An Address

ON

SOME INSUFFICIENTLY RECOGNIZED POINTS IN THE DIAGNOSIS OF DISEASE.

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BY

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GENTLEMEN,—The title of this address may not improbably excite some wonder in the minds of my audience. It may appear to be a bold step to assert in these days, even tentatively, that there still remain any points which elude or fail to receive full consideration in matters relating to diagnosis. It is now a mere platitude to speak of the extraordinary progress that has been made in the science and the art of medicine in the course of the last fifty years, for this is recognized on all hands, and we know well that fresh fields of research are being opened up daily by skilled and assiduous cultivators. So much so is this the case that it has become well-nigh an impossible task for the hospital teacher or the most diligent practitioner to follow up and utilize the products of these investigators. Never in the history of medicine was the first aphorism of Hippocrates borne in upon us with greater weight and solemnity than in this twentieth century, and we may well ask ourselves, Who, indeed, is sufficient for these things? To illustrate my statement, I will only ask you to compare the classical treatise of Watson—which was the textbook in medicine of most of the greybeards now in our profession—with the outcoming new edition of Allbutt's *System of Medicine*, for this provides us at once with a standard wherewith to measure the progress that has been attained in our day. It is, indeed, startling and amazing, and I venture to think that no such advance can be placed to the credit of any other learned profession. I grant at once that the engineer, the electrician, and the astronomer may point to remarkable progress; but, if we regard the relation of their splendid efforts to the general welfare of our common humanity, I am bold enough to declare that our progress is distinctly the greater and more important.

Notwithstanding this notable advance in our knowledge, we appear to have arrived at a curious stage as regards medical doctrines at the present time. We hear it widely affirmed that our old medical and surgical books are now effete, and rather misleading than helpful. We witness a spreading wave of incredulity as to the virtues and value of many therapeutic measures which were practised and commended by our predecessors. We are inclined to ignore the preparations of our *Pharmacopoeias* and to employ the latest products of synthesis and many extraordinary combinations of food and medicines for our patients.

We lend a willing ear to the last declarations of the laboratory experts, and proceed to adopt their suggestions. I suppose this has to be to some extent, but I greatly fear that in such proceedings we run the risk of crowding out a good deal of soundly acquired and proved experience—if, indeed, this has ever been acquired—and of becoming medical scientists rather than expert medical practitioners.

Let me not be misunderstood. I am in no degree averse from the best and most necessary researches of all our laboratories. These must be prosecuted, and especially those which are carried on in clinical laboratories, of which we still have too few in our great medical schools in this country; but I maintain that in the best interests of our patients there are limits to the too ready employment of such measures as are now month by month foisted on our attention. Let us consider what is the meaning of experience in our profession. What is it that we call for, and fall back upon, in any grave crisis in our clinical work? Is it not the opinion formed by the man who has seen much of disease, who has watched it and pondered over it, and best conducted his patients through their maladies?

Is it in such crises that we *instinctively* appeal to the last experiences of the laboratory operator? Some of you will doubtless reply to me in the affirmative, and tell me of the hopelessness of treating diphtheria, tetanus, anthrax, and other toxic invasions on any other basis than that of the assured benefits derived from the laboratory, and, if so, I am entirely with you. And if you have come to regard *all* disease as the result of microbic or otherwise toxic intrusion into the body, your pathogeny is at once simplified, and you are henceforth the slaves of the laboratory, and have become administrators of serums and antivaccines. Well, gentlemen, I, for one, am not yet prepared to declare myself a convert to such extreme views. A long clinical experience bids me to keep my head clear, if I can, in these days (and it is no easy task), and to see all things medical with a *mens sana* and a *mens medica*. I have found that a large part of our best art is still founded on empiricism, and that we cannot give reasons for our practice which will satisfy the purely scientific mind. In daily practice, for the best interests of my patients, I am content to be a medical artist rather than a medical scientist, but I am ever ready to avail myself of the best science that comes to me with the approval of careful experience.

If we are to wait for a scientific explanation of every step we are compelled to take in our daily work, I much fear that our patients will also have long to wait for relief, and that we too shall have long to wait for reputation. This is, indeed, one of the great difficulties that beset us. It renders it hard, and in some cases an impossibility, for certain men in our profession to practise efficiently. They cannot rest content with mere empiricism; they want a scientific reason to justify every dose of medicine and to support every method they employ. Not finding it, they leave our ranks, and are tempted to scoff at our efforts as mediæval, tentative, and uncertain. This is a clear indication, as I venture to think, of the absence in these men of the *mens medica*. No, gentlemen, the best practice of medicine is not yet altogether scientific, and if we are to help our patients, we must for the present do what carefully acquired experience justifies us in doing. Our art is, indeed, based upon many sciences, but it is not yet scientific. It is certainly becoming more so year by year, as we gratefully acknowledge. In the meantime we have carefully to cultivate the art of medicine, taking advantage gradually of every scientific fact which is of proved utility.

These considerations, preliminary indeed, bring me now to the topic on which I desire to speak this evening, namely, to urge the importance of a study of the patient no less than a study of his disease. I am strongly impressed with the belief that there is a tendency at present to pay rather too exclusive attention to the pathological process which is in progress, and to neglect the peculiarities of the individual and the conduct of his particular constitution in relation to the particular illness. In the case of a manifest toxic invasion, I think we are now rather too apt to be engrossed with the *microbe* and its delinquencies, and to forget the equally important matter of the *soil* in which these work out their mischief.

We are disposed to regard the soil, or tissues, of the patient as an indifferent medium, akin to the agar-agar of the laboratory, equally ready to react and encourage the development of such processes as may be expected to follow on inoculation. A little consideration of the matter plainly corrects this view, and clinical experience proves that the soil is commonly as much concerned in the process as is the seed. The personal factor in disease has always to be reckoned with at the bedside, and from the nature of the inquiry here this point cannot be solved in the pathological laboratory. This constitutes, indeed, one of our great difficulties in experimentation. I would illustrate this very simply by taking the case of the transmission of bovine tuberculosis to the human subject. As you are aware, there is a preponderance of scientific opinion to the effect that such transmission is not only possible but certain, in defiance of Professor Koch's strong view to the contrary. I am not aware that there are certain facts determined as to the direct infection by milk of tuberculous cows in the case of human subjects. We cannot deliberately administer obviously tuberculous milk to children to prove this, but we know that large quantities of such milk are daily partaken of by children and adults, since so many apparently healthy cows are found to be

good milkers, although obviously vulnerable to tuberculin when tested by it.

The experiment is thus being made for us in the homes of too many of our fellow-creatures, and if the results are not more serious than might be expected, we may explain the fact by assuming that there is often an inherited immunity which is protective, or by finding that the infection has been arrested in the mesenteric glands, and rendered inoperative for further development. Dr. Nathan Raw, of Liverpool, who has studied this question with much care, informs me that no actual proof exists of the production of tuberculosis in children, as no one has made the experiment of feeding with milk known to be tuberculous, but he believes that all the collected facts point in the direction that abdominal tuberculosis occurring in the milk-drinking period is due to infected milk, more especially as the bovine bacillus has been isolated from the mesenteric glands by both the British and German Commissions on Tuberculosis. He mentions further, that in those countries where raw milk is not taken, abdominal and milary tuberculosis is practically unknown.

Professor Koch appears now to be wavering in his opinion. He agrees that bovine tuberculosis may be conveyed, but is not yet prepared to admit that pulmonary tuberculosis is produced by it. Our position, as clinicians, is quite clear in the matter for the present, and we must stand by the British view of the infection from a bovine source, and act accordingly, and without delay.

The personal or individual factor is probably different in the case of the lower animals as compared with the human subject, but in the former there are found to be differences of susceptibility, even in individuals of the same class.

Those who see much of family practice over long periods become aware of family and personal peculiarities which tend to modify the incidence and course of various diseases, many of which are not so manifest to those who, as Sir Andrew Clark used to say, "see sick people, but have no patients" in the ordinary sense. A knowledge of the antecedent history, both family and personal, is necessary to a full understanding of this matter, and this is not readily secured amongst the poorer classes. I think we often fail to make, or try to make, what is called a physiological diagnosis of our patient—that is, to judge promptly, by practice of it, as to the habit of body or diathesis of the individual. Here, again, we are apt to regard most individuals as alike in tissue proclivity and tendency, and to expect the varied processes of disease to act indiscriminately upon each of them. Yet we find that this is not the case, some reacting mildly and others gravely; a toxic dose being borne well and disposed of favourably in the one, and in the other rapidly proceeding to fulmination and death. Again, we meet with absolute immunity in certain individuals, a condition not yet fully explained. We note the greater liability to some diseases at certain developmental epochs, a matter of importance, not seldom, in difficult and puzzling cases. Thus, amongst those which present unusual symptoms in children and young adults, we may often find the explanation in tuberculosis; in adults up to middle life, many strange and untoward symptoms are found to be dependent on unrecognized syphilis; while, in the aged, we find that some of the most puzzling conditions are sometimes ultimately resolved by a sharp attack of acute gout. Let us consider next two of the commonest diathetic conditions we have to deal with—the scrofulous or strumous habit, and the arthritic, often called rheumatic, habit. The facies in each is sufficiently distinct; the former presents two main varieties, known respectively as the "ugly" and the "pretty" struma. The "ugly" variety is that commonly described in textbooks. Its characters are the full, big face, swollen neck, sallow complexion, tumid lips, with catarrhal manifestations. The "pretty" variety presents finely-cut features, with delicate complexion, fine hair, long eyelashes, and various qualities which clearly indicate both beauty and delicacy. These two types present us with features which are both vulnerable, and a ready prey to the onset and development of tuberculosis should infective intrusion occur. The "ugly" variety it was which first gave rise to the term "scrofula," from the full and short, porcine quality of the neck. Note, that neither of these may be called in the first instance *tuberculous*. They present a soil ever ready to lead to tuberculous infection.

The "ugly" variety, indeed, is a veritable forcing ground for every kind of vicious microbic intrusion, so that such individuals are the worst subjects for all infections, and have every ailment in a grave form. They succumb in early life to these, or manifest very indolent recuperative power. They rarely recover from nephritis, and in later life are the worst subjects of syphilis if smitten by it.* This condition also modifies the course of many illnesses, and presents strange blends of morbid states when mixed infections occur. For instance, in the tropics, I am inclined to believe that such subjects are specially prone to suffer from hepatic abscess.

We now find these individuals described as tuberculous *ab initio*. This I regard as a grave error. *They need never become tuberculous, although they are a ready prey for the infection.* In the "pretty" variety, there is more particularly a tendency to mesenteric and pulmonary tuberculosis, and perhaps to articular disorders. I think we do not adequately recognize these facts at the present time. We may regard these subjects in respect of tuberculosis as the *ens in potentia*, and when definitely infected as presenting the *ens in actu*. The moral of it all is when detected as specifically vulnerable elements in a community to protect such subjects, even more than others, from tuberculous environment and infection.

Let us consider next the subjects of another habit of body—namely, the arthritic or so-called rheumatic. They present a peculiarity and delicacy of constitution very different from that of the scrofulous. We note in them an absence of the vulnerability of the lymphatic system which is so marked a feature in the subjects of struma. The facies does not suggest delicacy, there being neither tumidity nor sallowness. There may be even in early life some vascularity of the face and a general good development. The tendency is to suffer from the various manifestations of rheumatic poison, whether developed in the throat, the skin, the heart, the joints, and the brain, especially during adolescence, the tendency to rheumatic infection passing off more or less after the third decade. Later in life some of these subjects become gouty, and are apt to suffer from myalgia, fibrositis, lumbago, sciatica, and neuritis, and many of them present examples of arterial sclerosis and contracting kidneys about the fifth decade. The physiognomy in the latter case is very different from that of the subjects of chronic parenchymatous nephritis. One of the most marked features of this diathesis is its resistance to tuberculous infection—a point little regarded. Even if tuberculosis is acquired it has a hard struggle to progress, and is either arrested or held in check for many years as a rule. It has been humorously asserted, indeed, that one way of avoiding tuberculosis is to grow gouty; and truly what is bad for gout is very good for tuberculosis—namely, full dietary, or what the French call forced alimentation. The prognosis in arthritic subjects, if no cardiac complications have occurred in early life, is good for longevity provided a suitable environment is secured, and if what Sir Andrew Clark termed "physiological righteousness" is observed.

I have been speaking of what may be regarded as pure specimens of these two habits of body; but, of course, we meet with many examples where there are blends of constitutions, intermixtures of diathesis, in which one condition may be more prominent than the other, yet potential for modification. Hence we find blends of struma and rheumatism, a bad conjunction; and blends of gout and struma, a rather less grave coalescence—less grave, I say, because the gouty element generally acts protectively, and permits of a better prognosis for longevity. Many of these arthritic subjects manifest a high arterial tension in middle life, and so, as Sir T. Clifford Allbutt suggests, we may be concerned for them should they fall victims to pneumonia, or, in later life, suffer from cerebral hæmorrhage. Such tendency, of course, puts us on our guard and demands attention to habits of life and diet. Yet another peculiarity pertains to arthritic subjects, and it is their singular susceptibility to the gonococcal virus. This infection is apt to be more serious in some than in others, especially in those of gouty proclivity, manifesting a special vulnerability in

* I find that this point was noted by W. Heberden at the end of the eighteenth century. "Struma et lues venerea nonnunquam eundem hominem simul occupant: quod ubi fit, altera alterius nocendi vim plurimum augeat." *Commentarii de Morborum Historia et Curatione* (Cap. lxxvii, de Struma.)

their textures. In these subjects, too, we meet with Dupuytren's contraction and the curved little finger, known as "Landouzy's campodactylie," which in my experience have no origin in purely rheumatic conditions. They are further liable to a hyperinotic state of the blood, whereby there is a special tendency to phlebitis and blood-clotting, a point to be remembered in the puerperal state in the case of gouty women, who are more than others liable to pulmonary arterial thrombosis. A due consideration of clinical facts such as these surely lends support to the doctrine of the diatheses which is at the present time too much discarded, and even denied and derided by some. I regard them as *fundamental* and amongst the certainties of medicine, and to disregard them is, I venture to think, an indication of the absence of clinical acumen in the disputant. This tendency to ignore the personal factor in diseased persons is probably to be attributed to our modern devotion to laboratory revelations, and our less assiduous observation in detail of the human patient actually before us. Being over-engrossed with the seed, we are neglecting the soil, and, as physicians, we have to take good heed to both in our daily duties.

There is much to be said in favour of a nervous or neurotic habit of body, where we find plain evidence of instability of nerve centres, with tendency to break down in various parts of the cerebro-spinal axis—a condition plainly inherited and transmissible, often blending with other diathetic proclivities. For an example, take the case of chorea, well termed "cerebral rheumatism." Here we have the distinct rheumatic infection, together with nervous erethism and instability. The rheumatic and choreic child is commonly the nervous child in a family, as pointed out by Cheadle. Other members may be infected, but they do not present vulnerable points in their cerebral cortex.

Neurotic disorders do not present gross textural change, but simply perverted nervous action. In the group of "family" diseases of the nervous system we find the inborn tendency to a breakdown of some nervous tracts, which may occur definitely in several members of a family, in one or successive generations, in early or late adolescence.¹

We may acknowledge a difficulty here, because we have in many cases no particular physiognomy to guide us in determining the subjects of neurotic tendency. We have, however, other features in abundance to observe, and especially the personal and family life-histories, so far as we can gather them, to throw light on individual cases. There may be a solitary case in a family, but antecedent histories may help us to find a clue to the particular infirmity which has cropped up.

Myxoedema and cretinism in an early stage may long remain unrecognized unless we are on the look-out for the peculiarities of the disease as disclosed by a change of physiognomy and of habits. The same may be said in regard to early phases of paralysis agitans before the onset of the tremors; thus, the mask-face, and the slow gait, with mental hebetude, may long precede the characteristic tremors, the piping voice, and the sweatings. If we pay too much heed to the disease, and neglect to study the patient, we may miss many indications of early phases of disease. Where a long report has to be made on a person under examination, say for a life assurance policy, it is possible to be so much engrossed in recording answers to questions that we fail to study the individual before us sufficiently, and so miss the fuller appreciation of the particular features of the case. Some of these insurance papers, especially the American ones, are so full of details that they are apt to lead to an inadequate appreciation of the examinee before us, and to occupy too much time.

The study of localized pain requires extreme care, and a minute series of inquiries and examinations which demands much time and thought if an exact diagnosis is to be made. A flippant diagnosis of "rheumatism," or "neuritis," or "indigestion," may be very wide of the facts before us, but we are expected to form an opinion with rapidity and certainty, and it may be a matter of days before we can trace a pain to its true source. The patient and his friends do not understand our proper hesitation and postponement of a satisfactory opinion on the case.

In cases of apparently simple anaemia, especially in males in middle life, we may sometimes discover the

reason of our inability to promote recovery by attending to the minute histology of the blood, and, finding an undue eosinophile count of leucocytes, be led to the discovery of ankylostomiasis, which is perhaps more common than is generally believed, owing to the extended travels of our population at the present day.

The numerous varieties of skin diseases present us with a vast field for the study of constitutional states of body. They almost always bear on this question, and their removal commonly rests with a recognition of the particular personal habit, the strumous tendency to pus formation, the arthritic to serous effusion or scaling with pruritus, the bilious to xanthelasma, and the diabetic to vitiligoidea. Since the study of botany has been neglected in recent years in the medical curriculum, there has been, as I believe, a loss of discriminating power in our students, and less ability for accurate differentiation in common observation—a mental faculty greatly encouraged by early botanical study. The Edinburgh school has always recognized this, and insisted on its importance for the future physician, and, as I venture to think, with much benefit. Indeed, I believe that a little more botany and careful study of the *materia medica*, and a more assiduous observation of the action of some of our old approved remedies, would assuredly sharpen the clinical wits and acumen of our modern students and practitioners. Some measure of the over-elaborated studies in chemistry might well be curtailed to allow of this—certainly in the case of the average pupil, who has soon to make his way in general practice.

In my clinical teaching I have sometimes said to my pupils that there are almost certainly things that we are looking at to-day in our patients which, as yet, we do not see. Yes, looking at, without seeing, things which some one will some day see and recognize, and will wonder how we in our day failed to see and interpret them. This has been the case in the past, and it will assuredly be so in the future if we patiently carry on our physiognomical studies at the bedside. I find that Browning has put this idea in the following pregnant lines:

I tell you, men won't notice; when they do
They'll understand.

Lastly, allow me to hope that you will not regard these expressions of opinion as mere *obiter dicta*, suggestive of servile conservatism on my part, for I have come to these conclusions after a long experience and much thought. The views I have ventured to lay before you, imperfectly, as I well know, are not new. I think they are at the present time insufficiently recognized and applied in daily practice by many of us. At the end of the sixteenth century we find that they had not escaped the reflection of one of the greatest philosophers of all time, for in his *Regimen of Health* (1597) we may note the following passage by Bacon (Lord Verulam): "Some Physicians are so regular in proceeding according to art for the disease, as they respect not sufficiently the condition of the patient." This is the moral of my words to-night, and I will leave it to your careful consideration.

REFERENCE.

¹ Dr. Ormerod, Harveian Oration, 1908.

DR. F. W. TWORT, in a paper on the influence of glucosides on the growth of acid-fast bacilli, presented to the Royal Society on March 4th, described a new method of isolating human tubercle bacilli directly from tuberculous material contaminated with other micro-organisms. Among 43 glucosides tested with acid-fast bacilli it was found that one—ericolin—killed off a large number of species of micro-organisms, especially bacilli of the colon group and various cocci, but had very little effect on the acid-fast group of bacilli. By means of this glucoside, the isolation of tubercle bacilli directly from human sputum contaminated with other organisms became quite easy. A lump of sputum was placed in a 2 per cent. solution of the glucoside in distilled water, and kept at 38° C. for three-quarters of an hour to an hour. Pure growths of tubercle bacilli could then be obtained on Dorset's egg medium in fourteen to twenty-eight days. The tubes were sometimes contaminated with a few other organisms, chiefly tiny colonies of streptococci and slow-growing organisms of the streptothrix group, but they were so few that they in no way interfered with the tubercle colonies, which could be easily subcultured.