this Section, we have helped to cut down some of the scientific weeds of our own age, and not only so, but that we may have added something to the great store of knowledge which it is one of the great objects of this Association to accumulate.

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF SURGERY,

*At the Annual Meeting of the British Medical Association,
in Norwich, August 1874.*

By SIR JAMES PAGET, BART., F.R.S.,
President of the Section.

I OWE to being a Norfolk man the honour of being President of this Section, and certainly I owe it to that fact that when the honour was proposed to me I accepted it, for there was no other ground whatever on which I could have held such a position as this; but when on that ground it was proposed to me, I felt I was bound by a debt of gratitude to show myself, and endeavour, if I could by any means, to promote the success of the meeting of the British Medical Association in my own county. I very soon found that the honour brought with it, as honours usually do, a great responsibility, which was that I should give some reason for my being here, and say something of Norfolk or of what I had to do with it. And, naturally, I turned back to think of what Norfolk surgery was when I left it to be a student in London; and I soon found reason for having yielded to any invitation that could come to me from my county in the feeling of profound gratitude that I had to bear to those who were my first teachers, and among whom were found my earliest and now my oldest friends. I turned to my first case-book, commenced in 1830 and finished in 1834, when, just forty years ago, I left Yarmouth, where I was apprenticed, to proceed to the larger field of study in London. I had spent there four years, being apprenticed to learn the "art and mystery of a surgeon", whereupon let me say that the lawyers do us great injustice in the spelling of the word, which always implies that we have in our art some mystery or secret thing which other men should not dare to look into; whereas it is not the mystery but the *mestieri*, the *metier*, the mere business and art of the surgeon and apothecary. In my own case-book, I found nothing that in the present day would be worth recording—no more than the cases that might well be taken by any other boy, from sixteen to twenty, at his first sight of a large practice. But I found there some things which made me still fear (falling in very much with the tone of that most admirable and wise address that we heard this morning), lest in our progress we should have let fall some of the things which we had better have held, and are now regarding some things too lightly that then seemed very grave, and, perhaps, are so still. For among the records which I found are records of methods of practice now almost completely disused, and yet in which I feel confident there was much right. I find, for example, a large and almost uniform practice of bleeding for all acute diseases, for many chronic, for most of those that were unknown or uncertain, and for a large proportion of cases in which there seemed to be nothing the matter. Trying to learn something from these things, I came to one or two conclusions which I am sure the younger members of the profession need to have much impressed on them. One of them is that at the present day we undoubtedly overvalue the blood, and estimate too cautiously the loss of it. We heard after the address to-day of the impossibility of performing vivisections upon men. It was not impossible at the time of which I speak; for then there was a practice of vivisection performed on nearly all market days on those who wished to be bled; and the early part of my life was spent in this practice. On market days patients came, and I really scarcely know for what reason, except that it was a good old custom, and the county was then Conservative. They had some discomfort, and wished to be bled, and I bled them usually till they fainted, and the charge for bleeding was one shilling.

Now, looking at that as I can with a very clear memory of its results, and with a more scientific tone of mind than I then possessed, I can regard those as a series of vivisections fairly performed for the determination of what is the influence of the removal of blood up to the point of

syncope upon a comparatively healthy person. I think I can say surely that not one of these persons suffered harm. I kept my cases with much care, and I have a clearer recollection of that period of my professional life than I have of an equal period of any part of it which has since passed, and I verily believe that not one person suffered harm. There were a few who came afterwards with trivial inflammation about the wound, a few with light attacks of what might be called adhesive phlebitis: clots formed in the wound, and obstructed the vein for a time; but beyond that no damage, to the best of my belief, ever was sustained. Amongst these was a large number of pregnant women; for, in the eastern part of the county at that time, it was the general belief that pregnancy might not pass through its whole course without some mischief, unless a patient were bled; and in these cases, too, no harm whatever came. The custom seems like a comedy, but there are in it, and in the records of it, a series of facts of very great importance. They prove to my mind as clearly as anything of the kind can be proved, that the loss of blood up to fainting, and, in some cases I remember, with those epileptiform convulsions that come with the loss of blood, is absolutely harmless. I will venture to say, that there are few persons in this room who might not be bled to fainting, and tomorrow be almost unconscious of it, and, perhaps, in this week of hospitalities, might even be the better. That fact, I take it, may be held by the practice of former times as perfectly assured; and we, therefore, probably take vastly more care than we need of the saving of blood in healthy persons. I am far from suggesting that any good followed this method of proceeding. I speak of it literally, as if it had been simply a course of vivisections to determine the influence of the loss of blood to the point of fainting upon a large series of healthy persons. Among the cases recorded, I also find that all who suffered from acute symptoms, especially from acute pain, with the ordinary signs of fever, were largely bled. That was a custom scarcely without fail. As soon as the hot skin, and a rapid pulse, and a flushed countenance, were found with local pain or any other acute symptoms—as delirium, for example—at once bleeding was resorted to. I will not venture to say how much of real good was done by these cases; but this I will say, that I believe there is at present no remedy employed from which the immediate relief derived is so great or so complete as it was from bleeding. From time to time, I have the record, and many more times I have the clear memory, that after every bleeding there was a distinct expression of the very greatest relief, and commonly following that a firm belief that the bleeding had cured the disease. And this I remember—and I tell it as a guard against any suggestion that I should at once propose the same manner of treatment—that this relief of suffering followed in those who quite certainly were suffering from profound anæmia. Persons who were bloodless, pale, and faint, who yet complained of the continuance of the local symptom, were again and again bled, and again and again experienced temporary relief.

Call it again vivisection, if you will; at least the fact is worth recording, that, for nearly all the most pressing symptoms of acute disease, bleeding was at the time an almost complete remedy. It would be impossible, from those records, or from any others much better than mine of the same time, to determine now by any method of analysis what cases gained real and what gained only passing or apparent relief; but I cannot for a moment believe that there were none among those cases that were not thoroughly and fully relieved by the proceeding. For what have we to believe, if we suppose they were not? Why, that all those whose names we hold in honour as the most sagacious teachers of that time, the men whose acumen we praise, whom we speak of as the keenest observers of their time, were at this point profoundly and guiltily mistaken. Can we suppose that those who were teachers then—to mention only those who were the great leaders of practice in this city at the time—men like Crosse, and Dalrymple, and Wright, and Lubbock, and the rest, were keen in all their observations of the diagnosis of disease, could forecast what would be its course, and judge exactly of its effects, and yet, in respect of that to which they gave the best portions of their lives and minds, the treatment of it, were even stupidly in the wrong, and doing grievous damage to those to whom they were seeking to do good? I will take any hypothesis of pathology rather than believe that. That there were cases greatly benefited I have no doubt, and one of the chief inquiries we have to enter upon even now is which of the cases suffered harm, and which were really benefited by the treatment which we now neglect.

In another line was the almost constant use of mercury. For nearly all the chronic affections, for nearly all cases that were obscure, mercury was given to the point of producing some degree of salivation; and, amongst all the prescriptions that I dispensed most frequently, I suppose there was none nearly so frequent as a quarter of a grain of calomel and a quarter of a grain of opium every four hours. For that there was a traditional respect, which had been derived from a physi-

cian, I suppose, of very great sagacity, who had practised in Yarmouth some time previously—Dr. Girdleston. From all that is recorded of him, he must have been a very exact observer. He had seen much practice in Italy; and, although he was a man of remarkable presence—faith-giving in his mere appearance—I cannot but believe that he did great good among the sick; and he had handed down this prescription, which prevailed from that time many years onward, and throughout the whole of my apprenticeship, for all obscurities, for all those things for which, I will venture to say, with not at all fewer mistakes, some now give bromide of potassium, and some what is called a light tonic. I have no doubt, even from my own cases, that sometimes their practice was mischievous; but I am equally sure that it was in many cases very beneficial. The cases in which it did good I cannot now recognise; but let me observe that it probably did good in a large number of the cases of which the real nature was at that time not discerned, and in a large proportion of the chronic diseases of internal organs which we now assign to syphilis. At the time, there was no suspicion that syphilis affected any but the external parts: we know now a multitude of syphilitic affections of the liver, of the lung, of the spleen, and many more still of the nervous system, with syphilitic affections of the brain, which were at that time put down vaguely to chronic inflammation of unknown origin, or to tumours, thickenings and productions of substances that needed to be absorbed; therefore mercury was given, and therefore mercury cured a considerable proportion of cases for which it was given; and therefore, as in the present time, having this power of curing a certain percentage, we assume that it will cure all diseases that look like those that are cured by it. This leads me to suggest (and I know not where I can suggest it better than to a meeting such as this, which will soon spread itself over the land, and be actively studying everywhere) that, whereas we are at the present time rather apt to think that pathology should be the guide of therapeutics, there is a large number of cases in which therapeutics should rather be the guide of pathology. The fact that a medicine cures a given disease is as much a fact, and quite as significant a one, as is the employment of a chemical test for discerning the nature of a solution. It can be repeated from time to time, and with the same results. There is hardly anything in the chemistry of complex bodies more sure than that quinine cures ague, and, spreading from that, that it cures a large number of periodic diseases. And, as with quinine, so with mercury. If at that time the value of therapeutic tests for indicating disease had been fairly estimated, we should have come, by many years sooner than we did, to a knowledge of the syphilitic nature of a large number of internal chronic diseases. If it had been fairly said, “we have no clear knowledge of mercury as a remedy other than this, that it cures syphilis, and, therefore, that what it cures is likely to be syphilitic”, we should have come at once to the knowledge of that which it took twenty years more to ascertain. We are, I believe, too much under the guidance of what may be justly called inferential therapeutics; because we know something of pathology, we may, therefore, proceed at once from pathology to the recollections of the remedies of disease. It is a fair method of study if it be not carried to excess, but it should be studied side by side with the other facts that therapeutics may just as fairly be a guide to pathological knowledge. But, after all, my knowledge of Norfolk and Norwich surgery are more with persons than they are with things, and I am bound by all recollections of gratitude to record some of them here.

One whose name was perhaps never known beyond his own town, where perhaps now it has been forgotten, was my first and good master, Mr. Charles Costerton, a man of admirable sagacity in his profession, a good anatomist, a good practitioner, and, above all, one who set a good example of continual study, and helped me in all the work I was disposed, and some of that which I was indisposed, to undertake. Then, as guiding him, those that were the chief guides of the practice of surgery in Norwich, and amongst them all the leader, Mr. Crosse, of whom it is very hard to speak in measured language, for if I were to speak of one who, better than all I knew at that time, and equal to any I have known since, had the qualities both for being and producing good surgeons, it was he. A profound student of his profession in all its branches, a seeker of knowledge wherever it might be found, in any language within his reach, a recorder of his facts to the last degree of minuteness, and one so in earnest that he spread about him a contagion of earnestness which it was indeed most hard to resist; and in a yet more personal sense the producer of great surgeons in that one of my own pupils, the Vice-President, is his son and most worthy follower. With him as a colleague was one very unlike him, but who might teach other qualities not less valuable in surgery—Mr. Dalrymple. Our President, who, as you perhaps know, has been the author of admirable memoirs of his contemporaries and predecessors in the Norwich Hospital, has recorded of him that which we should be all so anxious to imitate,

the almost distressing sense of responsibility which he continually felt; the way in which he had almost to be driven to that which he knew to be right, lest, by some accident or oversight, he should do that which was wrong; the constant dwelling upon all suspicions that he had not fulfilled his whole professional duty—a tone of mind not wise perhaps to be the master process, but yet most wise to keep, lest others should take the lead. And with him there was another, the very type of that which a general practitioner should be—Mr. Johnson, who was at that time assistant-surgeon to the hospital—busy, active, and conscientious, and learning, as our admirable orator of to-day said it was most necessary to learn, the inner life of every family to which he went, the inner feelings of every patient he had under his care, till he passed, in nearly every house, from being not the mere surgeon or physician, but the wise, judicious counsellor, the other parent of the young, the intimate friend of all with whom he mingled. I remember others; for no man is in himself, I suppose, fit to be the whole and single study of any others, and our wisdom is rather to study that which is best in each, and make from all together the ideal which we would most imitate. There were men here with that happy audacity in surgery which enters sometimes as an element very necessary for the active life we have to lead. Of Dr. Lubbock, as a physician, I had no power of judging; but as a surgeon, which he was, I believe, much more than a physician, he was profoundly wise in his audacity, taking a lead where others might be wiser than himself, but often doing that which others dared not do, and doing it successfully.

There was another, too, from whom that at least might be learned, and of whom I must speak in that view with respect—William Webber of Yarmouth—a man not wise or right in some other things, but in that giving an example from which all might study. These are some of my best memories of Norwich and of Norwich surgeons, and they have been maintained in succeeding generations by my finding, amongst my oldest friends, those who have held the lead in surgery since, and, in a generation yet nearer than theirs, amongst my own pupils, those who still hold it. So that visitors of the British Medical Association when they come here, come to no common place for the discussion of surgery. Here it has been studied very gravely and very earnestly; here let it be so discussed, and not in a professional spirit only, but in a really scientific one. I owe to Norfolk, and chiefly to Norwich, much of that which led me in early life to go somewhat beyond the ordinary bounds of study into the range of science. It should never be forgotten that in Norwich was trained Sir James Edward Smith, the founder of the Linnæan Society, the leader of the botanical knowledge of his time. After him followed Sir William Hooker, best of all botanists of his time, the founder and outworker of the greatest botanical institution in the world, at Kew Gardens, whom it has been the great happiness of my life to have had in intimate friendship from my earliest years; who does Norwich the honour, let me say, of sending his own son to study surgery here. Then let me name Mr. Dawson Turner, a botanist, an artist, an antiquarian, one with whom any one might study and be helped to study all that might help him both to the tone and temper and the true knowledge of his profession. From all these it would be wrong if anyone had gone without, I will not say succeeding in his profession, but without a thorough knowledge of the manner in which he might gain success, and an honest ambition for the success that they achieved. And, if I put them all in my memory together, I should say that from them I learned that principle which I had tried, however vainly, to follow out in all my study, the principle upon which it will be well if we begin our present surgical meeting, that he that will seek the truth earnestly, with patience and humility, in the fear of God and the love of man, will surely find it.

LARVÆ OF FLY IN THE EAR.

I HAVE just met with a case in my practice which, so far as I am aware, is of rare occurrence.

J. R., a farm-bailiff, came to me on July 27th, suffering from intense pain on the right side of the head, which, he stated, had come on suddenly the previous afternoon. The external ear was red and irritated, and blood was oozing from the external meatus. Upon examining with the speculum, I found the meatus occupied by a moving mass of larvæ, which I carefully removed by forceps. There were twelve of these in all; and the largest was two-fifths of an inch in length, having all the characters of the larva of the common blue-bottle fly. After removing them all and syringing the ear, I found that, besides destroying the surface of the external meatus, they had penetrated the membrana tympani. The pain abated soon after the removal of the larvæ, but the patient still remains very deaf. He can give no account of how or where the parent fly deposited the eggs in his ear.

C. MOSS CAMPBELL, M.D., Staunton.