sneers if you tell him that quinine is useless? Will the patient who, when wrestling with an agony of pain, is promptly relieved and comforted by a hypodermic injection of morphine or a dose of laudanum or of ether, believe you if you tell him that it is not the drug that has wrought the change? Are anæsthetics not drugs? Yet what would the millions who benefit by them say if they were in the future to be denied their beneficent help? Are antiseptics not drugs? and would the surgeons of the world be content to go back to the old days and to the state of things which held sway before their discovery? Even of the more simple every-day remedies—the alkali, the gentle laxative, the drastic purge, the emetic, the febrifuge—can we venture to say that we dare to face the daily ills of life without their aid? No, gentlemen, we have advanced in knowledge, in culture, in observation, and correspondingly in power; but we cannot afford to cast aside the benefits we derive from our humble friends, the bottles on our shelves, as yet. Year by year we eliminate some of them, but only to replace them by others more directly efficacious than they; year by year we add to them, as we have recently added cascara, strophanthus, and others. Slowly but steadily we are learning to cast away their coarser, more distasteful, and useless elements, and to search for and depend upon their active and energetic principles; simplicity in prescription, portability, palatability, and efficacy are daily and everywhere more and more sought after, and in this department, as in all others, genuine progress is perceptible; but the day is yet far off, in the dim and distant future, when medical men will feel justified in meeting all the ills of life with folded hands, a sympathetic eye, and the genial influence of only a warm bed.

Gentlemen, I pray you pardon me if I detain you to add a few words more of a purely personal character. I commenced my address to you this evening in an unusual fashion. It is customary, on such occasions, to open with an expression of gratitude for the honour conferred, and a few commonplaces of that kind, which everyone understands, accepts, and smiles at; but to-night I have ventured to disobey this common practice of society, but only because I wished a little later on, more earnestly, more emphatically, and more directly, to offer you my personal thanks on a wider scale.

In the late election of direct representatives of the profession on the General Medical Council, the whole profession to which I have the honour to belong united to confer upon me a distinction which I shall ever be proud to remember, and returned me triumphantly at the head of the poll as its first member to that which is practically our professional Parliament. If I were not grateful for such an honour, I should be insensible indeed; and now that you, by electing me as your President for the year, have added your sign manual of approval to that election, I ask you to take it for granted that I heartily appreciate your kindness. I accept the office thoroughly imbued with this feeling, and I ask you to believe me implicitly when I tell you that I do so. And I go a great deal further still. I am requested by a body of men among whom I have laboured all my life, whose interests have been my life's study, whose profession is that to which I too have the honour to belong, and who are, therefore, my peers in the strictest sense of the word, to accept an office which implies that I will, in a position of unparalleled responsibility, protect their interests, seek to forward their rights, and guard their honour. Am I likely, do you think—is it possible for any man—to accept such a call either coldly or with indifference? No, gentlemen, I regard the late poll as a solemn call to a most sacred duty; and while I ask you to accept my most grateful acknowledgment and thanks, I promise that, God helping me, I will not be false to the trust you have reposed in me.

THE need has long been felt of a seaside home to which temporarily disabled policemen could be sent. An influential committee has been formed, including the Speaker, Lord Wolseley, the Dukes of Norfolk, Portland, Westminster, Grafton, Sutherland, and Manchester, and other representative men, for the purpose of urging upon the benevolent public the advisability of establishing such an institution.

ST. JOHN AMBULANCE ASSOCIATION.—Certificates of proficiency were presented on Sunday last by General D. Anderson, Governor of the Royal Military College, Yorktown, Farnborough, to fifty-five cadets who had attended a course of lectures and instruction on "First Aid to the Wounded."

IT is proposed to erect at Salford a new hospital for infectious diseases at a cost of about £90,000. This hospital is to supply the place of Wilton Hospital, which the Health Committee have sold to the North Western Railway Company for £21,000.

THE female scarlet fever wards of the London Fever Hospital, Islington, are to be enriched with decorative wall-paintings, the work of Miss Emily Wall whilst a patient in the hospital. These paintings are stated to be of a high order of merit.

AN ADDRESS

ON

THE DEVELOPMENT OF SURGERY AND THE GERM THEORY.

Delivered at the Annual Meeting of the Birmingham and Midland Counties Branch.

BY LAWSON TAIT, F.R.C.S.,

Surgeon to the Birmingham and Midland Hospital for Women; President of the Branch.

GENTLEMEN,—Your kindness in placing me in this position of distinction brings with it for all of us the burden of an address. I tried hard to find some new line for this, but I have failed, and I have to fall back on the retrospect inevitable in all such efforts, and, more serious still, the retrospect must be personal.

I am reminded that it is exactly twenty-seven years since I entered the University of Edinburgh as a medical student, and I have been trying to recall to mind, as accurately as I can, my impressions of what I then saw, to contrast them with what I see now; to recall the teaching and practice of 1860, and compare them with those of 1887, and to see wherein and how far we have improved.

In such a retrospect there will be this advantage, that I shall speak of events and contrasts which most of you have witnessed, and which can, therefore, be in no way regarded as ancient history.

My earliest surgical recollection is still very vivid. The operating theatre of the old infirmary was crowded, every seat even of the top gallery was occupied. There were probably seven or eight hundred spectators, for Syme was to operate on a gluteal aneurysm. He was then in the zenith of his fame, and in the very best of his powers, his hand as steady and his eye as true as it ever had been—incomparably the best surgeon I have ever seen. He entered the theatre with the recognised procession of assistants—house-surgeon and dressers—and was greeted with a subdued murmur of applause. The spectators included men of all ages and ranks in the profession. Very many who had come from great distances to see the great feat—like Bickersteth, of Liverpool, who came specially to assist, if I remember rightly—and, of course, there were many boys like myself from fifteen upwards. The patient was put to sleep, Syme buttoned up his dress-coat, turned up his sleeve, I saw a rush of blood, and in a few minutes the placing of the patient in the carrying chair and a round of applause announced the conclusion of the operation.

I have often wondered since, and now I wonder still more deeply, why we gathered in such numbers to see that operation. The boys did so from curiosity and in ignorance. I am sure I did, and my ignorance was in no way lessened, for I saw little of the operation, and I understood less. The older and more experienced spectators went mostly from curiosity, and perhaps to be able to say that they had seen the great surgeon do this great deed. Some six or eight men were there, like Bickersteth, who might some day be called upon to operate for a gluteal aneurysm, and they were the only legitimate spectators. All the others had far better have been away.

In those days the first-year students were turned into the operating room, and I believe this practice is still continued, to learn how operations are done before they know the names, far less the relations, of the structures concerned. In the dissecting-rooms our greatest feats were to display skilfully the fancifully numerous layers of fascia which, if we had been really operating for hernia, we certainly would never have troubled about; or to remember the varieties of troublesome perineal arteries with which not one per cent. of us would ever have any practical anxiety.

The strange thing was that this nonsense was encouraged by our teachers—nay, it was carried into the examination hall. At that time, as now, the College of Surgeons was the great manufacturing agency for those practitioners in England and Wales who had the care of all the ordinary ailments of British humanity, as well as the smaller number who operated on gluteal aneurysms. In order to receive a certificate which enabled the candidate to deal with scarlet fever and pneumonia, he had to answer questions about the internal iliac artery, and how it might be tied. Such a trifling accident as that upon which many of you depend for your night work, the process of parturition, had not been regarded as of nearly so much importance as the relations of the pelvic fascia.

Now all this is greatly changed, but it is not changed enough, and there is still room for improvement. It is perfectly true that a great bulk of surgical work (with which for precision and brilliancy of result, as well as for difficulty of detail, even Syme's gluteal aneurysm cannot for a moment compare) is now done in small rooms before audiences restricted to a few post-graduate pupils. All this—a quarter of a century ago—even if it had been possible, would have been the object of spectacle. Now two facts are recognised that were apparently hidden from our fathers; that the great bulk of human ailments are such as do not require operative interference, and that the acquisition of a surgical diploma is no guarantee of the possession of the manipulative skill necessary to be an operating surgeon. In my youth, the great object of the schools seemed to be to turn out men able to cut for stone and tie arteries, and the more solid and more frequently exercised qualities of the general practitioner were neglected. To some extent the reason of this was that the great bulk of the students went to the university with so large an amount of experience gained by the old-fashioned apprenticeship system, that many of them could really claim to be already accomplished practitioners. But the changes of the last quarter of a century have almost ended that most excellent method of training, and I find now that our young men leave the schools in large numbers in just such a state that they are ready to begin to learn the profession they have to follow.

My business at present is not so much with the process of medical education as with some of its results, but I cannot help joining my voice in the wail, which is getting pretty general, concerning the destruction of the old method of education by apprenticeship, and I most emphatically condemn the attitude taken by the two Royal Colleges in relation to the Apothecaries' Company, to which, whatever may be said to its detriment, we owe most assuredly the more solid and useful elements of education for very many past generations. During the time covered by my retrospect, some of the changes effected have resulted undoubtedly in making our students more learned, but I sometimes wish that the coming generation had less about them of the *savant* and more of the doctor.

In 1860 the battle of anæsthetics was nearly ended, and no one ever thought of performing a surgical operation without chloroform, though its application to midwifery practice was still stubbornly resisted by some of the old folk; but the traditions of the old surgeons of the days before chloroform still survived, centring chiefly round the memory of Liston, who must have been a man fulfilling the requirements of the operative surgery of his day more fully than any other of modern times. In those terrible days, when the operation had to be done in spite of the shrieks and struggles of the poor sufferer rapidity was everything, and accuracy had sometimes to be sacrificed to speed. But Liston seemed to excel everyone in his lightning-like movements, with all the accuracy of Syme. Thus he made a reputation, and leaves a memory more like that of an actor than a man of science, as he really was. Now-a-days, when accuracy is everything and speed a matter of little moment, advantaged as we are by our slumbering patient, we can form no notion of the work of such a man as Liston. As boys, we always spoke of him as of a hero. All the stories of him were treasured and handed down, and he formed the standard of comparison to the detriment of all his successors. We then spoke of a lithotomy not in relation to its results, but by the number of seconds it took to finish. Now-a-days, the man who hurries an operation for show is no credit to his art. By this, however, I do not mean that men with slow minds and shaky hands are to be encouraged to engage in operative work.

In a retrospect such as this it is an easy matter to speak of the wonderful advances made by the discovery of the anæsthetic properties of certain volatile drugs, as contrasted with the horrors of the days before this greatest blessing to mankind; but we do not as yet fully recognise the indirect advantages it has conferred upon ourselves. It is, like Mercy, twice blessed; it blesses the surgeon as well as the sufferer, and it has made possible operations which no surgeon could have faced without it. The whole realm of abdominal surgery gives an illustration of what I say. It is true that a few abdominal operations were done before the days of chloroform; but, in this country at least, they were nearly all removals of simple parovarian cysts. The first ovariotomy by Charles Clay did not occur till September 27th, 1842. Baker Brown had slumbering patients, and, without the unconsciousness to which we now so safely reduce them, not one of those many advances with which the name of Birmingham will ever be closely associated could ever have been possible. It is true that the deep sleep was first cast upon man in the Massachusetts Hospital; but the battle was fought in this island, and the victories which have followed it are due to the pluck and pugnacity of James Young Simpson. "It is not," says Sydney Smith, "the man who first says a thing who deserves the credit, but he who says it so long and so loud that at last he persuades the world it is true."

Having thus been able to do something more than snatch an occasional victory by sleight-of-hand like Liston's, the art of the surgeon made rapid progress, and about 1862 Simpson began to insist that we should know something about surgical results; that we should know, indeed, not only whether we were doing as much as we could do for the welfare of patients upon whom we had to operate, but whether the results obtained were at all in proportion to the labour, expense, and suffering involved. Curiously enough, the research was at first not made on the main lines, but on a side-issue—that of inquiry as to the best method of closing bleeding points. Simpson collected a mass of statistics which excited amazement at the terrible mortality of such simple operations as removals of the leg and forearm. He blamed the old method of ligature, and he led us astray about acupressure; but even that mistaken divergence was of infinite use, for it led us to discontinue the long ligature—an advance which has never been acknowledged, and never accredited to Simpson's work as it ought to have been long ago. It was an advance as great as Ambrose Paré's introduction of the thread itself.

In my youth every stump had a number of threads hanging out of it, and after a week they were pulled by the house-surgeon or dresser day by day until they came away. Sometimes they never came. Simpson's attack led to a reconsideration of the whole question; in fact, we owe to him an enormous debt for the whole advance of modern surgery in the three directions which I have indicated—anæsthetics, statistical research, and the arrest of bleeding. For all of these rich fields were lying ready. Baker Brown showed that we had no need to fear that for which we had all such a mortal dread—a little piece of dead stump inside the abdomen. The rivalry between Baker Brown and Spencer Wells induced the latter to adopt a method of recording his cases which has been followed ever since, and the method of proper statistical research was begun. Finally, the battle of torsion and ligature was decided in favour of short ligatures of animal tissue, and our present perfect methods were established.

But this was not all. Simpson's research on the mortality of amputations and hospitalism showed that enormous advances might be made in our hospitals, and the conclusion was established that, just as in a town, the larger and more crowded the population, the greater the factors of danger, the greater need for precautions of many kinds. Vast improvements in our hospital systems have followed; the old careless nursing has been banished; and where dirt and untidiness reigned supreme, all is now care and cleanliness.

Here, again, I am carried back to the memorable day when I saw Syme operate on the gluteal aneurysm. One of his assistants was his son-in-law, the recently-appointed Professor of Surgery in the University of Glasgow, Joseph Lister, a man who has exercised an enormous influence for good on the progress of surgery during the last twenty years—a verdict which will be accepted the more readily from me as one known to be hostile alike to his doctrines and his practice.

Let me remind you again that just then the great battle of biogenesis was being fought, the leaders of the two sides being Pouchet and Pasteur. The former died early, and his work—almost his name—is forgotten. But on my own mind an indelible impression was made by his wonderful little book, *L'Origine de la Vie*, which fell into my hands in 1868, and which has remained a landmark in my life ever since. I now know that many of his conclusions were incomplete, and many of his observations inaccurate by reason of his faulty apparatus, but his book kept me out of the errors of the school of Pasteur, and freed me from the dreams of Lister, into which so many have fallen.

Shortly after I had finished my curriculum, I was fortunate enough to be appointed house-surgeon to a small hospital in Yorkshire, little known outside its own district, but ever likely to keep a strong hold on my affections, founded by John Clayton, in the city of Wakefield. There I had an enormous mass of surgical material at my disposal, and a kindly and indulgent staff, under whose direction I was permitted to make full use of it. Lister had just published his first papers, and had hardly grasped, certainly had not fully formulated, his splendid idea of antiseptic surgery. From 1867 till 1870, Lister had no more faithful disciple, no more devoted follower, than the unknown house-surgeon of the Clayton Hospital. I spent my days with my hands soaked in carbolic oil, making carbolic putty and securing carbolic lac plaster. Compound fractures were saved, which in Edinburgh would have been condemned to amputation, and I did operations successfully which astonished others as much as they gratified me. Years after, when I had fallen away from the faith, the argument against me which alone caused me grief, was the assertion that

I had never seen and did not understand Listerism. I had been to Glasgow to see it, I had carried it out more scrupulously than the master himself, I had suffered painful attacks of hæmaturia from my misguided enthusiasm years before my metropolitan critics had known what carbolic acid was. But with all my success there occurred the old troubles, death from pyæmia, loss of cases which I could not understand if Lister's doctrines were true. I thought a royal road to surgical success had been opened to me, yet every now and then I found myself floundering out of it, and I began to fear that it was a science falsely so-called.

In 1870 I came to Birmingham in search of a wider field, and my present happy line of life was opened to me. For some years I had little to do in the way of practice, and I devoted myself to the following of the researches of Bastian, Tyndall and Pasteur; all my work, however, being tinctured by the strongest belief in the faith of Darwin. To such a mental altitude the ultimate doctrines of Listerism are an impossibility. This has been completely established by some of our most recent and best English workers; and the wildly extravagant speculations of German observers, whose powers are far more in the direction of metaphysical invention than of actual biological record, are blown to the winds by such a man as Dallinger. Büchner, for instance, carries his cultivating experiments of the bacillus anthracis to an absolute *reductio ad absurdum*, when he tell us that he can, by successive cultivations, change it into the bacillus subtilis; and again that he can change the harmless bacillus subtilis into the deadly bacillus anthracis by a similar process. Dallinger disposes of this briefly, in two sentences. "To one who has fully comprehended the meaning and operation of the Darwinian law, it will be at once apparent that there must be error somewhere in this matter."

"If the law of actual variation, with all that is involved in survival of the fittest, could be so readily brought into complete operation, and yield so pronounced a result, where would be the stability of the organic world? Nothing would be at one stage. There could be no permanence in anything living."

I could grant, at their first appearance, the conclusions of Tyndall, that the lowly organisms which are associated with, and which doubtless cause decomposition, do not and cannot arise save from pre-existing germs. Dallinger's observations completely establish this, even if it wanted anything more than an *a priori* proof. I followed Bastian's work, and soon found enough to persuade myself that his conclusions were based on incomplete experiments. Abiogenesis, as Bastian and Pouchet before him had proclaimed it, was to me, as an evolutionist, a doctrine which could not be adopted. I read with delight Mantegazza's dramatic description of this when he said: "Je dus me lever, brisé de fatigue, mais enchanté d'avoir surpris la vie à son berceau." But I could not bring myself to believe it. To me that was incredible; for the theory of Darwin—amply verified by every fact in Nature—has as a corollary the conclusion that changes are slow in a geometrical proportion to their primitive character. The curve of progress in civilised life is rapid. In the primitive forms of life, the biological record clearly shows that during that epoch the curve of progress is so slow that the human mind can devise no scheme for appreciating its direction at all; no conceivable measure of time can be stated for the period which must have passed during which the simplest chemical combinations existed without form of life, and that in which the simplest forms of life were changed into more complex. To assert, therefore, that out of a mere chemical combination life would be developed in a laboratory experiment, was to try to get us to believe in the reality of the juggler's deception. And yet the followers of the opposite school, with absolutely ascertained facts to guide them, went as far astray on the other side, and absolutely misinterpreted the truths before them. The fact that sterilised beef-tea can be kept indefinitely in a hermetically closed vessel has been known to the housekeeper for generations. Tyndall showed that cotton-wool could so filter air and so sterilise it that it might be freely and harmlessly admitted to the said beef-tea. That also cannot be doubted, and the conclusion is inevitable that if you keep away the germs of the minute organisms, the growth and life of which alone determine decomposition, you keep your beef-tea sweet. But they did not give sufficient prominence to certain other facts; I do not say they did not know them or that they forgot them, but the overwhelming importance of their increasing discoveries dwarfed to the biologists the importance of those old-fashioned facts. Thus the housekeeper also knows that the chemical constitution of the beef-tea has a great deal to do with its keeping sweet; that, in fact, the beef-tea may be put into such a condition as to defy the germs. Such a simple chemical process as putting a great deal of salt in it, or depriving it of its water, will effect this. Similarly, the presence of life in tissue will prevent decomposition, and not only in tissue, but in non-organised material which is in association with tissue. Thus, in an egg there is a very minute—a microscopical—mass of tissue, the germinal vesicle, which, when living, seems to possess the most extraordinary powers, not only of resisting decomposition itself, but of preserving from decomposition the non-organised mass of albumen with which it is associated. Try a very simple experiment with four eggs, and you will see the force of what I mean. The first egg shall be one laid by a hen to which the male bird has never had access, so that the egg is absolutely sterile. The second egg shall be one completely fertilised. If you put both of these eggs into an incubator, and keep them under precisely the same circumstances, in a week the non-fertilised or dead egg will be putrid and will swarm with all sorts of bacteria and spores, whilst the fertilised egg-contents will be quite sweet and the chick well formed.

The second part of the experiment is as follows: Another and precisely similar pair of eggs are to be dipped together for about twenty seconds in boiling water, so as to form a thin film of coagulated albumen immediately within the shell. They may then be put on the shelf of the storeroom for six months, and opened at the same time. They will then be found both quite sweet and fit to eat. This is the rough and ready method of the housekeeper to preserve eggs from the attacks of germs. Of this second pair of eggs, one had life in it and the other had not. The life in the former is destroyed by exactly the same process as that which prevents the access of germs to it and to the egg which never had life. Of the first pair of eggs not protected in this way, the presence of life protected the egg from decomposition, the absence of it allowed rapid and complete decomposition to be effected. There is not a henwife in the kingdom that does not know that, unless the access of the male bird is permitted in proper ratio, not only will the eggs not set, but they will not keep, and that such proper access will enable eggs to be kept for nearly three weeks. After that the embryo loses its life; there is no further protection, and decomposition at once sets in.

If these scientific workers had known as much as the henwife, they would have known that to apply the conclusions derived from beef-tea in the flasks of the chemist's laboratory to the phenomena of living tissue is nonsense. The phenomena of decomposition must not, therefore, be taken for those of disease.

We do not in the least know what life is. We call it vital power, and talk glibly about it, though our men of science seem to have neglected it. But let us suppose for a moment that when life ceases some chemical change takes place; until that change does take place the phenomena of putrefaction are impossible. Take a drop of fluid in which amœbæ exist; put it on a warm slide, and watch the marvellous phenomena—as I have often done for two or three hours at a stretch. The field swarms with bacteria and micrococci, and bacilli of numerous breeds, as well as amœbæ. Pick out an amœba and watch him. Allman used to call him the "all-devouring"—and he well deserves the title. So long as his sloth-like movements go on he is avoided by his neighbours. But his movements get feeble and very slow, and you will see a paramecium go at him. The movements cease altogether, and presently you will find him riddled with bacteria and bacilli, and soon all trace of him will be lost. Why did his enemies avoid him whilst he was alive? Why could they so easily attack him when dead? I cannot tell, but it shows that there is an enormous difference between tissue living and tissue dead.

The reasons which may be urged to show the futility of the arguments of the German school of bacteriologists are endless; but I must not be tempted into a discussion of any of them, for I have to deal with a much wider question. Let me only say that the best of all proof of the fallacy of their assertions is the fact that every attempt to elevate the germ facts of putrefaction into a germ theory of disease has miserably failed, and has failed nowhere so conspicuously as when obtruded into the realms of the treatment of disease.

Let me continue my retrospect on this subject. Increasing engagements gradually withdrew me from the practical study of these interesting questions, and I have not, therefore, closely followed the researches into the new field of work, the cultivation of germs and cognate lines of research, chiefly because they have trenched very little on surgical work. They have been practically confined to the class of diseases dealt with by the physician. They have unearthed many strange and striking facts, and have explained many occurrences of the utmost importance. I do not, therefore, either despise or deride them. I believe I estimate them at their proper value. I believe that John Law was really a philanthropist, and his Darien prospect was a very good thing in its way, yet the South-Sea Scheme did not make everybody's fortune. Neither will the germ theory of disease explain all the facts of pathology; nor will it enable us to deal effectually with every disease, as its disciples want us to believe.

Yet it has had some splendid successes; no one can dispute that who knows the story of the mortality of women in childbed. Many horrible tales have been told illustrative of this. Let me add another. A few weeks ago I made some visits to La Maternité with my friend Professor Tarnier. He directed my attention to a linear chart on the wall of his room, showing the total death-rate of the women confined in the hospital from 1792 till 1886. He divides this marvellous record into three periods, the first of which he calls the period of inaction, during which the mortality was 9.3 per cent.—in some years it was as high as 20 per cent., a perfectly murderous mortality. The second period he calls that of the battle against the causes of infection and contagion, without antisepsis, that is, by mere general hygiene; and he shows that by this an abrupt descent to a mortality of 2.3 per cent. was secured. In his third period, by the employment of antisepsis, chiefly, and now entirely, by solutions of corrosive sublimate of about 1 in 3,000, the mortality was reduced to 1.1 per cent., and in 1885 and 1886 it was not 1 per cent. Finally, the conclusion is this. When antiseptic precautions were used, the mortality almost disappeared, so that we now know that the raid against lying-in hospitals was a mistake. I have taken an active share in it myself, and therefore am bound to make this recantation. Destroy the germs on the hands of those who attend parturient women, and the women are safe. Women attended in their confinements by midwives have a greater chance of escape, because those midwives are but little likely to come across the deadly germs which excite the fatal diseases. But let the physicians and students of tumble-down and not over-clean buildings like La Maternité (a convent but little altered from the sixteenth century) wash hands and instruments in a solution of corrosive sublimate each time before touching the patients, and these women seem to be absolutely free from danger. I need not say that after this true infective puerperal fever ought to be banished from practice. It ought to be heard of no more.

Well, you say, this proves the germ-theory of disease. Certainly, for this one instance; but the case is only the exception which indicates the real road for inquiry. The puerperal woman is in a condition wholly different from that of any other patient; her very blood is wholly different in chemical constitution from the blood of any other human being. I believe that the very germs which are so disastrous to her might be, and often are, smeared over wounds in men and non-puerperal women without any harm. That there is a difference of this kind is proved by many illustrations, such as scarlet fever. No one can doubt that this disease is due to a specific germ which generally breeds true. I do not know what has been done in the way of cultivating it, but we know that it does sport when grown in abundance, for in epidemics the poison-results vary from slight sore-throats to malignant non-eruptive attacks, fatal in twenty-four hours. But look at the natural history of the disease. Each child goes through a period of danger, having its maximum period between five and seven years of age; and just in proportion as the individual approaches a period of life more and more remote from that age, so does the likelihood of becoming affected by the disease diminish, as the severity and fatality of the attack diminish if it occurs. Yet the germs are the same—it is the vital condition of the *nidus* which changes. So the curious fact of the immunity generally conferred by one attack against a second proves that we are expecting too much from our knowledge of germs, and that we are neglecting too much researches into the condition of the nest in which they breed. What is the chemical or physical change effected in the individual constitution which enables it for an indefinite time to resist the attacks of the germs, which at one time it was so ripe for, and which it cultivated so freely? Not any known method of research can show us the difference in the soil, but there is a difference, whilst the germs float about exactly the same.

You will see, therefore, that I accept the germ-theory of decomposition; its facts are indisputable. I also accept the germ-theory of disease concerning certain diseases; the facts there are equally beyond cavil. But it is when Lister comes in with his royal road to surgical success, still more when his German disciples, full of enthusiasm and quite empty of discrimination, appear on the scene, that I am in doubt and equally in fear. What I predicted in a paper read before the Royal Medical and Chirurgical Society, in 1880, has come to be only too well fulfilled. This failure in logic, this rushing to extremes, this undue haste to unravel all the mysteries of nature, and that greatest mystery of all—human disease—by every new little fact discovered by us, is no new thing.

No sooner had Schleiden and Schwann made out the rough outlines of cell-growth, than we got a full-blown system of cellular pathology about which the fights were bitter, just at the time when my retrospect begins. Then bigger and better microscopes enabled Bennett to see molecules, and a molecular pathology swept the cellular out of the field. Now our lenses enable us to study bacilli and their spores, and the bacillary pathology takes the prize.

But Dallinger has beaten the bacillus, and now our *savants* will have to give up their jellies and their soups, on which they feed their nasty beasts, for Dallinger has found a lesser flea:—

> "So naturalists observe, a flea
> Has smaller fleas that on them prey;
> And these have smaller still to bite 'em;
> And so proceed *ad infinitum*."

Let me go back again to my retrospect to show what I mean, by two illustrations taken from my own surgical work, in which I can speak strongly from a very abundant experience. I have argued this question very fully elsewhere from the views of abdominal surgery, and I shall speak in a few moments of that subject again. But we are told now that abdominal surgery is a thing by itself, apart altogether from the surgery of the rest of the body, and not governed by the same laws. That, of course, is nonsense—simply a repetition of the schoolboy proposal, "heads I win, tails you lose."

Let me speak of the only bit of general surgery which comes my way—the removal of tumours of the breast—operations of which I perform some scores every year. When I was a follower of Lister I used his every method, every detail recommended by him in turn—and they were endless in variety—and I rarely got anything like such satisfactory results as I get now without them. I used the carbolic oil, and the putty, and the lac plaster, and the carbolic spray, thymol, absolute phenol, corrosive sublimate, boro-glyceride, etc., *usque ad nauseam*. But the moment I gave all that up and used a little piece of Chassaignac's drainage-tube and plenty of Gamgee's absorbent wool, my results became uniformly satisfactory, and they remain so. Union by first intention is the rule, suppuration is the exception. The reason is simple. The drainage-tube removes the likelihood of any nest for the germs to breed in, and the absorbent cotton-wool makes their existence impossible; it removes the dead fluids. Therefore I care not a fig for the germs, and the advance in surgery in the direction of this success is due not to Lister, but to Chassaignac and Gamgee. It was my custom long before Gamgee's death to ascribe on all fit occasions this merit to the labours of our late townsman, and now that he is gone from amongst us it is my duty to claim for him a full recognition of his great discovery. Titular honours and weighty pensions have often been bestowed for far less important services to humanity, and I am strongly of opinion that the State should be moved to recognise to his widow in some substantial form the merit of his invention.

Let me now speak briefly of the arguments derived from the recent advancements of abdominal surgery, and as that is a field on which I must inevitably become wearisomely discursive, unless I exercise great care, let me confine myself rigidly to one case, that of inflammatory disease of the Fallopian tubes. I am induced to select this because I have been savagely attacked recently by several German authorities, notably by Dr. Sänger, of Leipsic, and by others at the recent Surgical Congress. Dr. Sänger's style of language is such that I could not emulate it, and should be sorry to attempt an imitation of it. Dr. Sänger has such a low opinion of my abilities and my work, that he goes so far as to recommend me to learn to read German, and to read the works of German gynæcologists. Unfortunately for me, that is just what I did some twenty years ago with infinite labour, for the language is neither simple nor easy. I have long since been convinced that my time in this respect was thrown away. The German mind, at least the German medical mind, is essentially different from the mind of the Briton; it not only evolves from its own consciousness descriptions of things other than the proverbial camel, but it wraps up its grain of wheat in such a bushel of chaff that the labour of getting a meal is intolerable. Nothing pleases it so much as metaphysical speculation, whilst we, on the contrary, are eminently pragmatic. From my early experience of German hospitals and the methods of work therein, I never wondered at the necessity of some such reform amongst them as was offered by Listerism.

To return to the Fallopian tube and Dr. Sänger. His chief trouble about me is that I will not recognise his subdivision of pyosalpinx into varieties, based on the peculiar kind of low organisations to be found in the contents of the tubes. He desires to establish three such varieties—the salpingitis gonorrhoica, due to the gonococcus of Neisser; the salpingitis tuberculosa, produced by the bacillus tuberculosis of Koch; and the salpingitis actinomycotica, produced by the actinomyces bovis of Bollinger.

My reason for refusing to follow Dr. Sänger is simple and my argument easy. Dr. Sänger is putting the cart before the horse. These organisms are the result of the inflammatory exudation, not its cause.

When Dr. Sänger is as accomplished in the use of the microscope as Dallinger, he will probably find in the contents of these tubes the still smaller beasts which, by repeated buttings against the fragments of dead tissue, prepare the food on which the bacteria live. The bacteria are the phenomena of decomposition and not the cause of the disease. If it were not so, how could we explain the familiar fact of rapid recovery after the opening of a superficial abscess in a person otherwise healthy? The evacuated pus swarms with bacteria; the pus is dead tissue. The bacteria do not attack the living tissue. The moment their food is removed they are starved into subjection; the abscess cavity heals and the patient recovers. But put this germ-theory of disease into the position of power, which its German advocates claim for it, and recovery is theoretically impossible. The whole economy must become an addled egg. Given the access of the bacillus tuberculosis to the Fallopian tubes, the infection of the peritoneum is a logical sequence. But I have opened the abdomen and removed chronically inflamed Fallopian tubes distended with dead matter infected by this bacillus, and with the infundibula not occluded, and the patient is now well and healthy, nearly three years after the operation. I have opened the abdomen, in many cases packed full of tubercular matter, and drained it like a common abscess, and have cured the patients. The same thing has been done by Esmarch, who has identified the bacillus in the orthodox German fashion. Does anyone believe that either of us removed every bacillus and every spore? I know I did not, for the tubercular masses in several of my cases kept coming out for weeks afterwards, yet the patients recovered. What I really did was to enable my patients to get rid of the dead or dying exudation on which the bacilli lived, of the decomposition of which they are probably the means and wholly the result. Apply this German notion of the germ-theory of disease to the facts of clinical medicine and surgery, and see how irreconcilable with the facts it is. If true, the amputation of a finger ought to be as fatal as the loss of the thigh at the hip-joint. But it is not so, in the same hospital and under precisely similar circumstances. Amputations have an increasing mortality everywhere just as the point of severance approaches the trunk. Why? Clearly on account of the "diminished vitality" of the general tissue due to the "shock." These two good old-fashioned terms are sneered at by your modern *savants* because they serve only to express our ignorance. The reproach is true; we are ignorant. But it is better to be ignorant and to confess it than to parade a lot of inaccurate conclusions in the name of science.

If the germ-theory of tubercle, that it is the result of the existence of the bacillus tuberculosis, were correct, none of us were safe from consumption, for we must meet these deadly brutes at every cab-stand, and at every gathering in the town-hall. If it were true, Bennett's fact that multitudes of people recover from consumption could not be sustained, yet every physician knows it is true.

We do not know exactly what we mean when we speak of diminished vitality, any more than we know exactly what we mean when we speak of an earthquake. But there is such a thing. A man's toes remain healthy until the deposits of lime-salts in the walls of the arteries deprive them of nutrition and diminish their vitality, till they can no longer resist the attacks of the ever-present germ. The germ attacks the dead and nearly dead tissue. Drysdale's flagellate monad probably begins the deadly work of senile gangrene, and the bacteria finish it. But there is a line of demarcation beyond which the little beasts are powerless by reason of the resisting vitality. Some indiscreet surgeon interferes, makes a clot in the artery; the clot runs up, nutrition stops, vitality is destroyed, and the germs wreak their evil wills.

In conclusion, let me again say that as the wrecks of the cellular pathology remain to us most valuable contributions towards the solution of the mysteries of disease; as the construction by Bennett of a scheme of molecular pathology did much to help us, so has the germ-theory rendered us great service. But as a final answer it is insufficient, and its wholesale and reckless application is alike mischievous and misleading.

THE new children's ward in connection with the Westminster Hospital was opened by their Royal Highnesses the Duke and Duchess of Connaught, who were accompanied by Princess Margaret, on July 13th. Archdeacon Farrar presented a cheque for £1,200, being the amount subscribed in connection with St. Margaret's Jubilee Fund, and sufficient to perpetually endow two cots in memorial of Her Majesty's Jubilee. Mr. James Hora, a parishioner of St. Margaret's and a governor of the hospital, has endowed a third cot by contributing £600; and a member of the committee has undertaken to maintain a fourth by an annual payment of £30. Four out of the twelve cots available have been endowed.

AN ADDRESS

ON

PLYMOUTH AS A HEALTH-RESORT;

TOGETHER WITH SOME REMARKS ON THE CONTAGIOUS DISEASES ACT.

Delivered at a Meeting of the South-Western Branch.

BY PAUL SWAIN, F.R.C.S.,
President of the Branch.

GENTLEMEN,—It is with no small pleasure that, on behalf of the profession here in Plymouth, I bid you a hearty welcome to our town. I presume that most, if not all, of you are to some extent acquainted with the beauties of Plymouth and its immediate surroundings. We who live here have, alas, to encounter that familiarity which breeds contempt. We live day after day amongst our lovely scenery by land and sea, and we fail to recognise what strangers tell us who have travelled around the world, that when they come to Plymouth they have seldom seen a lovelier spot. Looked at from the standpoint of public health returns, our town certainly does not apparently show a favourable position. In the first quarter of 1887, the death-rate of Plymouth stands at 27.6 per 1,000, the average death-rate of twenty-eight large towns being 22.0. The average infant mortality in these twenty-eight towns of infants under 1 year of age was equal to 145 per 1,000, whilst that of Plymouth amounted to 212. Without desiring to minimise the effect which these figures should have on our sanitary efforts, it is but just that I should point out that they are drawn from an area which by no means represents the population of Plymouth. They are taken from the population of the municipal boundary, and it has been shown by our medical officer of health, Mr. Greenway, that the population included in that boundary represents an average of 53.3 persons to the acre, whereas the average density of population in the twenty largest towns of England is only 38.6.

Dr. Bloxall, in his report on the sanitary condition of Plymouth in 1879, stated that in six streets most densely populated, the death-rate was 30 per 1,000, but that the rest of the municipal area had only a death-rate of 21 per 1000. But Plymouth should not, as far as her sanitary record goes, be judged only by the lowest and most densely populated portion of her area. She has stretched far beyond her municipal boundary. We have now fortunately a sanitary authority established on our outskirts, embracing the beautiful neighbourhood to the north; and I have before me the report of the medical officer of that district, Mr. Edlin, for 1886, showing a death-rate of only 11.3 per 1,000.

As to our climate, we may claim to take a good position. We have obtained a most unfair notoriety with regard to our rainfall; but in reality our rainfall bears comparison with that of other places of resort. Looking over *Symon's British Rainfall* for 1885, I find that in that year it rained on 199 days in Plymouth, and that our depth of rain was 34.57 inches. At Penzance it rained on 218 days, with a depth of 45.45 inches; at Dawlish, 210 days, 34.37 inches; at Torquay, 190 days, 33.45 inches; at Buxton, 201 days, 48.92 inches; at Bournemouth, 31.08 inches; at Brighton, 174 days, 31.42.

The mildness of our climate is proverbial, and during last winter, when all the surrounding world was clothed in snow, Plymouth enjoyed a happy immunity from the infliction.

Take her all round, there are few places I know more suitable as a place of resort for pleasure, amusement, or the maintenance of health, than Plymouth. But, from our own particular stand-point as a Medical Association, the town of Plymouth deserves our thanks. She has lately built a hospital which is not only a credit to the neighbourhood, but which, I venture to think, is a splendid addition to the great medical charities of England. It is much to the honour of the three towns that within the last quarter of a century they should have built two model hospitals—the Royal Albert Hospital at Devonport and the new South Devon Hospital at Plymouth. At the birth and early growth of the former I had the honour to assist. With the latter now my lot is cast, following in the steps of men whose names are well known in the annals of English surgery, and with whom, in the persons of their sons, Whipple and Square, I have the pleasure to work as my colleagues.

And this leads me to make a few remarks on the subject of medical education at provincial hospitals. I have for years past contended