AN ADDRESS

DELIVERED BEFORE

THE CLINICAL SOCIETY OF LONDON.

BY

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GENTLEMEN,—On taking the President's chair for the first time, I beg to thank you most heartily for the great honour you have conferred upon me—an honour which is much enhanced, in my estimation, by the circumstance that it is bestowed by my professional brethren; and that it is held in connection with a Society founded for the special purpose of advancing that branch of medical science upon the study of which my life-long thoughts and energies have been centred. When, indeed, I was nominated by the Council for the office of President, I felt much diffidence as to the propriety of accepting their flattering proposal, for I thought it probable that some other member might, perhaps, occupy the chair more advantageously for the objects of the Society. Looking at the ability of your former Presidents, and the admirable manner in which each of them has performed his duty, I feel that I have a difficult task before me. But I shall use my best efforts to promote the objects and prosperity of the Society; and I trust that I may be able, at the close of my period of office, to transmit it to my successor with prestige undiminished and dignity unimpaired. In this effort I well know that I shall be ably and cheerfully assisted by the executive officers of the Society, and I also feel assured that I may count upon your individual and collective support.

It has been customary for the President to address some preliminary words to the Society at the first meeting after his election. I feel that on the subject of clinical study I can add nothing to the wise and thoughtful words you have already heard from my predecessors, neither can I pretend to the eloquence which some of them displayed. Therefore, during the short time for which I shall occupy your attention to-day, I propose to take a humbler, though I trust not an altogether unprofitable, theme.

The Clinical Society has now entered on its twelfth session, and it appears to me a suitable occasion for inquiring into the nature and character of the work which it has done, and of ascertaining how far it has fulfilled the intentions with which it was founded. From such a retrospect of its past labours we may perhaps form some forecast of the direction which its future efforts should take. That the Society has, in some senses, been eminently prosperous, you are well aware. The report of the Council, read at the annual meeting a fortnight ago, was in this respect quite satisfactory; it showed how largely our numbers have grown; while the increasing interest manifested in our meetings indicates that the work done is generally approved. It is not, therefore, from any doubt or suspicion of failure that I propose this inquiry, but rather in the hope that we shall thereby be encouraged to make still greater efforts in future.

The Society was founded professedly for the cultivation and promotion of practical medicine and surgery by the collection of cases of interest, especially of such as bear upon unsettled questions in pathology and therapeutics. The field of its prospective labours was, therefore, a very wide one—for I suppose the time will never arrive when there will cease to remain such question as yet unresolved—but it could not well have been more restricted; for a field that did not comprise the whole extent of clinical research would have scarcely justified the formation of such a society. Our rules lay down more definitely the mode in which the main purpose of the Society is to be carried out. Communications of two classes are to be received. Those of the first class are to consist of written reports of cases already complete: written, in order that they may be accurate and definite; drawn up after a certain plan, in order that they may comprise every important particular. Communications of the second class are to consist of cases still under observation; and this class of papers was mainly designed to afford an opportunity for appointing a small committee of members to assist in drawing up a full record of the case. It is further provided that every communication shall conclude with explanatory remarks by the author, he being presumably the person most competent to afford an explanation of the nature of the case he has observed, and of its bearings either upon pathology or treatment; and to draw from it whatever lessons it is capable of afford-

ing. The history of the case is thus kept as much as possible separate from the opinions and conclusions deduced from its study. The facts accurately related would thus, it might be hoped, constitute a record valuable for future reference, even though the inferences drawn from them at the time of their occurrence should subsequently be set aside. It is obvious from these regulations that the founders of the Society had not only a definite idea of what it was to aim at doing, but also took considerable pains to secure the accuracy and completeness of the work to be performed.

For more efficiently carrying into effect the object of the Society, it was resolved, at an early meeting of the Council, that the President shall have authority to nominate, from time to time, committees consisting of not fewer than three members of the Society, for the purpose of carrying out the investigation of clinical and especially therapeutical questions.

Now let us endeavour to ascertain how far these plans have been successful.

With regard to the first class of communications, it has occurred, as might have been expected, that they have formed by far the greater proportion of the papers read at the meetings; and I venture to say that it is impossible to read the volumes of our *Transactions* without observing that they have been for the most part extremely interesting, and mainly in accordance with the scheme of operations laid down in the laws of the Society. This has been largely due to the energy, zeal, and good judgment of the several gentlemen who have successively held the office of Secretary; and, whilst referring to the services of these gentlemen as a body, I cannot help specially naming our first Medical Secretary, Dr. Burdon Sanderson, without whose active co-operation the Society never would have existed, and to whose logical and strictly scientific cast of mind we mainly owe whatever is best in the original scheme of the Society's operations. Having latterly devoted himself almost exclusively to a different kind of scientific work—in which, I need scarcely remind you, he has gained a world-wide reputation—we have unhappily lost him as an active fellow-worker among us; but I know that we still have his cordial sympathy in our labours, and feel sure that we may count upon his assistance on any suitable occasion.

It would manifestly be impossible to mention the many communications of this class in detail; but I may, perhaps, without impropriety, refer to a few of them as illustrations of the work that has been done. I must, however, premise that I shall select them without any view to their comparative excellence, but solely because they relate to cases which possess some special pathological or practical interest.

At our second meeting, held on January 24th, 1868, important communications were presented by Drs. Hermann Weber, Murchison, and Burdon Sanderson, upon sudden death in rheumatic fever with excessive temperature. These cases were read here some months prior to the publication of our associate Dr. Wilson Fox's very interesting monograph on the same subject; and, with the exception of three cases published in the *Medical Times and Gazette* by Dr. Ringer in 1867, they were, I believe, the first brought before the profession in this country. Several other cases of high temperature, either in rheumatic fever or other diseases, have been subsequently communicated to the Society by Drs. Hermann Weber, Southey, and C. T. Williams, and gave rise to very interesting discussions upon the use of cold baths in the treatment of hyperpyrexia.

At the first meeting in our second session, Mr. Hulke read the history of a case of ichthyosis of the tongue, upon which epithelioma supervened. I believe I am correct in stating that this was the first instance of the kind ever recorded, though our *Transactions* contain similar cases, reported at later dates, by Sir James Paget and Mr. Brown.

The volume for 1868-69 also contains a valuable paper by Dr. Peacock, giving an analysis of one hundred and forty-six cases of acute and subacute rheumatism, made chiefly in reference to the proportion of cardiac complications. Coming from so distinguished an authority on diseases of the heart, and founded on such an extensive and prolonged experience, this paper cannot fail to be of permanent value.

Among our more important and interesting communications, in an etiological and pathological point of view, was a case recorded by Mr. Langton in 1877. It was one of cutaneous eruption contracted by a groom from attending to a horse with cracked heels; and it singularly corroborates the facts adduced by Dr. Jenner, showing that the affection in the heels of horses called "grease" is capable of exciting, in the human subject, a disease which exactly resembles cow-pox, and which, like it, affords protection from the contagion of small-pox. Neither must I omit a passing reference to Sir William Gull's very striking cases of anorexia nervosa that appear in our seventh volume of *Transactions*.

Diseases of the skin and of the nervous system have, of late years, attracted a large share of the attention of some of our ablest and most

active workers. Upon the latter subject, especially, we have had many admirable and most valuable communications, far too numerous to allow of detailed mention in the brief time at my disposal; and where all have been excellent, it would be manifestly improper to make any selection.

We have had many very interesting surgical papers upon which I do not feel myself altogether competent to offer an opinion; but I may refer to those on aneurism in various situations as appearing to me peculiarly valuable. Whilst upon this subject, papers in the fifth and seventh volumes of our *Transactions* seem to deserve special mention. In the first of these papers, Dr. Cockle relates a case of aneurism of the thoracic aorta, in which Mr. Heath tied the left carotid artery, in the hope of producing consolidation of the contents of the aneurismal sac and bringing about what Mr. Holmes happily calls "a practical cure". In the second of these communications, Mr. Holmes reports a similar case, in which he also tied the left carotid artery for aneurism of the thoracic aorta. Both patients were exhibited here, and in both of them the result of the operation was "indubitable and very marked relief". Mr. Heath's patient lived for several years, and appears to have died in consequence of resuming hard agricultural labour, which brought on a recurrence of his disease. An instance reported in our seventh volume by myself, of spontaneous cure of thoracic aneurism, where some of the distal arteries were obliterated, seems to lend great support to the propriety of this operation.

Few important novelties in surgery have failed to be brought forward at our meetings. Mr. Pollock communicated to us in 1870 a series of fourteen cases in which he had practised skin-grafting, or transplantation, in patients suffering from indolent ulcers; and, on the same occasion, Mr. Lawson read the history of two similar cases. These cases were treated in St. George's and the Middlesex Hospitals soon after M. Reverdin originated this plan of treatment, and were the first of the kind recorded in this country. Papers on Esmarch's method of preventing loss of blood during surgical operations were brought before the Society by Mr. Henry Arnott in 1873, and subsequently by Professor Esmarch himself in the following year.

The modes of preventing hæmorrhage by acupressure, torsion, and various kinds of ligatures, were also fully discussed at several of our meetings, the subjects having been introduced by the late Mr. C. H. Moore and by Messrs. Cooper Forster, Callender, Gant, and Bryant. The employment of Sayre's jackets in the treatment of spinal disease was recently advocated by Mr. Berkeley Hill; and the management of wounds on plans calculated to prevent the occurrence of erysipelas and other diseases arising from uncleanness was brought before us at a very early date by our late valued coadjutor and Vice-President Mr. Campbell De Morgan, by Mr. Durham, and more recently by Mr. Callender, showing that our London hospital surgeons are as keenly alive as any others to the necessity of guarding against, and as successful in averting, such untoward sequences to surgical operations. Whilst upon this subject, I must not omit to mention that one of my predecessors in this chair, Mr. Prescott Hewett, showed conclusively, in one of his addresses, that pyæmia, one of the diseases said to be caused for the most part "by hospital air and the aggregation of surgical cases in the wards of our large hospitals", occurs also sometimes in patients placed in perfect isolation, in good houses, well ventilated bedrooms, and, in fact, to all appearance under the most favourable conditions. This would seem to indicate the existence of some state of the patient predisposing him for the disease, or possibly some peculiarity of season favouring its development.

As regards therapeutics, which was one of the departments to the advancement of which it was hoped the Society would conduce, I may refer to cases recorded in our *Transactions* by Mr. De Morgan and Mr. Hulke, on the treatment of cancer by condurango, showing the uselessness of that once much vaunted remedy; also to communications upon the value of nitrite of amyl by inhalation in angina pectoris by the late Dr. Anstie and Dr. Brunton. I need scarcely say that these latter communications have borne good fruit, as many of us have doubtless found in the treatment of similar cases.

One feature of our *Transactions* must not be overlooked; it is that on many subjects we have had a succession of communications. I have already referred to those upon aneurism. We have likewise had several instructive papers on the treatment of ascites, communicated by the late Dr. Hillier and by Drs. Bristowe, Henry Thompson, Robert Liveing, and Day; and others on the employment of tapping in pleuritic effusion, by Dr. Berkeley Hill and by Drs. Douglas Powell, John Murray, Cayley, and Broadbent.

If the work done by the Society, in the form of papers upon cases already complete, has been ample and satisfactory, that done by committees has been, I think, less than might have been expected. In looking through the volumes of *Transactions*, I find only eighteen

reports made by committees upon individual cases. No less than seven of these were made during the first brief session of 1868. Of the remainder, seven only were founded on cases brought forward with the view of being submitted to the consideration of a committee; the other four having been upon cases referred to a committee for examination, on account of some doubt being expressed by the Society as to the deductions drawn by the authors from the facts they related. The perusal of these reports well shows the advantage of submitting suitable cases to the investigation of committees, consisting of members selected for their special acquaintance with the subject under consideration. Seeing how largely the *Transactions of the Pathological Society* have been enriched, and doubtful questions of morbid anatomy elucidated, by the reports of the Morbid Growths and other committees of that society, I cannot help feeling that we have not in this respect carried out our programme so fully as might have been desired.

One of these reports on vaccine-syphilis touches on a subject so important as to merit special notice. It was founded on a case which, if not, as I believe, the earliest authentic case of the kind, was at least one of the earliest recorded in this country, and preceded by several months the more extended and complete groups of cases communicated by Mr. Jonathan Hutchinson to the Royal Medico-Chirurgical Society. Such instances are, in fact, so exceedingly rare that, whereas successful and healthy vaccinations can be counted by millions, these mischances—if I might employ such a term for unhappy results which can be entirely avoided with due care—can only be reckoned by units. The facts contained in these papers indubitably prove that carefully conducted vaccination cannot convey syphilis, even when the vaccifier happens to be the subject of that disease. In Mr. Smith's case, only one of the four punctures was infected with syphilis; the other three having escaped, although they presented the proper appearances of vaccination. Mr. Hutchinson's wider experience leads him to the conclusion (which is indeed but a necessary deduction from the facts he has so clearly recorded) that the vaccine virus and the syphilitic virus were present in totally different fluids; and that the one might be conveyed in all its vigour without any admixture of the other. He shows that it is "quite possible for vaccine-lymph and blood to be transferred at the same time, and for each to produce its specific results"; but, on the other hand, that, by suitable precautions, it is no less "possible to vaccinate successfully from a syphilitic infant, in the utmost stage of potency as regards its blood, without communicating syphilis".

Two most important lessons are deducible from the history of these cases: the one, that the greatest care should be exercised in the selection of children from whom vaccine-lymph is drawn; the other, that it should be the rule in every case to use only the transparent lymph drawn from pustules at the proper stage of the disease; and that, whenever blood is accidentally drawn by the puncture, the lymph from that vesicle should be rejected. I repeat, due attention being paid to these precautions, and also to the health of the child proposed to be vaccinated, "small-pox", in the words of Mr. Simon, "need cause no further fear, nor its antidote be accepted with distrust".

I hope that I may be pardoned this digression, springing out of Mr. Smith's paper, on account of the great importance of the subject. Of the many blessings conferred on mankind by the art of medicine, none probably has been so widely beneficial as the discovery of vaccination. It is not too much to say that it has been the means of saving hundreds of thousands of lives which would otherwise have fallen victims to small-pox, and prevented millions of persons from lifelong disfigurement. Those among us who can carry our recollection back far enough cannot fail to be struck by the inconsiderable number of persons now met with who present the seared visages so common a consequence of small-pox among the members of the older generation forty years ago.

The discussions consequent on the reading of communications have formed a very useful part of our proceedings. They have been most interesting, and have rarely failed to develop and add to the value of the subject under consideration. Indeed, they have in many instances possessed a very high degree of merit. It would be impossible to dwell at length upon this part of our work; but I may be permitted, amongst so many that have been good, to refer especially to one on so-called fibroid phthisis, held upon a paper read by Dr. Andrew Clark at one of our early meetings. To the discussion already referred to, on the employment of cold baths in the treatment of hyperpyrexia; to that on pyæmia, held after one of Mr. Prescott Hewett's addresses; to that on the use of salicin and its congeners in the treatment of rheumatic fever; and also to those on several important surgical questions; and especially to recent discussions on the subjects of aneurism, rodent ulcer, and gastrotomy, as having been, in my opinion, both excellent and most valuable. The interest taken in some of the discussions has been so great, that the propriety of printing them, either in full or in abstract, in the

Transactions has been more than once suggested to the Council, who have, however, uniformly, and I conceive judiciously, negatived the proposal.

Only four committees have been appointed by the President, under the regulation passed by the Council at one of its earliest meetings. Two of these have reported; two are still sitting, but we hope to receive their reports during the present session. One of the latter is inquiring into the "incubation period of scarlatina, diphtheria, typhoid fever, erysipelas, and allied diseases"; the other into the "deleterious effects which follow the prolonged and continuous use of chloral in ordinary doses". From the former of these committees, in particular, we may hope to receive an important and valuable report. We already know, with considerable accuracy, the period of incubation that elapses between exposure to the contagion and the evolution of the symptoms in small-pox and measles. It is probable that the period is less definite in some of the analogous diseases which form the subjects of inquiry; and it is also probable that the irregularity may vary among them, and be greatest in such diseases as typhoid fever and cholera, in which local insanitary conditions are mainly instrumental in spreading the disease. The state of the patient at the time of exposure will, perhaps, also be found an important element in determining the duration of the latent stage.

Whilst on this subject, I may observe that we already have, in the last volume of our *Transactions*, a most instructive paper on the period of incubation in scarlet fever and some other diseases, by Dr. Murchison; from which it appears certain that this period varies within considerable limits in some of these diseases. If this should be confirmed by the wider inquiries of our committee, it will become very important to inquire into the causes of such variations. Sir William Jenner is chairman of both these committees—a sufficient guarantee that the greatest care and widest experience will be brought to the examination of the facts and opinions upon which their conclusions will be founded.

Of the committees which have already reported, the first was appointed to "investigate the temperature in syphilis". It was composed of Dr. Bäumlér, Dr. Duffin, and Mr. Berkeley Hill, and their report was read at a meeting of the Society held May 27th, 1870. Although that report was only considered by its authors as a preliminary one, they were able to arrive at certain conclusions in reference to syphilitic periostitis and affections about the joints. I think it is much to be regretted that the investigation, thus commenced, was not extended, either by means of the same or of some other committee, to an inquiry into the characters and peculiarities of the febrile stage accompanying the onset of constitutional syphilis, more particularly the cutaneous eruptions.

The other committee to which I referred was appointed "to investigate the value of quinine as a means of diminishing bodily temperature and pulse in pyrexia". It comprised Dr. Murchison, Dr. Symes Thompson, and Dr. Hermann Weber, and their report was read on May 27th, 1870, and is printed in the same volume of our *Transactions* as that on temperature in syphilis. This report seems to me a perfect model of what such reports should be. On commencing operations, the committee laid down a uniform plan for conducting the investigation, and entered the results of each experiment in a tabular form, prepared beforehand for that purpose. Discarding a large number of preliminary observations, from which no conclusions could be drawn, their report is founded upon fifty-two cases of various kinds of acute disease; and their conclusions are unquestionably of great value. This committee, again, regarded their investigation as quite a preliminary one, and suggested a further examination of the subject—a suggestion which I should rejoice to see still carried out.

And now, gentlemen, in conclusion, what encouragement can we derive, or what suggestions may we gather, from this very imperfect sketch of our past labours, to guide our future course? The Society has done good and valuable work. It has given facilities for bringing prominently forward many questions that might otherwise have failed to obtain such wide diffusion; it has been most useful in affording an opportunity for the interchange of opinion and the free discussion of questions of practical interest; and we must all, I think, feel that these discussions have led us to listen with respect, and to consider with care, the opinions of others, even though we ourselves may have differed from them. For myself, I can honestly say that I have rarely heard a paper read, or listened to a debate, without deriving from it some valuable information or some useful practical hint.

But, although we have thus good reason to be satisfied with the past, we cannot regard our work as having been altogether perfect,—there is still room for improvement; and it would be well for each of us, when drawing up cases to be presented to the Society, to remember the regulations laid down in our laws, and to cast them strictly in the shape therein provided. As regards communications upon cases still under

observation, probably my reference to the subject will serve to remind us that, whenever it seems desirable, the Society will be ready to associate a committee to assist in investigating an unusual case, or to verify uncommon facts. It appears to me, however, that our chief development might advantageously take the direction of making original investigations by means of committees. We have already done so to a limited extent; but such work might with great advantage be increased. Subjects of scientific importance, or of great practical interest, could, undoubtedly, be satisfactorily inquired into by us. Any report emanating from a select committee of this Society would be received with respect, and would, unquestionably, carry great weight with the profession at large.

I may mention, as an illustration, one such subject that occurs to me, which it might have been advisable to refer to such a committee for investigation some time since, when we had a most animated debate upon the use of salicin and its congeners in rheumatic fever. A committee appointed at that time for the purpose of investigating the effects of salicin and its compounds, might have aided in placing it in its proper position as a remedy for acute rheumatism. Having, by your favour, held an important position on the Council since the formation of the Society, I feel myself largely to blame for having allowed so good an opportunity of usefulness to pass by unimproved. We have all, probably, now formed our own opinion as to the value of salicin from personal experience, for it has, I believe, been largely tried in all the London hospitals; and it is, therefore, perhaps no longer necessary or desirable to institute an investigation, which might have been useful and appropriate at an earlier period; but similar opportunities are certain to arise, and I trust that we shall use them, and so assist the advancement of our art.

In the early days of the Society, when its position was less assured than at the present, and its pecuniary resources were small, it was necessary to husband our funds, so as to provide for carrying on the operations of the Society for a year or two, even though we should on some occasion expend more than the income of the year. This state of things no longer exists. We have accumulated a small capital, and for several years, though our expenditure has been liberal as regards the cost of the annual volume of *Transactions*, our income has exceeded our requirements. It might now, therefore, be a proper question for the consideration of the Council, whether committees, consisting of not less than three persons, selected from among some of our able and energetic young members, might not be appointed, from time to time, to undertake the practical investigation of important subjects bearing especially upon therapeutics or etiology; receiving from the Society a moderate honorarium for their services. I merely venture to offer this suggestion for consideration, "without prejudice", as the lawyers express it.

If the Society continue as it has begun, and especially if it aim at doing higher and more complete work, its volumes of *Transactions* will in the future, even more than in the past, constitute a rich storehouse of information; gathered together, I trust, for the legitimate objects of the art of medicine—namely, "the glory of God and the relief of man's estate".

ON SYRINGING THE EARS.

I AM much obliged to Mr. Lennox Browne (BRITISH MEDICAL JOURNAL, January 11th, 1879) for calling my attention to two omissions made in my paper on the removal of foreign bodies from the ear.

1. Mr. Lennox Browne seems to think that, from the form of the nozzle, it was intended to block up the meatus; whereas, the syringe should be held at a little distance from the meatus, and not touch any part of the ear. Forty years ago, the ear-syringe found in the shops had long-pointed nozzles, which, introduced into the auditory canal, were liable, in awkward hands, to do mischief. Hence the nozzle of the modern syringe was made short and conical, to avoid this danger.

2. I took it for granted that no one would for a moment think of injecting the ear with either hot or cold water. Hence I did not insist, as I should have done, on the water being not below 98 deg. Fahr.

My friend Dr. St. John Roosa informed me, some time ago, that I was the first to formulate rules for syringing the ear to remove foreign bodies from it; and Mr. Lennox Browne says that these rules are now to be found in classical aural treatises. Notwithstanding this, the discussion in the BRITISH MEDICAL JOURNAL, last year, proved that the teachings of Toynbee and Hinton, and of St. John Roosa and others, had been for the time forgotten; and my examination of the ear-syringes found in the shops of London and Paris proved that the instrument-makers had lost the art of making such syringes as these authors recommended. These facts constitute my apology, if any be needed, for stepping aside from my own beaten track to call the attention of my brethren to this subject.

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