OBITUARY NOTICES

OF

FELLOWS DECEASED.

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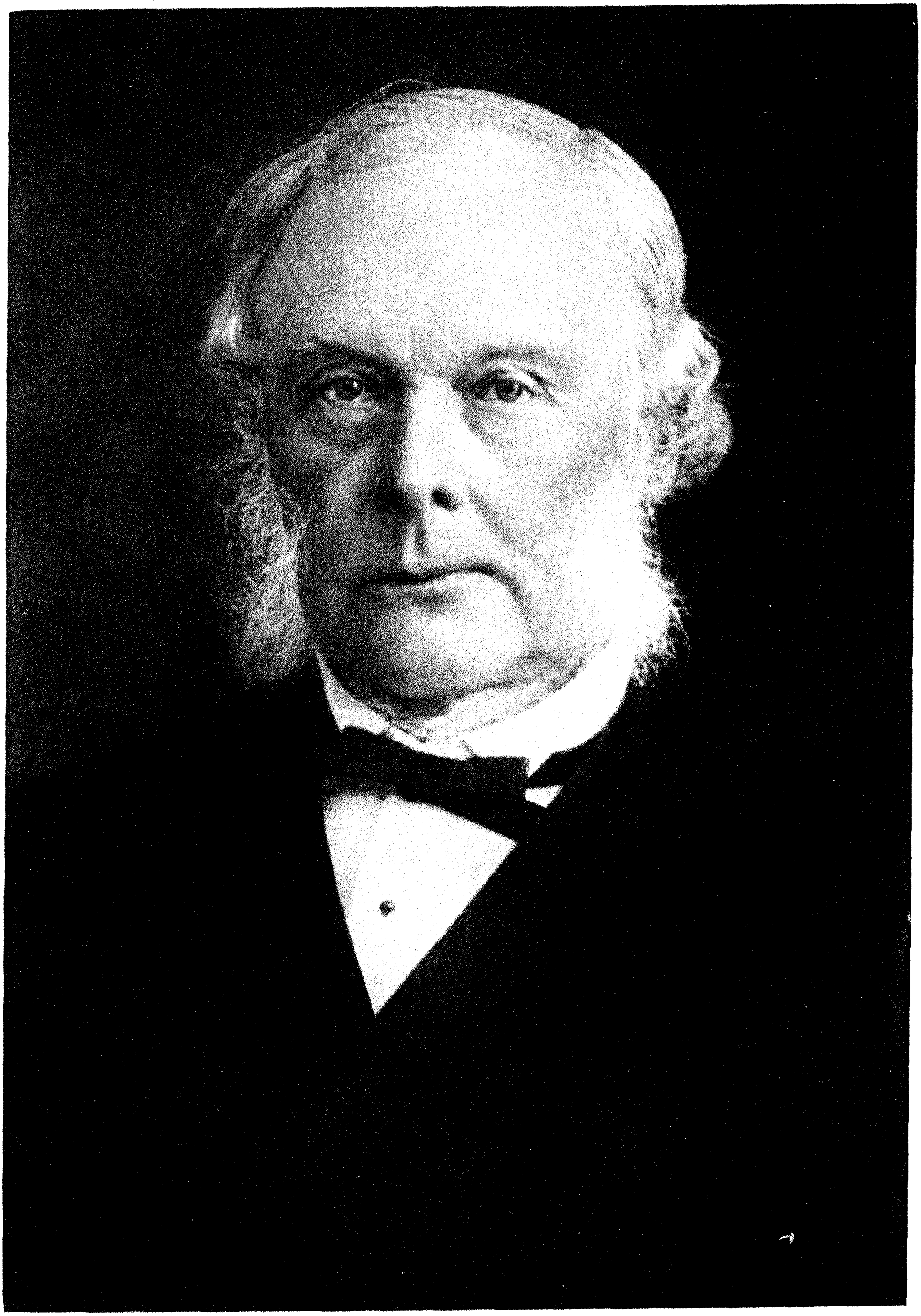


Lister

LORD LISTER, 1827–1912.

JOSEPH LISTER was born at Upton in Essex on April 5, 1827, and, like many other men who have attained great eminence, belonged to a Quaker family. His father, Joseph Jackson Lister, who was in business in London, occupied his leisure time in scientific researches, more especially in researches in connection with the perfection of the microscope; indeed, it is to his researches that we owe the final perfection of the achromatic lens, which has proved such an essential instrument for microscopical work. He was a man of great accuracy of thought, and a hard worker, and his influence on Lord Lister's character and career was very profound; indeed, Lord Lister himself was never weary of stating how much he owed to his father's early training. It may be noted, in passing, that his father was himself a Fellow of the Royal Society, and that his brother and one of his nephews have also received that honour.

Lister's medical training commenced at University College, and while there he came under the influence of Sharpey, who was then Professor of Physiology, and of Thomas Graham, who was Professor of Chemistry. Sharpey especially exercised great influence in directing his thoughts to the study of physiological problems, which ultimately formed the basis of his great life work; indeed, while still a student he made observations on the contractile tissue of the iris which attracted a considerable amount of attention among physiologists, both in this country and abroad, and he followed that up by work on the muscular tissue of the skin. Both these papers were published in the 'Quarterly Journal of Microscopical Science,' in 1853.

His career at the London University was a very distinguished one, and in 1852 he took the Degree of M.B. Towards the end of his time at University College he became House Surgeon to Mr. (afterwards Sir John) Erichsen, and House Physician to Dr. Walshe. It was more especially while he was House Surgeon that his attention became concentrated on the calamitous results which followed wounds, both operative and accidental, and the terrible cases which he had to deal with riveted his attention, even at that early period, on the subject to which he afterwards devoted the best part of his life.

After he had finished his studies at University College, and before deciding on his future career, he was advised by Dr. Sharpey to visit Edinburgh and see the practice of Prof. Syme, who was then one of the most distinguished surgeons of the age. Towards the end of 1852, therefore, he went to Edinburgh on what he intended to be a short visit. In Edinburgh he very quickly became an ardent admirer of Prof. Syme, who exercised upon him an influence and fascination which were never lost. He constantly quoted Mr. Syme's work and opinions up to the very end of his active surgical work. At first he was simply a visitor in Mr. Syme's wards, but in order to acquire more familiarity with his work and views, he became a dresser,

and subsequently his House Surgeon, a post which he occupied for over a year. He then decided to settle in Edinburgh, and in 1856 he was appointed Assistant Surgeon at the Royal Infirmary; at the same time he began to give lectures on surgery in the Extramural school. In preparing these lectures he found that the subject of inflammation and the changes which occurred in the tissues were so imperfectly understood that he made it his first duty to investigate the matter for himself. The results of these researches were of the greatest value in his subsequent work on the treatment of wounds, and enabled him to understand and follow up many things which occurred in wounds where sepsis was excluded, and to make deductions which were of great importance in his work. His researches on these matters earned for him the Fellowship of the Royal Society, to which he was elected at quite an early age. His papers on the early stages of inflammation placed the whole subject on a new and firm basis and remain as classics and remarkable examples of close observation and accurate deduction.

In 1860 the Chair of Surgery in Glasgow became vacant and Lister was appointed to that position, and left Edinburgh to assume his duties as Professor of Surgery. There he had also charge of wards in the infirmary, and it so happened that the wards allotted to him were particularly insanitary, and in fact all forms of septic diseases of wounds were constantly present in them. Indeed, the terrible results which followed injuries and operations were a cause of great distress to him, and he was constantly pondering over their causation and prevention. At that time many surgeons looked on these diseases as inevitable accompaniments of wounds, especially when treated in hospitals, and believed that nothing in the way of local treatment was of any real value in preventing their occurrence. As a result, however, of his reflections on septic diseases and of the scientific work which he had been carrying out, Lister had already, when he took up his work in Glasgow, come very near the solution of the matter.

In his early lectures, in Glasgow, he devoted a great deal of time to the subjects of inflammation and suppuration and their causes, indeed, to such an extent that the students were inclined to think that too much attention was paid to them, but Lister felt that it was the study of these matters which would ultimately give the key to the cause of the troubles which occurred in wounds and might lead to their prevention. He had already arrived at the following conclusions:—Pus is only formed in wounds as the result of irritation of the granulation tissue which covers the raw surface and the formation of granulations must precede suppuration. The most probable cause of this irritation of the granulation tissue is putrefaction of the discharges in the wound. This putrefaction occurs quite early after the injury. The cause of the putrefaction of the blood and serum is something which comes from without and is not an essential occurrence after a wound, because in subcutaneous injuries the tissues are cut or torn across in a similar manner as in open wounds, blood is effused, serum is poured out and yet no suppuration and no decomposition occur; but once the skin is broken, the

state of matters is quite different. Lister came to the conclusion that if only he could prevent the putrefaction of the discharges in the wound, no suppuration would occur and the wound would follow the same course as a subcutaneous injury.

Here, however, he came up against a dead wall; what it was that led to this putrefaction was unknown. It was evident that it had something to do with the exposure of the wound to external agencies, and the general opinion at that time was that it was due to the access of the air and especially of the oxygen in the air. But he had long before satisfied himself that it could not be the air itself, or any of the gases that composed it, because in certain subcutaneous injuries, for example in some fractures of the ribs where the lung was punctured, air might be present in the tissues in large quantity and in contact with effused blood and serum, and nevertheless no inflammation or suppuration or decomposition took place. Hence the conclusion he came to was that it was not the air itself, but something which was conveyed by the air, which caused the trouble, and in former times this something was spoken of as a *miasm.* It will thus be seen that he had advanced a long way towards the solution of the problem which was occupying his mind, and that all that was wanting was the discovery of what it was that, carried by the air or present in it, entered the wound from without and set up decomposition and irritation and the various troubles which, in his opinion, resulted therefrom.

During these early years in Glasgow he tried in all sorts of ways to prevent this decomposition of the fluids in the wounds. He paid attention more especially to personal cleanliness, which he carried out to such an extent that his scrupulousness in that direction led to very sarcastic remarks being made about him. At that time surgeons very often did not even wash their hands after handling a suppurating case and before dressing the next patient, and they were equally careless as to the cleansing of the instruments used; indeed nothing but superficial wiping of a knife or other instrument was done before it was used again. Not only were the surgeons careless in this respect, but also the nurses and dressers, so that infection was unwittingly carried from one case to another. One of the early innovations which Lister made on taking up his work in Glasgow was to institute scrupulous cleanliness, and to insist that the hands and instruments should be thoroughly washed before handling a fresh case. His wards were, therefore, provided with basins, water, and piles of towels, a thing quite unusual at that time.

Lister also carried this matter further and tried to diminish the putrefaction in the wounds by washing them thoroughly. For example, in the case of an amputation, the limb would be held up and kettle after kettle of warm water would be poured over the wound to wash away the pus and decomposing material; he also employed various deodorising substances, more especially Condy's fluid. None of these plans, however, led to any noticeable diminution in the frequency of septic diseases, and it became evident that something more was needed. The use of deodorising materials had also been tried by

other surgeons, especially in France, but no real improvement had followed, because the attempts had been solely directed against the smell and not against the bacteria which were the cause of it.

This was the stage which Lister had reached when his attention was called to researches which Pasteur had recently made on the supposed spontaneous generation of micro-organisms and on the process of fermentation. The presence of minute living bodies in decomposing fluids had been known for many years and their origin had become a subject of violent debate. One view was that these bodies arose *de novo* in the decomposing materials, in fact that we had here an example of spontaneous generation of life; others, on the contrary, held that there was no such thing as spontaneous generation and that these minute "animalculæ" were merely the descendants of preceding animalculæ which had been carried into the putrescible fluids along with the dust from the air. Experiments had been carried on for more than a century with the view of settling this question, and though evidence was steadily accumulating against the theory of spontaneous generation it was not till Pasteur took up the question that the final blow was given to this view and that the doctrine of *omne vivum ex vivo* was finally established.

That these minute living bodies, or "vibrios," as the larger forms were termed, had anything to do with the putrefactive changes in the organic fluids in which they were constantly present did not for a long time occur to anyone, and the investigations on spontaneous generation were looked on as of purely scientific interest and not of any practical value. For about fifty years before Pasteur took up the matter, the cause of fermentation had also been very extensively investigated by chemists, especially as regards the alcoholic fermentation, and it had ultimately been demonstrated that this fermentation was undoubtedly due to the growth of the yeast cells which were always present in the fermenting fluids. It had also been quite recently suggested that other fermentations in organic fluids, and among them the putrefactive fermentation, were due to the "vibrios" which developed in them; but here the final proof was furnished by Pasteur's researches. But neither Pasteur, nor anyone who preceded him, with the exception of Davaine, had imagined that these bodies had anything to do with the production of disease. Davaine had, indeed, some years previously, noticed that, in the blood of animals which had died of anthrax, large rod-shaped bodies or "vibrios" were constantly present, and he had suggested that they might have something to do with the disease, but the matter had not been carried further.

These researches had not attracted the attention of medical men; and, indeed, it is hardly to be wondered at that a surgeon, who had much to occupy his mind with his surgical and pathological work, did not happen to be familiar with these recondite and apparently useless researches on spontaneous generation; but when Lister read Pasteur's convincing proof, both of the fallacy of the theory of spontaneous generation and of the relation of living bodies which were derived from pre-existing bodies of the same

nature to certain fermentations, among others to putrefaction, and his evidence that these bodies were derived from the dust floating in the air and deposited on surrounding objects, he saw at once that he had found the thing that was wanting. Pasteur's researches showed him that this something for which he had been searching must be a micro-organism, similar to those which Pasteur had described, and a very cursory examination of the discharges from the wounds showed him that they were teeming with living organisms of this kind. Lister, therefore, at once concluded that the cause of putrefaction in wounds, and of the various evils which followed that occurrence, was the growth of micro-organisms in the wounds, that they were derived from the dust in the air and from surrounding objects, and that if he could only prevent the access of these organisms in a living state or destroy them after they had entered the wounds, he might possibly prevent a great many of the evils which followed injuries or operation.

At first, however, Lister had no idea of the far-reaching nature of this conclusion. All that he could say at that time was that, in his experience, these infective diseases did not occur in connection with wounds, such as subcutaneous ones, in which putrefaction of the discharge had not occurred, and he hoped that the same would be the case if he prevented putrefaction in his wounds; his whole aim, in the first instance, was, however, to prevent putrefaction. Nothing was known at that time of the nature of the various infective diseases which occurred in wounds, or their relation to bacteria; indeed, the idea that disease could be due to the growth of any such minute parasites as bacteria had hardly been suggested. It is true, as we have just said, that Davaine, some years before, had pointed out the presence of large numbers of rod-shaped bodies, or vibrios, in the blood of animals which died of anthrax, and he had somewhat timidly suggested that possibly the presence of these vibrios was an essential element in the production of the disease, but this suggestion attracted practically no attention, and it was not until some ten years after Lister's work began that proof was furnished that anthrax was due to the growth of these organisms, which were subsequently named "anthrax bacilli." That pyæmia, septicæmia, hospital gangrene, tetanus, erysipelas, diphtheria, and so on, were similarly due to the growth of micro-organisms in the blood and tissues had not occurred to anyone, and, indeed, did not definitely take root in Lister's mind for some time after he commenced his antiseptic treatment. Nevertheless, Lister had arrived at very definite conclusions on this point, as the result of his experience with the new methods of treatment, long before the part which the various micro-organisms played in the production of disease had been demonstrated.

Let us return, however, to the point when Lister, as the result of his study of Pasteur's researches on fermentation and spontaneous generation, acquired the certainty that the cause of putrefaction in wounds was the access of micro-organisms from without. On reviewing the matter, he saw that the problem of preventing the growth of micro-organisms in wounds was a complex one, and that two different conditions had to be dealt with. On the one hand, the

organisms might be already present in wounds before they came under his care, *e.g.* in wounds inflicted accidentally, such as compound fractures; in this case the problem was to kill them in the wound after they had entered, or before they had established themselves there. On the other hand, the wound might be inflicted by the surgeon through unbroken skin, and the problem then was to prevent them from entering in a living condition. In both cases there was the further problem of preventing their entrance during the subsequent progress of the case. His first attempts were made with compound fractures, in which the problem was to destroy the bacteria which had already entered and to prevent others getting in afterwards.

In looking for some agent which would destroy the bacteria and thus prevent putrefaction his attention was directed to some striking results which were being obtained in Carlisle about that time in the treatment of sewage. It was stated that as the result of treating sewage with large quantities of German creosote the material was effectually deodorised and further putrefactive changes ceased in it. Lister accordingly procured a specimen of this German creosote, which contained as its essential element carbolic acid, and he waited patiently till he got a suitable case of a compound fracture in which to test his views, and some months elapsed before such a case presented itself. In the meantime he was occupying himself with the examination of wounds and with the subject of spontaneous generation and other cognate matters. Finally, a case of compound fracture was admitted to his wards and in accordance with his instructions he was sent for at once. He took some of this liquefied German creosote, and introduced it into all the recesses of the wound; he also mixed it with the blood in the wound. The wound was then covered with a small piece of lint soaked in the German creosote, and outside that plain lint and towels were placed to soak up the discharge. The result surpassed his expectations; the patient remained well instead of developing temperature and high fever within a few hours and having a long illness, the wound did not swell or become inflamed, there was no pain and no suppuration. The mixture of blood clot and carbolic acid remained in the wound and a crust formed on the surface. After a time Lister began to try to detach this crust, and found that beneath the superficial layer epithelium had spread over the deeper portion of the clot and the greater part of the wound was covered with a layer of epithelium. The antiseptic which he had employed had prevented the occurrence of putrefaction, and the blood and the tissues in the wound had behaved exactly as if they had been covered with skin, in fact, exactly like a subcutaneous wound. No more attractive scientific paper was ever written than his first publication in the 'Lancet' in 1867 on this new method of treating wounds, and one is struck with the acuteness of his observations and with the rapid and sound deductions which Lister drew from everything that he saw.

As the carbolic acid evaporated from this mixture with blood and might thus leave a putrescible substance, his next step was to paint the surface of

the clot daily with fresh carbolic acid, and later on he covered the crust with a tin cap so as to retain the carbolic acid. The character and progress of these wounds was something quite new to him and to everyone else, and it is not surprising that as case after case followed the same course he and his staff became most enthusiastic over this revolution in surgery. After a time he proceeded to apply the same principle to operation wounds, and the first case in which he used it was a patient with a large spinal abscess. This was opened, and the pus caught and mixed with carbolic acid to form a paste, which was applied over the wound with a tin cap outside in the manner just described. When he came to dress the wound next day he found to his surprise that no pus was coming from the wound, but only a little clear serum. This was a new experience, and a thing which had not occurred to him before, and he was in a difficulty, because he wished to apply a fresh mass of the carbolic acid paste outside, and he had no pus or blood to make it with. By this time he had found that carbolic acid was soluble in oil, and he therefore sent to the dispensary for a solution of carbolic acid in 20 parts of linseed oil and some whiting, and proceeded to make a paste or putty with which he covered up the wound, covering the putty with a piece of tin. This acted admirably and the abscess healed up without any fever or general disturbance—a result of which he had had no previous experience.

He was now able to get rid of the undiluted acid with its caustic effects, and had at his disposal oily solutions of carbolic acid and the carbolic putty, and with these he proceeded to extend his system, not merely to accidental wounds but to all sorts of operation wounds, and with the most remarkable success. His plan at that time was to cover the skin with carbolic oil before the operation, to soak his instruments in carbolic oil, and to fill up the wound from time to time during the operation with the same oily solution. It is doubtful if any better results can be found even at the present day than those which were obtained at that time by the use of carbolic oil and putty. At the same time the method had great inconveniences; the putty, for example was very apt to crack and crumble, and the carbolic oil obscured the view.

After many experiments, Lister introduced a specially prepared lac plaster as a substitute for the putty. This lac plaster was made of shell-lac and carbolic acid covered with a very thin layer of caoutchouc. This mixture was spread on suitable material and wrapped round the wound; outside this cloths were applied to soak up any serum which might exude. About this time also he obtained purer carbolic acid and was able to make watery solutions, and these were substituted for the oily ones. He was now able to carry out a more thorough disinfection of the skin than had been possible previously, for he fully realised that, apart from the dust in the air, bacteria might grow on the skin and so spread into the wound. From the first the necessity of disinfecting instruments and everything which came in contact with the wound had been fully realised by him, and at this time

the disinfection was carried out by immersion for a considerable time (1–2 hours) in 1/20 watery solution of carbolic acid.

While the lac plaster was a great improvement on the carbolic putty it had the disadvantage that it did not absorb the discharge, and the next point to which Lister turned his attention was to obtain some material as a dressing for wounds which would absorb and retain the discharges while at the same time preventing putrefaction in them. He tried oakum and various other materials and ultimately fixed on the fine gauze material which is still used at the present time, and he did a great deal of work with the view of converting it into a suitable dressing. Though the gauze could be disinfected by soaking it for a sufficient length of time in carbolic solution, the discharge in passing through it very soon neutralised or washed out the antiseptic and quickly underwent putrefaction in the gauze. It was necessary, therefore, to store up the carbolic acid in the gauze, so that it should on the one hand yield up enough to prevent the discharges which passed through it from undergoing immediate decomposition and on the other hand retain enough to avoid the necessity of frequent changing of the dressing. He had already found that carbolic acid had a great affinity for resin and that a resinous mixture only parted with its carbolic acid slowly. But gauze impregnated with resin formed a very sticky material which was unsuitable as a dressing. This difficulty was overcome by mixing paraffin with the resin, and gauze impregnated with this mixture was found to answer the purpose very well. For the sake of economy he placed a piece of jaconet, previously sterilised by immersion in 1/20 carbolic lotion, outside the gauze which was applied over and around the wound so as to prevent the discharge passing straight through it opposite the wound, but to compel it to travel over all the gauze before reaching the surface.

At this time he still laid great stress on infection from the air, and in operating he constantly poured carbolic lotion into the wounds so as to destroy any bacteria which might fall into them from the air, and in dressing wounds a stream of carbolic lotion was kept flowing over the incision for the same reason. It may be said here that, contrary to what has been generally supposed, the lotion was never syringed into the wound after it had been once closed, it merely flowed over the surface so as to prevent living dust gaining access. It was soon found that although septic diseases were now absent, such a free use of carbolic lotion not only obscured the view, but caused a good deal of irritation of the wound, as evidenced by the large amount of serous discharge afterwards, and also frequently led to carboluria, though seldom to any serious signs of poisoning. Hence he recognised the necessity of diminishing the amount of carbolic acid which came in contact with the wounds, while at the same time preventing the access of living organisms, and after many experiments, he at length introduced the carbolic spray. The spray producers were worked at first by hand- or foot-bellows, but later by steam, and a very fine cloud of spray containing about 2½ per

cent. of carbolic acid surrounded the wound during the operation and at each subsequent dressing. The idea was that the fine particles of the spray coming in contact with the bacteria floating in the atmosphere would kill them before they fell into the wound, while at the same time only a small amount of carbolic acid would come in contact with the wound and so the local irritation by the carbolic acid and the amount of absorption would be very much less than by the former plan.

With the view of preventing irritation of the line of incision by the carbolic acid contained in the discharges, many experiments were made to find some material which could be interposed between the dressing and the line of incision, and which would be impermeable to carbolic acid. Ultimately a satisfactory "protective" was obtained by covering the ordinary oil-silk used for surgical purposes by a layer of copal varnish. The surface of this prepared oil-silk was then painted over with a layer of dextrine and starch, so that when placed in carbolic lotion in order to disinfect it, it should be wetted all over. A narrow strip of this "protective" was laid over the line of incision before the gauze dressing was applied.

While Lister was thus engaged in improving his methods for destroying bacteria and preventing their access in a living state to wounds, he also spent much time in improvements in the treatment of wounds apart from the question of asepsis. The most important of these improvements was the introduction of absorbable ligatures. Up to the time when he commenced his great work the blood vessels were tied with silk or hemp, and as these ligatures had to separate by ulceration and suppuration before the wound could heal, they were left long and hung out of the wound in bundles, so that they could be pulled out when they became loose, usually about the eighth or tenth day after the operation. As the result of the prevention of suppuration, one of the first things observed was that these ligatures did not separate and there was much trouble in getting rid of them; in fact they sometimes had to be cut short and left in the wound.

Lister accordingly turned his attention to this matter and made many experiments in order to see what happened to the ligature in aseptic wounds, and whether it might not be cut short and the wound closed over it. He found that this was the case and that silk ligatures might be cut short and left without causing any trouble. On examining these ligatures after they had been buried in the tissues for some months he found that they were undergoing destruction and that the threads of the silk were being broken up and absorbed by the cells of the body. The process was, however, a very slow one and after more than a year considerable fragments of the silk were still present. He therefore looked for some material which would be suitable for ligatures and which would be more quickly absorbed, and tried various animal substances, ultimately concentrating his attention on catgut. (Although Lister was not aware of it, catgut had been tried as a ligature material many years previously, but discarded as quite unsuitable. At that time the wounds were septic and the catgut was extruded from the wounds just as silk was,

and, besides, as the catgut was not hardened, it very quickly swelled up in the tissues and the knot became inefficient.) Lister very soon found that catgut as it came from the makers was quite useless, it swelled up in a few hours, the knot got loose and when applied to a vessel in its continuity the lumen of the vessel opened up again. He therefore made many experiments on the preparation of catgut so that it should remain firm in the tissues, should not be absorbed for some time, and should at the same time be aseptic. His researches on the preparation of a suitable catgut ligature were carried on intermittently for many years.

Another point to which he devoted much attention at this early period was the drainage of wounds. One of the first points which became evident was that as the result of the irritation of the carbolic acid a large amount of serum was poured out during the first two or three days after the operation and this had to be got rid of, otherwise it distended the wound and interfered with the healing. Drainage of the wound was therefore necessary in order to allow this serum to escape. At first he carried this out by introducing a piece of lint and later of gauze into one corner of the wound, but this proved to be inefficient and objectionable in many ways, and later on he resorted to indiarubber tubes. It is interesting to note that the first patient on which he used an indiarubber tube was Her late Majesty Queen Victoria, for whom he was called on to treat a large axillary abscess. I may quote Sir Hector Cameron's description: "In due course it (the abscess) was opened, with all antiseptic precautions, the line of incision in the skin having first been frozen by the use of Richardson's spray apparatus. Up to that time it had been Lister's practice in such cases to introduce a narrow strip of lint dipped in an oily solution of carbolic acid (1 to 4) through the incision, with the object alike of preventing primary union and of acting as a drain. This practice was followed on the present occasion. Next morning he was disappointed to find that little or no drainage had taken place and on withdrawal of the lint, thick pus, similar to the original contents of the abscess, escaped in quantity. Local tenderness and fever still also persisted. The same state of matters was found at one or two subsequent dressings. During a walk in the open air (a favourite practice with him when trying to solve a knotty problem), it occurred to Lister that if he could make use of some aseptic tubular drain, instead of the oiled lint, matters might progress favourably. Accordingly, on retiring to his bedroom that evening, he cut out a piece of the indiarubber tube of the Richardson's spray apparatus of suitable length and, having cut holes in it and sewed into one end of it a piece of silk thread, he placed it to soak all night in some watery solution of carbolic acid (1 to 20). In the morning he was pleased to find that the rubber was in no way weakened or altered in structure and, when changing the dressings, he substituted the tube for the strip of lint. At the next dressing he had, as he said to me, 'the inexpressible joy' of finding that not only had free drainage occurred into the antiseptic dressings, but that the discharge was now very thin and watery. Soon it became entirely serous in character, while it rapidly diminished in quantity. All constitutional

disturbance disappeared and very soon the abscess cavity was obliterated and complete healing secured. This was the first occasion on which he ever made use of a rubber drainage tube. On returning to Edinburgh, he repeated the experiment in a case of amputation of the thigh, with the best possible results. He immediately had rubber drainage tubes made by the manufacturers and ever afterwards used them constantly. Similar tubes had been devised and used by Chassaignac early in the century for carrying off accumulations of putrid pus from deep-seated situations; but it is my impression that the idea occurred to Lister quite independently. Whether this be so or not, the use of them, when rendered aseptic, proved a valuable addition to antiseptic treatment."

Attention was also directed to the best methods of approximating the edges of the skin so as to obtain primary union. Previously, when wounds were stitched up, it was done in a very perfunctory manner, and the stitches were generally pulled too tight and caused much irritation. Lister took the greatest care to bring the edges together accurately without pinching the skin under the stitches; these stitches were termed "stitches of co-aptation." When there was a moderate degree of tension two or three stitches of fairly thick silver wire were first inserted so as to relieve the tension (stitches of relaxation) and then the stitches of co-aptation were introduced. When much skin was removed he had a further arrangement of "button stitches," pieces of lead placed on each side of the wound connected by a piece of silver wire passing through the wound from one side to the other and fastened to the lead plates. The silver wire was pulled tight and caused marked approximation of the edges; in these cases the other two forms of stitches were also employed. The materials used for stitches were silk, catgut, horse-hair, silkworm gut, and silver wire, according to the circumstances of the case.

In many operations on the extremities he also took steps to render the limb bloodless before the operation. This was done by elevation of the limb for three or four minutes so as to empty it of blood, and then a tourniquet was applied at the upper part of the limb. At a later period than that of which we are speaking, Esmarch introduced a method of obtaining a bloodless limb by first bandaging it firmly from below upwards with an elastic bandage, and then applying an elastic band just above the termination of the bandage and removing the latter. There are various objections to the use of the bandage, and Lister performed a number of experiments on animals which showed that his method of elevating the limb was quite as satisfactory as the bandage (see Lister's 'Collected Papers,' vol. I, p. 176) and free from objection; he, however, adopted Esmarch's elastic band in place of the tourniquet.

This was the state of matters when the author first began work as dresser in Lister's wards in 1873. We had carbolic lotions, carbolic spray, and carbolic dressings, protective and jaconet; the vessels were tied with catgut, which was cut short, the deeper parts of the wound were in some cases approximated with

catgut, and great care was taken to unite the edges accurately without tension. We had also drainage tubes, and in many operations on the extremities the limbs were rendered bloodless. The instruments, sponges, and anything which came in contact with the wound were sterilised by prolonged immersion in 1–20 carbolic lotion, and the surgeon's hands and the skin of the patient in the region of the operation were thoroughly disinfected by carbolic lotion. As a result septic diseases were entirely abolished. There was no pyæmia; erysipelas was hardly ever seen, unless in cases already suffering from it when admitted; there were no cases of hospital gangrene, nor of tetanus developing after operation. It is true that suppuration did occasionally occur in a wound, perhaps three or four cases in the course of a session, and when this took place very thorough investigation was made as to the source of infection, so as to avoid it in future. In this way the treatment was steadily improved, and with experience the necessary precautions became more and more automatic.

When one bears in mind that these advances had been made in the treatment of wounds in some seven and a half years, and that the methods, starting with the use of undiluted impure carbolic, had been constantly improved till they had reached the stage described above, one can realise what a prodigious amount of labour and thought had been expended on it. During all this period improvements in the surgical procedures, as apart from the question of asepsis, were also constantly being carried out in all directions, as will be presently referred to. Further, Lister carried on his duties as Professor of Surgery and Clinical Surgery most efficiently and conscientiously, and also his hospital and private work. When we consider all this it is clear that the results could only have been reached by a man of splendid physique, and gifted with extraordinary mental endowments and powers of observation and deduction.

Although at this time (1873) septic diseases had been abolished and the range of surgical work had been greatly widened, Lister was not satisfied that he had worked out the best possible method and was getting the best results. The model which he had always before him was the subcutaneous wound, and his aim was that as soon as the operation was over and the wound stitched up it should become practically a subcutaneous wound and that only a certain period of rest should be necessary to complete the cure. The perfect result would be that at the end of the operation a dressing would be put on to protect the line of incision, and that when it was removed at the end of a suitable length of time (say, eight to ten days) the wound would be found healed, not only at the surface, but in depth, the stitches would have become absorbed, and nothing further in the way of dressings would be required.

To attain this ideal two things were necessary, firstly, that living bacteria should be completely excluded from the wounds, and, secondly, that the means employed to keep out the bacteria should not unduly injure or irritate the tissue. The first had already been attained almost completely,

and there could be no doubt that with further experience the exclusion of infective bacteria could be relied upon. But as regards the second point, the means employed to secure asepsis of the wound did irritate the tissues sufficiently to prevent the realisation of the ideal and led to the exudation of serum to such an extent that drainage was as a rule necessary for two or three days, and so the wound could not be closed and left alone under one dressing. Further, carbolic acid is a poison, and if absorbed into the system in large quantity might give rise to disagreeable results. As a matter of fact symptoms of serious poisoning from absorption of carbolic acid were very rare, but a good many patients had carboluria, which showed that a certain amount of absorption did take place. Further, the method was complicated, and many men would not give the time and care which was required in order to ensure success. The disadvantages of the treatment were, however, not of very much importance in contrast to the fact that septic diseases could be completely avoided by its use, but nevertheless Lister was not the man to rest content with anything short of his ideal, and consequently his further work was in the direction of simplifying the methods and of reducing the irritation of the wounds as far as possible.

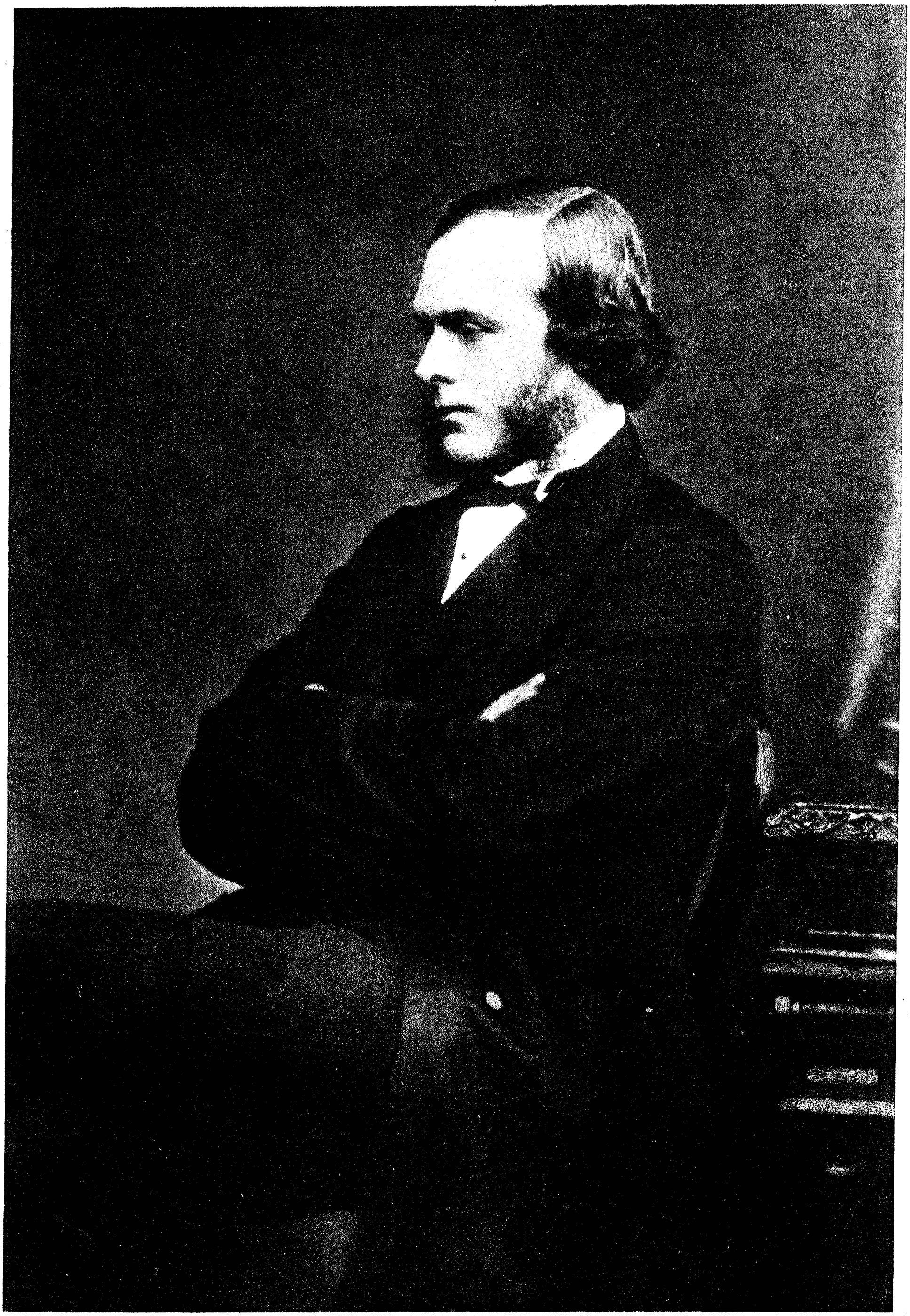
With this object Lister examined every fresh substance which was suggested as an antiseptic, testing them bacteriologically and observing their effects on wounds. He was constantly coming to the hospital with mysterious packets containing all sorts of gauzes or ointments, with which suitable wounds (*i.e.* those in which there would not be any serious danger if the asepsis were not quite perfect) were dressed, and the results as regards asepsis and irritation were carefully observed. He generally tested them as regards irritation on his own skin in the first instance, and he was frequently seen wearing patches of dressings on his arms in order to test if they irritated the skin. A great variety of substances were tried in this way, such as boracic acid, salicylic acid, picric acid, eucalyptus oil, thymol, various mercurial preparations (bichloride of mercury, biniodide of mercury, albuminates of mercury), iodine, iodoform, chinosol, and so on; in fact, whenever a new antiseptic substance was suggested it was carefully tested bacteriologically, its effect in preventing putrefaction of blood was examined, its irritating properties and its utility as an application to wounds were tried. The great majority of those substances were rejected for one reason or other, but a certain number were retained and are still employed in surgical practice; such as boric lotion, boric ointment, and boric lint, eucalyptus ointment, lotions of bichloride of mercury, double cyanide of mercury and zinc gauze, salicylic wool, etc.

When the mercurial preparations were introduced the use of carbolic acid was very much restricted, and ultimately it was only employed for the disinfection of the skin, and for keeping the instruments sterile during an operation. Sublimate lotions were substituted for carbolic acid, for the cleansing of the hands and sponges during the progress of an operation, and carbolic dressings were replaced by gauze impregnated with mercurial salts.

In the case of the mercurial dressings it became necessary to disinfect them before use, because, the mercurial salts being non-volatile, bacteria falling on them were not destroyed; this was at first done by wetting them with 1/60 carbolic lotion, and later by heat. The greatest simplification was the abolition of the carbolic spray. As bacteriological science advanced it was shown that the bacteria floating in the air were rarely pathogenic, and, indeed, were unable to grow in wounds and therefore might be disregarded. Further they were present in the air in the spore form, and the carbolic spray was shown to be ineffective in killing spores. When these facts were demonstrated it became evident that the spray was unnecessary and did not fulfil its object, and Lister accordingly gave up its use. No one was more pleased than Lister to get rid of the spray; so long as it was possibly of use he did not dare to discard it, but once it was shown not to be necessary he gladly gave it up.

Lister had always a strong belief in the *vis medicatrix naturæ*, and was always pointing out facts which indicated how the tissues, if in a healthy state, could to a certain extent prevent the growth of bacteria, and actually destroy them. Indeed, he was the first to furnish definite proof that what he termed the "vital action" of the tissues was a very potent agent in protecting the body from bacterial invasion. He showed by experiments with urine and milk how organisms cannot spread up canals lined with healthy mucous membrane. He disinfected the orifice of the urethra and the glans penis, and passed urine into a sterilised flask, which was then plugged with cotton wool. This remained sterile for years, and one flask is still in existence. A similar experiment was performed with milk; the udder and teats of the cow were disinfected and milk drawn into sterilised vessels. This is a much more difficult test, as the air of cowsheds is full of bacteria of all kinds, but several tubes of milk procured in this way remained sterile, showing that bacteria were unable to penetrate up healthy milk ducts.

By the time that Lister gave up work he had simplified and perfected the treatment of wounds to a very great extent. Antiseptics were no longer brought in contact with wounds in any appreciable quantity, and the antiseptics which might possibly get in were much less irritating than those formerly employed. As a result there was much less discharge from the wounds, and drainage was no longer necessary unless in exceptional cases. Consequently, dressings might be left on for days, and when removed the wound was found to be healed, while if easily absorbable catgut stitches were employed, they came away of themselves. In fact, his ideal of converting an open wound into a subcutaneous one had been practically attained. Since that time further changes of a minor character have been made which have added to the security of the aseptic result, such as the use of boiling for disinfecting instruments and other materials, the sterilisation of dressings, gowns and so on, in high-pressure sterilisers, the use of sterilised rubber gloves, masks, etc. Other changes have also been made which,



however, do not always show the profound insight and thoroughness of the Master, but, nevertheless, aim at still more perfectly attaining his ideal.

The principles on which Lister worked were on the one hand to keep living bacteria out of wounds, and on the other to remove as far as possible anything which would hinder nature to carry out its work, viz., the repair of the wounds, as quickly and satisfactorily as possible; in fact, as has already been said, he aimed at converting an open wound into a subcutaneous one. We have sketched the methods by which these aims were gradually attained in the course of a long and most laborious research. What then were the results? They were the complete abolition of suppuration and the septic diseases associated with wounds.

Previous to the introduction of his methods a surgical ward was a most depressing and painful sight. The odour in the wards, in spite of ventilation, was nauseating and often foul, the patients were suffering, in pain, flushed, feverish, and very ill; some were dying of one or other of the septic diseases of wounds. Union by first intention was a very rare occurrence; suppuration occurred in practically every case; indeed it could hardly be otherwise, as in most cases bunches of ligatures were hanging out of the wounds; associated with these ligatures was the danger of secondary hæmorrhage when they began to separate.

Still more serious than these local troubles was the frequent occurrence of general septic diseases, such as septicæmia, pyæmia, erysipelas, tetanus, or hospital gangrene. In a large proportion of the cases in which a wound of any considerable size was produced, whether by accident or by the surgeon's knife, the patient suffered more or less severely from one or other of these surgical diseases. After major amputations, for example, the mortality was very high; the average in the practice of various surgeons at that time varied from 30 to 50 per cent. We may here quote from the introduction to Lister's 'Collected Papers':—

"Lister collected his statistics of amputation for two years (1864 and 1866), just before he introduced the antiseptic method of treatment, and found the mortality to be 45 per cent. The causes of death are not definitely stated, but almost all the deaths were due to infective diseases; for example, of six deaths following amputation of the upper extremity four were due to pyæmia and one to hospital gangrene. In his paper on excision of the wrist-joint, published in 1865, he refers to fifteen cases in which he had performed this operation, and incidentally remarks that six were attacked by hospital gangrene, while one died of pyæmia.

"Volkmann, in one of his earliest papers on antiseptic treatment, stated that for the four years preceding the adoption of Lister's method, that is down to 1872, he had left his wounds entirely open. During the first year in which this method was carried out, the results were very favourable, and he was thoroughly convinced of its superiority over the plans which he had formerly adopted. As time went on, however, and as overcrowding of the wards became unavoidable, infective diseases of wounds increased

progressively, and at last, in the summer and autumn of 1871, the deaths from pyæmia and septicæmia were so numerous that he made up his mind to close the hospital altogether for a time. Before resorting to this desperate remedy, however, he determined to try the Listerian method for a few weeks, and the result of this trial was entirely to alter the aspect of affairs.

"Similar facts were published by Nussbaum of Munich, who commenced the treatment two years later than Volkmann. The hospital at Munich, a building by no means satisfactory as regards sanitary arrangements, became a hotbed of septic infection, to so great an extent that almost every case of open wound was attacked by one or other of these diseases. Pyæmia was rife, affecting nearly all cases of compound fracture, wounds of bones, and amputations. Erysipelas was constantly present. During 1872 hospital gangrene also appeared and steadily spread in spite of all the precautions which experience dictated or ingenuity could devise; in that year 26 per cent. of all the wounds were attacked by this dreaded disease; during 1873 the proportion increased to 50 per cent., and it ultimately reached 80 per cent. Erysipelas, too, which in 1872 was of a comparatively mild type, became much more virulent as well as more frequent. All this occurred in spite of the use of antiseptic lotions, of the open method, and other devices. In 1878, after he had put Lister's method to the test of practice, Nussbaum published an essay entitled 'Sonst und Jetzt,' in which he drew the following striking contrast between the previous state of affairs and that which followed the introduction of Listerism :—

Formerly.	*Now.*
Injuries of the head, compound fractures, amputations, and excisions, in fact almost all patients in whom bones were injured, were attacked by pyæmia. For example, of 17 cases of amputation, 11 died from this cause. Even patients with severe whitlow died from it.	No pyæmia.
Hospital gangrene had got the upper hand to such an extent that, in spite of the open method, in spite of continuous water-baths, in spite of the use of chlorine water, or the actual cautery, finally 80 per cent. of all wounds and ulcers were attacked, large arteries being opened into.	No hospital gangrene.
Almost every wound was attacked with erysipelas	No erysipelas.

"It would be easy to produce a great cloud of witnesses to the appalling state of matters in various hospitals before the introduction of the Listerian method, but their testimony would merely be a repetition of the above statements. It is true that these untoward results were witnessed most often, and in their direst form, under hospital conditions of a particularly insanitary kind, and that their frequency and severity varied considerably, according to the methods of wound treatment adopted. Nevertheless, these infective diseases were present everywhere, and it will readily be understood that the dread of them, never absent from the surgeon's mind, was a serious bar to progress."

But the results were much more far-reaching than this. The disappearance of septic diseases after operations opened up a new and large field of surgical activity Prior to the aseptic period, only operations of urgent necessity were performed, and these chiefly consisted of amputations, strangulated hernia, colostomy, a certain number of excisions, operations for stone in the bladder and for diseases of the urethra, opening abscesses, ovariotomy (just in its infancy) and a number of minor operations, especially such as could be performed subcutaneously. When it was demonstrated that one need no longer fear septic diseases after operation, it became evident to Lister that the whole subject of surgical treatment must be reviewed, and that many conditions might be relieved by operation which were formerly left alone, or that operations might be more thoroughly done than had previously been the case, and that many operations might now be performed which had not been thought of, or, if suggested, had been condemned as impossible and even criminal. Into this matter Lister threw himself with enthusiasm, and the treatment of every case was carefully considered from the point of view of whether something better might not be done for it under the protection of asepsis than had hitherto been the case. Lister thus not only abolished septic diseases, but was the pioneer of modern operative surgery, and for years surgical work could be seen in his wards which was not done elsewhere.

Reverting to the period when the writer began to work under him (1873), we had, on the one hand, the Professor of Surgery and several surgeons at the Edinburgh Infirmary who had not adopted Lister's practice, and ridiculed his views, and on the other, Lister himself. The teaching of the two schools was often diametrically opposite, and those of the anti-Listerian school were not sparing in their scathing and sarcastic denunciation of his work. His teaching was, however, clear and logical, and when we (the students) compared the practice and results of the two schools there was no question in the minds of anyone which was right and which was the surgery of the future. It would take too much space, and might be wearisome, to narrate the many alterations which Lister made in surgical practice, but long before his views on the treatment of wounds had been generally accepted, he had entirely altered the operative treatment of the surgical diseases which came under his notice. We may mention, however, a few points.

A good deal of his early advances in practical surgical work was carried out in connection with injuries and diseases of bones and joints. Ununited fractures of bone had previously been occasionally treated by operation with the view of obtaining union, but generally with disastrous results. Lister, however, had no hesitation in carrying out operations of this nature and with great success. Even if the operation involved opening a neighbouring joint, as in fractures of the olecranon, he did not hesitate to perform it. Malunited fractures were also operated on, pieces of bone being removed as required and the ends being brought into proper position and fixed by silver wire. Recent

fractures were also treated by operation if the bones could not be got into position. Deformities of bone, such as those which occur in rickets, were corrected by open operation, and old-standing dislocations were reduced by operation, the joint being freely opened and prepared for the reception of dislocated end. Healthy joints were opened for the removal of loose bodies. Joints were drained for obstinate hydrarthrosis. Tuberculous joints were laid freely open and drained. Extensive operations were performed for mammary cancer, the axilla being freely opened up. Attempts to cure hernia by operation were made. He reintroduced suprapubic lithotomy in preference to lateral lithotomy; and, indeed, in many other diseases too numerous to mention, he introduced operative treatment long before others had taken up aseptic work and had rid themselves of the former teaching that such operations were too dangerous. Had he published the work which went on in his wards he would be known, not only as the greatest scientific surgeon and the greatest benefactor of humanity who has ever lived, but also as the greatest practical surgeon of his day.

Although he was often urged to publish his practical work he always refused to do so, the reason being that he knew that there were very few surgeons who had at that time taken up aseptic work, or who were capable of keeping a wound aseptic; indeed, there were very few surgeons who did not look on his theories and practice as a species of insanity. He felt that if he recommended in the medical press procedures, the safety of which entirely depended on the rigid asepsis of the wounds, he might tempt other surgeons, who were not skilled in aseptic work and who did not, indeed, believe either in its theory or its practice, to undertake similar operations which, without the protection afforded by his work, would have ended in the most terrible disasters.

Another direction in which his activities found vent was in bacteriological work, much of which he also did not publish. He constantly made bacteriological observations and experiments in connection with his work on the treatment of wounds, and he very soon saw and taught that there was a good deal more to be considered than the simple putrefaction of the discharges in the wounds. He found, for example, that although there was no putrefaction in the discharge as tested by the sense of smell, there might still be pus, and he promulgated various theories as to the production of pus apart from the growth of micro-organisms. He, however, before long gave up those theories and had to take another view, namely, that the organisms which produced putrefaction were not necessarily the same as those which produced suppuration, and that organisms might produce suppuration without any putrefaction being present in the wound.

As a result of the disappearance of the various septic diseases in his cases, he naturally adopted the view that they must be due to the growth of micro-organisms, not merely in the wound, but in the tissues of the body. He was the first to point out that the tissues of the human body have a powerful action in destroying bacteria and in preventing their spread, and

that they were not like dead inert tissues, ready to become the home of organisms if only the latter gained access to them. He further came to the conclusion that these various septic diseases must be due to different forms of organisms, and some years after the commencement of his work, he with great diffidence promulgated the idea that tetanus, which at that time was looked on as a disease of the nervous system, was really nothing more than an infective disease of wounds, a view which was laughed to scorn by his contemporaries. It is remarkable how many of the views which he was constantly expressing to his intimates, and indeed to his students, have been verified by the progress of bacteriological work.

Apart from bacteriological work directly bearing on the best methods of excluding bacteria from wounds, Lister made several bacteriological researches in other directions. He made many experiments on spontaneous generation to test the accuracy of the previous results, and also extensive observations on the germ theory of fermentative changes. At the beginning of his work, he spoke of bacteria in connection with wounds as if they were all the same, but very soon he saw that there must be many different kinds of bacteria, each with their own special effect on the medium in which they grew. He was the first to demonstrate this point in his beautiful research on "Lactic fermentation," published in 1877, in which he showed that this change in milk was due to a particular organism which he called the *Bacillus lactis*, and which he was able to isolate by a very ingenious method from the many other forms of organisms which are present in milk.

In the course of his work on the best antiseptic materials for dressings (which he usually tested by packing them loosely in a tube, saturating them with fresh blood and then inoculating the blood at one end of the tube with putrefying material) he made the remarkable observation that a definite quantity of the putrid material must be inoculated in order to start the putrefactive process in blood, and that if the quantity was too minute, even though it contained a number of bacteria, putrefactive changes might not occur, indeed the organisms would not grow. The writer does not remember if this observation was ever published; it may have been mentioned in some of his papers, but it led the author to investigate whether anything of the kind occurred in infective diseases in the living body. These researches showed that a similar condition prevails in infective diseases, and that the occurrence and the virulence of the infection in individuals not extremely susceptible to the disease depend to a very large extent on the dose of the infective material in the first instance, a fundamental and very important point in the natural history of these affections.

In addition to his antiseptic and scientific work, Lister wrote several papers on surgical subjects which were characterised by the same care and thought as were his other works. Of these, his articles on "Amputations" and "Anæsthetics" published in Holmes's 'System of Surgery,' and his paper on "Excision of the Wrist," may be specially mentioned. These articles alone would suffice to stamp him as a good practical surgeon.

This short sketch will give some idea of the great work which Lister carried out and which has completely revolutionised surgery. He had the great good fortune to live to see his views universally accepted and the revolution completely accomplished, and those who were associated with him well know the great joy which it was to him to realise the enormous saving of life and diminution of suffering which resulted from his work. But although he had this supreme satisfaction he still remained to the last the modest man which he was at the beginning, always ready to accept anything which seemed likely to improve matters and to listen to and ponder over the views of others. Perhaps he was too ready to accept the work of others, only to find when put to a thorough test that it failed and had to be abandoned; this was especially the case in his search for more satisfactory and less irritating bactericides. More than once he announced to his class that such and such a surgeon had discovered the ideal antiseptic, only to find on thorough examination that it was not so.

Lister's patience and industry were extraordinary, indeed without these qualities he could not have carried out his work. From early morning till late at night he was always at work, patiently testing his conclusions, trying new plans and materials with the view of improving and simplifying his methods, looking after his patients, teaching his students and explaining his views to the visitors who crowded his lectures. As a teacher he was unrivalled. His diction was clear and simple, he gave his reasons fully for everything which he did, his lectures were thoughtful and made his hearers think, and his earnestness was inspiring. He did not teach the students to pass examinations, he taught them to think, he inspired them with high ideals, and those who had the honour and good fortune to work with him worshipped him and humbly set themselves to follow his example.

One of his great characteristics was his conscientiousness. He never acted on impulse, but always carefully considered any step he took. This is very evident in his scientific work, as a study of his writings will show. He made rapid deductions, but he tested them and considered them carefully before they took a place in the edifice which he was building, and he was always open-minded and ready to modify or abandon his positions if they proved to be wrong. In his private life, and in his dealings with patients, his conscientiousness was always in evidence. The advice which he gave was always the result of careful deliberation, and the welfare and comfort of his patients, whether private or hospital, were his first care. He spent a great deal of time in his hospital work, not only in dressing and making observations on the cases, but in seeing that every detail in the treatment was thoroughly carried out and that the patients were made as comfortable as possible. If a patient complained that a bandage was too tight or that his dressing was uncomfortable he would stop and rectify it himself, no matter what other engagements he had made. As a result he was often unpunctual in keeping his engagements. His humanity and his desire to benefit his fellow men, whether medically or financially, were very striking

traits of his character. He was ready to believe any tale of distress which was brought to him, and was very frequently imposed upon to a great extent by professional beggars. Indeed it was often difficult to persuade him that the recipients of his charity were unworthy; he had a childlike belief in the veracity and honesty of mankind.

Lister was elected a Fellow of the Society in 1860. He served on the Council in 1881–3, 1893–1901, and 1902–3. He was Foreign Secretary in 1893–5 and President during 1895 to 1900. He was also a Vice-President in 1900–01. In 1880 he received a Royal Medal and in 1902 the Copley Medal.

It is not possible to close this review of Lord Lister's work and life without referring to the great part which Lady Lister took in his work. She was an ideal helpmate—quiet, unassuming, and wrapt up in his work. The notebooks of his experiments are almost entirely in Lady Lister's handwriting, and not only did she act as his assistant, but she aided him in discussing and criticising the results obtained. Her loss to him was irreparable, and he was never the same afterwards. Fortunately her death did not occur till his work was finished.

W. W. C.

GEORGE ROBERT MILNE MURRAY, 1858–1911.

George Robert Milne Murray was born on November 11, 1858, at Arbroath, Forfarshire, Scotland, and after receiving a general education at the High School of that town, proceeded to the University of Strassburg, where he made a special study of Cryptogamic Botany under the direction of Prof. De Bary. On his return to England, in 1876, he was appointed assistant in the Botanical Department of the British Museum, the Botanical and other Natural History Departments being at that time located at Bloomsbury; he was placed by the Keeper, Dr. W. Carruthers, F.R.S., in charge of the Cellular Cryptogams.

While Murray had charge of the Fungi and Algæ, and more especially after the removal of the botanical collections from Bloomsbury to South Kensington, the cryptogamic herbarium was not only rearranged on modern lines, but largely extended through the acquisition of various important collections for the Nation. In this connection may be mentioned Broome's valuable herbarium of Fungi, Dickie's marine Algæ, and Wheeler's extensive series of water-colour drawings of British Fungi. He realised fully the

importance of authentic microscopic preparations to the student of Cryptogams, and was instrumental in acquiring several valuable series of mounted slides, for instance, those illustrative of the Fungi and Plant Anatomy of De Bary, the Red Algæ of Schmitz, and the Diatom collections of Deby and of Comber. His first published note dealt with the reproduction of the Ascomycetes and appeared in 1877. He contributed to the 'Encyclopædia Britannica' an article on Fungi in 1879, and one on Vegetable Parasitism in 1885. In 1882 Murray was asked by Prof. Huxley to investigate the Salmon disease, and afterwards published three reports on that important subject. He was lecturer on Botany at St. George's Hospital Medical School for four years (1885–9) and afterwards at the Royal Veterinary College (1890–5). In 1889 Murray (in conjunction with A. W. Bennett) published a Handbook of Cryptogamic Botany. From 1891, onwards, he was secretary to the West India Islands Exploration Committee—a Joint Committee of the Royal Society and the British Association—which was instrumental in the collection of much valuable material, comprising more especially Cryptogamic Plants and insects.

In 1892 he initiated and edited 'Phycological Memoirs, being Researches made in the Botanical Department of the British Museum,' and several parts were published in that and succeeding years. In 1895, on the retirement of Dr. Carruthers, he was appointed Keeper of the Botanical Department of the British Museum, and two years later elected a Fellow of the Royal Society.

Murray was born within a stone's throw of the sea and was at his best and happiest on that element. Hence he was led to make a special study of the marine side of Botany, publishing an Introduction to the Study of Seaweeds in 1895. As a collector of the minute vegetable organisms present in sea-water he was indefatigable, and in 1897, for the special purpose of collecting plankton, he obtained a grant from the Royal Society and crossed the Atlantic to the West Indies and Central America, accompanied by his Museum colleague Mr. V. H. Blackman, now Professor at the Imperial College of Science and Technology; one important result of this expedition was the observation of the hitherto mysterious organism Coccosphæra in the living state and the discovery of its green algal-like nature. This voyage yielded also a rich harvest of new forms of Peridiniæ.

He had previously visited the West Indies in 1886 as naturalist attached to a Solar Eclipse Expedition. From the Scotch Fishery Board steam yacht, the "Garland," he collected diatoms in most of the Scotch lochs; from material obtained in this way he made a very important discovery, that of the reproduction of a diatom by asexual spore-formation. Not only was he an enthusiastic collector himself, but his enthusiasm was of so infectious a nature that he was able to persuade several captains of ocean-going steamers to learn the methods of collecting plankton, and to bring him material from the Atlantic, Pacific, Arctic and Indian Oceans, and from the Red and China Seas. In 1898, aided by grants from the Royal Geographical Society, the Drapers' Company and the Fishmongers' Company, he chartered a tug (the

"Oceana") and went in the month of November to a part of the Atlantic, 300 miles west of the Tearaght Light, off Dingle Bay, Ireland, where the depth quickly increases from 500 to 1800 fathoms, to collect material at numerous measured intervals between the surface and the ocean-bed; on this expedition he was accompanied by V. H. Blackman, J. W. Gregory, L. Fletcher, his Museum colleagues, and also by J. E. S. Moore and Louis Sambon. That part of the Atlantic is so far from the usual lines of traffic that no other vessel was sighted during a space of ten days. Just as the work was being finished, a storm came on of such violence that a train on the nearest land was blown off the rails; the captain allowed the tug to run before the gale and eventually made for Queenstown, where the vessel was detained on account of the storm for thirty-six hours before the return voyage could be continued.

In 1901 Murray, as Scientific Director of the "Discovery" National Antarctic Expedition (under Captain Scott), was extremely busy with the provision of stores and apparatus; further, he edited the 'Antarctic Manual,' which was prepared for the use of the staff and published; he sailed with the "Discovery" from Gravesend and continued the organisation of the scientific work until the arrival at Cape Town, farther than which his duties in London would not allow him to travel. For a year or two previously he had been in unsatisfactory health as a result of repeated attacks of influenza, and it had been hoped that the sea-voyage would act as a restorative, but the heavy strain of the work he had undertaken proved too much for his weakened constitution. Then came two heavy blows; his wife died suddenly, from heart failure, in 1902, and his only brother, Alexander Stuart Murray, after a very short illness (pneumonia), in 1904; his brother had been Keeper of Greek and Roman Antiquities in the British Museum since 1886. Murray's health then broke down completely, and in 1905 he retired to Stonehaven, Kincardineshire, where he passed the brief remainder of his life; the immediate cause of death, which took place in his fifty-fourth year, on December 16, 1911, was cancer in the throat. He has left a son and a daughter. Murray was an excellent and cheery companion, very kind-hearted, and always ready with sympathy and help for those who needed them.

L. F.
W. C.

ADAM SEDGWICK, 1854–1913.

ADAM SEDGWICK was born in 1854 at Norwich, where his parents were temporarily residing. His father, the Rev. Richard Sedgwick, was vicar of Dent in Yorkshire, and it was there that Adam's childhood was passed, and he always regarded himself as a North-countryman. His father's family had been connected with the neighbourhood for many generations; they were landowners of the kind locally known as "statesmen," *i.e.* they farmed the land which they owned. To this family belonged also Adam Sedgwick, Professor of Geology in the University of Cambridge, and great-uncle of the subject of this sketch. The older Adam was one of the founders of British geological science, and his name is immortalised in the Sedgwick Memorial Museum of Geology at Cambridge. The mother of the younger Adam also belonged to a land-owning family whose seat was in the neighbouring county of Lancashire, and there is no doubt that Sedgwick owed many of the sterling elements in his character to this double strain of country-bred ancestry. Though his studies as a scientific man led him to radical views on many subjects, those who knew him best were never in any doubt as to the existence of an underlying stratum of conservativism on social, religious, and political matters, which formed, so to speak, the bed-rock of his mind.

Sedgwick was educated at Marlborough and entered King's College, London, with a view of qualifying himself for the medical profession, but his stay there was brief, and in 1874 he came up to Cambridge and entered Trinity College as a pensioner. At that time there were being laid at Cambridge the foundations of that school of animal biology which has brought so much fame to the University, and the founders were connected with Trinity College. Michael Foster had left Huxley a few years before in order to introduce the new science of "biology" into the old University; he was Prælector of Physiology and Fellow of Trinity College. Amongst his first pupils was Francis Balfour, later created Fellow and Lecturer of Trinity College, who threw himself with enthusiasm into the study of Comparative Embryology, in which he achieved a world-wide fame. Adam Sedgwick early fell under the spell of this brilliant genius, and soon abandoned the idea of entering the profession of medicine, but took up instead the precarious occupation of a teacher of pure science. In 1877 he took his degree in science with first-class honours. In 1878 he was made Foundation Scholar of Trinity College and he became demonstrator to Balfour.

In 1882 the University, fearful of losing Balfour, created a special Chair of Animal Embryology for him; but, in the same year, Balfour lost his life on a mountain-climbing expedition in the Alps and the University declined to continue his Chair. It seemed as if the newly-created School of Comparative Embryology was doomed to extinction, but after some delay the

University consented to create a Readership in Animal Morphology at the small salary of £100 a year, and to this Adam Sedgwick was appointed. He had already been elected Fellow of his College in 1880 and he was now created Lecturer, and so he was enabled to earn an income sufficient to support himself. It is greatly to the credit of the Fellows of Trinity College that, by their action at this juncture, they endowed biological science and enabled the work of Balfour to be carried on.

The Chair of Zoology was occupied at this time by Alfred Newton, who represented the systematic side of the science, and so for twenty-five years, until Newton's death in 1907, Sedgwick acted as Professor of Morphology and Embryology without either the emoluments or the University status of a Professor. The work begun in Balfour's little laboratory (originally a couple of rooms in the department of Physiology) grew in importance, and room was created for it by raising the roof of the engineering laboratory by means of jack-screws, and thus intercalating a new series of rooms between the roof and what had previously been the uppermost storey of the building. In the lofts thus improvised, Adam Sedgwick, by his perseverance and enthusiasm, built up one of the finest schools of zoological research in the world. The teaching of animal biology, especially on its practical side, was systematised by him to a degree that had never been attempted before, and was converted into a thoroughly sound intellectual discipline. Around him were gathered an eager band of researchers, amongst whom were students from the United States and from Japan. Indeed, the School of Zoology at Tokio may be said to be the child of the school at Cambridge, for Mitsukuri, its founder, began his researches in Cambridge. The pupils of the Cambridge school went all over the country to occupy important positions in zoology; amongst them may be mentioned Weldon, late Professor of Zoology in Oxford; Bateson, Director of the John Innes Horticultural Research Institution; Graham Kerr, Professor of Zoology in the University of Glasgow; Hickson, Professor of Zoology in Manchester. Sedgwick literally lived in the laboratory, bringing the influence of his splendid personality to bear on all his pupils, kindling their enthusiasm, guiding and supervising their researches, and making a most enduring impression on their minds.

In 1883 Sedgwick made a voyage to the Cape, where he remained some months in order to study the embryology of that strange animal Peripatus, which by some naturalists was (and is to this day) classed as an Annelid, whilst others regarded it as the lowliest member of the group Arthropoda. The fruits of this expedition were a remarkable series of memoirs published in the 'Quarterly Journal of Microscopical Science,' which demonstrated beyond cavil that Peripatus is indeed an Arthropod, but one of such primitive character that it might also be regarded as the veritable "missing link" between Annelida and Arthropoda. The relation to one another of blood-spaces and body-cavity were made clear once for all, and a whole mist of misinterpretation cleared away from the ideas which had previously prevailed on the structure and relationships of the Arthropoda. Whilst the publication

of his results was still far from complete Sedgwick was elected Fellow of the Royal Society, and subsequently he had twice the honour of serving on the Council. In fact, he was regarded as one of the leading zoologists of the country, and he served on every important Committee connected with the subject. Amongst his staunch and life-long friends he numbered the doyen of British zoology, Sir E. Ray Lankester; for whom he had the greatest respect and admiration. He lived also in the most friendly and harmonious relations with Newton, Professor of Zoology in Cambridge, who, instead of regarding the development of the newer side of zoology with suspicion or jealousy, aided and abetted Sedgwick in every possible way.

In 1892 Sedgwick married Miss Laura Robinson, daughter of Captain Robinson, of Armagh, and in 1897, with some misgiving which was shared by his warmest friends, he accepted the post of Tutor of Trinity College, a position which he held until 1907. The duties of his new office not only rendered it impossible for him to prosecute research, but even made it difficult for him to spend much time in the laboratory. Although he devoted his vacations to the production of a 'Text Book of Zoology,' of which three volumes were published, and which is the most complete so far produced in the English language, yet this was a poor consolation for the withdrawal of his stimulating presence and influence from his students. A comprehensive text book of Zoology is too vast a work to be completed by one man, and although the great interest of Sedgwick's work was the decision of his matured judgment on the infinitely varied facts of the science, yet for the completion of his third volume he had to call to his assistance the help of his friends J. J. Lister and A. E. Shipley (now Master of Christ's College).

In 1907 Newton died and Sedgwick was appointed Professor of Zoology in his stead, but he had hardly settled down to the duties of his new office when events took place which were destined to transfer him to a new sphere of activity. In 1908, the Governors of the newly constituted Imperial College of Science and Technology resolved to re-endow and equip the Department of Zoology in the Royal College of Science, which was one of the three constituent colleges of the new institution. This department had been rather neglected since the death of Howes, who was Huxley's successor; arrangements had indeed been made for the carrying on of the teaching temporarily, but no Professor of Zoology had been appointed. Sedgwick was asked to join the Committee whose duty it was to select a new professor of zoology for the college. When the Committee presented their report, the Governors unanimously besought Sedgwick to accept the post himself and to undertake the duty of reorganising the department. Sedgwick promptly declined; but a short time afterwards the request was renewed, the Rector of the College travelling down to Cambridge in order to press the wish of the Governing Board on him. At last, impressed by a sense of the duty which he owed to the science of zoology in general, he yielded to the pressure put on him and, in 1909, he severed his life-long connection with the University of Cambridge and took up the duties

of Professor of Zoology in the Imperial College of Science and Technology. He was succeeded in the Cambridge Chair by one of his ablest pupils, Stanley Gardiner. Once established in London, he threw himself with the utmost zeal into the organisation of the Department of Zoology. He established series of lectures in those branches of zoology which had the most direct bearing on the economic applications of the science. In this way more students were attracted to the department than had ever been in it before, and when Sedgwick died a flourishing sub-department of Entomology, with an able professor at its head, had been brought into existence. The initial task of organisation necessarily involved a considerable expenditure, but Sedgwick's efforts were bearing fruit when sad signs became evident of the failure of his health.

Already, in 1904, he had had some attacks of pulmonary disease, but these, it was thought, had been, completely overcome. There was left, however, some pulmonary weakness, and before accepting the professorship in London, Sedgwick consulted his medical adviser and was assured that he had no disease and was quite competent physically to undertake the duties of the new post. Until the spring of 1911 his health seemed indeed to be improved, and his friends were delighted by his vigour and spirits. In 1911, however, whilst travelling on business in the North of England, he contracted a severe attack of influenza, and from that date until his death his pulmonary troubles increased; he lost flesh, and his health visibly failed. At the end of 1912, yielding to the urgent advice of his physician and his friends, he obtained leave of absence from his teaching duties and went abroad to winter in the Canaries. The change seemed at first to afford some relief, but a relapse supervened, and, despairing of any improvement in his health, he returned to London, and died in his residence at South Kensington on February 27, 1913, thirty-six hours after his return.

Besides a widow he left three children, two sons and a daughter. The elder son has already entered on an academic career and is a Foundation Scholar in Trinity, his father's college.

In reviewing Sedgwick's contributions to science, his eager and ardent nature must be constantly borne in mind. He was for ever seeking new points of view and was apt to be somewhat impatient of those who clung to older views. Hence he was wont to state his ideas in somewhat strong language, which roused opposition and delayed recognition of the side of the truth which he was seeking to emphasize. But as old controversies pass into oblivion the essential truth of many of Sedgwick's ideas comes more and more into prominence. His early work was concerned with the structure and development of the vertebrate kidney, especially the kidney of the higher vertebrates. In this he was following up and completing the researches of his teacher Balfour. Sedgwick's views were not accepted by Continental zoologists and the great authority of Gegenbaur caused them to be regarded as unsound, but a few years ago their truth was completely vindicated by the masterly researches of Schreiner. It was the same with his work on the

kidney of Mollusca: here again his statements were confirmed after a lapse of many years by Plate.

His great work on Peripatus, of fundamental importance for structural zoology, also encountered mistrust on the Continent, but it is not too much to say that all subsequent work, not merely on Peripatus but on the development of Arthropoda generally, has tended to support and confirm Sedgwick's views. Out of this work arose, however, a wider controversy, no less than the validity of the cell-theory itself, and on this subject Sedgwick took up a position which can only be described as revolutionary. The orthodox view of the subject which was then held, and which is still held by many zoologists, is that the higher animal is a cell-republic and that each cell is equivalent to a Protozoon. To this view Sedgwick opposed the idea that the cell of the higher organism is a mere unimportant and imperfect sub-division of the cytoplasm and that a nucleated sheet of protoplasm can and often does replace a layer of cells. Sedgwick's views were received with almost universal repudiation, but the work of Driesch in developmental mechanics has certainly gone far to confirm them; it has been definitely shown that an entire and perfect organism can be built up out of half an egg and that such an organism contains half the normal number of cells, so that each cell in the smaller organism must bear a totally different relation to the whole from what it does in the organism of normal size.

At the beginning of his career Sedgwick was a strong supporter of the recapitulatory theory of development. As the presence of other factors than the ancestral one in the determination of development became more and more obvious, Sedgwick put forward the only plausible theory that has as yet been advanced to explain the existence of the recapitulatory element in developmental history. Later in his life, disgusted with the facile and unsupported assumptions which characterised too much of the reasoning on this subject, he passed into an attitude of entire hostility to the theory and published a series of trenchant criticisms of it in the Darwin Memorial Volume which was issued in 1909. Those who still hold firmly to the truth of the recapitulatory theory can only rejoice in the publication of these able criticisms, for they will compel the data on which the idea of recapitulation is based to be thoroughly sifted, and it is to be hoped that closely considered reasoning will be substituted for the wild guesses which have too often done duty for argument when recapitulation has been discussed.

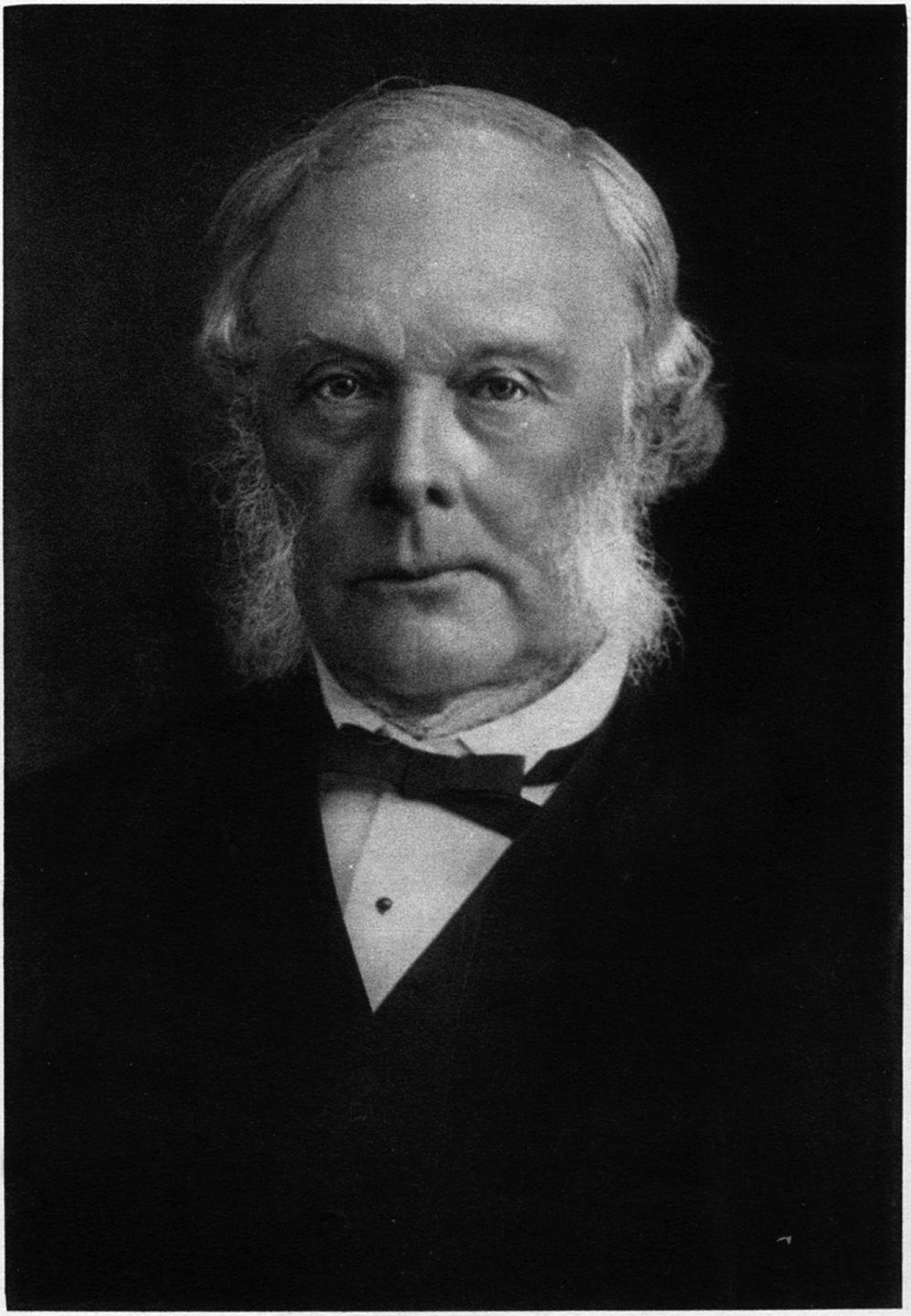
In appearance Sedgwick might be described as a typical English squire—of medium height, ruddy complexion, somewhat thick-set in build, direct and sincere in his manner. As one got to know him better, his sterling qualities evoked such admiration from his pupils that their feeling towards their master can only be described as the warmest affection. A friend of the most constant and unswerving loyalty, and a friend who would bluntly tell the truth even when the truth was painful to youthful vanity; a wise counsellor, the moderation and carefulness of whose advice were in sharp contrast to the exaggerated language in which he "blew off steam" when off duty: such

was Sedgwick, and to all who knew him and loved him his death is an irreparable loss. His very weaknesses endeared him to his pupils when they came to be regarded as symbols of his personality. He had a hasty temper which used to relieve itself in somewhat violent language with a plentiful admixture of expletives—but the storm was over almost as soon as it arose, and the expletives left no sting behind; indeed, they were gleefully repeated and with embellishments served as the basis of a number of Cambridge legends.

Nothing caused more secret gnawing apprehension to his closest friends than the disappearance of these ebullitions; they felt as if the old Sedgwick were slipping away from them, and this proved only too true.

Sedgwick's memory will live for ever in the zoological school at Cambridge, which, though founded by the genius of Balfour, was built up to its leading position by Sedgwick's efforts. Whether the school he founded in the Imperial College will be equally successful remains to be seen, but his memory and example will serve as an inspiration to all those who were brought into contact with him there.

E. W. M.



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