AN ADDRESS ON COLLECTIVE INVESTIGATION OF DISEASE.

Delivered at a Meeting of the Lancashire and Cheshire Branch.

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MR. PRESIDENT AND GENTLEMEN,—We are met to help on, if we can, a very excellent scheme for the more systematic and general observation of disease and its treatment. We do not, of course, suggest that there is anything novel in the idea of combined effort amongst medical men for this object. All our professional societies exist with this design, and most of them appoint, every now and then, committees with the special intention of collecting information relative to some given topic. Government commissions have not unfrequently collected evidence on a large scale as to such maladies as plague, typhus, syphilis, small-pox, leprosy, and other diseases, which have assumed a national importance. I believe I might say that some idea of a collective scheme of investigation was present to the minds of the founders of our Association. At any rate, in its early days, and whilst still retaining the honourable name of "Provincial," it published some elaborate reports, which were the result of combined effort on much the same plan as our own. I well recollect one, thirty or forty years ago, on arsenic, which, I believe, did much to establish the repute which that remedy afterwards obtained in England, to the great advantage of thousands of its population. You have also the pleasure of knowing that Manchester, some years ago, under the initiative of Dr. Ransome, did some very useful work in this manner. I may even claim that, in a small way, I have myself had something to do with collective investigations. For many years I edited, in the *Medical Times and Gazette*, "Reports of Hospital Practice," and "Statistics of Operations," the materials for which were obtained much in the way that we now contemplate. I am free to confess, however, that the collective labour often fell upon myself; and I may thus claim some practical familiarity with the drawbacks as well as the advantages of the scheme which we now propose. Permit me to say then, that I cannot for a moment doubt that our distinguished associate, Professor Humphry, when he persuaded our Association to organise the large scheme which we are here this afternoon to promote, has done that which will prove of great and lasting advantage to medical and surgical science. It is a scheme which deserves from us all the best help that we can give it.

I do not think that it would serve any useful purpose for me to say anything about that which our investigation committees have already done. Most of you have seen the documents which have been issued, and can judge for yourselves. Many of you have already made contributions to the reports. If you have thought with me on the matter, you will have concluded that the preparation of these schedules must have been a most useful task to those who undertook it; that no one could even read them without obtaining valuable instruction; and those who attempt to use them as guides in case-collection, must secure, in so doing, a good knowledge of the most recent facts and opinions as to the several maladies which they concern. Thus, quite apart from their final object, we find much good done. It may be that the final outcome may prove somewhat less than is expected; that, however, is really not of much moment, for collateral objects of scarcely secondary importance will most certainly have been secured. I am glad, however, to learn that thus far our committees have experienced no disappointment, and that the answers received have exceeded in value their utmost expectations.

If I were to criticise at all what has already been done, I should incline to suggest that too much of detail has been, in some cases, attempted. If the collection of facts in this way should prove to some degree impracticable, it will probably either be because many are deterred, by the largeness of the task proposed, from making any attempt to fill up the schedules, or that, when filled up, they may prove unwieldy in the hands of those who undertake the labour of collating them. There existed once in London a Society for the Improvement of Medical Observation, which published a little book entitled *What to Observe at the Bedside*. Both society

and book came, I believe, in some measure to grief, by an error in this direction, over-elaboration. When we are told in detail how to observe everything, it but too often ends in our observing next to nothing. In order, however, that I may avoid the ungracious task of criticising our work, and the unnecessary one of praising it, I will, with your permission, devote the rest of my half-hour to some suggestions of my own.

It seems to me very desirable that we should avail ourselves of this organisation in order to obtain large records of rare diseases. The study of what are called rare diseases is by no means a matter of mere curiosity. Apart from the fact that the subject of a rare disease has an equally strong desire to be cured as the sufferer from a common one, we must remember that in rare maladies we often find the most instructive lessons in the laws of pathogenesis. Rare maladies seldom or never really stand alone; they are neighbours or cousins to others, and differ often from them only in being very strongly pronounced. We can often far better study the laws of a whole group from a rare and exaggerated example, than we can from dozens of the more common. Certainly we cannot, either as pathologists or as practical men, afford to leave one single rare malady uninvestigated. The only way to investigate them without danger of error is to collect a large series of each one for contrast and comparison. Now this is often impossible to the individual, but will be well in reach of such an organisation as that now proposed. Let me give a few examples of the kind of cases which I would propose to collect.

The distinguished President of the Clinical Society, in his opening address a few weeks ago, mentioned that he had frequently encountered a form of illness, usually ending fatally, which he could not find described in books. It consisted in the development of very serious symptoms, pus in the urine being always one of them, in consequence of the first employment of catheters for residual passive retention caused by enlarged prostate. The patient is, to begin with, in good health; there is no complete obstructive retention; the use of the catheter is ordered simply as a precaution; and yet, under these apparently simple conditions, an acute and rapidly fatal illness ensues. Sir Andrew Clark asked the experience of the Clinical Society on the subject, stating that he had seen several fatal cases of this kind every year for some time past. Now, here is an excellent subject for collective investigation. It is definite, and not too large. It is one upon which almost all have facts, and could send them without much trouble. For my own part, I have no doubt that the form of illness described is simply acute cystitis, developed in consequence of the decomposition of the urine in atony of the bladder. I have not myself often seen such end fatally; but, in common with all surgeons, I am quite familiar with them in minor degrees. However long continued the habitual retention of a certain residual quantity of urine, there is, I think, seldom any tendency to decomposition until the catheter is used. As soon as you use a catheter, the patient usually becomes dependent upon it; he often cannot afterwards pass the least quantity. Often the bladder is so weak that you are obliged to promote the flow of urine by gentle pressure, even when the instrument is in. Within a few days of the beginning of catheterism, the urine, which was clear and bright before, begins to be dull and opaque, and to abound in bacteria; and after this it but too often becomes purulent. I have sometimes suspected that the extreme atony permits the introduction of air by the catheter, and thus favours putrefactive changes in the urine. There is no doubt that it makes it almost impossible ever really to empty the viscous. It would be desirable to enlarge Sir Andrew Clark's interrogation, so as to make it include the results of treatment of prostatic retention generally. Precisely the same events often follow the use of catheters for prostatic retention attended by complete stoppage. Although the introduction of instruments may be as easy as possible, and although the softest and least irritating of instruments may be used, with the utmost gentleness, yet the result is decomposition of urine, cystitis, and, but too often, a painful death.

Notwithstanding the improvement of instruments of recent days, the results of the treatment of prostatic retention still remain exceedingly unsatisfactory. It is most desirable that they should be, in the first place, widely investigated and candidly recorded, and, in the next, that we should try whether we cannot, by their aid, construct a better system of treatment. It would take me too far, were I to venture to suggest some possible modifications of our present methods; one hint, however, allow me to give, since it may perhaps serve as a spur to prick the sides of our intent. The victims of prostatic retention are very often taken from the most intelligent part of the medical profession; men who, by judicious care in their

mode of life, contrive to live a little beyond the usual average; just such men as I see before me. It is most desirable that we should improve the treatment of prostatic retention.

I have gladly availed myself of a suggestion by a physician of a surgical subject which needs elucidation. Permit me to return the compliment, and mention a medical one. I do not think that enough is known about catarrhal laryngitis. It appears to me to be a very definite affection, in minor forms tolerably common, but in its severe ones rare. It is, in a certain way, quite distinct from croup, being entirely non-membranous. It seems to me to be more common in adults, or in those past middle life, than in others. There is a tradition that Washington died of it, and I believe that a former colleague of my own, on the staff of your JOURNAL, did so. If Dr. —'s illness were really such, he is the youngest victim to it within my knowledge. Acute oedema of the larynx is scarcely a suitable name for it, since it may be caused by a variety of causes, whereas the malady to which I desire to call attention is always due to exposure to cold, and always obeys the law of true catarrhal affections in passing off completely and rapidly if the patient survive. In almost all the fatal cases which I chance to remember the cold was caught by going to church, and all were in men past middle life. In one, a gentleman of 55, who but one year before had insured his life for a very large sum, being apparently in excellent health, died on a Wednesday, having caught his cold on the previous Sunday evening. In this case the death was in spite of laryngotomy, and it is possible there was pneumonia also. In the last which I have seen, and the one which has made the greatest impression on my mind, the patient, a gentleman of 50 years, attended an evening service in cold wet weather, complained of his throat immediately afterwards, had paroxysms of extreme difficulty of breathing, and died in one of them, within forty hours of his having joined in the duties of the choir. So distinctly paroxysmal had been his dyspnoea, and so complete the remissions, that he died in his clothes, not having gone to bed. This feature of complete temporary relief had led his medical attendant into the mistake of telegraphing for me to do tracheotomy, instead of at once doing it himself. The patient in the end died, before either of us could get there. We were allowed to examine the body within an hour of death, and found the larynx and adjacent parts in a state of extreme congestion, with much oedema, but without a trace of membrane. I by no means insinuate that catarrhal laryngitis has been wholly neglected in systematic works, but I feel sure, from much conversation with those of large experience, that collective investigation would be likely to bring out its features much more definitely, and to add an important chapter to the clinical history of catarrhal ailments in general.

At the first meeting of the present session of the Pathological Society, a week ago, the most interesting communication was one from Mr. Sutton, who, by his zealous work at the Zoological Gardens, has shown that monkeys are very liable to a peculiar form of general softening of the bones, allied to rickets, possibly almost identical with it. This observation is, I believe, almost new, and it is exceedingly important. It was well known before that some of the lower animals did, in early life, under certain conditions of diet, become rickety, though, even on this point, the evidence was scanty, and the results of experiment by no means uniform. In Mr. Sutton's patients, however, the age of the monkeys was not usually known, and none of them were very young. In this respect, and possibly in others, the disease appears to be as much like osteomalacia or mollities ossium as the true rickets of early life. Here, however, the question arises, is osteomalacia, as Trousseau and others have called it, a form of rickets, allied to it naturally; by which term I mean acknowledging, more or less remotely, the same causes? It is so rare a disease in English practice, that there appears little or no hope of giving a satisfactory answer to this question within the present century, unless collective investigation can give us help. I have not myself seen a good typical case of mollities ossium for twenty years. The last with which I was made acquainted was one in which Dr. Ormerod, of Brighton, had preserved the skeleton, and of which he kindly gave me a photograph, together with a detailed history, read, I think, at one of our Association meetings. In this case, all the man's children were rickety in childhood, and there appeared some justification for the name "rickets of the adult."

You will probably remember that Trousseau says that he cured a case of osteomalacia by giving cod-liver oil, just as he would have cured rickets. Now it cannot be doubted that there are at present under the observation of our members, scattered here and there amidst the crowded population of our islands, a few cases of osteomalacia. Let us make it known that we want the records of these,

more especially in reference to the family history; and I doubt not that we should, before very long, obtain a respectable series of facts.

About two years ago, I saw, at one time, three examples of a malady which was wholly new to me. I did not at the time know that it had been described, but I learned subsequently that examples of it had been accurately recorded by Hebra under the name of xeroderma. Since then I have seen but one other example of the malady, and to see that I had to go to Paris to the wards of the Hôtel St. Louis. My surgical friends there had seen but one other example; Hebra has recorded but three; you may judge, then, that the xeroderma of Hebra is a rare malady. It belongs, so far as I can appreciate it at present, to the lupus group, but it differs very remarkably from other forms of lupus. In the first place, it begins in early childhood, and is clearly a family disorder, in that sense of the words which implies that it usually affects several out of the same family of brothers and sisters. In the case referred to there were three, two sisters and a brother. It consists chiefly in a diffuse inflammation of the skin of the exposed parts (face and upper extremities), resulting in induration, ulceration, and scars, and very peculiar development of nevroid-like tufts of vessels. It is said to sometimes end in cancer, and histologists have found elements like those of the rodent ulcer form of epithelioma. Such are the features of this most curious malady, and such the facts of its clinical history. It is a well-defined disease; each case is like its brother, and, once recognised, there is no difficulty as to its diagnosis. Probably, of Hebra's xeroderma it is true, as of osteomalacia, that there are somewhere, within cognisance of those who read our JOURNAL, a few other examples. It is most desirable that they should be unearthed, and submitted to the inspection of trained observers, and placed on record in full detail. The malady is clearly a connecting link, and one which possesses most remarkable and instructive affinities.

What I would suggest in reference to collective investigation for such rare maladies as this is, that our committee should prepare concise descriptions of each, just as an entomologist would of a newly observed moth, printing, if needful, its distinguishing features in italics. A little book might be prepared, containing these descriptions printed on one page, and the opposite left blank for notes. Probably, by a little care and thought, a list of a few hundred rarities might be compiled. I mean rarities in such an extent, that it is very desirable every single case should be put on record. Let me mention just a few.

1. Rhinoscleroma, of which as yet no good English case is on record.
2. Hebra's prurigo, a form of prurigo beginning in infancy and lasting, in spite of treatment, through the whole life; of which also, I am glad to say, no English case has yet been recorded.
3. Morphoea.
4. Alibert's keloid, keloid of scars.
5. Addison's disease.
6. Hodgkin's disease.
7. Myxoedema of Gale and Ord.
8. Congenital absence of special bones (such as radius and tibia, with associated portions of carpus or tarsus).
9. Cases of spina bifida, illustrating either results of treatment or survival without it.
10. Cases of sacral tumour.
11. Cases of "the Recurring Iritis in Young Persons."
12. Aneurysms in the orbit.
13. The osteitis deformans of Paget.
14. Disease of joints in ataxy ("Charcot's joint-disease").
15. Non-malignant growths in the tongue.
16. Hemiglossitis.

I mention these simply as examples. Respecting each one, it is clearly to be desired that some explanations should be given as to what is meant by the term used. If this were carefully and concisely done, the recognition of each would not, I think, be difficult, even to the inexperienced.

One good reason for undertaking the collective investigation of rare diseases (or the collection of rare diseases for investigation) is, that it is impossible that it should be done in any other way. I may confess myself no believer in new diseases. What we consider new are simply what have in the past been overlooked. I do not in the least doubt that there have been in the past just as many instances of osteitis deformans, myxoedema, Charcot's joint-disease, etc., as there are now. Nor do I believe much in the restriction of rare diseases to special localities. If Hebra could find, in the practice of the Vienna Hospital, a dozen examples of rhinoscleroma, it

was simply that he knew what he was looking for, and had before him an immense field of observation. If we can reproduce these two conditions, our search will be attended by a like success. It can be done only by collective enquiry. We cannot, in the pursuit of examples of rare morbid phenomena, do as a geologist would who wished to procure a new fossil. Our zeal may be just as great as his, and quite as well justified, but we cannot go from infirmary to hospital, and from dispensary to workhouse, as he would from cliff to cliff, or quarry to quarry, until he finds his treasure. Success to us is possible only by the employment of many searchers well instructed for their work.

Great good would also, I think, result from the collective investigation of the more exceptional forms of idiosyncrasy. There are those who cannot take the smallest dose of iodide of potassium, and others upon whom mercury, quinine, arsenic, etc., exercise effects totally disproportionate to the dose. Better knowledge respecting these personal peculiarities, and in reference to almost all our more powerful drugs, is very much to be desired. How far are such idiosyncrasies hereditary? With what other personal peculiarities are they usually correlated? And what inferences may we make from discovery of any one of them? Such are some of the problems which are of great importance, and which can be solved in no other way than by the collection and sifting of a large amount of evidence. Similar work ought also to be undertaken in reference to idiosyncrasies as to articles of food. Instances of egg-poisoning, tea-poisoning, honey-poisoning, and the like, are, I am sure, much more frequent than are generally supposed. Many examples of liability to disease, otherwise unexplained, would be cleared up by correct knowledge on this head. In some cases, especially in reference to such articles as honey, milk, and fish, it is probable that not only must we examine as to special susceptibility in the individual, but as to unwonted peculiarities in the article itself. Honey, for instance, is not a secretion of the bee, but is collected from various flowers, and varies in its qualities according to the flowers used. Wholesale and fatal poisoning is related to have occurred in the United States from eating honey collected by bees from a species of *kalmia*. If such severe results may be produced in rare instances, it is but fair to suspect that less urgent ones may not infrequently escape notice. It is the same with milk, which varies with the food of the cow, and to some extent the same remark extends to the flesh of domestic animals and of game.

A brief manual, stating the facts already known in these matters, and inviting new observations, would, I think, be very likely to meet with a fruitful response. I had intended to urge the importance of self-observation in these matters on the part of medical men, but our President has spoken on this subject so much more ably than I could that, will say no more than that I most fully agree in his appreciation of the importance of this kind of work.

The field of therapeutics is of such vast extent, that I hesitate to suggest its general cultivation in connection with our present objects. Who can doubt, however, that, in respect to the action of drugs and methods of cure, we are in great need of better and more definite information? In some directions, there is even some cause for anxiety lest we may be tripped up by eclectics, homeopaths, and other charlatans. For the present, however, should anything be undertaken in this direction, I would earnestly advise to avoid elaboration. It might be very well every now and then to print, under the auspices of a drug-committee, a short synopsis of information respecting the best accredited new remedies, with questions suggesting further inquiries as to their use. I am tempted to say that something of this kind is the more necessary, because it is now about eighteen years since our energetic Medical Council has issued a *Pharmacopæia*, and the profession is left to itself for guidance as to all novelties introduced since then.

I have already alluded to the circumstance that our Association once, long ago, prepared a valuable report on the therapeutic uses of arsenic. The almost unanimous testimony given in it to the general harmlessness of that drug greatly increased its employment. Possibly, a few simple queries, which would take but little time in answering, might produce replies which would do good service to the reputation of other valuable drugs. Respecting mercury, for instance, we might ask such questions as the following. Have you ever known a long course of small doses do any permanent harm to the patient's health? Has it often come under your notice that such courses have cured chronic dyspepsia, removed the liability to habitual constipation and sick-headaches, and left the patient in better health than he ever had before? Have you seen cases in which mercury, rapidly pushed to its full physiological effect, seemed to be beneficial in acute inflammations, such as pleurisy,

peritonitis, synovitis, meningitis, or pneumonia? Have you observed any facts in support of the popular prejudice against mercury in scrofulous subjects? I mention these simply by the way of examples. It would, of course, be easy to multiply them in reference to mercury, and to many others of our most important drugs.

If the scheme of collective investigation now before us contain any germ of comparative failure, I will foretell that it lies in the possibility that materials may accumulate beyond the power of any committee to make use of them. We all know how note-books get filled one after the other, and how much easier it is to write out cases than to read and collate them afterwards. What is difficult with our own manuscripts is ten times so with that of others. I foresee that this may lead to dissatisfaction on the part of contributors, who, thinking that their materials have been lost, may refuse to contribute more. To obviate this, I would suggest a plan which I have repeatedly practised myself, and which, when I edited your JOURNAL, I made an abortive attempt to introduce into general use. It is the publication of detailed abstracts, instead of the cases themselves. A well-condensed synopsis is often almost as instructive as the case at full length; it is more easily read, and it may be made to serve as the clue to the detailed notes in manuscript, should any investigator wish to hunt them up. As regards the majority of what we may call rare cases, full abstracts would be amply sufficient for publication. I have a vision of a time when our Association will possess a large museum of manuscript cases—a sort of archives-chamber—well arranged, classified, and labelled. It would, of course, require curators constantly at work to make abstracts and indices, and to secure that those who wished to consult the collection should be enabled to do it with the least possible trouble. I am sure that such a museum is a want. It would be quite as valuable as the bottled fragments of pathological anatomy which we collect so eagerly, but which, in but too many cases, tell us but very little as to the maladies of which they formed a part. The opportunity of contributing to such a collection, and the certainty that his material would not be lost, or would be shelved only in the sense of being properly pigeon-holed, would be a great encouragement to the note-taker. It is, I think, just the thing that our scheme wants to make its life permanent, and its success large and certain.

I hope I have not unduly wandered from the scheme proposed to me by those who did me the honour of inviting me to address you. It seemed to me the best service that I could render the cause they have so zealously taken up, if I could succeed in pointing out directions in which their scheme was likely to be useful. This I have found a very easy task. In conclusion, let me say that I sincerely hope that those who manage our finances will be very liberal as regards expenses of printing, etc. I believe they have been so. It would be false economy indeed to be otherwise, for surely never did our Association engage in a more important work than that which it is now taking up. Year by year we are beginning to perceive more and more to how great an extent the welfare of nations, and the happiness of individuals, is bound up in the word health; and never, I think, since the days of Caxton, was the printing-press better employed than in forwarding the knowledge of the laws of human life, and improving our methods for the prevention and cure of disease.

ST. BARTHOLOMEW'S HOSPITAL MEDICAL COLLEGE.—The Senior Scholarship in Science, of the value of £130, tenable for one year, has been awarded to Mr. Major Brown, B.A., of Downing College, Cambridge, and Mr. James George Ernest Colby, B.A., of Wadham College, Oxford—equal; the Junior Scholarship in Science, value £130, tenable for one year, to Mr. Horatio George Adamson and Mr. Frederick Frost Blackman—equal; and the Jeaffreson Exhibition, of the value of £50, to Mr. John Wilkie, B.A., of Trinity College, Cambridge.

MR. JOHN P. HOWARD of Burlington, Vermont, has just given 40,000 dollars to build a new medical department at the University of Vermont. In ten years Mr. Howard has given 400,000 dollars to this institution.

THE EARL OF JERSEY has been elected President of the Radcliffe Infirmary, Oxford, *vice* the late Duke of Marlborough.

THE GENERAL HOSPITAL, BRISTOL.—The Martyn Memorial Entrance Scholarship of £20 has been awarded to Mr. H. F. Davis.

VACCINATION.—Mr. A. Charlton, of Brabourne, Ashford, has received a grant of £4 16s. from the Local Government Board for successful vaccination in his district of the East Ashford Union.