actually suffering the acute phases of the disease but rarely consult us. There are other infective persons who never consult us at all. For the question arises: By whom is gonorrhoea spread abroad? There may be persons in whom gonococci can live for a long time without causing any symptoms, just as there are "carriers" of the organism of enteric fever—they have not "typhoid" themselves, but they can give it to others. Carriers are not said to have "chronic enteric fever," and possibly it would be wise to drop the term "chronic gonorrhoea."

It is probable that most of the persons who transmit gonorrhoea are frequently reinfected with fresh strains of gonococci, and really are the subjects of repeated mild attacks of acute gonorrhoea. Man and wife, for example, may continue to reinfest one another time after time, so that it is only after separation that either of them ceases to be infective. After you have entered upon the practice of medicine you will avoid many complications if you always think at least three times before stating that a person is free from venereal disease. It can be acquired again so quickly. There is not much gained by seeing an habitual drunkard through one drinking bout, and it is rather futile to attack venereal disease by founding clinics where all comers can be treated at the public expense. You cannot cure drunkenness unless you can keep people from drinking.

PULMONARY SPIROCHAETOSIS.

A PRELIMINARY NOTE ON SPIROCHAETES IN THE SPUTUM OF SOLDIERS ADMITTED TO SPECIAL MALARIA WARDS.

BY

J. A. THOMSON, CAPTAIN R.A.M.C.

HAVING been for some time in charge of the laboratory at a malaria centre, my attention was drawn to the frequency with which samples of sputa were sent to the laboratory accompanied by requests for examination for tubercle. The number of the cases of tubercle was small.

Later, being put in charge of both wards and laboratory of a special hospital for malaria, I came into more direct contact with the patients and was able to see and study for myself the conditions which had caused medical officers to suspect tuberculous infection.

Briefly expressed, these conditions were:

- (a) The chronic debilitated state of many patients.
- (b) Complaints of coughs of long duration made by many of them, but not associated with definite physical signs in the lungs.
- (c) That, although sent into hospital as cases of malaria, blood examination for malaria was very frequently negative, and, although kept in hospital under antimalarial and tonic treatment, many patients did not make satisfactory progress towards recovery, but remained suspects of some other chronic condition, such as tubercle, neurasthenia, or D.A.H.

Having found spirochaetes in considerable numbers in the sputum of one patient, I systematically examined the sputum of all patients complaining of coughs, by simple stains and by the special stain for tubercle. Unfortunately I had not proceeded with this investigation for more than about three weeks when I was transferred to another sphere of duty; yet in that short time I was able to collect sufficient material to show that pulmonary (or bronchial) spirochaetosis in a chronic form was apparently prevalent amongst the group of patients I had been dealing with.

I reported my findings to Colonel Sir Ronald Ross, Consultant in Malaria, who has advised me to publish a note upon them. This is rather premature; the findings may or may not be of much pathological importance, but the subject seems to be one of interest and deserving of further investigation.

The first case in which I found spirochaetes was one that might very reasonably have been suspected of tubercle. He gave a history of chronic cough, never very severe, but dating from his period of service in Salonica. He was sallow, had lost some weight, but was neither markedly wasted or anaemic. He did not feel fit, was easily tired, and got short of breath after moderate physical effort. He had been invalided home on account of malaria, and since his arrival in this country he had been in several hospitals, with short intervals at his dépôt.

For periods of three to four days, and at about three weeks' intervals, he described himself as feeling "queer"—a feeling, as far as I could understand, of malaise and feverishness, which he distinguished from his former malarial attacks. When admitted to my wards, although sent in as a case of malaria, his blood was negative, and although kept under observation for over a month, frequent blood examinations always gave negative results. He had a morning cough, not very troublesome, but accompanied by expectoration. Physical signs in the chest were indefinite—a râle here and there, but nothing to implicate any particular area of lung. The sputum was clear and jelly-like, but he said that during his "queer" periods his sputum was blood stained, and this statement was twice confirmed whilst he was in hospital by the sputum assuming a pink jelly-like character. His temperature in hospital zigzagged above and below the normal line from 97.5° and 98° F. to 99° and 99.5° F.; there were no acute rises or rigors. Repeated examinations of sputum for tubercle were always negative, but spirochaetes were always found in considerable numbers and more abundantly when the sputum was blood-stained.

There was no pyorrhoea, but to eliminate the possibility of mouth spirochaetes instructions were given to wash the mouth and gargle the throat with weak permanganate before samples were collected, and to collect only what he definitely felt to come from the lungs.

Taking my cue from this case I proceeded to examine systematically the sputa of all cases in which cough was present. All were given the same instructions about cleansing their mouths and throats before collecting samples. Films of each case were stained for spirochaetes and for tubercle. From October 3rd, 1918, to October 21st, 1918, 79 cases were examined; 39 cases showed spirochaetes and in 2 tubercle bacilli were present.

The character of the sputa of cases in which spirochaetes were present varied. The most common type was clear, jelly-like, and non-aërated. A few cases were described as purulent, more as muco-purulent. In three cases (all without tubercle bacilli) the sputum was blood-stained; they included:

- (a) The case already described as having intermittent expectoration of pink jelly-like mucus.
- (b) One in which there were three frank hæmoptyses whilst in hospital; never very much blood at a time—about one to two teaspoonfuls amongst a quantity of jelly-like mucus.
- (c) One case of muco-purulent expectoration streaked with blood on many occasions. This case, however, had definite pyorrhoea to a moderate degree, and I was never quite satisfied that the samples were not mixed with mouth secretions. He was peculiar in that at intervals of ten to fourteen days he had definite sharp rigors, with temperature up to 102° and 103°; the febrile periods were more prolonged than those usually seen in cases of ague, lasting as a rule into the following day and terminating in profuse perspirations. I examined his blood frequently during rigors, during the febrile periods, and during the intervals between his attacks. Malaria parasites were never found, and according to his own statement, although invalided home as a case of malaria, repeated blood examinations in Salonica never showed the presence of malaria parasites.

In addition to complaints of cough many of the patients exhibited a combination of symptoms which is often seen in the wards of a special malaria hospital.

- (a) Poor general physique, weakness, and listlessness.
- (b) Tachycardia either after or without physical effort.
- (c) Shortness of breath after slight exertions.
- (d) Systolic cardiac bruits both at apex and at base, which if not heard when the patient is at rest in bed, develop readily after slight effort.
- (e) Low vasomotor tone, made very obvious by lividity and coldness of hands when the patient has been standing, and by the soft inelastic feeling of his body tissues.
- (f) Nervous debility—tremors of hands and tongue, low spirited and disinclined for mental or physical effort.
- (g) In many the temperature zigzags slightly above and below the normal line.

Although low and weak in health these patients do not show the cardinal signs of malarial cachexia so often seen amongst natives of malarial countries. There is no appreciable enlargement of the spleen, and no severe degree of anaemia. As a rule the blood is negative on admission, and remains so during their stay in hospital, but in this connexion it must be remembered that practically all patients admitted to special malaria hospitals are readmissions from other hospitals, and have already been treated with quinine.

These cases do not tend to show the marked improvement in health which frank malaria cases do when put upon antimalarial and tonic treatment; they remain suspects of some other disease.

All my cases in which spirochaetes were found were men invalided home from Salonica on account of malaria. All had pretty much the same history of chronic invalidism, months spent in hospital with occasional short visits to dépôts where they apparently never did any useful work and where they were regular attendants on sick parade. The total amount of quinine which had been taken by many could be reckoned in pounds rather than ounces. It was not always easy to determine if these men actually got fever at their dépôts; whatever their febrile attacks were it is certain that in many cases they were not characteristic of the regular malarial rigors; in many cases the men were able to say definitely that the attack was not like their old malarial attacks.

I am inclined to believe that many men suffering from the train of symptoms I have described above do get attacks of fever when exposed to the physical and physiological strains of camp life and that these attacks are in many cases not malarial in origin.

How far the above train of symptoms is due to organic degeneration of tissues produced during the acute stage of malaria abroad and never recovered from, is difficult to say. Other factors, too, go to confuse judgement, such as the general physical and mental stress and strain of war service, and the possibility of other infections such as trench fever. All I wish now to do is to point out that chronic pulmonary (or bronchial) spirochaetosis is present in many of these cases. It may be a cause of the debility, or the debility may merely afford favourable conditions of environment for spirochaetes. Further investigation and research would be necessary to estimate the true significance of the presence of spirochaetes in these cases—research along therapeutic lines with drugs known to be antagonistic to this group of organisms.

So far as I could ascertain from the physical signs, which were always of an indefinite character, there appear to be no serious pulmonary lesions.

The spirochaete has tapering extremities and a gently undulating outline without spirals. It affects various attitudes, sometimes extended, often bent on itself; when numerous they often appear entangled with each other. The spirochaete stains easily with ordinary stains; fairly strong carbol-fuchsin (one part carbol-fuchsin to two or three parts water) for a minute or two gives good results, so also does a strong watery solution of gentian violet. Silver nitrate preceded by tannic acid as for *Treponema pallidum* is good, but the films must be very thin to get satisfactory results, and for that reason, when spirochaetes are scanty, a much longer search is required.

NOTE ON THE EPIDEMIOLOGY OF AMOEBIC DYSENTERY.

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WENYON AND O'CONNOR, while in Egypt, demonstrated* that amoebic cysts (including those of *E. histolytica*) can pass successfully through the alimentary canal of house-flies, to be voided in the faeces, wherever these may be deposited; and they also found the cysts in the intestine of caught "wild" flies. As a result of their experiments, these workers concluded—somewhat hastily, perhaps—that house-flies are a very potent factor in the spread of amoebic dysentery. Since then there has been a general disposition to regard the fly as the chief agent by which this infection is spread.

During the past two years I have had the opportunity of studying the incidence of amoebic dysentery in various parts of Egypt, and before I had been long at work I formed the opinion that far too much stress had been laid upon the part actually played by flies in the transmission of this disease. Some factor other than flies is essential—a factor, moreover, which does not appear to be

of nearly so much importance in the spread of bacillary dysentery.

In the southern canal area, where I was stationed for over a year, flies were abundant, especially during autumn and spring. Amongst British troops bacillary dysentery was very common, whereas the amoebic type was rare. I gave details of the various infections in the *Journal of the Royal Army Medical Corps* for September, 1917, so it must suffice here to state that about 75 per cent. of the total stools examined were those of bacillary dysenterics. Barely 2 per cent., on the other hand, were amoebic. Among Indian troops, it is interesting to note, amoebic dysentery prevailed to a considerably greater extent, 12½ per cent. of the dysenterics showing active *E. histolytica*, while this parasite was found, in one phase or another, in 15 per cent. of the total stools. Nevertheless, there was, I consider, little or no increase in the number of infected men—that is, no appreciable spread of the infection in this area—because the percentage of cases of amoebic dysentery was distinctly less than the percentage of Indians (about 20 per cent.) who were found to be normally carrying the cysts of *E. histolytica*; and the great majority of these men had come direct from India to Egypt. That is to say, amoebic dysentery among the Indians occurred, at any rate mainly, in men who were already carriers. The hardships and stress of active service in the field had tended, in a number of these men, to lower the vitality and impair the resistance of the bowel, and as a consequence an attack of amoebic dysentery resulted.

Now, if flies are the principal factor in the transmission of amoebic infections, how is it that, while flies were abundant and bacillary dysentery very common amongst the British troops, amoebic dysentery, and incidentally all intestinal protozoan infections, were nevertheless rare? It cannot be maintained that the sanitation (using the term comprehensively, to include fly-prevention) was so perfect that the cysts had no opportunities for dissemination, because there is the fact that bacillary dysentery—which is, most probably, in the main fly-borne—was prevalent. It is evident that, whether the fly is or is not an important vehicle of transmission in the case of amoebic dysentery, some essential factor was lacking. This factor is water—moisture and humidity—which in my opinion is more important than any number of flies; and latterly there has been too great a tendency to overlook this essential factor in the transmission.

It has long been known that amoebic cysts cannot withstand drying. On the other hand, it was shown by Penfold, Woodcock, and Drew¹ that the cysts of *E. histolytica* can retain their vitality—proved by their ability to excyst—in water for at least a fortnight, and probably for a longer period. And the significance of these two opposing biological points in their bearing on the successful propagation of the species has not hitherto been sufficiently recognized. A fly very rarely (unless accidentally) alights on a liquid surface; its minute droplet of faeces is practically always deposited on a dry surface. Hence in hot countries—such as those in which amoebic dysentery is common—the droplet dries up in an instant or two. The chances of the survival of the cysts and successful infection of a new host by this method of contamination are therefore extremely slight. Now, under what climatic conditions is amoebic dysentery found to be most prevalent? Under conditions such as those obtaining in India and Mesopotamia, for instance, especially in areas which possess during certain seasons of the year a very damp climate with a high degree of humidity. In such lands there is an excellent chance for the survival of the cysts deposited in human faeces and for their successful dissemination by the agency of moisture in various ways.

In this connexion Egypt occupies a particularly interesting position. Over a large area (including the southern canal zone) this is a very hot and dry land, with scarcely any rain and a low degree of humidity. An exception is provided by the coastal fringe bordering the Mediterranean, where the humidity is much greater, owing chiefly to the moisture-laden northerly winds. Hence, on the above view, one might expect to find a higher percentage of amoebic infections in this northern district than in that in which I was at first working; and I soon had indication that this was the case. At Suez I found that the percentage of carriers amongst the native population was only 4 to 5, considerably less than that noted by Wenyon and

* In a Memorandum published in Egypt in 1916, and reprinted in the *Journal of the Royal Army Medical Corps*, May, 1917.