WAR EXPERIENCES IN DYSENTERY, 1915-18.

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Now that the campaign in the Eastern theatre of war has come to a close it would not be inappropriate to call attention to some recently published articles on this disease as it occurred in the Egyptian Expeditionary Force.

The war in the East has, broadly speaking, proved to be a medical more than a surgical one. Medical officers serving in the Forces in Egypt soon discovered that, whereas a well-considered scheme for the diagnosis and treatment of surgical cases existed, there was no such well-ordered one for the diagnosis and treatment of tropical diseases, which have claimed by far the greater number of the casualties. The need for special instruction of pathologists in these diseases has been a great desideratum and it cannot be said that at the end of four and a half years this has been accomplished. An example of what we mean—founded apparently on insufficient knowledge or investigation—has been the almost universal usage of emetine for all cases of dysentery; in fact, this drug has become almost a fetish. At the commencement one may, with some justification, point out that the study of dysentery is one little understood by the majority of medical officers and the very nature of this group of diseases, of such varied etiology, entails an intimate knowledge of protozoology, bacteriology, and cellular pathology, in the details of which not every bacteriologist engaged in the diagnosis of war dysentery has had the advantages of a special training. It must be remembered that the ultimate diagnosis of a case of dysentery has remained almost entirely with the bacteriologist.

The remarks which ensue must then be read as an attempt not so much at destructive as at constructive criticism.

The first paper which has attracted our attention is one by Lieutenant-Colonel Cowan and Captain Hugh Miller, R.A.M.C., in the JOURNAL OF THE ROYAL ARMY MEDICAL CORPS for September and October, 1918. From a clinical point of view, this paper strikes us as one of the soundest we remember to have read on this subject. But there are certain inferences and deductions drawn from the bacteriological or pathological standpoint, which seem to us to call for some explanation. What proof have we for the statement that at "Cape Helles amoebic dysentery was rife during July, August, September and October; while at Suvla Bay the main cause was bacterial." Apparently there is very little to support this view, which
is against all previous experience. Probably the bacterial cause of the disease came to be recognized the moment proper laboratories with trained bacteriologists were established.

It is not too much to say that the type of disease did not alter, but the methods of diagnosis did.

Furthermore the same argument holds good for the subsequent statement on hearsay evidence, that while the dysentery in East Africa prior to the war was wholly amebic, the bacillary affection has been frequently seen there since the arrival of Indian troops in the autumn of 1914. Surely there is no proof that bacillary dysentery has been "imported" into any tropical country of recent years; we know that this disease has an almost universal distribution and was most certainly present in East Africa before the war.

Amebic Dysentery.—When discussing the etiology of amebiasis the exact meaning of the endemic carrier rate of cysts of Entamoeba histolytica has yet to be determined. Does the presence of histolytica cysts in an otherwise normal stool necessarily indicate that the individual who has passed that stool is suffering from an undetected amebic ulceration of the bowel? From the frequency with which these cysts can be detected, often in small numbers it is true, in the faces of people residing in Great Britain, where amebic dysentery is not known to occur commonly as an indigenous disease, one rather gathers it does not (Yorke, Transactions of the Society of Tropical Medicine and Hygiene, July, 1918, p. 298, found 5·2 per cent of Army recruits and 19·5 per cent of the inmates of a lunatic asylum were carriers of these cysts. It was found, moreover, that whereas the carrier rate for convalescent dysenteries was 11·5 per cent it was almost as great, 7·8 per cent, in the case of convalescents from diseases other than dysentery. In discussing this paper Sir Wm. Leishman stated that he could testify that there had been no outbreak of amebic dysentery in France.

It would therefore be dangerous to infer on clinical grounds that British soldiers in Egypt were suffering from amebic dysentery solely because the carrier rate for E. histolytica cysts in native Egyptians is rather a high one, 13·5 per cent (Wenyon and O'Connor, "Human Intestinal Protozoa in the Near East, p. 39). Again, is there any real basis for the statement that a double infection with the E. histolytica and the dysentery bacillus is by no means uncommon? We do not think there is. In an experience of ten years laboratory work on dysentery one of us (P. H. B.) has come to regard cases of this kind as being extremely rare. So much depends upon what one is going to dub a dysentery bacillus. The designation dysentery bacillus should be reserved solely for one or other of the two classical types agglutinating in high titre to specific Shiga or Flexner-Y sera.

Should a bacillary dysentery stool contain a few histolytica cysts, this does not indicate to our minds that the patient is suffering from an acute attack of both diseases.
Should active destruction of the bowel wall be in progress it is our experience that the amoebae found in the stool will be large and will contain ingested red cells. This same view has been previously well expressed by Wenyon and O'Connor in their monograph (op. cit., p. 46).

We entirely agree that in the microscopic diagnosis of amoebic dysentery a single negative examination is of little value unless due attention is paid to the marked differences in the cellular exudate in the two diseases (cf., Willmore and Shearman, *Lancet*, August 17, 1918, pp. 200-206).

In considering the question of a double protozoal and bacillary infection, due regard should be paid to the characters of the bacilli isolated. Is there any proof that certain bacilli quoted *B. faecalis* alkaligenes, *B. paracolon* A, streptococci, and *B. C. L. A.* 1-6 (dysentery-like bacilli isolated by the Central Laboratory, Alexandria), possess any pathogenic power at all in human beings? None of these organisms as far as we know, have been recognized as being capable of causing intestinal lesions. None of them satisfy Koch's postulates. Is there any proof that they cannot be found in abundance in normal stools, and even in typical bacillary dysentery stools after a period of decomposition? We contend that there is not, and the arguments in favour of this view are being published by one of us (P. H. B.) shortly.

We would emphasize once more that the real test of recognition of the true specific dysentery bacilli can only be based upon their agglutinability in high titre with specially prepared rabbit sera.

*Bacillary Dysentery.*—These authors make a statement without adducing any proof that carriers of the dysentery bacillus are by no means uncommon.

This appears to be quite contrary to the experience of other observers (cf., Fletcher, *Journal of the Royal Army Medical Corps*, June, 1918; Whitehead and Kirkpatrick, *Lancet*, August 3, 1918, pp. 143-144). The latter investigators, out of an examination of 5,000 specimens, isolated Flexner-Y bacilli only twice from fecal stools and Shiga's bacillus never at all. This coincides almost exactly with the experience of one of us (P. H. B.) during his work on this subject extending over a period of ten years.

That there appears to be a similar lack of appreciation of the pitfalls which beset the isolation of the dysentery bacillus, is evident in the statement that over half (58.8 per cent) of the cases with blood and mucus in the stools examined in Central Laboratory, Alexandria, no dysentery bacilli or entamoebae were detected and the cause of the illness remained undetermined. Why? Because many factors (such as the period of the disease, length of time of decomposition of stool before examination, nature of any contamination, feces, urine, etc.) all affect the probability of successful isolation of the dysentery bacillus. Some of these factors have been appreciated in standard works on the subject, as "Besson's Bacteriology," and Stitt's "Tropical Diseases." In fact, in investigating cases near the front
line, before the third day of the disease, the percentage of our successful isolations by P. H. B., in a series of 107 cases in September, 1917, was as high as seventy-eight per cent.

In no other disease are the niceties of bacteriological technique so necessary as in bacillary dysentery. For instance, it has been our experience that failure to isolate the bacillus has been due to the common practise of placing a small amount of cresol in the bedpan.

Taking this as well as the other factors that we have mentioned into consideration, it is obvious that the proper location of a dysentery laboratory should be in close proximity to the dysentery ward.

To our minds the statistics quoted from the Central Laboratory, Alexandria; from April to November, 1916, should show a far higher percentage of bacillary cases; in fact, a percentage which would approximate to the results obtained in a field laboratory in Palestine in 1918; viz., in 1,874 cases, seven per cent were amoebic and ninety-three per cent bacillary, which in our experience has been the proportion between these two diseases throughout the whole course of the campaign in the E.E.P.

We agree that, even in typical cases, the agglutination reactions of patients' sera with typical dysentery bacilli isolated from their own stools are indefinite, and as an aid to diagnosis untrustworthy.

The authors appear to be sceptical as to the existence of a "pure chronic bacillary dysentery." We agree that in properly tended and treated Europeans this condition is rare; however, it undoubtedly does occur in the much neglected Turk. One of us (P. H. B.) had the privilege of examining post-mortem fifty-five cases in Turkish prisoners in which the disease had lasted from two to three months. This condition showed as a sinus transverse ulceration of the lower bowel with inflammation and sometimes tunnelling of the intervening mucous membrane. Out of twenty bacteriological examinations of ulcers of this type, Shiga or Flexner-Y bacilli were isolated in fourteen by means of scrapings from the floor of the ulcers. As a further proof no amoebae were found in microscopical sections or in scrapings of the gut.

Another paper by Captains J. G. Thomson and L. F. Hirst, in the Journal of the Royal Army Medical Corps, working in the same area, may be read in conjunction with the clinical study already quoted. The only point in this paper with which we disagree is the stress laid upon the importance of these atypical strains of dysentery bacilli in military hygiene. As quoted in the paper, one of us (P. H. B.) was inclined to lay stress upon these organisms in his original work in Fiji during 1910 (Report London School of Tropical Medicine, 1912), but with the recent improved methods of technique in the recognition of intestinal bacteria he has seen reason to lay less stress upon the atypical organisms and to confine himself, when talking of dysentery bacilli, to the two well recognized types.

These investigators appear to have isolated nearly as many atypical as typical strains of B. dysenteriae. By atypical strains we infer they mean
Gram-negative, non-motile bacilli, fermenting the sugars with acid, but without gas production. Having produced thirty-nine atypical strains divisible into eleven types by employing six sugars in their peptone tubes, how many more one wonders, could be produced by employing twelve or more?

It is our experience that colonies of the so-called atypical organisms are easily recognizable on MacConkey plates, as well as on agar slopes, by their denser, more luxuriant and slimy growth. The remarkable point about the true dysentery strains is that they are often to be found only in small numbers on a MacConkey plate (cf. Martin and Williams, B.M.J., 1918, April 20, pp. 447-448). They appear to be very delicate organisms and cannot compete with other more vigorous bacilli on cultures, such as B. coli, B. acidi lactic, B. faecalis alkaligenes, and B. pyocyaneus. The statement holds good, that, because the bacteriologist, after a single attempt cannot isolate a certain organism from a small portion of the patient's stool, it does not necessarily indicate that the bacillus is non-existent in the intestinal canal of the patient. How often are enteric bacilli isolated on the second or third attempt from the feces of carriers? We would here refer to Professor Browning's letter to the Lancet (May 25, 1918, p. 749) calling attention to Captain R. J. Mackie's work, performed also in Alexandria, on these organisms. He suggests that they do not have the epidemiological significance of the classical Shiga and Flexner-Y types.

With this we agree, but we do strongly differ from him that a considerable proportion of cases of dysentery in the East are attributable to these organisms. We assert once more that the proof for this statement is lacking.

We believe that the only test of the specificity of a bacillus is its agglutinability, and if this shows a markedly positive result it will always give the same classical biochemical reactions (sixty agglutinable dysentery bacilli of the two types tested by P. H. B. in September and October, 1917).

Could any bacteriologist at the present day be certain that a bacillus isolated from normal stools, and giving the sugar reactions of a paratyphoid bacillus, but non-agglutinating with a homologous serum, was in fact an enteric organism.

In considering the cases of dysentery which we have seen among the units of the Egyptian Expeditionary Force from 1915-1918 several points in diagnosis and treatment have impressed themselves on our minds.

All over the sphere of army operations dysentery of the bacillary type has accounted for the very large majority of these cases. We wish to emphasize this statement, as in our opinion too great stress has been and still is laid on the prevalence of the amebic type.

We disagree with those observers, many of them in England, whose writings tend to show that it was the amebic type that was mainly responsible for the illness among the troops in certain areas and at certain times. In our opinion, although cases of amebic dysentery are always
present, yet the epidemics that have occurred throughout the war area have been solely due to the *B. dysenteriae*.

We are sceptical of the existence of cases of the so-called "mixed" type because we have never seen a case where active *E. histolytica* has been found in the stools from which *B. dysenteriae* has been isolated. We admit that cases have presented themselves where the cysts of *E. histolytica* have been found in cases of undoubted bacillary type (case of Savage and Young in the *Journal of the Royal Army Medical Corps*, September, 1917, vol. xxix, No. 3, pp. 249-275).

The figures which we show prove conclusively to our minds the truth of this contention; and once this is admitted then the whole clinical aspect of things is changed.

What was the attitude of the temporary officers of the Royal Army Medical Corps towards dysentery in 1915?

Leaving out of consideration those who were trained protozoologists and bacteriologists and those who had some previous experience of tropical diseases, it may truly be said that the large bulk of the medical officers in the Egyptian Expeditionary Force had only the most vague ideas on the subject. Most men knew that there were two distinct types of the disease because they had different names.

Their knowledge ended when they had stated in the words of an officer, senior in years though not in service, "In amebic dysentery you give emetine, in bacillary dysentery you give mag. sulph."

"The Discussion on Dysentery" held in Alexandria and published by the Army authorities at the end of October, 1915, amply shows the absence of accurate knowledge on the subject.

It may be said that the diagnosis was entirely in the hands of the bacteriologists. It still is, and rightly so, but at the same time certain features ought to be known by all medical officers to help them to deal with the cases to the best advantage, and to avoid indulging in a routine treatment, which, in our opinion, must have done considerable harm.

What was the routine treatment in 1915? It is summed up in the word "emetine," for it is true that the vast majority of cases of diarrhoea with blood and mucus stools got emetine by some method or another, irrespective of the type of the disease and very often without any serious attempt at accurate diagnosis having been made. There were two factors which made this treatment almost inevitable: (1) The enthusiastic claims made for the drug by Rogers after his experience with it as an improvement on pulv. ipecac. in India; (2) the inclusion of tubes of emetine tablets in the Army box of tablet medicines issued to all regiments and medical units.

This latter was a temptation which few could resist.

In view of our figures of the relative frequency of the two types, we believe that this practice must have done harm for this reason: It is generally admitted that emetine has rather a depressing effect on the patient, and, although in a long series of *amebic* cases no evidence of
cardiac dilatation was observed (Savage and Young), when the drug was given in varying form; yet in bacillary cases, where the patient is fighting a toxin of varying power, the administration of emetine will probably act as a severe handicap, and may turn the scale against the patient. All writers agree that many of their bacillary cases die of cardiac failure.

What then can be urged to take the place of this treatment which has become a routine? In our experience, it would be much more accurate to give to all cases, where no exact diagnosis can be made on the spot, an early dose of anti-dysenteric serum. Our experience, like that of many others, has been that this serum, when given in moderate dose in the early stages, is likely to be followed by the best results.

The war in Palestine has differed from the war in France in this respect: in France it has been a surgical war, in Palestine a medical one. The surgical prophylactic procedure is, that every wounded man gets an injection of anti-tetanic serum at the soonest possible moment. This we believe is generally done in a field ambulance. If this can be done for the many casualties in a surgical war, surely it is equally feasible, that in a medical war every case of clinical dysentery should have an initial dose of anti-dysenteric serum on admission to the field ambulance.

Someone is bound to ask, "Is this not prejudicing the chances of any amœbic cases that may crop up?" The obvious reply is that the very nature of the two complaints suggests that a minimum of harm is likely to result to the amœbic case left without emetine for another day or two.

Bacillary dysentery is an acute toxic disease which may kill in a few days under the best treatment, or, if it does not do so, it may result in irremediable damage to the mucosa of the gut and one cannot give the patient by any medical means a new intestinal canal.

Amœbic dysentery on the other hand is a much less acute disease, which frequently does not kill even after the most casual treatment. Emetine given ruthlessly and indiscriminately to all and sundry must handicap the chances of a bacillary case, in the absence of other treatment, and may do so even combined with proper treatment. Anti-dysenteric serum on the other hand has never, as far as we know, been known to act prejudicially on any case, whatever the nature of the disease. Army orders have been issued dealing with the amount of the initial dose of serum, but none dealing with the time it should be given, with the result that fully half of the cases of bacillary dysentery in the field do not get serum until after the third day of illness.

This we consider to be the crucial point, for in our experience a small initial dose (40 to 60 cubic centimetres), given whenever the patient is known to be passing blood and mucus, is of much more value than the larger dose (80 to 100 cubic centimetres) laid down by the Royal Army Medical Corps authorities as the initial dose, without respect to the day of the disease on which it should be given.

Our figures show that in the cases coming under observation early it
is comparatively easy to isolate the bacillus and our experience in the wards shows the immense value of early administration of serum.

But, if the work is to be followed out to a successful completion, there must be some correlation of treatment throughout the units comprising the medical lines of communication.

Unfortunately, this has not been the case in some instances and it is sometimes found that amoebic cases which have been given a good start by early diagnosis and preliminary emetine treatment are handicapped farther down the line by the withholding of further emetine treatment by some medical officer; probably on the grounds that the diarrhoea had ceased.

Comparatively few medical officers take an intelligent interest in dysentery. It has also been our fate to isolate a Flexner-Y bacillus from the stool of a patient who was then given sixty cubic centimetres of anti-dysenteric serum within thirty-six hours of the onset of the illness. This patient did very well; indeed, one might say that the one dose was a cure in this case. He was evacuated towards the base as he was unfit of course to return to duty for a while, and could not be retained occupying a bed in a busy clearing station. It can hardly be credited, that in a general hospital to which he went this man was subjected to a course of twelve daily one-grain doses of emetine hypodermically, and this without any evidence of amoebic dysentery having been found in the stools, and in face of the Flexner infection and consequent serum treatment clearly marked on his medical record.

Had this occurred in 1915 it would have excited possibly little comment, but, unfortunately, we have to admit that this happened during the last weeks of war.

We therefore suggest that attention should be paid to the following points by those responsible for the health of communities or armies in countries where dysentery is endemic.

(1) Blood and mucus stools, with or without tenesmus, mean dysentery, and at the earliest possible moment an experienced pathologist should report on the nature of the exudate.

N.B.—Amoebae may not be found at first examination, but at least the exudate will indicate the probable type of the disease.

(2) If immediate microscopical diagnosis is impossible, give a moderate dose of anti-dysenteric serum and base further treatment on the result of laboratory findings when procured. Further serum treatment depends on the result of the initial dose.

(3) In amoebic cases emetine treatment must be consistent and a minimum of twelve grains given in daily doses of one grain each.

(4) Whatever the type of case treated by emetine or serum is only slightly more important than treatment by rest and dieting. In this connexion we wish to express our thorough agreement with the very complete scheme of diet laid down in the article already quoted by Cowan and Miller, and we suggest that it be taken as a standard.