most disastrous results, and yet have recovered in spite of nothing having been done. It is not upon exceptional cases that scientific surgery will be built.

That carbolic acid is a specific against purulent infection, or that it acts by destroying septic germs, I believe to be a fallacy, an *Idolus Theatri*; but that it is an useful agent, among many others which have a like effect with it, in coagulating animal matters, in accordance with its strength stimulating the parts and contracting the smaller vessels, so as to arrest the oozing of blood and serum, I readily admit, and, if the claim for it were limited to this action, I believe no one would feel inclined to doubt its value. The practice of applying analogous substances to wounded parts, more especially when lacerated and contused, is a very ancient popular one, as every reader of sacred and profane

shown his imagined proofs as to the existence of atmospheric germs to be altogether erroneous. We may conclude, therefore, that living germs are not necessarily the cause of putrefaction and fermentation; neither is it necessary to believe that ferments are living at all—they may be dead. This, if not admitted, seems to be implied by Pasteur himself, who tells us he can now excite these processes, not by fresh yeast, but by the ashes of yeast. That they may be induced by the dead organic matter, which has been subjected to a direct temperature of 150 or 200 deg. centigrade—a heat utterly incompatible with the existence of life—we have seen to have been proved by Pouchet, Musset, Solly, and others.

"The idea that these imaginary germs were the cause of putrefaction, of disease, of blights among vegetables, and other evils, originated with Kircher and the pathologists of the seventeenth century. It has been frequently revived, but always shown to be erroneous. In 1852, cholera was supposed to be occasioned by a fungus that really existed in the dejections; but which Mr. Busk pointed out was the *uredo segetum* of diseased wheat, which entered the body in the form of bread. Certain well known parasitic diseases are spread by contact, such as scabies, which, as it depends upon an insect burrowing in the skin, may be understood to crawl from one person to another. Favus, also, I succeeded, in 1841, in proving might be made to grow on diseased surfaces of otherwise healthy persons; but many of our unquestionably infectious diseases, such as small-pox, scarlatina, measles, and typhus, have no such origin. It has been attempted to be proved, indeed, by Lemaire that, in the condensed vapours of hospitals and other putrid localities, vibrios may be found; but that vibrios are the cause of these various diseases is not only not proved, but, from what has been stated, is highly improbable. What then, it may be asked, is the origin of the infusoria, vegetable and animal, that we find in organic fluids during fermentation and putrefaction? In answer to the question, I say they originate in oléo-albuminous molecules, which are formed in organic fluids, and which, floating to the surface, form the pellicle or proligerous matter. There, under the influence of certain conditions, such as temperature, light, chemical exchanges, density, pressure, and composition of atmospheric air and of the fluid, etc., the molecules, by their coalescence, produce the lower forms of vegetable and animal life."—*Idem*, pages 23, 24.

* *L'Origine de l'ie*. Par Georges Pannetier. Avec une Préface par F. A. Pouchet. Paris: 1868.

† "This" (*i.e.*, the composition of dust) "has been tested in various ways. The dust has been ransacked to discover organic germs; collected and carefully examined with the microscope, near the soil and on the summits of the highest buildings; not only in frequented, but in desert places; in crowded assemblies, as well as in Gothic cathedrals and ancient vaults; in the ancient palace of Karnak, on the banks of the Nile; in the tomb of Rhamesis II, at the extremity of the Desert, as well as in the central chambers of the great pyramid of Ghizeh. The chief element of the dust collected in these places has been found to be starch-corpuscles. Large quantities of air have been drawn through tubes by aspirators, and collected on cotton, in distilled water, or projected on glass. The feathery snow which, falling through the atmosphere, may be well supposed to collect its contents, has been melted and the precipitate carefully collected. The emanations of marshy places, such as those of the Maremma, in Tuscany, have been specially investigated. The larynges and mucous pulmonary surfaces of numerous animals have been explored, even to the innermost bone-cavities of birds. On the summit of Mont Blanc, amidst eternal snow, on the glaciers of the Jura and of the Pyrenees, and in the deep crevasse, on the burning plains of Egypt and in the markets of Constantinople, the dust of the atmosphere has been microscopically examined—and in all with a like negative result as to the existence of germs. Nowhere could they be seen; or if a few, in the opinion of some, were visible, could they in any way account for the multitude of minute infusoria, which, in all these localities, not only readily spring up in putrid fluids, but in every instance are identically the same."—Bennett, *On the Atmospheric Germ Theory*, 1868, p. 14-15.

"The only conclusion I can draw from the numerous contradictory and ingenious communications presented to the Academy of Sciences during the last eight years on this matter is, that not the slightest proof is given by the chemists, with M. Pasteur at their head, that fermentation and putrefaction are necessarily dependent on living germs existing in the atmosphere. They rather tend to show that these are phenomena of a chemical nature, as was ably maintained by Liebig. Did we, indeed, confine our reading to the papers of M. Pasteur—that is, to one side of the case—we could easily persuade ourselves of his correctness; but every one of his experiments has been repeated by several independent investigators, who have

history well knows.* One or other of such substances, more especially the resins, balsams, camphor, musk, and various alcoholic preparations, alone or combined, enter into all the more celebrated vulnerary preparations of former surgery, and for generations out of mind have formed the bases for the nostrums of the village doctress and skilful horse-leech, into whose hands they doubtless descended from earlier authorised practitioners. In the hands of an old lady, who I know inherited the practice from her grandmother, in my boyhood, well bathing a wound or bruise with old rum was a sovereign remedy, which I have many time seen applied to others and smarted under myself, and to its efficacy I can even now bear testimony. I have always taught my pupils, where parts have been seriously injured and had their vitality lessened in a great degree, as smashed fingers and other badly contused wounds, that one of the best applications to them is thin linen lint well saturated with some balsamic tincture, as benzoin, which, uniting with the blood, forms a hard sheath, often only to be removed when cicatrization is complete. Doubtless scores of surgeons have realised like results. This I have always attributed, as Hunter (who was well aware of the effect of the application of spirit to a wound) did, to the coagulation of the albumen, and its forming, with the blood, an impervious cover, by which the air is excluded and decomposition prevented, and not to the exclusion of septic germs. The albuminous compound, if in the wound, is innocuous and is capable of being absorbed. Even where there is a mere abrasion, and only a little oozing of serum or liquor sanguinis, a fast impervious glaze—an artificial epidermis—is formed, and thus an anæsthetic effect is produced, and the desiccating influence of the air prevented.

In large wounds, as amputations, this same effect has, in one form or other, been long sought for. It forms one of the most essential conditions for union by the first or the second intention. If bleeding in the flaps at once ceases, they may be brought together without delay, for the intervening layer of blood, coagulable lymph, or liquor sanguinis, will be so thin that its vitality will be maintained, and direct union will take place; but, if the oozing do not cease, the effused mass is too considerable, its vitality is not maintained, the flaps are too widely separated to influence it, and decomposition of it occurs. It is to obtain this arrest and "glazing of the flaps" that these have been, and still are by many surgeons, exposed to the action of the air for three or four hours before being adjusted to each other. Yet how can this long exposure of such large tempting surfaces to the myriads of craving septic germs be reconciled with their refusal of the inviting offer? for such exposed surfaces often neither suppurate nor furnish foetid pus; indeed, some good surgeons declare it to be the best mode of treating flaps so as to secure primary adhesion of them. Such, also, is doubtless the explanation of the action of a solution of the chloride of zinc, reported so favourably of by Mr. De Morgan, Mr. Moore, and other surgeons, and of the perchloride of iron as long used and preferred by myself and others. The simple washing of the flaps with rum or alcohol, simple, or holding in solution some of the balsamic gum-resins, has the same effect; and such, I believe, is the identical action of a solution of carbolic acid. If it be applied as a wash, as the other agents have been, to two clean raw surfaces, no impediment is presented by the interposition of a bulky foreign substance to their direct and immediate union; and, like them, if sufficiently diluted, not being noxious, its absorption is not injurious. If it be mingled with effused blood in a cavity, coagulation takes place: a solid, unirritating, not easily decomposed substance is formed, which, like many other organic compounds, undergoes absorption, and a cure is the result.†

* "Is there no balm in Gilead? is there no physician there? Why, then, is not the health of the daughter of my people recovered?"—Jeremiah, chap. viii, v. 22.

† "Go up into Gilead and take balm, O virgin, the daughter of Egypt; in vain shalt thou use many medicines, for thou shalt not be cured."—Jeremiah, chap. xlv, v. 2.

"And he went to him and bound up his wounds, pouring in oil and wine, and set him on his own beast, and brought him to an inn and took care of him."—Luke, chap. x, v. 34.

"The very word balsam seems in all ages to have had an idea of excellence, and efficacy affixed to it above any other branch of the materia medica; for the ancient physician by this word meant any species of medicine which powerfully recommended itself by a grateful and delicious fragrance, and whose use, both internal and external, was of singular efficacy in preventing putrefaction and resisting corruption."—See the very learned and elaborate article Balsams, in James's *Medicinal Dictionary*.

† I have instituted numerous experiments by adding various substances to blood just drawn and still fluid, to coagulated blood, and to the serum alone; also, I have applied several of these, as pure alcohol, perchloride of iron, chloride of zinc, tannic acid, tincture of benzoin, sulphate of alumina, etc., to wounded and lacerated parts, for the purpose of ascertaining their effect, and the power which the resulting compound has of resisting decomposition and of being absorbed. Some of these, as the strong mineral acids, rather change and decompose the blood than simply coagulate it. Though the resulting compound, even when freely exposed to the air, will remain for many months unchanged, they are unsuited for employment; for, if strong, they destroy the tissues, and, if weak, are not effectual. Chloride of zinc forms with

In this sense, carbolic acid is an antiseptic, and the treatment is antiseptic; and so it is, just in the same manner, with the other substances I have named, and also many others, but it is not antiseptic in the sense in which it has recently been so prominently spoken of. It does not destroy putrescent producing germs; for, as I believe, they have little or no influence. This speculation of organic germs is, I fear, far more than an innocent fallacy; it is a positive injury, for, teaching, as it does, that those desperate consequences which so often follow wounds result from one cause alone, and are to be prevented by attending to it alone, and that in a very simple manner, it leads to the ignoring of those many and often complicated causes, which I have indicated as influencing, for good or evil, the progress of a wounded person, attention to which has hitherto been considered to be one of the most essential parts of a surgeon's duty, and, as achieved or not, in a great degree to distinguish the able and skilful practitioner from the inexperience and unskilful man.*

The advocates of the antiseptic theory, however, triumphantly bring forward successful cases in support of their hypothesis, and declare that the efficacy of the treatment indubitably proves the truth of their theory. We may readily admit the truth of many of their facts without believing their premises. Every one of the reported cases may possibly be true, but they may be mere sequents, and by no means necessary consequences. Nothing is more fallacious than such a mode of reasoning. It is very easy and very captivating, but most erroneous. It has been too frequently practised, not only by medical men, but by all classes. The *post hoc* is constantly confounded with the *propter hoc*, yet it often happens that they have no necessary connection with each other. The one follows; the other has merely gone before. They do not stand in the relation of cause and effect. No cognisance is taken of any but one favoured antecedent, whereas there may be many others inseparably connected with the result. Look at the false theories, on every imaginable topic, supported by this kind of evidence, which have inundated the world. Every medical man cannot but recollect how such fallacies have retarded scientific medicine in all generations. There has hardly been a single disease, or the treatment of it, which, at one time or other, has not been subjected to this process. Thousands of remedies of every kind, which we now know to be absolutely useless, if not worse, have been vaunted as the true cause of cures. Never was such loose and inconsequent reasoning more prevalent than at the

the blood a solid mass, which is not so dense or resisting as that formed by carbolic acid or perchloride of iron, and a greater proportion of the substance is required to produce a corresponding effect. Alcohol forms a dense coagulum; tincture of benzoin answers still better, forming an odorous, solid, moderately unchangeable mass, and, I believe, deserves much of the credit so long attributed to the balsams; carbolic acid at once coagulates the blood, and forms a dense leathery mass, which decomposes very slowly; with the serum it forms a similar, but, as might be supposed, a less dense mass, from which fluid exudes. Perchloride of iron, of all these substances, appears to answer best. It acts promptly, a small quantity only is requisite, not more than one-sixth of the quantity of pure carbolic acid being required; it does not destroy or injure the bed-linen, as the acid does. The compound is dense, adherent, very unchangeable, and, being composed of only those substances which normally enter into the composition of the animal body, is more adapted for innocuous absorption, if not for actual assimilation and incorporation with it. Alum forms a firm coagulum, and tannic acid answers admirably, forming a soft leathery mass, which is very unchangeable. Though the compound of blood and iron will remain without decomposition for months, a white mould forms in it much sooner than in the compound of blood and carbolic acid. It is also more soluble in water than is the latter. A small portion of a solid mass of three ounces of blood, with which two scruples of perchloride of iron had been mixed two months before, and exposed to the air with only a paper covering, was put into water, in which it became partially dissolved. A like portion of a compound of three ounces of blood, with two drachms of pure carbolic acid, was similarly placed in water, and, after a month's exposure, hardly any, if any, was dissolved. If solution be necessary for absorption, this experiment would lead us to infer that the iron compound is more readily absorbable than the carbolic acid compound.

To show how theory influences practice, and how readily even able men are liable to be led away by hasty conclusions, it is worth calling attention to the extension of the putrefactive germ theory to the existence of pus in the urine. It has been alleged by learned and well known professors, in more than one of our universities, that the alkalinity of, and pus in, the urine arises from the presence of these septic germs, vibrios, and such like organisms, which have been introduced into the bladder by badly washed catheters, and the cure is asserted to be "carbolic catheters" to kill them. This idea is so opposed to all that is known of such affections as only to require being mentioned to be thereby refuted, and wondered at ever being adopted. What surgeon or physician has not frequently seen persons with alkaline urine containing pus, who have never had a catheter passed in their lives? Who has not seen such a condition improve at once, and in the end disappear altogether, by the use of frequent daily catheterism? Who has not known scores of cases where persons are, going about their daily avocations, and introducing for themselves catheters, which are carried about loosely in the pocket, where they cannot be clean, and yet with the urine perfectly free from pus? Who does not know the frequent consequence of nephritis, cystitis, enlargement of the prostate gland, injury to, or disease of, the medulla spinalis, and stone in the bladder? How are vibrios to get into the bladder in such cases, where no instrument has been used? and if not in them, why in others? Is calculus or abscess in the kidneys, or a rugous mucous membrane in the bladder, to be ignored as the origin of pus in the urine? If such condition were the result of septic germs, why, having once entered the bladder, and lived there, do they not seize upon the whole system and destroy it, as they are alleged to do when they enter by a wound

present day. Look alone at the assertions within the last few years as to the cause and proper treatment of plague, cholera, yellow, typhus, and typhoid fevers, and other epidemics affecting man and animal. See even how it has influenced, and not unfrequently led to hasty and injurious legislation on such subjects. It is the foundation for most of the evidence upon which every empiric relies, and vaunts and parades his success as infallible. He has done a certain thing to a certain part; it has become well; *ergo*, his remedy has cured the disease. If such be correct, every homœopath has good ground for his assumed success. Recollect the evidence and cases of success which lately were brought forward in support of the asserted cure of cancerous tumours by the injection of acetic acid. Yet who now believes in the truth of the doctrine? Sir J. Simpson, Drs. Pirrie, Keith, Fiddes, and others, have brought forward cases quite as numerous, and undoubtedly quite as true, in proof of their declaration that the use of fibrous material as ligatures upon arteries is the true cause of all those disastrous consequences which so often follow operations, as have Mr. Lister and others in proof of their assertion, that it is the introduction of septic germs which does all the mischief. They have performed large, important, and dangerous operations of all kinds, merely substituting acupressure in closing the arteries for threads. They have never dreamed of applying antiseptic treatment for the destruction of septic germs, to which, by discarding all dressings, they allow the freest entrance to the wounded part, yet they have reported such a succession of marvellous cures as have fallen to the lot of few surgeons. Now, will Mr. Lister allow that their success was dependent upon acupressure alone? Setons have been used in surgery from time immemorial, and rowels still are employed in veterinary surgery, not only in the cure of abscesses, sinuses, and fistule, but to establish a drain in many external and internal complaints. M. Chassaignac has found many followers all the world over, in the use of drainage-tubes, made of vulcanised India-rubber, having plenty of holes stamped in them to render them still more patulous, in the treatment of recent abscesses, suppurating joints, and even effusions into the thoracic cavities; cures without number being reported as the happy result of thus affording a broad, direct, and easy road, with food by the way, for the free ingress of septic germs into all these varied cavities. Yet, surely, it can hardly be affirmed, if this treatment and the cures, as is stoutly declared, stand in the relation of cause and effect, that the antiseptic treatment and its cures also do so. If the one which acts by keeping the discharge in, and the exclusion and destruction of the germs, be true; the other, which acts by not only letting the discharges out, but the germs freely in, cannot also be true. Taken as sequents, the reported cures in both may be true, but regarded as consequences this is impossible.

That wounds, whether simply incised or lacerated and contused, often do well when dressed with carbolic acid, is perfectly true; but that they do better than wounds differently treated, I deny. It is useless to bring forward cases, and compare them with other cases, where the circumstances are altogether different. In private practice, as well as in large hospitals, especially in the latter, circumstances so vary, that to decide by a name alone is worse than useless. Every surgeon knows that, besides the individual varieties of age, constitution, condition, habits of life, the mode of receiving the injury or disease, there is a general law affecting all. At one time, all his operations do well; he hardly loses a case, whatever the operation may be; and at the same time all wounds heal quickly and well, without suppuration; or, if pus do form, it is limited, and is laudable: while, at another time, precisely similar cases do as badly, so that even very trivial wounds and operations are followed by death.* In our infirmary, a reference to the records of operations will show this difference in a striking manner; and everybody also knows quite well that at such times certain erysipeloid diseases are prevalent in the district, and that patients without wounds will fall into a bad condition, and not unfrequently exhibit

similar constitutional symptoms. There are undoubtedly phases of unhealthy atmospheric (or, I would rather suppose, telluric) influences, which, in spite of every known hygienic precaution, will make their influence most grievously felt. Hospitals have not unfrequently had to be wholly or in part temporarily closed from this cause. Therefore, to get at a fair comparison of the results of any particular treatment, the same class of cases should be treated at the same time and place. This I have carried out. During the last three years, since "the antiseptic treatment" has been in vogue, I have not allowed one of my patients to be treated with carbolic acid; while my colleagues have very extensively employed it, and I may say, at least at one time, possessing the full amount of faith necessary for securing success, fairly tried it. The result is, that my cases without it are as good as theirs with it. They have had some capital cases with it; but I am confident I am not in the least overstating facts, when I declare that for every successful case with it I can show one as good without it. This I consider to be a fair comparison, as the class of cases and other circumstances have been identical. Had I during this time found their success with the antiseptic treatment was greater than mine without it, I should gladly have availed myself of it; but I did not. On the other hand, as an additional proof that it has not been so, it may be mentioned that lately the omission of it, even in large operations, has been more and more frequent, until now its employment has become the exception, instead of the rule; which, had any marked benefit resulted from its use, most certainly would not have happened. Had carefully covering the cut surfaces with carbolic acid yielded great benefit, we should not have witnessed many large stumps left entirely exposed to the air, without any covering whatever upon them. That the two plans are wide as the poles asunder, is obvious to every one. If freely exposed stumps heal up readily and well, it must be at once apparent that those which do so when most elaborately swathed in carbolised wrappings, do so rather in spite of, than as a consequence of, the treatment.

One word more, and I have done. I have spoken plainly on the antiseptic treatment, because I have felt decidedly. Let me, however, not be misunderstood. I have no other object in view than the advance of our profession. I willingly pay deference to, and acknowledge with thanks as warm and as cordial as those who have believed and adopted the antiseptic treatment, what good has been done, and I trust will still be done, by some of those who have been the most decided advocates of the method. To them I would say, in the words which Shakespeare puts into the mouth of Brutus, when speaking in the Capitol to the Roman people over the dead body of Cæsar, "If there be any in this assembly, any dear friend of Cæsar's, to him I say that Brutus's love to Cæsar was no less than his. If then that friend demand why Brutus rose against Cæsar, this is my answer: Not that I loved Cæsar less, but that I loved Rome more." So would I say, not that I value their works and labours less, but that I value surgical science more. When I believe doctrines and practice to be true, I venture to acknowledge their value; but when I am convinced that they are wrong and injurious, I presume to say so.

[Since the reading of this Address, Mr. A. Prichard of Bristol has informed me that the operation of removing the entire eye-ball, instead of the anterior half of it, only originated with him, and not with Dr. O'Ferrall, to whom it is so often attributed.—T. N.]

A CASE OF EPITHELIOMA OF THE LARYNX.

By THOMAS COLE, M.D. Lond., Bath.

A FINE boy, aged 5, was placed under my care in January 1869, for laryngeal dyspnoea and a spasmodic barking cough. Otherwise, he was in good health, and seemed remarkably strong. He had no pain, nor expectoration. At one year of age, the breathing was observed to be rather difficult; and it became more and more so till I saw him. At four years of age, he fell into the river, and shortly afterwards the cough commenced. This, I think, was merely a coincidence. I was suspicious of some kind of growth in the larynx, and cautioned the parents that the poor child might die suddenly. He would submit to no examination. Bromide of potassium was given for a month, with marked relief from the dyspnoea. On March 3rd, the boy died almost instantaneously. The larynx, on *post mortem* examination, was found to contain two large yellowish-white growths, each occupying one of the ventricles, and obscuring the vocal cords. The right tumour was as large as a horse-bean; the left, as a hazel-nut. The latter was somewhat hollowed out by the pressure of the former. There was a third smaller growth behind the left one. It seemed that no air could have entered the trachea, except by a very small passage left between the anterior surfaces of the tumours and the pomum Adami. Microscopically, the tumours proved to be good examples of epithelioma. Dr. Martyn of Clifton, who

* In the General Infirmary at Leeds, from May 1st to October 31st, 1865, out of 100 recorded operations, there were 21 deaths. In the following six months, Nov. 1865 to May 1866, out of 118 operations, there were 23 deaths. From June to end of Nov. 1866, there were only 10 deaths out of 115 operations; while from October 1st, 1868, to end of March, 1869, out of 140 operations of all kinds, not, however, including the trivial, there were only six deaths; and, in the last three months of the period, only one death is recorded. In each of the periods, there was no material variation in the number of operations, and none, so far as I can ascertain, in the nature or gravity of the cases, and yet in the last six months the deaths are not quite 1 in every 23 operations, while in the first the deaths amounted to over 1 in every 5; in the next six months to nearly as many; and in the six months from June to November 1866, the rate sunk to 1 in 12. Seeing the number of patients in the hospital was the same in each period, there surely must have been some potent unseen cause influencing the result; mere "hospitalism" will not account for it. In the last period, when there was the largest number of operations and the greatest crowding, the fatality was the least. Moreover, the last months of the hospital, which had been above a hundred years in use, were far more healthy than many periods which had occurred during its long occupation. Only operations of some magnitude are included in this record.