CROONIAN LECTURES

ON

DISEASE AND ITS MEDICAL TREATMENT.

Delivered at the Royal College of Physicians, London.

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LECTURE II.

MR. PRESIDENT AND GENTLEMEN,—In my former lecture I endeavoured to explain what I understood by the word "disease," and, by reference to various complex diseases, to show in what sense, as applied to them, it might be regarded as including within its scope the various phenomena which flow, immediately or mediately, from the primary cause, and in what sense again it is applicable to each or certain of these various phenomena. I ventured, also, to make some kind of arrangement of diseases in groups; and, guided by this arrangement, to discuss the processes by which diseases affect the body injuriously, and those by which their cure or alleviation is accomplished.

To-day I enter upon the subject of the treatment of disease. The art of prescribing is unquestionably a very important medical art. I recollect when, some years ago now, I read that fascinating work, the *Pharmacologia* of the late Dr. Paris, I was struck with the logical (I had almost said plausible) way in which he classifies the objects which we should endeavour to obtain, by mixing and combining medicinal substances. He arranges them under five heads, and under each head includes several subdivisions. The five heads are as follows:—1. To promote the action of the basis or principal medicine; 2. To correct the operation of the basis by obviating any unpleasant effects it might be likely to occasion, and which would prevent its intended action, and defeat the objects of its exhibition; 3. To obtain the joint action of two or more medicines; 4. To obtain a new and active remedy, not afforded by any single substance; and 5. To afford an eligible form. Now, I suppose that each one of us has endeavoured, according to his lights, to carry some such principles as these into operation; at all events, every one will admit that it is in many points of view not immaterial that our combinations of drugs should yield "elegant" results; and, still more, that the drugs which we administer together should neither neutralise one another chemically, nor aim at producing incompatible results within the body. But, admitting all this, it must also be admitted, I conceive, that such rules are more adapted for those who thoroughly understand, or who believe they understand, the actions of medicines in relation to the processes of disease, than for those who are investigators of the actions of drugs, and of their influence over morbid processes; and that, in proportion as we combine drugs habitually, so we deprive ourselves of the power of acquiring any precise acquaintance with the specific effects of the individual drugs which we employ.

But indeed the difficulties which beset us, in our endeavours to ascertain the effects of medicines upon disease, are almost insuperable, and are by no means limited to those which arise out of the cultivation of the art of prescribing. I do not propose to discuss these difficulties fully, however interesting and important such a discussion might prove; I do nevertheless intend to make a few brief remarks upon some among them.

One of our greatest sources of error lies in the fact that nearly all, if not all, diseases tend to run a certain course, which varies within certain limits, according to the intrinsic severity of the attack, the patient's constitutional peculiarities, and the influences to which he is exposed during his illness; which tends (as the case may be) towards convalescence, towards relapse, towards permanent impairment of health, or towards death; but which is yet, in the main, typical for each disease.

Thus we treat a case which, under ordinary circumstances, inclines after a while to get well, say a case of pneumonia, or of typhus, or of chorea; we push the treatment we have elected to adopt, until presently the patient begins to amend, and then soon regains his health. What more natural than that we should attribute his recovery to the medicines which we have given him, especially if we have sedulously watched his progress from day to day, have regulated our doses from time to time according to his supposed requirements, or have gone on increasing our doses until some effect—convalescence—follows? What more natural than that our belief in the efficacy of our treatment should be confirmed,

if (without clearly recognising the fact) we have to deal with a case of exceptional mildness? or that, knowing such diseases are sometimes inevitably fatal, we should regard an occasional unsuccessful case as no valid argument against the general efficacy of our treatment? Or, again, we have to deal with an affection, which is more or less chronic in its character, but is marked by alternate intervals of sickness and of apparent good health. A patient has gall-stones, or kidney-stones, and passes one occasionally, with severe pain and more or less functional disturbance; or he is the subject of psoriasis, which, we know, is apt to come out every spring or autumn, and after lasting for a few months to subside. Now can anything be more easy than, in the treatment of such diseases as these, to mislead ourselves into the belief that the subsidence of symptoms, which follows upon our treatment, is due to our treatment? Would it not be difficult indeed for an enthusiastic therapist to escape imposition? And even in cases of permanent illness, and those which are hastening on towards death, such self-deception is at least equally easy; slight apparently favourable variations are only too apt, both on the part of the patient and of the medical man, to be put down to the credit of the medicines which are being employed, and to be magnified in importance until the patient (who has been getting better from day to day) finally slips through our fingers.

This danger of misconception is, of course, very greatly increased when, as too commonly happens, we fail to recognise accurately the nature of the disease which we are called upon to treat. How often has a specific fever been cut short by an emetic, which fever never was, and never would have been, the disease it was assumed to be; how often has an hypertrophied heart been reduced in bulk by a course of medicine, when functional disturbance only was present; how often has phthisis yielded to treatment, when we have had merely to deal with pneumonia or bronchitis! And, again, misconception can only too easily arise (especially with regard to so-called functional diseases) when, in their course, natural but unanticipated changes take place, and happen to take place at the very time during which medicines are being administered. It is thus undoubtedly that many cases of epilepsy, hysteria, and neuralgia become cured, and that warts so often are charmed away. I have notes of several cases which bear upon this latter point; and two, which are especially striking, I will venture to quote in illustration of my remarks.

A coal-miner from Durham came to me at St. Thomas's, some years ago, with well-marked tic-douloureux of the right side of the face, from which he had suffered for fourteen years. The paroxysms of pain lasted from a few seconds to a few minutes, and came on night and day, at intervals varying between one and ten minutes. The right cheek was curiously wrinkled, and all the hair growing on it, as well as that on the temple, was worn down, as though shaven, by the constant rubbing with which, by means of a pad, he had endeavoured to relieve his pain. All the teeth (some of which had been carious) had been removed, from time to time, from both the upper and lower jaws of the affected side. During a residence of nearly four months in the hospital he was submitted to various kinds of treatment, with, so far as could be discovered, only temporary benefit. During the last ten days he was treated with three grains of extract of Indian hemp thrice daily; and then he left the hospital at his own request, and, according to his own statement, somewhat better than he had been on admission. But I wrote on his bed-ticket "Nil," as the result of his treatment. He returned to Durham; and about twelve months afterwards the medical man (a friend of mine) who had sent him up to me, called on me in London, and congratulated me on the marvellous cure which I had effected. He told me that the man had continued entirely free from neuralgia since his return from London. I have tried Indian hemp often since, but have never with its aid cured another case of tic.

The second case was that of a boy, aged fourteen, who was also in St. Thomas's under my care, more than ten years ago. He had had fits for two years, but for a week before admission had had them night and day, with scarcely any intervals; indeed, for the last three days he had been quite unconscious. He was unconscious, and thought to be dying on admission, but recovered shortly. When he came to, it was found that his lower extremities were in a state of locomotor ataxy; and not only that, but that, whenever he attempted to move them, a feeling of tingling starting from the toes crept upwards, and, after a second or two, was succeeded by a loud distressing scream, which ushered in an epileptic fit of about half a minute's duration. This sequence was quite constant; and during the day he managed, in great measure by keeping himself still, to avoid having any fit; but in the night, when he was asleep, and his legs were not under voluntary control, they were pretty frequent. The fits gradually increased in number, especially by night; and at the end of twenty-three days they were so frequent that the patients in the ward with him were kept awake all night by his rapidly recurring screams. The next morning, I ordered him five minims of liquor arsenicalis to be

taken three times a day. For eight days from that time he had no fit, and only for the first two or three of them a little tingling of the legs on movement. Then he was visited by his friends, and immediately after they left him he had a recurrence; he had many fits that evening, and two or three in the night, and on the following night also one or two. They then again ceased, and he was discharged from the hospital apparently well in about a fortnight. I have given arsenic in epilepsy both before and since the above case was under my care, but have never seen it act with such miraculous efficacy. I say miraculous, for in fact it acted by anticipation. For, although the fits ceased from the time at which the change of treatment was made, he only began that treatment in the evening, and therefore some hours after the cessation from fits had commenced.

Then, again, how constantly we deceive ourselves, or permit ourselves to be deceived, by the mode in which we exhibit our drugs, and the circumstances under which we administer them. I have already adverted to the fact, that combinations of various drugs necessarily interfere seriously with our power of estimating the value of the separate ingredients. I am not in the least objecting to such combinations, for I know that in a large number of cases they are judicious—aye, and absolutely essential, if we are to attain our object; but they are not always judicious or essential. We know that iodide of potassium promotes the removal of certain forms of deposit, especially those connected with bones; that colchicum cures the gouty paroxysm; that carbonate of soda tends to render the secretions alkaline; and straightway we prescribe for a rheumatic patient (who has some inflammatory deposit about his bones, whose case has a considerable superficial resemblance to gout, and whose urine is acid, and whose perspirations have a so-called "acid" smell) a combination of these drugs, without having any practical experience, and without even trying to gain any, as to their relative efficacy. We know, too, that strychnia causes spasmodic action of the voluntary muscles; but, without knowing whether it causes similar action of those of the alimentary canal, or, if it do, whether that action could ever be beneficial, we assume its value, and add it to our purgative medicines. Surely, too, it is on grounds not quite identical with these, and yet not quite dissimilar, that sarsaparilla has been so long esteemed a valuable remedy for syphilis and various forms of cachexia; that *pareira brava* has been so highly valued for the treatment of chronic inflammation of the bladder; that demulcents (of which mucilage, which cannot pass through the kidneys, forms an essential part) have been so long recommended in similar affections; that the combination of liquor potassæ and hyoscyamus (which, as Dr. Garrod has proved, renders the hyoscyamus wholly inert) has had amongst surgeons so high a reputation for the cure of clap. Further, we usually combine with our medicinal treatment various forms of hygienic treatment: thus we admit patients into the hospital, where they are for the most part better fed and cared for than they were in their own homes; or we send them to the sea-side; or we encourage them to spend a few weeks at some inland spa, where they drink waters containing homœopathic doses of iron or of chemical substances, which, from their known inefficiency, we have long discarded from the *matéria medica*, but where they enjoy perfect rest from toil, wholesome living, and fresh air; under all of which circumstances improvement of health is apt to take place, wholly irrespective of the medicinal treatment which is adopted.

But perhaps the greatest impediment of all to the advancement of clear therapeutical knowledge is offered by the various theories of disease which spring up from time to time, and influence our judgment of the nature of diseases and our mode of action in regard to them. I shall not presume to enumerate, far less to discuss, even the most celebrated of these theories, or to consider how and to what extent they have swayed the minds of medical men, or how far they influence us still. There was a time, however, when most physicians regarded diseases as sthenic, and when the natural tendency was, consequently, to bleed, and to adopt other forms of lowering treatment. There was a time (and it has not yet passed away) when an opposite opinion prevailed, and when, therefore, stimulants became the fashion; and again there was a time when it was believed that all the morbid processes of disease take place in the fluids of the body, and when, consequently, an evacuant or eliminative plan of treatment was looked upon as essential. Now it is obvious that, dominated by these and other such theories, men would shape their plans of treatment in accordance with them, and, finding that patients recovered under their treatment, would look to that fact as a confirmation of the correctness of their theory and practice. The man who bled in fever, would observe the disease rising to a certain height of intensity under his treatment, and would then see it gradually subside under a continuance of the same process. The man who pursued the stimulating plan, and he who followed out the eliminative method, would equally observe the same sequence of events; and all might point to their results with not unnatural triumph.

But whether in a series of cases, treated on one plan, recovery was more protracted by some few days, than in series treated on either of the others; or whether the percentage of deaths was rather higher under one of these plans than under the others; or whether a greater amount of weakness and incapacity for work, or a greater proclivity to secondary affections marked one class of patients than the other classes; are considerations which, however important for the determination of the relative values of different lines of treatment, would scarcely strike the man who is carrying out a theory in which he implicitly believes, and would scarcely reveal themselves in statistical tables. That apparent success has attended the most opposite plans of treatment is, at all events, undoubted, and is so far admitted that we endeavour to explain it away. We acknowledge the fact, that diseases were treated on a different plan in the days of Sydenham from that which we follow now, and that on both plans a sufficient measure of success has been obtained to satisfy practitioners that their treatment has been beneficial to their patients. We acknowledge that there has been a change; but we find it easier to believe that there has been a change in disease than a change in ourselves; easier to believe that Sydenham's theory with respect to the change of type of disease, which is mainly founded on imperfect knowledge, and which has never been shown to be true in any large sense, is applicable here, than that there has been a gradual change of medical opinion and practice, and that, consequently, diseases have been cured, or rather have been recovered from, under the most opposite plans of treatment; easier to believe that the table has been moved by spiritual agencies than by the unconscious operation of our own muscular force. But theories like these are not the only ones which sway us. When we observe the different plans on which different practitioners (who have been educated, probably, in the same school, and have imbibed the same doctrines) treat the same diseases; how the one insists on principles or details of treatment which the other thinks of no importance; and how also they carry their several peculiarities more or less through their whole practice; we cannot help recognising (what is certainly a fact) that there is a tendency among us, as time goes on, to invent small theories of our own for our own private guidance—theories which we both form and obey half unconsciously, and which would probably be at once repudiated if we took the trouble to think them out and put them into form.

I will not pursue this subject further; I ought not, perhaps, to have pursued it so long; for I am conscious that I have said nothing which is not familiar to you; have suggested no difficulty which you have not considered; have brought before you no cause of error against which you have not taken precautions. The difficulties adverted to are not the less, however, real difficulties, and I may admit that for myself they are almost overwhelming.

Functional Diseases constitute a very large proportion of the maladies with which we have to do. We know that, in a large number of them, the nervous system, and more especially its vaso-motor portion, is importantly involved; but in what degree it is involved and how, and whether primarily or secondarily, are matters which, for the most part, lie beyond our knowledge. We know, too, that in a large number of them, there are important disturbances of circulation, disturbances which are manifested either in the centre of the system by palpitation, or irregularity of action and the like, or towards the periphery, by dilatation or contraction of the smaller vessels, and consequent congestion or anæmia, rapidity or slowness of flow of blood. And we know, too, that in many of them again, there is profound impairment or modification of the processes of nutrition and secretion; that, for example, albumen and other matters are rapidly converted into sugar within the system, or that the blood-forming organs fail to carry out efficiently their destined purpose. But in these latter examples, as in the first, and as in all others that we might quote, we know them as diseases mainly by certain symptoms; we recognise, it may be, in some cases anterior functional disturbances, to which some of the characteristic symptoms may be directly due; but we remain perfectly ignorant of the fundamental causes on which all the phenomena of each disease hang; remain perfectly ignorant of their intimate pathology. It is not to be wondered at, therefore, that our curative treatment of functional disease should be for the most part empirical and unscientific, and that, even when it is scientific, all the science is bestowed in counteracting, or relieving, or preventing secondary phenomena or symptoms. I am by no means saying this with any covert sneer; for, in the first place, so long as we can cure a disease, it matters little, so far as that disease is concerned, whether we can explain the mode of operation of our remedies or not; and, in the second place, there is no reason why, if we cannot dig up the roots of a disease, we should not lop off its branches, especially when we know that in many of these cases it is the branches, so to speak, which render the disease distressing or terrible; nor is it quite certain that in diseases, as in plants, this constant process of lopping

off does not, in some cases, lead ultimately to the dying out of the root. At all events, if we can rob epilepsy of its most terrible features, as Dr. Marshall Hall suggested, by the operation of tracheotomy; or relieve spasm of the glottis by the inhalation of chloroform; or assuage the agony of angina pectoris, and arrest impending death, by a few whiffs of nitrite of amyl, as seems to have been experimentally proved by Dr. Brunton; or cut short the neuralgic spasm by the application of galvanism; although we may still be unable in any degree to cure the morbid conditions on which these symptoms severally depend, it must be admitted that we are very far indeed from being powerless for good. At the same time, there are many of these diseases over which we have no kind of influence, and many in respect of which our curative power is illusory. Among these latter I may name chorea. How far this is a functional disease, may of course be disputed. We know its almost, if not quite, invariable association with cardiac valve disease, and that there are plausible arguments which have been especially dwelt upon by Dr. Hughlings Jackson in favour of the dependence of the choreic symptoms on embolism of the smaller arteries of the brain, probably those of the corpus striatum. But there are difficulties in the way of accepting this explanation, and it undoubtedly needs the confirmation of morbid anatomy. Provisionally, therefore, I think it may still be looked upon as a functional disease.

Now chorea, as we all know, is essentially a malady of childhood, which ends as a rule in recovery in the course of a few weeks, yet leaves the patient with a liability to relapse. There is no disease for which so many remedies have been vaunted as specifics; and there is no remedy, probably, which has been employed in it, whose use has not been attended with what may seem to have been satisfactory results; indeed, the equality of results from the most opposite plans of treatment is a remarkable feature in its history, and has struck many physicians besides myself. Sydenham treated his patients with alternate bleedings from the arm to eight ounces (more or less), and purgings (each being repeated three or four times), followed by a course of purgatives and alteratives; and his patients recovered. Iron, if not first recommended, was, at least I believe, first brought into prominence as a remedial agent in chorea by the late Dr. Elliotson, who administered it in the form of the insoluble sesquioxide, and in large doses; and his plan of treatment was long followed, and found successful. But, curiously, iron has fallen into comparative disuse as an antichoreic, since physicians have replaced in their prescriptions that almost inert powder by other ferruginous preparations of greater solubility and potency. Zinc, again, is a remedy which of late years has enjoyed a wide reputation; and, like the sesquioxide of iron, has been given in large doses; but the mode of administration which has been adopted in regard to it, is one which (except in an occasional fatal or incurable case) could not avoid leading on to a favourable result; it is to commence with small doses, and to increase them day by day as the stomach becomes tolerant of the drug, until a certain effect appears to have been produced upon the patient as indicated by the amelioration of the choreic symptoms, and then the dose is diminished. It is scarcely possible to conceive a plan of treatment more calculated to deceive the physician. Few remedies have found more favour than that last hope for all intractable diseases, arsenic; and it can at least be said of it, that it has proved as successful as any other medicine that has been tried. Those which have been enumerated are among the chief, but they form a very small proportion of the total number of remedies for chorea, which have been from time to time recommended, adopted, and fallen into more or less disrepute. I, like others who have been some years in practice, have tried many of the plans of treatment which have from time to time cropped up; and have especially employed largely the ferruginous, the zincic, and the arsenical methods, and I am free to admit that, for a long time, I had nearly perfect faith in their efficacy; and it was only when I began to compare my own results, and to study the experience of other people, that I began to doubt. Some years since I made some experiments with regard to the treatment of this disease, a summary of which I published in the second volume of the *Practitioner*. I adopted the plan of first allowing my patients to remain a week under observation in the hospital, in order to ascertain, by personal observation, if their disease were undergoing abatement, or if it were stationary, or becoming worse, and at the end of that time of selecting those only for experiment who were obviously not convalescent. "In the second place, I gave each remedy on which I wished to experimentalise alone, and for successive periods of a week or ten days or more, considering that, by the end of such a period, the patient would be fairly under the influence of the drug which he had been taking during it; and that, if that drug possessed any specific influence over the choreic movements, that influence might be expected then to have manifested itself in some appreciable degree. In the third place, when decided signs of convalescence showed themselves, I discontinued either at once, or at the end of the allotted

period of seven or ten days, the medicine, during the administration of which convalescence had been established, and gave in its place either a little compound infusion of gentian or a mere placebo, assuming that, if that medicine had brought convalescence about, its discontinuance ought to be followed, if not by some exacerbation of the disease, at all events by impeded convalescence."

The results of these experiments I summed up as follows.

"They all point more or less clearly to the same conclusions: the first and most obvious being that, so soon as convalescence has begun, it makes little or no difference as regards the progress of the case, whether the specific treatment which had been employed be continued or disregarded; the second, and less obvious one, perhaps (at least, if only the cases there quoted be considered), being that, the association of any specific remedy with the commencement of convalescence is a purely accidental coincidence"; and I added "that, to the best of my belief, my experimental cases, if judged by the event, may be regarded as cases of fairly average success." Since that time I have rarely used, or at all events have rarely used persistently, any specific plan of treatment, but have contented myself, for the most part, with the administration of some simple tonic and dietetic, and hygienic measures calculated, as I have believed, to restore the patient to, or maintain him in, good bodily health, and I cannot perceive that I have been in any degree less successful than I was formerly.

As regards *Diseases due to the Action of Poisons, and those originating in Excess, Deficiency, or Improper Quality of Nourishment*, I need say little here. The principles of their treatment are for the most part well understood; although unfortunately, for many reasons, we fail in a large number of cases to bring them into successful operation. It may, however, be worth while to remark, that often, when poisonous substances introduced from without are acting within the system, we are able, before we actually get rid of the poison, before we actually cure the disease, to obviate some of the more serious symptoms which it produces, and thus not only to bring relief, but to prolong life, and it may be save it. Thus, there is no doubt, that with the aid of opium we may relieve the colic due to lead; and thus also, by various means, we may to some extent neutralise the specific influences of the more potent poisons.

[To be concluded.]

PROPOSITIONS IN ANIMAL MECHANICS.

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THE complete mathematical proofs of the following propositions will appear in my forthcoming work on *Animal Mechanics*.

1. Each muscle is constructed in relation to its joint, in such a manner as to perform one kind of work only; and it performs that work to maximum advantage.
2. The number of muscles employed is determined by the number of distinct actions required from the limb.
3. The shape and form of the bones employed are the necessary consequence of the shape and power of the muscles in action.
4. The smallest muscle in the combination is as carefully adapted to its conditions of maximum work as the largest muscle.

From these propositions, supposed to be extended to the action of every muscle and joint, it appears to me to follow as necessary consequences—

1. That a foreseeing mind planned the type of the limb and of its actions.
2. That, the idea of the limb and of its necessary actions being given, the number, shape, and arrangement of the necessary muscles can be calculated and predicted with as much certainty as an astronomer can predict an eclipse.
3. That the shape and arrangement of the bones follow, of necessity, from the necessary arrangement of the muscles.
4. That any alteration, however slight, in any part of the combination of bones, muscles, and joints, would entail a loss of work, and lead to a less perfect mechanism.
5. That, hence, the permanence and stability of each species (so far as relates to bones, muscles, and joints) is absolutely secured, on the principles so admirably laid down by Mr. Darwin.
6. That the profound study of the mechanism of joints lends no support to the postulate that the similarities in the bones, muscles, and joints of animals, may be explained by common descent from a supposed common ancestor.