An Address

ON THE

DIAGNOSIS OF FEVER IN PATIENTS FROM THE TROPICS.

DELIVERED AT A MEETING OF THE WESTMINSTER DIVISION
OF THE METROPOLITAN COUNTIES BRANCH.

BY

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I HAVE twenty minutes in which to speak about certain points which have to be attended to in attempting the diagnosis of fevers in patients coming from the tropics. The time is very short. I shall not waste it, therefore, in preliminaries, but proceed at once to my subject.

Sources of Fallacy.

The first point I shall urge is a very important one. It is the necessity for the diagnostician to disabuse his mind of the very natural idea that because a fever has been contracted in or is occurring in a patient from the tropics it must necessarily be a tropical fever, symptomatic of some infection or condition peculiar to the tropics. This in my experience is one of the commonest and most misleading diagnostic fallacies. It so happens that my line of practice lies in great measure among patients from the tropics; but I am bound to say that half the patients from the tropics sent to me for an opinion or who come to me under the idea that they are suffering from tropical disease are not so suffering, although very likely they have fallen sick in the tropics or soon after return from the tropics. When you have dealings with a Scotsman you are apt to be obsessed with the preconceived idea of the national reputation for canniness, forgetting that, in the main, Scotsmen are very like other men, having the same physical, moral, and mental attributes. Just so, and perhaps even more so, in our contemplation of disease from the tropics. The major portion of a Scotsman's attributes is that of other men; only a very minor portion is peculiar. The major portion of disease in and from the tropics is ordinary disease; the minor portion special. Therefore when you encounter a fever in a patient from the tropics, think last and least (unless the diagnosis be glaringly obvious) of a tropical fever. Think first of and carefully test for those great and pandemic conditions—tuberculosis, syphilis, typhoid, malignant disease, and sepsis. If the seat and nature of the disease are not at once obvious, make it an inflexible rule to go over all the organs systematically, one after the other, beginning at the vertex and ending at the soles of the feet. I could tell many a story illustrative of the wisdom and necessity for this precaution—so obvious when stated thus pointedly, but, like so many other obvious things, so frequently overlooked or ignored. This is the first and perhaps the most important point I would make.

The next and equally obvious point I would impress on you is not to be misled by the diagnosis of malaria which in many instances the patient is nearly sure to volunteer. Patients' statements in this respect are apt to be very positive and correspondingly impressive. Such tropical cases come to you not so much with the idea of your diagnosing their fever as with the idea of getting you to treat them on their own diagnosis. I have seen many cases of tuberculosis, of endocarditis, of liver abscess, of pyelitis, of syphilis overlooked for this reason. It should be an axiom with us never, without a thorough and independent examination, to accept another man's diagnosis, least of all a patient's diagnosis.

Having excluded as far as you can these sources of fallacy, then, and only then, you may conclude that the fever, let us suppose, you are trying to diagnose is probably a tropical one. Your next step should be to put to yourself the question, What are the tropical diseases likely to be brought to this country and which are associated with fever? Of course, we may safely exclude such acute and short fevers as yellow fever, dengue, and so forth; these need not be considered.

Tropical Fevers.

Let me enumerate what I might designate the important tropical fevers in the approximate order of the frequency with which they present themselves in practice here. First of all, of course, comes malaria; next, perhaps, hepatitis and liver abscess; then Mediterranean or Malta fever; next, and at a long interval, kala-azar, trypanosomiasis and sleeping sickness, relapsing fever, elephantoid fever, and probably other infections about which we as yet know nothing, but only suspect their existence. Each of the fevers I have mentioned has some feature or features by which it may be recognized, or, at all events, suspected.

Tests of Malaria.

Manifestly our first duty is to recognize or to exclude the commonest of them all—namely, malaria. Fortunately, this is easily done. Provided we set about it in the proper way and have a little time allowed us there is no disease so easily and so surely recognizable as malaria, for of this infection we have not one or two, but three absolutely pathognomonic tests. I am in the habit of describing these tests as, *first*, the clinical test of periodicity; *secondly*, the therapeutical test of the action of quinine; and *thirdly*, the microscopical test, the determination of the presence or absence of the malaria parasite or of its product, malarial pigment, in the blood.

There are other indications of malarial infection, such as leucopenia with relative increase of the large mononuclear leucocytes, enlargement of the spleen, and anaemia. These are only of relative value. Their absence is strong evidence against malaria, but their presence, seeing that they occur in other tropical diseases, does not prove the presence of malaria. They are not absolutely diagnostic in the same sense as are the three tests I have just mentioned, and need not be further considered.

The most important clinical test of malaria is *periodicity*—the periodic recurrence of the febrile or other phenomena. Practically all fevers, whether malarial or not, exhibit a periodicity. In tuberculosis, in typhoid, in sepsis, and so forth, there is a regular evening rise and morning fall of temperature, often quite as marked as in malaria. There is very definite quotidian periodicity. Quotidian periodicity is therefore not peculiar to, is no diagnostic mark of, malaria. We do meet with quotidian malarial fevers, especially in malarial countries. But quotidian periodicity, if taken alone, does not justify a diagnosis of malaria. So far from doing so, it is actually misleading. It is perhaps the most frequent cause of erroneous diagnosis in tropical patients. This you can readily understand. A patient from India, for example, comes to you with a story that every afternoon he has a shivering fit followed by a rise of temperature to 103°, and this again after some hours by a drenching sweat. He may mention no other symptoms. You may be in a hurry. You pump for malaria, and you prescribe quinine. The patient does not improve. You make a careful physical examination, and you discover signs of tuberculosis, or of liver abscess, or of some other form of visceral disease.

Quotidian periodicity, therefore, should be absolutely ignored in most cases as an indication of malaria. The periodicity characteristic of malaria, and absolutely diagnostic of that infection, is either a *tertian* or a *quartan periodicity*. These you find in no other condition, and are sure indication of malaria. The only circumstance in which quotidian periodicity may be a help in diagnosis is when the recurring fever sets in very late in the night, say after midnight, or before 12 or 1 o'clock during the day. Such a time for the commencement of a daily fever is almost peculiar to malaria.

The quinine test for malaria has usually been applied more or less intelligently before the case comes under the observation of the consultant. It is reliable if properly applied. Rarely does a malarial fever, in this country at all events, resist adequate dosing with quinine. Ten grains two or three times a day is almost sure in forty-eight to seventy-two hours to tell us whether we are dealing with malaria or not. But in employing this test we must be sure that the quinine is given properly, and that it is absorbed. Very often the quinine is given in adequate dose, but in some insoluble form, as in coated pill, flinty tabloid, or insoluble sulphate. In catarrhal conditions of the stomach given in any of these forms the

drug may not be dissolved, much less absorbed, and cannot therefore be regarded as efficiently testing for malaria. When it is of importance that we should be certain of its action, quinine should be given in solution, or, in highly catarrhal or irritable conditions of the stomach, intramuscularly in doses of 7 to 10 grains. If no impression is made on a fever by quinine given in this way, do not blame the drug; revise the diagnosis.

Even more reliable than the clinical or the therapeutical tests of malaria is the microscopical test. If the malaria parasite or its product—haemozoin, or melanin, as it is usually called—is found in the blood, diagnosis is sure. The parasite of malaria is necessarily present at one time or another in the course of all malarial infections; it is always present in the visceral blood, nearly always in the peripheral blood, and, given certain conditions, can be readily demonstrated in the latter. It is necessary, however, to secure these conditions. In the first place the patient must not be under the influence of quinine, in the second place the person who searches for the parasite must know his business. Even a small dose of quinine—one, perhaps, quite insufficient to check the fever—may cause the parasite to disappear temporarily from the peripheral circulation. The possession of a microscope, and even skill in other departments of microscopy, do not always imply ability to recognise the malaria parasite. To do so satisfactorily requires experience—special experience—and long training. It is not a difficult matter, but, as with everything else, you must know how to set about it, and be familiar with the fallacies. It would be a comical list were I to enumerate all the various objects that have been brought to me as specimens of the malaria parasite.

I would warn you, therefore, to be careful about accepting a diagnosis of malaria from an inexperienced microscopist, but I would encourage you to have absolute confidence in the positive diagnosis, and in ninety-nine cases out of a hundred in the negative diagnosis of malaria from an experienced and conscientious microscopist.

Liver Abscess.

Assuming that we have to deal with a tropical fever and that by one or all of the tests I have enumerated we have excluded malaria, the question comes to be, which of the several tropical fevers I have mentioned are we dealing with?

Is the case one of liver abscess? The first and all-important question we put is—has the patient had dysentery or diarrhoea? If so there is, to say the least, strong presumption in favour of such diagnosis. We search, therefore, for local signs, for enlargement of the hepatic area, especially upwards, for local pain, oedema, or even redness. We inquire as to anaemia, progressive emaciation, for irritability and depression of mind; we look for a muddy complexion; we inspect the stools, looking for slime or other indications of a former or an existing dysentery; we inquire for a dorsal or right dorsal decubitis, for shoulder pain, and we make a count for the white corpuscles in the blood—a leucopenia being against, a leucocytosis being in favour of, liver abscess. Finally, if the symptoms are reasonably suggestive we explore the liver under chloroform, being prepared to operate at once if abscess is discovered.

Mediterranean Fever.

We may suspect Mediterranean fever, more especially if the patient has come from Malta, although this disease is by no means unknown elsewhere—as in India, China, and even in Central Africa. The points in favour of a Mediterranean fever diagnosis are an undulant type of the fever, profuse sweats, the occurrence of marked rheumatic pains or of orchitis, and the absence of indications of other disease.

Apart from the symptoms mentioned the evidence for this fever is principally of a negative nature. The fever may assume all sorts of characters. Often it is undulant in type, but as often it is distinctly intermittent and quotidian, often of a low continued type, often a medley of all of these. The serum test is reliable under ideal conditions, but my experience of it in London is the reverse of favourable. When I employ it I usually send the blood to two different laboratories; as often as not I get "positive" from one and "negative" from the other. So I do not trust it here, although, where fresh cultures are

obtainable, it is quite as trustworthy as the corresponding test for typhoid, and even more delicate.

Kala-azar.

We have a patient from India, from China, from the Soudan, or from North Africa. He has a chronic fever, his spleen reaches to near his umbilicus, and his liver is very much enlarged. He has been ill for months; he is anaemic; his tongue is clean and his appetite and digestion are good; he has taken quinine by the pound, and he is gradually going downhill. Probably that patient is suffering from kala-azar—the disease produced by the Leishman body. To make sure of the diagnosis we study the fever chart—a four-hourly one; very likely we note that there are two distinct rises of temperature in the twenty-four hours. We examine the blood; there is a very marked leucopenia, more marked even than in malaria, and there is a relative increase in the large mononuclears. Possibly, though this is not likely, we may find a Leishman body or two, if we search long enough, in the white blood corpuscles. In the presence of such a fever and such a history we are entitled to puncture the spleen or liver and to search for the Leishman body in the juice or fragments of pulp so obtained. Such a procedure is not free from risk and must be done carefully, aseptically, and with a dry needle and syringe. I say "dry needle and syringe," for if a trace of moisture be present in these it will, by endosmosis, so distort the parasites that, though present, they may be hard to recognize. Of course, one must be familiar with the technique for their demonstration and also with the details of the structure of the parasite, for it is exceedingly minute and might be mistaken for a micrococcus or a blood platelet.

Trypanosomiasis.

The patient comes from tropical Africa. He complains of irregular fever, of great physical and mental lassitude, headache perhaps, tenderness of the limbs when he knocks them against any hard body. You suspect trypanosomiasis. You strip him and inspect his skin. You see great patches of erythema, many inches in diameter, usually having a ringed appearance and looking slightly puffy; you palpate the glands in his neck, axilla, or groin, and you find that some or all of them are enlarged—perhaps only slightly enlarged. The pulse, as a rule, is abnormally quick, and easily excited. That patient is almost surely the subject of trypanosomiasis, and may die of sleeping sickness. Examine his blood with a sixth objective, examining it especially during one of the recurring attacks of fever, and you are almost sure to find the trypanosome. It will not be found in every field of the microscope, and you may have to return to the hunt several times, but in the end you are almost sure to find it. If you fail to find it in the blood, puncture with a hypodermic needle one of the enlarged cervical glands, and examine the lymph so obtained; in it you have even a better chance of finding the parasite. The blood count is very similar to that of kala-azar.

Relapsing Fever.

The patient comes from India, from tropical Africa, from North Africa, or even from Gibraltar. He tells you that he has attacks of fever, perhaps violent fever, regularly about once a fortnight; that the individual attacks last from three to five or six days, that they subside nearly as suddenly as they begin, and that he is quite free in the interval. He may have had three or four or even eight or nine such attacks. What are they? The blood is negative for malaria; there is no marked leucopenia. Examine the blood during one of the fever paroxysms, and probably you will find the spirochaete of relapsing fever. In the African variety it takes some looking for. If you find it diagnosis is established. Such cases I have seen more than once in recent years in London. They were imported from Africa, from Gibraltar, and from India.

Elephantoid Fever.

Another patient may tell you he has attacks of violent fever coming on at irregular intervals of weeks, months, or years, that the attacks last for two or three days, and may be attended with severe rigor, delirium, high temperature, and be followed by profuse sweating. If he comes from the West Indies, particularly from Barbados, he will call this disease "fever and ague," but it is not

fever and ague as we understand it. It is not malaria, but elephantoid fever for the most part, and if we inquire as to the occurrence of inflammation and cellulitis of limb, scrotum, or acute lymphangitis, we are sure to find that such is the case. The patient is suffering from elephantoid fever, and is or has been the subject of filarial invasion.

The possibility of these various and very different infections should always be present to the diagnostician when he is called on to treat a tropical fever in this country.

Multiple Infections.

I began with a word of warning; I shall conclude with another word of warning, and it is this: Do not infer because you have found in your patient's blood or elsewhere the malaria or some other parasite, that you have the complete and full explanation of the case. In tropical disease malaria is apt to complicate everything, so that multiple infection of patients is rather the rule than the exception.

When you find the malaria parasite the patient has certainly got malaria, but that does not exclude other infections. I have sometimes been "caught" in consequence of ignoring this obvious precaution. Some years ago I was asked to see a patient just returned from Portuguese West Africa. He was said to be suffering from fever and dysentery. He had dysentery sure enough, and his spleen was enormously enlarged. He had taken much quinine; as it seemed to irritate his bowels I stopped it. At my first visit he had no fever. I found nothing in his blood. I left instructions that I was to be sent for should he have an attack of fever.

Some days afterwards I was sent for, his temperature being over 103°. I took a slide of his blood, expecting to find in it the parasite of malaria. Judge, however, of my horror when, instead of the malaria parasite, there was an unquestionable trypanosome staring me in the face! After a few days the fever disappeared, and, with the fever, the trypanosome also.

A fortnight later there was again a return of fever, and I again examined the blood, expecting to find the trypanosome. I found no trypanosome, but I found plenty of tertian malaria parasites. And so the case went on, every now and again a fever spell with trypanosomes in the blood, and every now and again a fever spell with malaria parasites in the blood. By the persistent use of atoxyl and of quinine both infections were finally expelled from the circulation. The patient is now, I believe, quite well.

Last year I had in hospital a patient from an African colony who carried about with him the malaria parasite, the trypanosome, the *Spirochaeta pallida*, the filaria, besides an assortment of intestinal parasites, including *Ascaris lumbricoides*, *Trichiurus trichiurus*, and *Ankylostoma duodenale*—a veritable museum, which, as long as it remained with us, we appreciated very highly at the Tropical School. He could always supply us with a subject for demonstration or for a clinical lecture.

I fear my exposition of the subject has been very sketchy and inadequate; it is necessarily so in consequence of the time limit imposed on me. I trust, however, I have given you the leading points for reliable diagnosis.

COMMON MISTAKES IN OPHTHALMIC PRACTICE.*

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IN casting round for a subject on which to write a paper which would prove interesting and useful, the idea came to me that I should formulate my impressions, gathered from twenty-five years of eye work, of the mistakes most frequently made by the medical men who have seen, and usually treated, eye cases before they came under my notice.

I use the word "impressions" because I have no statistics to give you. I do not commit to paper any mistake that I may think another man has made; but

yet, when one sees, as one does see, a considerable number of cases wrongly diagnosed and indifferently treated, they in a way sort themselves and occupy separate pigeon-holes in the recesses of one's brain. I should be sorry if this paper led any one to think that I pass a harsh judgement on a man who makes a wrong diagnosis on an ophthalmic case. I have lived too long and made too many mistakes myself to do this, and, moreover, I know very well the limited amount of experience in eye work that in the ordinary run of work falls to the lot of the general practitioner; and so, if any one finds in this paper a cap that he knows fits him well, let him not think that it is specially designed for his head, but rather consider that his head is fashioned on somewhat cosmopolitan lines, and is therefore capable of being fitted at the first ready-made and reach-me-down hat shop that he chances to enter.

Refraction.

The class of cases that provide the most numerous mistakes are undoubtedly refraction cases. One would expect this because refraction work forms such a large proportion of eye work, but I should think that, taking mistakes in this branch alone, they are quite in the proportion of 9 to 1—that is, that in 10 mistakes in eye practice 9 are due to bad refraction work. Nor is it astonishing that so many mistakes are made in this department, for while it has always seemed to me that constant practice at it is essential to its satisfactory performance, the public, and not a few of our profession, seem to consider that any one who sells spectacles is qualified to advise upon refraction troubles. When I occupied—as I did for twenty years—the post of assistant surgeon at the eye infirmary, where the bulk of the work is refraction, I always found that when I returned from my summer holiday my work took me half as long again as it was doing when I went away. Moreover, accuracy comes with speed, and you decide without hesitation such points as whether you shall give full correction in a hypermetrope, or whether it is worth while to give this child cylinders, or wise to give that myope a second pair of spectacles for near vision. These, however, are minor points, though they have oftentimes an important influence upon the reputation of the man whose work is bettered. The gross mistake, and the one which one most often sees, is where ciliary spasm in a hypermetrope is treated with -2 D. spectacles or -1 D.

The type of this is a young dressmaker, or perhaps a tailor's apprentice, who comes complaining that after a few hours' sewing things become misty and her eyes ache. She gets some relief by holding her work nearer to her eyes; but now this manoeuvre fails to relieve, and she is sure she needs spectacles. You test her, and find that she can read perhaps $\frac{2}{3}$ distant vision. You try her with a -1 D., and she says, "Oh, that's lovely; that's what I want"; or "Oh, yes, the print is beautifully clear." You provide her with the -1 D. or -2 D., whichever she sees best with, and she goes away seeing better than she has done for months, while you proceed to your next patient with the satisfactory feeling that "that is a good case." Next day your patient goes to work with a light heart, and finds everything very nice for a few hours; then the eyes again ache and smart. She thinks that they are not yet quite recovered from the bad treatment to which she subjected them before she obtained the glasses from you, and perseveres with them. But she finds that the aching returns at an earlier hour each day, and that her headache, which used to pass off when she left work, now persists all through the evening, and that only sleep, and even that not always, cures it. She then seeks other advice, is put under atropine, found to have hypermetropia, given her proper correction, and is thereafter comfortable, and wonders why you ever gave her those spectacles, which make everything look so tiny. The explanation of course is that you were deceived by the ciliary spasm. She came to you on account of this spasm. Her ciliary muscles, irritated by months of overwork in focussing correctly in a shortened eye had got to a condition of "tonic spasm" and over-accommodation, producing temporary short sight. You had easily corrected this short sight, but by doing this had added to her hypermetropia by precisely the strength of the concave glasses you prescribed. This led to greater efforts of accommodation, and to results which neither the eyes nor the patient could possibly put up with.

*Read before the Devon and Exeter Medico-Chirurgical Society.