of that vivid and intimate conception of cell life struggling against irritant agents which directed his surgical strategy afterwards.

INHIBITORY ACTION OF VISCERAL NERVES.

Another of his physiological investigations concerned the inhibitory nature of certain visceral nerves. He wrote: "I have been led to make an experimental inquiry into this 'so-called inhibitory agency' because it appears to me to have an intimate bearing upon the question how inflammation is developed through the medium of the nervous system at a distance from an irritated part." His main conclusion was that inhibition resulted from more energetic action of those same nerves which when working mildly produce increased action.

Then, more closely related still to his pathological inquiries, followed two papers on clotting of blood, confirmatory, as he himself remarks, of his conclusions arrived at about inflammation. These papers and the year 1863 practically concluded the tale of Lister's researches devoted directly to physiology. Perhaps their main interest now is their revelation of their author in his earliest research career. Conspicuous in them is capacity for noting detail and the rarer power of sifting out from the detail its broader meanings. Another of their qualities is a voracious appetite for seeing for himself phenomena many would be content to take on trust from the excellent descriptions given by others. Lister was satisfied by nothing less than seeing for himself, and his eye never staled. Moreover, even at this stage, though he might appear engaged on diverse problems, he was in fact centred upon one problem, of which from his point of view the several were but several aspects. Thus, these early papers show that in reality quite early in his career he had not only found but had whole-heartedly flung himself upon what was to be his life-work and his life's problem—a problem we may almost subsume under the one broad term "inflammation." That was the study which he was to follow to a practical solution so magnificently, and in doing so exhibit what these earlier papers had less opportunity for showing—the splendid power of establishing, and systematically following through its consequences, a great generalization. Hence from these earlier researches we can see spring that triumphant career of experimentation and observation, in the laboratory and the ward, which, with a really surprising speed of mastery, winged Surgery for that which she is to-day—a far-flighted angel of healing the civilized world over.

ANTISEPTIC SURGERY IN PHYSIOLOGICAL EXPERIMENT.

But these papers whose features we have been examining far from exhaust the gift made by Lister to physiology. In addition to and beyond them he enriched it with the con-

tribution of enhanced means towards its own most cherished aims. He put into the hands of physiology for all time a superlative refinement of its method, and made possible observations otherwise impossible. Sir Berkeley Moynihan has eloquently insisted on the importance of surgery as a means of discovery. In physiology, how could Pavloff have achieved his epoch-opening study of the digestive processes except by leaning upon Lister's surgical principles? Ferrier initiated his study of localization of cerebral function by means of Lister's surgical principles. How could the physiologists of Toronto, less than four years ago, have bestowed upon diabetic sufferers that merciful remedy insulin, had it not been for the legacy of operative principle and science bequeathed to them and to the whole world by Lister?

And Lister's own words leave us in no doubt as to what a source of rejoicing it was to him that among benefits accruing from his work was this one, an enhanced scope and power for physiology and experimental medicine. He dwelt both in public and in private upon the need and importance of such experimentation for the progress of knowledge necessary to civilization. He rejoiced that he had contributed to its power. And indeed the experimentalist owes to Lister an instrument of research whose beneficent future the boldest imagination may well halt to set limit to.

And of the gifts of Lister to humanity one which the experimentalist is with especial gratitude ever mindful of is that, while helping man to mastery over disease alike for animal and man, he contributed to free that necessary experimentation from the infliction of pain. Man, sacrificing animal life as he does to satisfy the material needs of humankind, has the right to regard the intellectual need impelling him to search means of saving human and animalkind from disease as justified in its resort to animal experimentation. He does so with the less heart-searching and with clear conscience because largely owing to Lister he can do so without inflicting pain and suppuration.

Thus it is that through years to come, after indeed it may be that the actual papers contributed to physiology by him may have become matter chiefly for the historian and the antiquarian, Lister will still receive unflinchingly his meed of commemoration from the physiologist and experimentalist, and in a manner which to himself would, of all ritual and offering, have been the most congenial!—namely, in their daily observance and trust of methods which he discovered and inculcated, and in their practice for the alleviation and prevention of disease. It is therefore with peculiar gratitude that Physiology brings its tribute of admiration and veneration to the memory of one great even among the greatest of the benefactors of humanity, Joseph Lister.

LISTER AS A PATHOLOGIST AND BACTERIOLOGIST.

BY

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WE are met to commemorate the centenary of the birth of Lord Lister, and I have been asked to address this representative gathering on his contributions to pathological science.

Proud as I am of the great honour which has been bestowed on me, I am painfully conscious of my own imperfections and limitations for such a task. My qualifications are twofold. In the first place, I have read all Lord Lister's writings and have made a critical study of some of them, not only to discover their intrinsic merits, but to compare them with similar studies of his predecessors and contemporaries.

In the second place, I had the great privilege of knowing Lord Lister for thirteen years, and in a manner which, considering the great disparity of our positions, was, if I may presume to say so, almost intimate. As bacterio-

logist to the British Institute of Preventive Medicine I had to visit Lord Lister, the chairman of the Institute, almost weekly in order to report to him the progress of the work in the antitoxin department. After I left the Institute he frequently asked me to call upon him to discuss various scientific problems in which he was interested. In this way I was a good deal in his company, and among the younger men of that day I probably saw as much of him as anyone else.

When I first knew him, in 1895, he was 68, and I frequently saw him down to 1907. I last saw this great man in a bath-chair by the roadside at Walmer on a lovely sunny morning—December 26th, 1909. He was then 82 and was approaching the lonely end of his splendid career. Both then and now he stands out to me as a most impressive personality. I have known very many of

the leaders in our profession at home and abroad during the last thirty years, and have no hesitation in placing him *facile princeps Medicorum*. -

Up till old age he continued to be deeply interested in all medical advances and was almost young and boyish in his enthusiasm. Although leading a busy and strenuous life, he was still striving to keep abreast of bacteriological literature, which at the time was pouring forth in an unbroken stream. On several occasions I was hours in his company trying to unravel the intricate papers of Ehrlich and Bordet on the nature of haemolysis or the constitution of diphtheria toxin. Our hour of work finished, he would relax into reminiscences of his life and work, the difficulties he had to contend with, and how he had overcome them. On one such occasion—the date was October 23rd, 1905—he referred in great detail to his work on pigmentation of the frog and on inflammation, and he said to me—“I wrote the words down at the time—‘If my works are read when I am gone, these will be the ones most highly thought of.’” These were not the mumblings of senility, for he was still alert and fresh, but I took it to mean that he wished to be regarded as a scientist rather than as a surgical craftsman, notwithstanding his pre-eminence in the latter branch of human activity.

It is therefore a peculiar pleasure to me to have to refer to some of these contributions which he himself prized.

In estimating Lord Lister's scientific work, it is essential to remember that he had no properly equipped laboratory as we understand it to-day. His laboratory was his study in his private home, and perhaps the best part of his scientific work was that done in 11, Rutland Street, Edinburgh, during his first stay (1853-60) in the northern capital. To this period belong the papers on pigmentation of the frog (1857), the function of the visceral nerves (1858), the coagulation of the blood (1858-63), and the early stages of inflammation (1857). The construction of his antiseptic system took place mainly in the Glasgow period, 1860-69, while his chief bacteriological studies are to be referred to the second stay in Edinburgh (1869-77), when he resided at 9, Charlotte Square.

The hours for his scientific work were early in the morning and far into the night, following a harassing day of active surgery in private or in hospital. He undertook his experimental work so that he might speak with first-hand knowledge of the themes he had to teach. Many of the problems, although of paramount importance practically, were obscure and complicated. Some were not capable of solution then, others have not yet been definitively settled.

COAGULATION OF THE BLOOD.

The coagulation of the blood has at all times excited wonderment, and the theories to explain the process have been innumerable and are still being brought forth. In this country very notable advances were made in the eighteenth century by William Hewson, who unfortunately died before he was 35. He proved that the red blood corpuscles were bi-concave discs; he discovered the white corpuscles and performed a large number of admirable experiments to explain the process of clotting, although he never quite cleared up the mystery of its nature. His successors in the nineteenth-century were not more happy. Coagulation was early studied by Lister, as it was constantly being brought to his notice in connexion with closure of the vessels, putrefaction, and secondary haemorrhage in surgical wounds. When Lister began, the prevailing theory was that of B. W. Richardson, who referred coagulation to the escape of ammonia, which was believed in some way to hold the coagulative elements normally in solution. In a long series of experiments conceived and carried out in masterly fashion, Lister showed that the ammonia theory was untenable. He clearly saw the necessity for separating the nature from the cause of coagulation, and while baffled with the former he revealed by his experiments that the latter—the cause of coagulation—is really due to the influence exerted on the blood by the contact, even momentarily, of ordinary matter of some kind. He considered that this contact brings about

a reaction between the solid and fluid constituents of the blood, so that the corpuscles imparted to the liquor sanguinis the disposition to clot. As regards the cause of clotting, it cannot be said that we have materially advanced during the seventy years since the publication of Lister's work.

EARLY STAGES OF INFLAMMATION.

Another basic pathological process which he studied was inflammation. The extraordinary changes which we call inflammation have always attracted attention, and the explanations given constitute a large part of the history of medical doctrines. When Lister began to study inflammation there was already a considerable mass of accurate knowledge on the subject. The older pathological anatomy, so ably created by Morgagni and followed up by the French school down to the time of Cruveilhier, was being replaced by the new knowledge which accrued from the discoveries of Schleiden and Schwann. At the time Edinburgh was a great centre of medical activity and a nursery of medical talent. Many of her graduates migrated across the border to attain fame in the arenas of the South. I need only mention names like Richard Bright, Hodgkin, Thomas Addison, C. J. B. Williams, Marshall Hall, and two who had a marked influence on Lister's life at University College and afterwards—I refer to William Sharpey and the exceedingly able if eccentric Wharton Jones. Their work was carried forward by Augustus V. Waller and the little known but highly successful worker William Addison. In the inflammatory process the margination of the leucocytes was taught by C. J. B. Williams and W. Addison in 1841. In 1846 Volnay Waller rediscovered the wonderful process of emigration of leucocytes, previously known to Dutrochet (1827). In a most scholarly manner Wharton Jones summed up all the knowledge on inflammation down to 1846, and himself gained the Astley Cooper prize in 1850 for his splendid essay on the pathology of inflammation.

When, therefore, Lister began to study the process he was traversing ground already trodden. He early realized, however, that much that had been done concerned the later stages of inflammation, whereas its real essence was most likely to be found by analysing the earliest changes. Examining the frog's web and the bat's wing, he took elaborate precautions that they should be in a perfectly healthy condition. By the application of irritants he passed to the study of the morbid, and investigated especially the aggregation of the red corpuscles, their increased adhesiveness, and the structure of the arterioles and capillaries. He found that the capillaries alter in size, but referred the change to something inherent in their elasticity. While admitting the existence of contractility in the capillaries, modern writers have not accepted his explanation. Lister regarded irritants as acting in a twofold manner. The primary effect was a dilatation of the vessels, brought about through the influence of the nervous system and not limited to the *locus* of the irritant. The secondary effect, on the other hand, was the direct outcome of the irritamentum itself, in consequence of which the blood becomes altered physically. The red discs become more adhesive, they accumulate in masses, and may bring about a condition of actual stasis. Strange to say, Lister made no reference to diapedesis of the leucocytes, and probably missed it altogether. The earlier discovery of Waller had left, apparently, little impress on the knowledge of the day, so that when Cohnheim described diapedesis in detail in 1867 the discovery was looked upon as something altogether new. It is to Lister's credit that he discussed inflammation from a new point of view, which Cohnheim nine years later carried to a conclusion.

BACTERIOLOGY.

When Lister embarked on his extended study on the cause of suppuration and the complications which follow surgical operations, his previous work on blood and inflammation was a great help to him. He was continually groping for the causes of the surgical disasters which were habitually happening, but light began to peer through the obscurity. This was early during his Glasgow term. In

1865 his attention was directed by a colleague to the work which had been done on fermentation and putrefaction by Pasteur from 1857 onwards. This work came upon him as a revelation, and almost at once he grasped the full significance of the Frenchman's discoveries for surgery. Ten years later he tells us specifically that the work of Pasteur "long since made me a convert to the germ theory, and it was on the basis of that theory that I founded the antiseptic treatment of wounds in surgery."

Although he frequently reiterated his indebtedness to Pasteur, it is, I think, a vulgar error to regard Lister as a mere imitator of his great French contemporary. As early as 1861, and before his acquaintance with Pasteur's papers, he was getting near the truth about suppuration, and afterwards he advanced beyond the point where Pasteur had led him. From now on he became ardent in the pursuit of bacteriology. This was about 1870, when the study of microbes had scarcely emerged as a distinct science. At the time there were two conflicting views. The one, supported particularly by the eminent botanist Ferdinand Cohn, held that bacteria exhibited a constancy in form which might be used as a basis of their classification into genera and species.

According to the other view, there is no such morphological stability, but rather a pleomorphism whereby one and the same microbe could assume different forms according to the environment. In his earliest bacteriological work in 1873 Lister ranged himself among the pleomorphists, and it has to be admitted that he suffered shipwreck when he stated that Ehrenberg's and Cohn's classification was "entirely untrustworthy." Lister's mistake was one made by a great many others at that time, and serves but to show how unwary the earlier bacteriologists were of the pitfalls lying in their path. There exists an exceedingly interesting correspondence which passed between Lister and Pasteur on the subject. Pasteur clearly saw where Lister had erred and advised him to repeat his experiments with added precautions. This, Lister did, and, profiting by his now experience, became one of the foremost bacteriological technicians and pioneers of his time. So imbued was he with lofty ideals that, instead of covering up his tracks, he handsomely admitted his error. "Next to the promulgation of truth," he said, "the best thing I can conceive that a man can do is the recantation of a published error." This sentiment was almost exactly that which was given to us sixteen hundred years ago by Celsus, who, however,

added that "such a confession is suited only to a great genius whose splendour is such as to survive the sacrifice, especially in the performance of a task which is to be handed down for the benefit of posterity as a beacon of truth to warn them against similar errors." From the theory of Cohn and Pasteur it was to be presumed that in some way—at that time unknown—bacteria might be separated from each other and cultivated in a pure state. The great mycologist Brefeld early pointed out the necessity of raising pure strains or cultures from one single germ. Lister attempted this, and by unsurpassed technique, involving a remarkable understanding of the whole problem, he succeeded in obtaining a pure growth of a microbe which he called *Bacterium lactis*, and which was a cause of the so-called lactic fermentation. He grew the microbe in sterile milk and raised a pure strain, constant morphologically and physiologically, from a single bacterial cell. This was possible by the aid of an ingeniously constructed syringe of his own invention.

He also introduced several of the methods of sterilization which are in constant use to-day. His work on lactic fermentation is a classic, and stands as a model of what a scientific research should be.

Like Pasteur, Lister had the supreme faculty of seeing as if by instinct the exact experiment required to eliminate an element of doubt or to advance ahead. He was a master of the experimental method—a rare and precious gift which the Abbate Spallanzani truly said "has always been confined and always will be confined to the few."

And now I am at the end. No one could come into contact with Lister without being impressed by his noble personality, his magnanimity, his liberality, and his modesty. His aims were of the loftiest kind, and probably no medical man ever kept more steadfastly before his gaze the covenants of the Hippocratic Oath. He venerated his teachers, he taught his art to his pupils, he kept nothing secret from his profession, and followed that system which, according to his ability and judgement, he deemed best in the interest of his patients. With purity and holiness he passed his life and practised his art.

When one looks down the long vista of time and contemplates the art of medicine, three names stand prominent, and we should all feel pride in our English stock which gave birth to Harvey, to Jenner, and to the great Lister—a name to resound for ages.

LISTER AS A SURGEON.

BY

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SELDOM is a great discovery the product of one man's mind. In the work of other men it has ancestors; fore-runners possessing one or more attributes whose final and felicitous association within new work constitutes new truth. When that new truth is fully disclosed its relation to many other truths familiar to all for perhaps many years and in many associations becomes clear. The claim may then arise that those who have revealed isolated and antecedent truths have priority in the final discovery. Such truths are, however, only progenitors with no claim to be regarded as their own descendant, the new truth itself.

SURGERY A CENTURY AGO.

The immortality of Lister, whose centenary we commemorate to-day, is due to discoveries which did for the craft of surgery what John Hunter had done for its science. The oldest human remains bear evidence of surgery. Before Lister came operations had been relatively few because of their heavy mortality and their almost insupportable burden of terror and of suffering. Every operation risked the life of the patient from one single cause—putrefaction in the wound. So frequent was this that it was always expected; and whenever it appeared

no man might foresee or measure its consequences. Evidence of this we find on almost every page of the old textbooks and in the biographies of the great surgeons. It is startling to read that when, in the year 1821, Astley Cooper operated upon George IV for a small sebaceous cyst on the head, so tortured was he by anxiety lest erysipelas or pyaemia might develop that he sought to put upon others the responsibility of the operation—on Cline, on Everard Hume, on anybody but himself. He speaks of the operation in terms which to us now appear absurd, fearing that "it might by possibility be followed by fatal consequences." He says, "I saw that the operation, if it were followed by erysipelas, would destroy all my happiness and blast my reputation"; and "I felt giddy at the idea of my fate hanging upon such an event"; and again, "I am certain that if anything happened to the King that at any rate I should leave London and live in retirement." It is hard to believe that a surgeon eminent enough to be chosen for service to the King should be so deeply moved at the prospect of what was to him, as to us, technically the simplest of operations. The exercise of the art of surgery brought terror then where it now brings joy, to surgeon no less than to patient.