AN ADDRESS ON NEUROLOGY AND THERAPEUTICS.

Delivered before the Medical Society of London.

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MR. PRESIDENT AND GENTLEMEN,—I count it no small honour to occupy the position in which I am placed to-night, not only as the first recipient of a distinction which must ever be one most highly esteemed by the profession, but as having the duty placed upon me of inaugurating the new arrangement for the Fothergillian Prize. The duty, indeed, is one which I feel carries peculiar responsibilities, because it involves a precedent of no small importance, and lays before me the task of setting an example of no light responsibility. I wish that I could feel satisfied that the duty would be fulfilled and the example set in a manner worthy of the occasion. This satisfaction I cannot have, but I must rest content with the endeavour to do my best, and with the consciousness that I may safely reckon on your indulgence.

These are, however, not the only difficulties which present themselves to my mind as encircling the duty, pleasurable though it be, which lies before me. The founder of this Society, in whose honour the prize was established, lived and worked in the days when every physician was, before all else, a practitioner of his art, and when the birth throes of abstract medical science had scarcely begun. The practical aspect of the work of the physician has not yet vanished, and indeed it never can vanish, except in the case of those pure scientists to whom the name ceases to be applicable; but it has changed in relative predominance, and it has changed especially as regards the different periods of the physician's working life. Here, as everywhere, "the old order changeth, yielding place to new." The physician was then a practitioner from the very first—from the earliest days of his career—but now this is true only of some. The change has been inevitable. Science is an exacting mistress; medical science is not the least exacting, and with advancing years she has renewed her youth and multiplied her demands on the devotion of her servants. If many physicians did not devote their early years to her abstract worship, our knowledge could not have reached the point it has attained; the prospect of future advance could not be so bright as it is, whatever the actual degree of that brightness may be, did they not still do so.

But the different character of the work of physicians in the early and middle period of their active efforts to further the progress of medical science, is reflected in the character of the various societies which exist, and of which they are members. The founder of the Medical Society of London, whose memory this prize will always preserve, possessed, to a larger degree than most others of the first rank, the practical characteristics which dominated the work of the physicians of his day. The Society has always kept this aspect of medicine in constant view, and has thus been of peculiar service to all practitioners of medicine—a service on which it may justly pride itself. Hence it seems to me to be incumbent on those who venture to address it, and especially on those who may occupy the honourable post I have the pleasure of occupying to-night, to keep in view this aspect of its work, confirmed by the practice of so many generations, and possessing, as it certainly does possess, a high degree of peculiar utility.

But with it there is associated a duty which is not, I think, always recognised or always fulfilled, and which is yet of extreme importance. In the present day the abstract cannot be ignored; it asserts itself at every turn in scientific work; and with it another fact is asserted—is whispered, if you like—but in tones too clear not to be heard, and too distinct to be mistaken—the fact that there is no abstract work which does not at some time or in some way find a practical application. True in each department of science, it is con-

spicuously and almost invariably true in medicine. This application is moreover more frequently immediate, less frequently deferred, than in the case of most science; and in this fact there is involved another duty, which I would ask you to permit me to emphasise. The practical application of the abstract investigations of medical science can be indicated by none so well as by one whose researches have supplied the abstract results, and who can, if he is inclined to do so, at least indicate the practical application of his investigations in a way and with an efficiency that is possible to no one who comes after him, and takes up the thread of his labours in order to weave it into the texture of the practitioner's daily labour.

But this requires deliberate effort. It requires, also, diversion of effort from the more attractive and perhaps entrancing continuation of the work which lies close at hand. The result is not always visible, but it is not then the less certain, it is not the less important; and my reason for thus dwelling on what may seem somewhat irrelevant is to urge on the abstract worker the extreme importance of endeavouring, at each stage in his labours, to perceive what applications his results may have, and to point them out for the benefit of those who devote themselves to the practical application of the abstract, and who might not, and probably would not, perceive them without the aid thus rendered. There are, indeed, instances in which the abstract and the practical are united in one person. Instances will occur to you without my aid, and a conspicuous example is before you in the person of your President.

I must ask you to allow me to preface my special remarks regarding the treatment of disease of the nervous system with some remarks of a more general character on the pathology of those maladies, or rather on the change which has come with changing times on our conceptions of their nature. In doing this we cannot separate physiology and pathology. The two subjects will not be divorced, and this is true now to a degree and extent to which it was never true before. The more we learn of each the more intimate, the more essential, the more radical we must perceive to be their union. Disease is health gone astray, pathology is but perverted physiology, and we can only obtain an adequate idea of morbid processes by acquiring first a knowledge of those of health. This fact is of very great importance in regard to therapeutical procedures. We shall see this in a moment.

Of necessity our view must be restricted to those points which are related to the topic that is before us. We cannot range at large over the whole wide subject, attractive and even entrancing though it may be and is; but the special matters from which proceed the diseases we have to treat we cannot ignore, and we must consider the changes that have come over our conception of the normal processes, and which have entailed corresponding changes in our conceptions of the related processes of disease, before we can, with any prospect of success, attempt to ask ourselves even elementary questions regarding their treatment. Hence you may perceive that the method I have to ask you to pursue may seem circuitous, and the path we have to take may seem to be indirect. The method may not prove the less effectual for all that, nor the path less sure.

Without doubt the present generation has witnessed a vast change in our conceptions of the structure of the nervous system and its functions, and of the disorder of these. From these has proceeded a necessary change in our practical thoughts, and in the measures we adopt in endeavouring to alter that which is wrong, and in our efforts to substitute order for the disorder we can observe. If we try to perceive the nature of the change in our conceptions, and try to epitomise it in a brief expression, I think we should say that its essential nature has been the substitution (forgive me if I seem to use an expression of perplexing ambiguity and obscure brevity, it will clear itself in a moment) of the analytical for the synthetical method of thought, of analysis for synthesis, in our ideas alike of health and disease. Expanded into somewhat simpler terms, it may be said that the change has involved a perception of the many where before we saw the much. The single function, complex or complicated, we now perceive to have had only a spurious simplicity, and to consist of a combination of distinct elements, working together or working apart, but not to be confounded, except

with the certain result of being confused. Here, as elsewhere, we must, as a great thinker once said, distinguish where we cannot divide; and I may add we must divide where we cannot separate.

You may observe that I have been only saying that which is familiar, but saying it in recondite phrase—a process that is apt to be annoying, but yet is not without its advantage. It tends to give the familiar a fresher aspect, and to fix its features on the mind in a more effective manner. Grasp the change of conception which has taken place in the terms in which I have described it; realise that it has generally consisted in a substitution of the particular for the general. The gain has been immense, because the elements were seldom all united, and their partial effect produced too often insoluble difficulties when the constituents had not been discerned and when we could only see an imperfect whole instead of perfect parts.

Consider the functions of the cortex of the brain as an illustration of this truth. You can do so without my aid. I need not dwell upon its features, I need not point out the details of the illustration it affords, because these are well known to every one of you, and I have already, perhaps, spent too much time and too many words over that which is superfluous to discuss, and I shall have to consider another illustration on account of its special significance.

The point to which I would bring your thoughts—that for the sake of which I have dwelt on the nature of the change which has taken place in our ideas—is the probability, or rather the certainty, that this process of analysis is far from finished. Absolutely complete we must not expect it ever to become; but we cannot even conjecture at present how far off may be the point which we may reasonably regard as that of relative completion; we cannot yet guess how far we may be from the point at which we shall reach the ultimate of our practicable analysis, the point beyond which we cannot go with the confidence that science demands and a scientific habit of thought must have.

(To be continued.)

DISCUSSION ON THE DIAGNOSIS AND TREATMENT OF ACUTE CROUPOUS PNEUMONIA.¹

INTRODUCED BY

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DR. GOODHART, after referring to the interest and importance of the subject, and having observed that there were really no differences between the general symptoms in childhood and those of adult life, raised the question whether pneumonia in childhood was often ushered in by either vomiting or convulsions. He had no doubt, considering the high authority upon which the assertion was made, that it was correct, but he could not say that he had seen it so sufficiently often to say that it was a very common occurrence. He would lay more stress upon other cerebral symptoms such as a rather noisy delirium and high fever, the cerebral pneumonia of Rilliet and Barthez, a condition which he had thought more often associated with pneumonia at the apex, but which was not now generally thought to be so, though it was certainly a more common symptom in children than in adults. The term "apical" ought to be enlarged to "upper half," for it was not proved that the involvement of the actual apex was so very common, or that the upper lobe suffered more than the apex of the lower lobe. There could be no question that with this qualification it was very common in children. In 120 cases there were two apical to every five at the base. Henoch gave one to two, but Sanné, on the other hand, made an actual preponderance of apical cases. Adopting, however, even the figures that were least favourable to it, it would seem that pneumonia at the apex was more common in children than in adults. This might be due to the fact that the disease began

more often at the root of the lung and implicated the bronchial glands more readily, or rather that the bronchial glands were so sensitive in childhood to disturbing conditions of all sorts, that the position of the implicated part was in some way determined by them. Both sides were affected, much more frequently in children than in adults. Very often, if the chest were auscultated carefully, although the main disease was situated, say in the scapular region on the one side, there would be heard some evidence, more or less pronounced, of disease on the other, and usually somewhere about the root of the lung. Some earlier affection of the bronchial glands might be the determining cause of this double affection. This frequent incidence of pneumonia upon the root of the lung probably explained its frequent latency, the general symptoms being present, and yet the definite physical signs wanting for the first three or four days. The next difficulty was abortive pneumonia. The healthy working of the lung was bound up intimately with that of other viscera, particularly of the stomach and abdominal organs. The high fever of sudden onset, the flushed face, the delirium, the dry cough, were all common enough from some sudden gastric upset, and in certain families was the natural reaction to some trivial informality of diet, excitement, or what not. In such there was also often some added harshness of respiration in one part or another. Another element of obscurity was the ready production of collapse in children.

Not only did this ready occurrence of collapse tend to facilitate the attributing to the pneumonic consolidation a larger area than belonged to it, it was also inseparable from another feature of some pneumonias, namely, the absence of the physical signs when the consolidation was in reality there. This condition had been termed silent pneumonia, because, although the general symptoms were present, and with these the dulness on percussion characteristic of consolidation, the application of the stethoscope revealed nothing. Apart from the occurrence of collapse, there was a very ready explanation of silent pneumonias to be seen often in the *post-mortem* room, namely, the presence in the smaller bronchi of gelatinous casts of the pneumonic exudation. These casts were doubtless the cause of most cases of silent pneumonia, but they were equally valid causes of acute collapse of the lung, for they seldom completely filled the lumen of the bronchia, and thus readily allowed of the expulsion of the residual air, while they as readily refused the admission of any fresh intake; still there was no reason why pneumonia should not sometimes abort, and whether such abortion was possible or not was a very important matter to settle before discussing the further question whether drugs or treatment of other kinds were effective in controlling the disease.

On the whole there seemed no doubt, from a careful and even judicial scrutiny of the effects of treatment, and of the records of earlier times, and even occasionally now, of the effects of bleeding in adults, that pneumonia did abort, and possibly frequently so. In relation to these points the question arose whether such a pronounced organic change as acute pneumonia could be produced by nervous influence such as sudden shock. Dr. Goodhart had seen several cases of the kind that left little room for doubt upon the matter; the last case of the kind was a particularly interesting one.

A boy, aged 14, so far as is known in perfect health, was returning from school on December 14th, when a railway collision occurred, and he was thrown violently from one side of the carriage to the other, striking his head on the opposite side. He got out and walked home, a distance of a mile or two, but on reaching home he was violently sick, and was indisposed the next day and the next. On the fourth day after the event he was seen by Mr. Williams, of Woolwich; the patient was then complaining of vertex headache, and pain and tenderness over various dorsal vertebrae. He had also slight cough, a pulse of 120, and a temperature of 104.5°. He was conscious when roused, but was irritable, and curled himself up in bed like a cerebral case; his bowels were confined. Dr. Goodhart saw him on the eighth day after the accident, by which time the left apex was becoming dull on percussion, and tubular breathing had just appeared. He went on well; crisis supervened on the thirteenth day, and the temperature was normal for three days afterwards. He then suddenly went to the bad again; his temperature ran up to 104° or 105°, and presented this puzzling feature, that it was so high in the morning and fell to 95.4° at night; and this for three days in succession. After that it remained at 105° and 104°, till Dr. Goodhart saw him again seventeen days later. He was then practically moribund, but the symptoms seemed to point more to ulcerative endocarditis than to anything else, for although the disturbed action of the heart rather suggested renal disease, yet the hectic temperature was all against renal disease, at any rate pure and simple. The pneumonia had by this time cleared up. He died an hour or two later, and Mr. Williams found, *post mortem*, masses of dark-looking vegetation upon the aortic valves. In this case

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