Original Articles.

EXPERIENCES AT THE VARIOUS HOSPITALS
IN THE DIAGNOSIS AND TREATMENT OF
THE DISEASES PREVALENT IN THE ARMY.¹EXPERIENCES IN THE CARE OF THE SOLDIERS AT
THE MASSACHUSETTS GENERAL HOSPITAL. BY H.
F. VICKERY, M.D.

I INTEND to speak only a few minutes about this subject, which has been already so thoroughly gone over, and I would like, first of all, to say how much I am indebted to my chief assistant, Dr. G. W. Moorehouse, for very careful work in preparing the data for a review of my cases. Under my care came ninety soldiers in all, of whom three died — one from malaria of the remittent form and dysentery, one from typhoid fever with a very severe hemorrhage, one from typhoid fever complicated by that rare form of pneumonia in which the bacillus capsulatus is found — Friedländer's bacillus — which is a very fatal form of the disease.

About the general appearance of the soldiers, I was very much struck by the change in them, from what they were when they left the United States. They looked almost like foreigners, were very much emaciated, and very much pigmented; not merely sunburned, as seen upon covered parts of the body. This I ascribed to the destruction of the blood cells and a consequent deposit of blood pigment. In very many cases the nails showed this pigmentation. I never to my knowledge saw the nails after yellow fever, but the large majority of the men from Cuba had a line on their nails of natural color near the root, corresponding to the time since they had left Cuba, and nearer the end of the finger a pigmented portion acquired in Cuba. Almost all of them had enlargement of the epitrochlear and other glands, and almost all, without regard to the disease they had, had big spleens; and it was our experience that whatever disease they had, quinine seemed to be good for them. If quinine was omitted from the treatment, some malaria popped up before they left. With the majority a very rapid improvement was noticed, beginning visibly the next day after arriving. The malaria of all kinds in our experience seemed to be benefited by quinine and the largest doses we used were fifteen grains at a time. That was given six hours before a chill if we knew when that was, or as the temperature began to fall after the chill. It was given in one large dose at either of those times, and at other times a common way was three grains three times a day, or four times a day, and sometimes as much as five grains three times a day. One or two cases had bisulphate of quinine injected subcutaneously. The one who died was treated so. The places where it was injected looked rather bad, looked something as I have seen places look where fluid extract of ergot has been injected. None of them came to sloughing in this man, perhaps he died too soon, but the benefit was not very great. Fowler's solution, in doses of about three minims, seemed to be of value in the severe cases, or those with crescentic forms. We had cases of dysentery, but never found any amebæ, and did not find any

advantage in quinine as a rectal irrigation compared with other things, using also nitrate of silver, and normal serum solution and pure water. Much benefit seemed to come from a dose of castor oil in some of the cases of dysentery. The worst cases of dysentery did not seem to mind much anything we did; they kept on and apparently stopped when they got ready. It may be there were deep ulcers which took time to heal, but it was rather discouraging watching the patients. Diet did not seem to have much influence — I mean whether it was a diet of boiled milk, or, on the other hand, nitrogenous diet — it could not, to my mind, be proved that one was better than the other, but I was inclined to think that a complete change, as with a baby from a milk diet to a purely nitrogenous diet, seemed of some value, merely on the theory that if one kind of germs were being well nourished on, for instance the milk diet, a complete change in diet would cause that kind to wane or disappear. It had been said about these cases that a great many of them had had their stomachs all upset by quinine. So far as our experience went there were only one or two who vomited much, so that we did not find this injury from quinine. What was a remarkable thing, we seemed to get a good deal of benefit in the malarial cases from quinine, and yet cinchonism was not caused in more than two or three of all our cases. The benefit was got without those symptoms of the drug. Dr. Moorehouse has done an immense amount of work, which I mean to put in better shape to print, but it is no time to-night to read it. I think some of these charts might illustrate facts in a simple way, if they were passed around.

As to the prognosis of these soldiers, I think it is very likely that they will be troubled more or less off and on for a long while; the malaria will come and go, as they have bad luck. Here is the chart of a man who had the Widal reaction when he came in, and no plasmodia, and then, after almost three weeks, he had a chill and the malaria organism was found. Here is a typhoid that all of a sudden had a very typical malarial rise, although nothing was found. And here is a man who went out about ten days after he came in, his temperature having been normal a week, but I am sorry to say he was a hard drinker. He went out the 8th of August and came back the 22d of September, after a spree, very ill indeed. The crescentic forms had developed meanwhile. I will send the two charts around — the innocent one and the way he was finally. He got well. Here is a chart which shows how apparently amenable to quinine this crescentic form was. Here is a man who said he could not bear quinine in any dose, who took ten grains every morning for three mornings and it cured him. He did not feel it at all until he was told what he had had, and then he said he had had a headache every day. Here is a man who had severe dysentery and tertian fever. He had about six weeks of diarrhea and finally got well.

DISEASES PREVALENT IN THE ARMY AT THE BOS-
TON CITY HOSPITAL. BY HENRY JACKSON, M.D.

I was asked by the Secretary of our Society to say something on the soldier cases that I have seen this summer. I told him it was an immense piece of work to take hold of, and that the time was too short in which to write a paper giving a full account of the various phenomena; all I could do would be to give

¹ Read at a meeting of the Suffolk District Medical Society, October 19, 1898.

a short *résumé* of the general types which I had seen, and to say a few words on each of the types.

In the City Hospital there were, from the end of July to the first of October, somewhat over 400 cases admitted from the army. Of those, 126 were under my care. I have classified them roughly as follows: 65 cases of malaria, of whom 2 died; malaria and dysentery, 5 cases, of which 3 proved fatal; typhoid fever, 22 cases, of which 3 died; typhoid fever and malaria, 3 cases, one of which died; dysentery, 7 cases, none of which died; amebic dysentery, 1 case, which I will speak of later; he seems now on the fair road to recovery. I classed in a general way as debility 15 cases. In almost every one of those cases there had been a preceding history of fever, probably malarial: a more proper definition might be recovery from malaria. Two cases I classified as collapse: both of them got well; 2 cases were convalescents from yellow fever: they both recovered. In this preliminary list I have made I put down 3 cases as of doubtful diagnosis. The list perhaps ought to be much larger; some classified as malarial and typhoid fever ought to be thrown into the indefinite class. There was 1 case of simple jaundice.

Now to take up the 65 cases of malaria. The cases which I saw may be divided into four types: First, an irregular type of fever in which the temperature might on entrance be 101° or 102° F., in a day or two practically normal, in a day or two more rising to 101°, then normal, and again rising to a moderate point. I will pass about some of the charts. To refresh our minds I pass about on another sheet the ordinary tertian malaria we see about us. On that is also a chart of fatal typhoid fever from Chickamauga. In many of our cases of malaria the course of the fever was undoubtedly very much modified by quinine which had been given in Cuba in moderate doses and oftentimes in large doses in the ships on their return. Those on the *Bay State* were treated with large doses of quinine; those on the other transports received a certain amount of quinine. Then there was a remittent type of fever in which the fever ran along normal a few days, and then again passed through such a period of which I send around two charts. One is a very fair sample of remittent fever. It is not typical in that the first rise of temperature was not long continued though it was high on entrance, and we could not say what it was before he entered the hospital. The second part of the chart of his course in the hospital is perfectly typical of remittent fever. Then there was a type of fever of which I had a good many which I should call a sub-class of remittent fever, in which the course of the temperature was very long continued. These charts represent three weeks, and as you can see at a distance the temperature ran a course throughout that period, running at no time very high, averaging 101° to 103° F. A third type of malaria which we had was a type of what we might call intermittent fever, and yet there was not a single case in which we had the pure, absolute intermittent chart which we see in the Northern cases. I have no charts here to-night. The temperature would rise on one day, sink not absolutely to normal on the second day, as it does in the ordinary Northern type, that night be absolutely normal, to rise again the next day. The cases of this type, three in number, were very mild. They were amenable to treatment by quinine. After a single large dose of quinine the fever did not recur,

which was very different from the other cases; the other cases had quinine during the whole course of the fever. The organisms found in these cases were apparently the tertian organisms; that is, it was not possible to differentiate them. In one case the diagnosis was made by the house-officer. He reported the tertian organism before he saw the course of the fever. Then comes the pernicious type. After we had had a good many cases of malaria I was talking with Dr. McCollom one day, and he said: "You must look out for the pernicious malaria. Before you know it some of these cases will develop a tremendously severe type of disease and die."

I had of what I call the pernicious type three cases; two died, one recovered. One of those who died was apparently in a fair way to recover, but he developed pneumonia. Whether that pneumonia was dependent upon the malarial organism or not I cannot say. The Southern writers claim that there is a pneumonia dependent upon the malarial organism in itself. I pass around two charts of this malignant type. The first man came in, as you will see by the chart, with a temperature of 98.6°, with a pulse of 90. In the course of the afternoon there was a tremendous rise of temperature to 105.8°, his pulse rose to 130, he was unconscious, his circulation was extremely sluggish, he was bluish and continued in an unconscious condition for the better part of two days. On the third day he was partially conscious, noticed the physicians, appeared to recognize his brother and we felt very hopeful about the case. Then he became very much worse. I found at each lower axillary region bronchial respiration and râles. I did not examine the backs. The autopsy showed he had a double lobar pneumonia, the process extending throughout both lower lobes. The second one came in unconscious and made a fairly rapid recovery, the temperature dropping in the course of three days to 100° and then to normal. Judging from this single case, which was the only one that recovered of the three, having passed through these severe paroxysms, they were apparently safer from subsequent fever than those of the remittent type.

Now the organisms which were found in these cases were the very fine hyaline intracorpuseular organisms and crescentic organisms with black dots in the centre. With the crescentic organisms go the oval organisms, sausage-shaped organisms. When the crescents were found at all they were found in great numbers. I have seen many fields of the microscope in which there were two crescents, and on moving the slide about with a little care you would find cases where there were three of these crescentic or oval organisms in a single field of the microscope. Associated with these hyaline and crescentic forms there were, in many of the cases, a few intracorpuseular, pigmented organisms. The pigmented organisms when present were different from the tertian pigmented organisms, in that the granules were larger; they were more motile, and the organism itself was smaller. In a very large number of the cases no organisms were found. I judge that the cause of that was that in so many of these cases quinine had been given; in many in large doses, and in most of them in some dose. I am more sure of that position in that in those cases that stayed a long time in the hospital organisms were often found late in the disease which had not been found previously, — the patients who stayed two or three weeks. Also, two cases went out of my wards and returned after two

or three weeks. During the first time that they were in the hospital no organisms were found, and on their return organisms were found in abundance.

Treatment of the Cases.—I treated all of these cases by large doses of quinine. I used what I should call a mixed treatment, that is, combining the so-called Dock method, of giving a single large dose of quinine when the temperature has fallen a degree, with the method of giving the continued doses of quinine. All of these cases received twenty grains of quinine when the temperature had fallen one degree after a rise in temperature. The temperature was taken every two hours. When it had dropped one degree twenty grains of quinine were given, and given always in capsule. I gave it in capsule because I was afraid to give it in solution on account of the danger of causing vomiting simply from the taste of the quinine, and I did not feel confidence in giving quinine in pills. To give twenty grains you have to give four capsules, it being so light. In addition to this single large dose which I tried, I think in every case (and in some cases I succeeded in stopping the temperature, though not in many) I gave fifteen grains of quinine during the day. Some of my colleagues gave that in one dose. I usually gave it in divided doses during the day. That was during the course of the fever. To every case that came into the hospital I gave two grains of quinine three times a day, on the theory of the Southern physicians, that you cannot cure anything in Florida, as I have heard them say, without quinine. Then, as to the treatment of these severe cases with collapse, the pernicious types. One case died, I am sorry to say, before I had known of the efficacy of treating pernicious malaria by subcutaneous injections of quinine. I found out afterwards that the Southerners say, that is the only way you can save them. I gave the case that recovered quinine subcutaneously. I gave it in large doses subcutaneously to the man who apparently was on the high road to recovery from his malaria, though he subsequently died from pneumonia. Quinine is very difficult, I found, to give subcutaneously. I had to give many large injections subcutaneously. I think about five grains to the drachm is the best I could do, adding tartaric acid to the bisulphate of quinine. I found that I could not dissolve the hydrochlorate of quinine in anything like the proportion of water which Thayer claimed. I was in Baltimore lately and hoped to ask him about it, but he was not there. He says the hydrochlorate of quinine is soluble in equal parts of water. I went to several apothecaries, who told me that was impossible. The form I used was five grains of bisulphate of quinine and five grains of tartaric acid in the drachm of water.

As to the course in these cases, it varied extremely. Some ran a very mild course, represented in two charts I passed about, perfectly well, clamoring to eat and get out, the temperature every now and then rising to 101° or 102° F. Some of the cases reacted very quickly to quinine after a single large dose, having no further fever. But of those there were only a few—some half-dozen out of the 65. The average number of cases ran a course of several days, so far as I could see, the course of the disease unmodified by the quinine given.

In malaria and dysentery, of which there were five cases, the diagnosis was made by the finding of plasmodia or by post-mortem examination. One of these cases was a typical case of remittent fever, with tre-

mendously severe dysentery. The others represented the atypical forms of malaria, the temperature rising only occasionally. The treatment of dysentery was as follows: I gave them all irrigation, boric acid or soda or saline, bearing in mind the danger of rupture of the gut. In giving the injections the fluid was not forced into the bowel. In none of them did I raise the hips high, as you would raise them in giving an injection in a perfectly healthy gut, or in a gut that was not ulcerated. Morphine was necessary in most of the cases to relieve pain. So far as I saw, it had no effect whatever in controlling the movements; they would go on uninterrupted, though morphine was given in as large doses as we cared to give it—I mean morphine and the various opium compounds. I had in several cases a good deal of success in treating them with a combination of mild saline with very small doses of morphine. I did not in most of these cases use a treatment. I suppose most of us here wash out the bowels thoroughly with large doses of salines, and attempt to keep the bowels quiet by morphine. I did not use that, as most of the cases were so sick that I felt any such method as that would wash the patient away as well as the bowel. In four of the cases I used the entirely empirical method of treating dysentery, which is employed so much in India and in China, namely, to treat by morphine and ipecac. I was very much surprised to find that I got fairly good results in two cases, and that in one case there were very good results; the character of the stools changed within forty-eight hours. Whether it was due to the treatment or not I do not even pretend to say, but it did no harm. I gave it to two extremely sick men—men as sick, I felt, as men could be. They were not nauseated; they were not depressed by the ipecac. I gave one-sixth grain of morphine, and half an hour later twenty grains of powdered ipecac in capsule. It apparently caused no nausea in any of the cases; there was no vomiting excited by the ipecac. In one case there was vomiting, but he had vomited for several days before that treatment was instituted.

Of the diet, all I can say is that I stuck as nearly as I could to albuminous diet, avoiding milk in large quantities. When I used milk it was peptonized.

There was one case of amebic dysentery. This case was of great interest to me, in that it was, so far as I could tell, the only case that I had seen shortly after the process had arisen. Such cases of amebic dysentery as I have seen have been cases that have, through months and sometimes through years, run the course of so-called chronic diarrhea, the amebæ being found late in the course of the disease. This was a perfectly healthy man, went to Cuba the first of July, taken sick some time the latter part of July, and so came under observation within a month after the process had started. There was no fever in this case; the leucocytes were normal. I gave him the treatment of amebic dysentery usually employed—quinine by rectum, and also quinine by mouth. He improved rapidly and went out yesterday apparently well. I feel there can be no doubt as to the amebæ being present. The diagnosis was confirmed by several observers, fortunately by Dr. Councilman, who, of course, is the most able to make the diagnosis. Also, the course of the disease was entirely different from ordinary dysentery. He would pass little bits of blood and mucus without any pain, without tenesmus.

Herpes was common in dysentery—herpes facialis.

In one patient I had a tremendous case of herpes zoster.

In general, in all these cases of malaria, and malaria and dysentery, one great factor was very prominent, — they had no chills. The temperature would shoot up to 104° or 105° F. without chill, without rigor. I think there were only two or three cases in which there was rigor, and when present it was not severe. There was not a single case where the bed would shake as it does in the ordinary case of stay-at-home malaria. Many of the cases were very anemic, several were severely anemic. In one there was intense anemia, so marked that there was apparently as a result of that a general edema. The urine was at no time abnormal. As soon as he recovered his color the edema disappeared. The average pulse in the cases of malaria was not very high, running 80 to 90 and to 100. Only in the pernicious cases was it very high; in those it ran to 130, 140 and 150.

Of the typhoid cases, several of them were convalescent when they appeared. The diagnosis was practically made by the Widal test, and, as a rule, all of the cases were mild; the fever was not high. One of the fatal cases arose late in the course, after the men returned from Cuba, and it was complicated by delirium tremens, which was apparently the immediate cause of death. The cases of typhoid which came from Cuba were not as severe as those which came from Chickamauga.

Of typhoid malaria there were three cases. In two of the cases, during convalescence, there was a sharp rise of temperature, and the organisms were found. In the other case the disease was not apparently modified by the malaria; the organisms were found, the Widal was positive, and the general condition of the patient was that of typhoid and not of malaria.

Of the cases that I have classified as debility, many of them were probably convalescent from malaria; many of them were markedly anemic. There were two cases only that were complicated with a tenderness which would suggest the presence of neuritis. Dr. Bullard was much interested in that process at the time, trying to find out whether the severe exposure these men had been through could start up a neuritis; it apparently had not.

Two cases I classified as collapse; and collapse they were without any definite etiology. The first case, my house-officer rightly said, was a case of cholera infantum. The man came in unconscious, vomiting everything, having movements constantly; he was cold, the temperature not quite 97° when he came in, and his pulse could not be felt. I arrived within an hour, and at that time there was just a flutter to be felt. That man recovered. He was not extremely anemic; he was not extremely emaciated; it seemed to be a sudden breaking-down. The second one was not so severe. I do not believe the first case I have spoken of would have stood a ghost of a chance of living if he had not been met at the station by Dr. Stedman, who gave him powerful stimulants subcutaneously, and he reached the hospital just alive. Stimulants were continued there; no food was given, and when I did give him food I treated him as you would a laparotomy — with half-teaspoonful doses of boiled water, as hot as he could take it. The other case was unconscious, but the pulse not very rapid.

All I wish to say about the two cases of yellow fever is to draw attention to what Dr. McCollom told

me about, namely, that after convalescence from yellow fever you would find a yellow discoloration of the nails. It is rather uncanny to see about half of the nail yellow, and the pink at the lower part of the nail appearing, which had grown out since the convalescence. I do not know whether that is an original observation, but original or not it does not appear in the works on yellow fever. Both the cases showed it to a marked degree. Dr. McCollom said that that had enabled him many a time to make the diagnosis.

Then the mild cases of questionable etiology — of course there is nothing to say about these. The case of jaundice was apparently simple, mild catarrhal jaundice, and had nothing whatever to do with the fever.

Two or three points I wish to speak of in general. In the first place, as this is the first meeting of the Society, I think I may be allowed to speak of the general *morale* of these men. It is the most impressive thing I have seen in my life. The character of the men impressed me deeply. Their character was borne out by what I can tell you, that not only did they make no complaint of the treatment which they had had in the war, or in the hospitals or transport ships, but, what to me was still more remarkable, there was not a single case in which those soldiers boasted to me of what they had done, or even offered the suggestion that they had given their life for the country, and that therefore everything ought to be theirs. I think that is more remarkable than that they did not complain. That from a social point of view. From the medical point of view, the saddest, and yet the most encouraging, point about these soldiers was their vitality, the strength of their pulse — the most encouraging thing, because it enabled these men to pull through diseases that would seem impossible that men could live through, and the sad sight that men with such constitutions as that should be so reduced. Man after man came into my wards that were mere skeletons, men emaciated to a degree that I had not seen, except in the most advanced cases of lingering cancer, where the cancer may be situated in such region that it would not interfere immediately with life, and allow the patient to live as long as there was any vitality left. That is what a good many of these cases lived through; they lived as long as the heart could go. I was surprised to find the good condition of their skin. No cases came in with evidences of abrasions, or with evidences of lesions from vermin.

The diet was a very important factor in these cases, as it is difficult to feed those who have gone a long time without food in any walk of life, and we had the same factor to deal with in these soldiers. Their stomachs and intestines were not in a condition to stand large quantities of food, and we were obliged to feed them with the greatest care.

EXPERIENCES AT THE LONG ISLAND HOSPITAL. BY J. J. MINOT, M.D.

I had at the Long Island Hospital about seventy cases all told, none of them as severe as some of the cases Dr. Jackson and Dr. Vickery have spoken of. I have almost nothing to say after these gentlemen have spoken, as they have covered the ground entirely. I saw all the different types that Dr. Jackson has spoken of except that I did not have any of the pernicious ones and none of my cases died. They were

all regular soldiers with the exception of a few recent admissions from the Ninth Regiment discharged from other hospitals and who had had a subsequent attack of malaria. They were all in pretty good condition, and have all done pretty well. There were about ten cases of typhoid fever. The interesting part was, that in those ten cases there were no plasmodia found until after they got through with what appeared to be the typhoid process, and had had a normal temperature a week or ten days, when several had what appeared to be a malarial process, and in some of these the plasmodia were then found. The chill in some was very distinct, and in one or two very severe; the severest were in men who had their first chill while in this hospital. A great many of them vomited in place of the chill; many had headaches, and many had no symptoms at all, that they or we knew, except the temperature. One of the interesting things was the length of the rise and fall of the fever. That varied in different cases; in some cases about twelve hours, and in some six, after it had started to rise until it returned to normal. Sometimes these rises would occur every day, for a number of days, sometimes every other day, and again irregularly. Many of my cases have stayed a good while, and some have had constant rises, some had no rise at all until they had been in hospital two or three weeks, but were very anemic and in wretched general condition on admission. They all got quinine in varying doses, and by all the methods usually employed. I could not see it had any immediate effect on them in any way, whether given in single large dose or a large dose combined with doses of five or more grains three times a day, or given in smaller doses. It seemed to me they had their temperature go up about when they got ready, and I could not have any effect on it at all, except of course as time went on the rises in temperature became less and less frequent and I suppose will eventually disappear. Some of the cases were interesting to me, in that they showed the Widal reaction, but did not have symptoms of typhoid fever. Where they got their Widal reaction from I don't know. One admitted he had typhoid fever three years ago, but that was the only one. I should think I had five or six of that kind, but they were unable to give a previous history of themselves; some had been sick a day or two, some had never been sick until they got on board the steamer coming to Boston, when they began to vomit, — quite a common history. One of the things that has impressed me very much was that we were able to distinguish inside of twenty-four hours, practically in a very large number of these cases, whether they were typhoids or malaria, malaria of the remittent type, where the temperature runs a number of days. One case in particular had no plasmodia nor Widal reaction, though repeatedly examined, but he had a temperature almost exactly like one of Dr. Jackson's remittent cases and like one of the cases at the Carney Hospital, which is now under my charge, where both Widal reaction and plasmodia were found.

THE CARE OF THE SOLDIERS AT THE CARNEY HOSPITAL. BY H. F. HEWES, M.D.

About one hundred and twenty of the soldiers who saw active service in the late war in Cuba were treated at the Carney Hospital. Of these eighty-eight came under my charge at some time in their illness. Of these eighty-eight cases seventy-three had malaria alone, none

fatal; ten typhoid, none fatal; and eight dysentery, none fatal; seven of the typhoids had malaria also, and six who had the dysentery had malaria also; three of the cases had no disease which could be diagnosed at the time of their entrance. Of the eighty cases of malaria all had malarial organisms in the blood. I saw the blood of each case admitted at least once myself. If the finding was negative, the house-officer looked at the blood twelve hours afterwards; if this was negative, the blood was examined every third or fourth day as long as the case remained in the hospital. But eight cases remained in which the malarial organism was not found at some time. Of those, three were typhoid and two dysentery. The organism was found in the majority of cases upon the first examination, but in several it was found only on the fourth or fifth examination. Of the eighty cases of malaria sixty belonged to the type known as estivo-autumnal; six were pure tertian and six showed a mixed infection of the estivo-autumnal and tertian organisms. The differentiation between these forms of malaria was made entirely on the blood finding. A great variety of forms of parasite was found in the blood in looking over all the cases. In the thorough examination of the blood of as many of the eighty cases as possible, both in fresh and stained specimens, the following forms of organisms were found: In the first place, by far the most common form of organism was a small form known as the hyaline form, an intra-corpuscular, slightly motile, non-pigmented form, as a rule, eccentric and perhaps one-fifth or less of the size of the corpuscle in which it lay. This was either in the form of a sphere or had to the eye the form of a ring, frequently of a signet ring. Perhaps the next most common form was an irregular form, often slightly pigmented, in some cases looking very much like a cross; in others like a ring with one side broader than the other. Then we found in a great many cases clearly pigmented forms. These were either very sluggishly motile, with a few pigment granules of light color, typical of the estivo-autumnal cases, or perhaps of the same size or larger, but much more pigmented and actively motile; very different looking organism, although the size might be the same — the tertian form. Then there were found also in a few cases — the tertian cases — a form of this kind filling the corpuscle, full of pigment, very active; and also in some cases the segmented or sporulating form with a good many segments, and in a few cases even the flagellate form. In a considerable number of cases, in addition to these forms, there were found a crescent-shaped organism, sometimes taking an ovoid shape, intra- or extra-corpuscular, pigmented, the pigment as a rule bunched, but not always. In some cases a large extra-corpuscular form, fully as large as a red corpuscle, with pigment in it, very sluggish; in some cases forms which had the pigment all collected in the centre in a block. That represents, practically, the variety of organisms found in fresh and stained specimens.

Now as I put these down on the board they represent the cycles of the two types of malaria I have been describing. This upper line will represent fairly well the regular cycle of the estivo-autumnal organism seen in the circulating blood; the rest of the process takes place in the internal circulation. This line down here represents the cycle of the tertian organism very fairly, except that in addition we did find in one or two of the tertian cases a form like this, which is the very earliest form of the tertian, a kind of thin spider form

with thread-like processes. Now, as you see, the hyaline and ring forms are common to both. The hyaline form of the estivo-autumnal form is slightly smaller than of the tertian, but unless you measure them in each case you cannot tell the difference. These irregular forms are common to both. The pigmented form, one-third to one-half the size of corpuscle, is common to both, but has such different characteristics that the differentiation can be made. When you get beyond this point in the tertian you have forms which never can be confused with the estivo-autumnal, the full-sized pigmented and the sporulating forms. The sporulating forms occur in the estivo-autumnal, but I have never seen them in the peripheral blood. The crescentic form is the form which occurs out of the regular cycle. It may appear spontaneously after the other forms have been present. Several observers claim that they had seen the pigment in the crescent, not collected as in this drawing, but moving, and had seen the crescent change shape, become round and sporulate. This view is not held by most observers of American estivo-autumnal malaria, and Koch has recently published the result of his investigations on tropical African malaria, in which he says definitely that the crescent form is innocuous; may last in the blood an indefinite time, but cannot give rise to paroxysms or to auto-infection. He finds it unaccompanied by other forms.

MacCallum considers it to have the property of reproduction by a sexual process, distinct from sporulation. His investigations do not suggest that the crescent is innocuous.

To differentiate the estivo-autumnal and tertian types of infection we work upon the following lines:

If pigmented forms one-half to full size of the corpuscle, with fine pigment granules, numerous and very active, are present, or if sporulating forms are present, the infection is tertian. If pigmented forms less than one-half size of corpuscle, with few pigment granules with slight activity, be present, the type is estivo-autumnal. If hyaline or small ring forms only are present the development must be watched for diagnosis. If the development in the peripheral blood ends with the non-pigmented large ring, or the sluggish, slightly pigmented form less than one-half the size of the corpuscle, the type is estivo-autumnal. If the full-sized or active pigmented forms develop it is the tertian. If crescents are present the type is estivo-autumnal.

Both varieties of organism may be present together when we have mixed infection.

The afebrile period is the time to take the blood for diagnosis of estivo-autumnal. Within a few hours of the paroxysm is the time to diagnose tertian.

The characteristics of the blood in the two diseases differ in some other ways than in the organisms. The estivo-autumnal blood, as a rule, showed much deeper anemia. I have never seen such deep anemia in the blood of malaria about here as in many of these cases. The loss of hemoglobin in the corpuscles was particularly marked, and blasts were present in many cases. The tertian blood showed the large red corpuscles, larger than normal, characteristic of that malaria which did not occur in the estivo-autumnal. The estivo-autumnal blood showed a large amount of pigment in the leucocytes, which is characteristic of this form of malaria against the other forms. The blood seen in periods of quiescence, between weekly paroxysms, showed often only the small ring form.

I should say, that in the examination of fifteen cases the rings were the only form found on first examination, and it was impossible to make the diagnosis at once. In later examinations other forms were found in eight of the cases, but in eight cases we were still in doubt. In the apyrexia, after a paroxysm, or during the days of fever, the larger pigmented round forms and rings were found in these cases, showing them estival. The temperature charts suggested that these eight belonged to the estivo-autumnal type. I have some of the temperature charts of the estival cases, which illustrate the different temperatures which you get with practically the same blood finding. All these cases had the simple ring form at the first finding, and two later developed pigmented forms, two did not. You see how the temperature differed. These were under quinine throughout. One received quinine for two doses, and then for some reason the quinine was omitted, and he ran for two weeks without quinine. For that reason I suppose his chart is more like the regular form of fever. The temperature stayed up continuously in some, in others it would be up for two to four days, then normal, or 99° to 100° for ten days, then up again.

A comparison of the temperature charts emphasizes a very important point, namely, that the method of diagnosis by the blood examination is the only reliable method of differential diagnosis between the two types of malaria, included in these cases. Here are estival charts resembling the ordinary tertian. Here are tertian with quotidian charts, with remittent charts, and one with constant pyrexia.

In regard to the other symptoms I will say nothing, save to mention that one case had a malarial paroxysm which was associated with an acute jaundice. The man, who had been of perfectly normal color before the attack came on, became jaundiced inside of an hour—it seemed so—the liver was intensely painful to pressure, and the blood was full of organisms at the time, although previously nothing but crescents had been found. The man was dosed with quinine, and the whole thing subsided in twenty-four hours. Another case came in with acute nephritis, marked general anasarca and edema of legs, the urine containing one-half per cent. of albumin and typical sediment of acute nephritis. The malarial organism was found in his blood, and he was dosed with quinine, five grains every hour until he had taken twenty-five, and then five grains every four hours continuously. At the end of five days the amount of albumin in the urine had diminished to a trace, and the edema was disappearing. The quinine was then omitted by mistake, and at the end of a week he had another malarial paroxysm, and the urine contained nearly one-half per cent. of albumin again. I call this nephritis, because the man had edema associated, which is uncommon if not unknown in hyperemia. The duration of the condition does not suggest acute nephritis, but we have had little experience with malarial nephritis here.

The result of treatment was much the same as Dr. Minot has mentioned. I should say, however, the average case was benefited distinctly by quinine, but I should not attempt to claim I had cured anybody, except the tertian cases, and not all of these. I had cases go three weeks without paroxysms, and develop paroxysms on removal of quinine. I gave to a certain set of men five grains every hour, until they had

taken thirty-five grains on the falling temperature, when the large estival forms were in the blood, and then five grains every four hours continuously for two weeks, and in some of these cases paroxysms developed, and during cinchonism organisms were found active in the blood. A number of other cases never had any temperature after the taking of the quinine. Chart No. III is one of the cases where the paroxysms came on after long omission of quinine. Where these paroxysms came on they seemed to follow exertion or exposure. This man walked a mile and came back, and had a paroxysm with the organisms very thick in the blood. This one sat by an open window. The absence of relapse seemed proportionate to the improvement in general condition. In closing, I will mention that in one case of typhoid (Widal positive) I found pigmented malarial organisms during the active typhoid—a finding which is rare, I believe.

ANIMAL VS. VEGETABLE FERMENTS.¹

BY A. E. AUSTIN, A.M., M.D.

It has long been known that vegetables and plants have digestive processes, almost, if not quite, analogous to those of animals; the sprouting grain, at certain temperatures, develops a ferment, the diastase, whose enzymogen doubtless exists latent in the seed until such time as conditions of temperature and moisture develop the fully active ferment from its parent or forerunner. Nor does the analogy end here; the sprouting plant can no more use the undigested starch than the animal body can, and hence the starch which the seed contains must be converted into maltose, in order to feed the young plant until it can obtain its nourishment from the earth and air in which it is placed.

The intermediate products between starch and maltose are also undoubtedly the same as those which exist in the digestion of starch by animal ferments, ptyalin and amylase, that is, soluble starch, erythro-dextrin, achroodextrin and true dextrin, as well as isomaltose; (1) Papayotin, from the juice of the Carica Papaya, has likewise a strong digestive action upon albumins, converting them to albumose-peptone, but differing from diastase in that it seems to play a constant part in the digestion of the plant, (2) and thus differs from the action of the diastase which seems to simply meet an emergency and to be discontinued as soon as the plant has reached the point where it can provide for itself, much like those birds which instinctively digest the food for their young and feed them with the regurgitated contents of their own crops until such a time as the young can eat and digest the food in its natural state.

Nor do these vegetable ferments stop with the conversion of sugar, but some of them have the power of splitting up the glucosides into dextrose and other components. Emulsin, for instance, found in bitter almonds, has the power of splitting up, with the aid of water, the amygdalin, likewise contained in the kernels, into benzaldehyde, hydrocyanic acid and dextrose. (3) A fat splitting ferment has also been found in the castor-oil bean, in linseed and in corn. Nor do many of the plants stop with a single ferment but possess at least two and perhaps more. The juice of the fig-tree.

pineapple and papaya, for instance, not only convert albumin into albumoses and peptones, but also have the power of coagulating milk in exactly the same manner as rennet from the calf's stomach, without change of reaction.

As a general thing, these ferments have been regarded by plant physiologists as having no importance, and as playing no part in the economy of the plant. This is clearly disproved by the fact that albumose-like substances have been found in the juice of papaya (2) and in that of the pineapple (2). It appears, therefore, very probable that plants have digestion exactly analogous to that of animals though the intermediate products may differ.

Becoming interested in this subject, it occurred to me that it would be of interest to compare the intermediate and final products as well as the relative strength of both kinds of ferments, and by the kindness of Messrs. Parke, Davis & Co. I was supplied with sufficient taka-diastase to carry on my experiments.

This substance is an almost white powder, and is claimed by the manufacturers to be made from a Japanese seaweed, which apparently has this power of digesting its amylaceous food like an animal.

At first my efforts were directed to determining its strength as a starch-converting medium as compared with pancreatin and saliva. In each experiment one gramme of starch was made into a clyster with 100 c. c. of water, stirred up cold, and heated to boiling while stirring. It was then allowed to cool and to one was added one-tenth gramme pancreatin, to another one-tenth gramme taka-diastase, and to the third one c. c. saliva. The flasks were then stoppered and allowed to remain in the thermostat at 40° C. for twenty-two hours. They were then taken out and the contents filtered, by which any undigested starch was removed, washed and the previous volume of 100 c. c. re-established. The sugar was determined by Fehling's test, and the sugar calculated as maltose, that is, 10 c. c. Fehling's is equivalent to .0778 grammes maltose, instead of .050 grammes as in dextrose, on account of the weaker reducing power of maltose. Then the amount of starch was calculated from the maltose found, an easy process, if we consider two of the simple molecules of starch, $C_6H_{10}O_5$, as uniting with one molecule of water to form one molecule of maltose $C_{12}H_{22}O_{11}$, from which the percentage of starch from a certain amount of maltose would be 94.736 per cent.

The result of this tabulated is as follows:

First experiment, — Conditions, etc.			Presence of starch was determined by Iodo-potass. iodide solution, when no blue was found starch was reported as absent.			
Ferment.	Starch converted grms.	Per cent. of total.	Starch.	Starch converted.	Per cent.	Starch.
Taka-diastase	.613	61.3	No	.9331	99.31	No
Pancreatin	.267	26.7	Yes	trace	..	Yes
Saliva	.199	19.9	Yes	.1553	15.53	Yes

For some reason the pancreatin in the second experiment failed to act with any energy.

It was next deemed important to increase the burden placed upon the ferment by increasing the amount of starch and to vary the period of digestion so that

¹ From the Chemical Laboratory of Tufts Medical School.