Observations on Filaria sanguinis hominis in south Formosa / by W. Wykeham Myers.

OBSERVATIONS

ON

FILARIA SANGUINIS HOMINIS

IN

SOUTH FORMOSA.

BY

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FILARIA SANGUINIS HOMINIS

IN

SOUTH FORMOSA.

ON taking charge of the native hospital at this port (Takow) in August 1879, I immediately began to carry out the request of my friend Dr. P. MANSON, that I should examine any cases of Filaria sanguinis hominis that might come under my notice, with a view of investigating and, if possible, confirming the discoveries and observations made by that gentleman.

My object, then, was as follows:—

1°. To observe the proportion of filaria-infected patients in relation to the whole number of admissions to the hospital, further noting the external manifestations present, *e.g.*, elephantiasis, lymph scrotum, etc.

2°. To note the periods of appearance and disappearance of the embryos from the blood of infected persons, with a view of corroborating or disproving Dr. P. MANSON'S recent discoveries as to the periodicity displayed by the parasites in their appearance.

3°. To account, if possible, for the disappearance of the embryos at certain hours, discovering, if feasible, whether this was final as regarded the swarm, or whether they lay dormant and adherent during certain periods in the lungs or other organs of the body.

4°. To attempt by experiments on monkeys to produce conditions similar to those observed in filaria-infected men.

Such were the designs with which I entered on my investigation; but I was somewhat crippled at the outset by the difficulty I met with in getting an infected subject, and for this reason I found the efforts made with a view of attaining my aims considerably restricted.

I have repeatedly examined the blood of patients admitted on the general list, at various hours, both day and night, devoting particular attention to those suffering from malarial disease, but have as yet only succeeded in finding three filariated persons. One of these is the subject of the observations recorded below, and the other two declined to permit more than occasional examination. From these and other conditions noted and hereafter detailed, I am led to think that this state of blood infection is not common to nor favoured by residence in Formosa, and the following are my reasons for thinking so:—

1°. The number of patients and others I have fruitlessly examined at various hours in the day and night.

2°. The almost total absence of those diseases which Dr. P. MANSON has proved to be dependent on parasitic obstruction, such as elephantiasis and lymph scrotum.

Of the former I have seen but one case, and she informs me that she contracted it "years ago in Amoy," and has been to this hospital before for treatment; I therefore assume this is one of the cases recorded by my predecessor, the late Dr. DAVID MANSON.

The only statistics available from the native hospital date from April 1871 (Customs Reports and Hospital Records), and during that period I find upwards of 15,000 patients have been seen by Drs. DAVID MANSON, RENNIE, and MYERS. The cases have been very carefully classified under their proper heads, and from these records I observe that only two of elephantiasis have been noted, and none of lymph scrotum. (I do not include the case of elephantiasis mentioned above, for, as I have stated, I believe she represents one of the two mentioned by Dr. D. MANSON.) From this fact, coupled with the convincing observations and discoveries made by Dr. P. MANSON as to the etiology of these diseases, I am strongly inclined to think that Filaria sanguinis hominis does not exist in the blood of the natives of this place, otherwise it would seem probable that there would have been, during the nine years reported on, very many more of those manifestations which Dr. P. MANSON has shown to be the results of filarial infection.

On the mainland these diseases abound, whether one takes the earliest symptoms shown by the enlarged glands, the elephantised part, or lymph-secreting scrotum. It may be asked how is it that any cases of filaria-infected blood are to be found here, if the disease has no local origin, and to this I would reply that the sufferers come from Amoy or thereabouts, as undoubtedly was the case in the only three instances I have met with. As is known, there are a large proportion of the "natives," or residents here, who are, comparatively speaking, recent colonists, coming originally from Amoy and its surrounding districts. With characteristic adherence to the place and province of their own or their ancestors' nativity, the immigrants settling here do neither themselves nor in succeeding generations become entirely separated, in mind at least, from their original locality, and it is apparently the ambition of many to return, if only for a short stay, to the spot which family tradition styles their home; hence we find, as a fact, that most of the people hailing from the mainland make periodical visits there, and even many of those who have become by length of residence apparently more closely attached to Formosa would seem to some extent to keep up this rule.

The Pepohuans and Hakkas, on the other hand, who have more right to be looked on as regular and permanent natives of Formosa, and of whom a goodly proportion attend the hospital, ought, one would think, if filarial disease was common, to present some cases, but this does not appear to be so; nor can I hear that elephantiasis or lymph scrotum is met with either amongst these people, the uncivilised aborigines, or those whom, for distinction, I will call the immigrants. Again, with such numbers coming from the mainland, where filarial infection and its consequences are known to abound, it is not improbable that some afflicted persons come over with the rest—as happened in the instances I have given,—and if the circumstances were favourable for further propagation, by this time the prevalence of the disease would have been sufficient to admit of its existence being a matter of more certainty. The woman suffering from elephantiasis had, as she said, contracted the disease when young, and at Amoy, and though she and her children and grandchildren had long been settlers here, she and they made periodical visits to the mainland, where several of their relations still lived. She told me she was a member of a large village the residents of which were more or less connected, or, to speak more correctly, were of the same clan, and though they had been a great many years settled in Formosa, and some of them born here, still they continued to consider

themselves "Amoy men." She had no filariæ in her blood, though I looked for them several times, but two were discovered in the sanguinolent lymph drawn from an enlarged inguinal gland. There were no other cases of elephantiasis as far as she knew.

This is in effect a similar result to those accruing from the inquiries I have made or caused to be made in other parts of the country, and although I have heard of a case of elephantiasis scroti, I am told he also traces his disease to the period of his residence on the mainland. There is, however, another fact in reference to this man which may be thought to have a little bearing on the rarity of the disease, and that is, his case appears to be known far and wide, and is spoken of with wonder, which probably would not happen were the disease more common. On making inquiries of the patients as to the existence of elephantiasis, I have been in more than one instance told of this case, and this by persons belonging to districts very remote from that where the afflicted man lives.*

In this connexion it may be convenient if I introduce an account of my efforts to filariate monkeys, as I think the observations made on embryos after their withdrawal by mosquitos may tend to suggest a reason for the non-propagation of the disease in this island. I assume that all now take for granted the fact that the mosquito plays an essential part in completing the cycle of genesis, and that if this medium is absent or incapable, the further propagation of the parasite is suspended. It will be recollected that Dr. P. MANSON, in his search for the medium by which the canine blood filariæ attained their freedom, found that the mosquitos which nurtured the filariæ sanguinis hominis, when made to feed on an infected dog, *digested* the embryos they thus obtained, showing that if a mosquito was the intermediary host in the case of dogs, it must be a different species to that which acts as go-between in man. With these preliminary remarks (the bearing of which will be seen directly), I pass on to describe my experiments on monkeys.

The man from whom I obtained the embryos is a native of Amoy, at present employed as a boatman in Takow; of himself and his disease I will speak more fully further on, when I come to the observations made on him with reference to periodicity and other matters. The plan I followed was to make him sleep under a large gauze-covered cage ("mosquito house"), into which, each night, were put numbers of mosquitos freshly collected from all parts of the settlement, and to whose operations he cheerfully submitted himself, being apparently quite indifferent to both their numbers and their bites. Besides this, I had in the cage a breeding trough, also covered with mosquito netting, into which from time to time I put mosquito larvæ got from different places, even Taiwan-fu, the capital city, 30 miles north of this. As the mosquitos were matured, they flew up into their netting, from which I let them escape into the larger cage. I took care to carefully cover up the breeding trough, so that none of those which had fed could return and deposit their ova. For these latter I had a trough suspended in the

* I cannot help still thinking that further search must result in the discovery of the actual presence of more cases of elephantiasis, as one can hardly imagine but that amongst such numbers as are constantly coming and going to and from the mainland, some of the affected from so generally tainted a region would cross over, notwithstanding the prejudice entertained against travelling by Chinese who may be thought to be or are ailing to however slight an extent; but, of course, such instances can in no way affect the supposition as to the absence of disease traceable to local influences. Again, were the local influences favourable, these importations, so far from remaining inconspicuous, would, as centres of propagation, soon make their baneful presence markedly observable.

darkest part of the house, and filled with water as required. I then obtained five monkeys, four of which were young, lively, and apparently in good health, but the fifth, though highly intelligent, and trained by its previous owner to perform several tricks, was phthisical, and eventually died of pulmonary disease.

As soon as the water in the cage became sufficiently covered with ova, I gave it to the monkeys to drink; and here I may state a fact which I have not seen noticed elsewhere, and that is the strong antipathy these animals have to drinking water which appears impure or in which there are objects in motion. Besides the ova, there were always numerous mosquito larvæ darting about, these the monkeys would try to remove, but finding this impossible, would jump about screaming, and, attempting to upset the dish, utterly refuse to drink. Wishing to see whether, if impelled by thirst, they would get over this fastidiousness, I took care that they obtained no fluid but that contained in the bananas on which they fed; and in the case of one animal, a male, 10 days elapsed before any water was spontaneously drunk. Driven desperate by thirst, after making many attempts to brush away the ova and catch the larvæ, he suddenly dived in his head, took two or three deep draughts, and then sprang away screaming and chattering. With the others I had commenced daily drenching, as soon as I found their antipathy insuperable, and I eventually followed the same plan with the remaining monkey. We know there is a considerable amount of fluid in succulent fruits, but even with this, the interval that elapsed before the animal could bring himself to drink the water showed how strong must have been his instinctive objection to other than pure liquid.

For more than six weeks I continued to administer the water in which the mosquitos were daily depositing their eggs. After the first week I examined the blood of all the monkeys each day, both night and morning, but without result. In five weeks one of the subjects got fever, with cough, and died.

No signs of filariæ nor anything which could be attributed to them were found. About seven weeks from the commencement of the experiments another monkey died from pneumonia, and the postmortem was as barren of results as regards filariæ as the previous daily examinations had been.

About this time the man had to cease sleeping in my house, but I continued giving the water for a few days longer, until the mosquitos had disappeared from the cage and ova had ceased being deposited. The daily examination of the blood, however, still continued, but as fruitlessly as before, and this I continued to carry out up to the end of the fourth month, about which time another monkey (the educated one) died. Her body I put in spirits and sent to Dr. P. Manson, who told me that he found nothing but extensive tubercular disease.

The surviving monkey is still (November 1880) well and lively, and there are no traces of filariæ in his blood.

I attach no importance to the death of the other four animals in as far as the object of these experiments is concerned, as they all died of pulmonary affections, a not uncommon occurrence with monkeys kept in captivity. I may mention here that pulmonary disease is very prevalent amongst captive monkeys in Formosa, but whether this condition is frequent amongst the animals when in a free state I am not as yet in a position to say. Every day during the time the man slept at my house I caught a certain number of gorged mosquitos,

which I kept alive in bottles and duly labelled. On examining those who had fed on the previous night I readily found several lively embryos, but at no later date could I find other than semi-digested remains, which at a subsequent stage, however, were not to be seen. I inspected great numbers of mosquitos each day, examining several specimens from the lot caught and confined at the same date and hour. Thus I feel justified in speaking with the necessary certainty as to the results of the observations I describe.

I have no doubt that in all the cases which came under my notice the mosquito was an inhospitable host, digesting where it should have nurtured. As I have said before, the mosquitos were collected promiscuously without reference to locality, and therefore I am inclined to think that if the species which entertains the man-infecting filaria were common, I should have got it; and if my suspicions are correct, then it will be readily intelligible why we have no cases of elephantiasis or lymph scrotum referred to this place, and also why the only filariated individuals I could get hold of were those hailing direct from Amoy. I purpose, if practicable, getting supplies of the desired mosquito ova from the mainland, and will then continue my experiments.*

In the meanwhile I intend trying the local mosquitos on dogs. I hear from foreigners that "worms in the heart" are a cause of canine mortality here, although I have not been able to be sure that the deceased dogs have not been imported from, or at any rate lived for some time on, the mainland.

Should it turn out that in Formosa (an island only separated by a channel 180 miles wide from the mainland, and in constant communication with Amoy) there is immunity from filarial disease, and that this is due to the absence of a fit intermediary host, then a curious addition will have been made to our knowledge of the geographical distribution of this disease and the conditions necessary for its general existence and propagation.

It may, perhaps, chance that an investigation extending beyond the limits offered by this hospital might result in detecting a few more filariated individuals, especially as such a proportion of them originally come from or often reside in districts where Dr. P. MANSON tells us 1 in 10.8 of the population have filariæ in their blood;† but anything further that can be discovered might, I am inclined to conjecture, possibly serve as exceptions, which would the more firmly establish the fact of Formosa being, from some cause or another, unfavourable at least to the reproduction of the imported disease. Of course, after only 18 months' personal experience I cannot speak with any authority, nor indeed should I have ventured an opinion did it not seem that the carefully recorded experiences of those much more able to speak on the subject than myself confirm the conclusion which my comparatively limited observations have a tendency to support.

Before passing from this portion of my paper, I should state that (speaking from memory) the only mosquitos I have seen here appear to be much larger and darker than those in which the (embryo) filaria sanguinis hominis is supported, and which, through the kindness of Dr. P. MANSON, I was frequently able to see when on a visit at Amoy. At this

* I am also at present engaged in closely examining all the different species I can find, with a view to future description and classification.

† *See* Customs *Medical Reports*, xiv, 1.

time I also enjoyed the great privilege of having many of Dr. P. MANSON's discoveries personally demonstrated to me by that gentleman. I mention this to show that I have not entered on this investigation without previous instruction as to the method of procedure from the one best qualified to impart it. The benefit this is only those who have been so favoured can fully realise.

And now to detail the results of my observations for the purpose of investigating subject No. 2, viz., "To note the periods of appearance and disappearance of the embryos from the blood of infected persons, with a view of corroborating or disproving Dr. P. MANSON'S recent discoveries as to the periodicity displayed by the parasites in their appearance."

For this purpose I was able to persuade the boatman TO AH to submit himself for experiment, and although this is the only case from which I was able to get consecutive observations, still, I was at various times able to make such investigations upon two others as led me to hope that I might be justified in applying the rule *ex uno disce omnes*, and thus (subject to future confirmation) reduce the requirements of the present experiments within practicable limits.

As may be supposed, it is no little trial of a man's resolution to submit to almost hourly pricking, to say nothing of the irksomeness of being always the subject of close observation; and, I imagine, had not TO AH'S association with foreigners made him understand that no harm would be done, I should have found him as refractory as the other two, whom I could with difficulty persuade to allow me to make occasional corroboratory examinations. Besides which, TO AH, being in good health and doing his daily duty, seemed even more suitable than would be one of the patients, perhaps suffering from some affection that might influence the records. TO AH was under more or less supervision for six or eight months, and although I only give one series of examinations, I do so because these were the most consecutive; but for all that, I from time to time was able to get him to let me tap him, thus making sure that the records I had got, and have here tabulated, were true of his condition generally. The history he gave of himself in October 1879 is as follows:—

He is 28 years old, and a native of Amoy, where he resided until he was 21. From the time he was about 14 or 16 he has suffered at various periods from "fever and ague." At about the age of 18 or 20 he first noticed swellings in his groin, which, however, have increased but little; in fact, he thinks they show a tendency to lessen in size. He suffers during the hot season from sharp attacks of "fever and ague;" otherwise he is in good health, well nourished, and generally fit for his work. He was not aware he had filariæ in his blood, and does not think much of the fact, although he watched the embryos under the microscope with much interest. He has visited Amoy twice since he first came to Formosa, but as his friends and relations have all died off, he thinks of permanently settling here. He does not suffer from any inconvenience whatever when pulling, even long distances, in the gig, nor does he find that he is unfit for considerable exertion of a pedestrian kind, and often accompanies his master shooting, carrying a tolerable weight all the time. Is quite willing to allow me to make the experiments explained to him, and will be glad if he can be cured of his tendency to "ague" altogether, as "then he would be quite well."

There is nothing abnormal to be seen about his scrotum or legs, and in every way he appears an athletic, well-developed man.*

* With reference to his attacks of so-called "fever and ague," I must state that these are probably seizures of the characteristic ("lymphatic") fever, and differ from the malarial disease, which they resemble, in the absence of marked periodicity, there being generally a long interval between the attacks.

As to the other two cases which came under notice, they were admitted, one suffering from ague, and the second from a callous ulcer of the leg. No enlarged glands could be detected, nor any other ailment which might be attributed to filarial infection.

The blood parasites were in both cases found in considerable numbers at night, and up to about 7.30 or 8 A.M.

The blood was drawn from one or other of the fingers by slightly congesting them with a tape, and then pricking the skin with a sharp needle. At least two slides were charged at each observation, the maximum "find" being recorded. My Chinese assistant (whose observations I invariably checked) had been trained by Dr. P. MANSON, and had, under his supervision and direction, aided in similar work at Amoy.

TABLE I.

DAILY RECORD of FILARIA EMBRYOS found in a DROP of BLOOD taken from TO AH, a BOATMAN; and of the TEMPERATURE under his TONGUE at the MOMENT of TAPPING, showing PERIODICITY in APPEARANCE of EMBRYOS.

Days.	4 A.M.		6 A.M.		8 A.M.		10 A.M.		12 Noon.		4 P.M.		6 P.M.		8 P.M.		10 P.M.		12 Midnight.	
	No. of Embryos on Slide.	Temperature.	No. of Embryos on Slide.	Temperature.	No. of Embryos on Slide.	Temperature.	No. of Embryos on Slide.	Temperature.	No. of Embryos on Slide.	Temperature.	No. of Embryos on Slide.	Temperature.	No. of Embryos on Slide.	Temperature.	No. of Embryos on Slide.	Temperature.	No. of Embryos on Slide.	Temperature.	No. of Embryos on Slide.	Temperature.
		°		°		°		°		°		°		°		°		°		°
1	24	...	0	...	46	...	0	99.2	1	99.4	0	99.4	0	100	48	99.4	28	100.2	35	100
2	23	99.3	32	99.1	3	99.4	...	...	1	99.1	0	100	0	100.1	29	100	...	...	114	98.4
3	39	99.1	49	98.4	...	...	...	...	...	...	...	...	8	100	25	99.4	38	99.1	41	99.2
4	49	99.1	43	98.4	7	99.2	0	98.3	0	99	0	99.4	1	100.1	35	99	94	100	45	99
5	41	98.4	32	99	6	99.1	0	99.3	0	99.1	0	99.3	0	100	20	99.3	42	99.2	34	99.1
6	45	99	41	99.1	2	99.3	0	100	0	99.2	0	99.4	0	100	20	99.2	40	99.4	38	98.2
7	36	98.4	59	99	0	99	0	99.2	0	99.4	0	99.2	0	99.4	46	99	42	99.4	28	99.1
8	40	99	29	98.4	0	99.1	0	99.4	0	100	0	100	0	100.2	7	99.4	38	100	260	98.5
9	81	98.4	28	98.3	0	99	0	100.1	1	100.1	0	100.1	4	100.1	10	99.2	...	...	...	...
10	22	98.3	14	99	0	99	0	100.1	0	99.2	0	99.4	1	99.4	8	99	36	99.4	27	98.9
11	38	98.5	12	99	0	98.9	0	99	0	99.3	0	99.4	0	99.6	6	98.9	22	99.4	35	99
12	33	98.4	15	99	0	99	0	98.9	0	99	0	99.1	0	99.8	3	98.6	40	99	120	99
13	41	98.5	10	98.8	0	99	0	99	0	98.9	0	99.2	0	99.7	12	98.5	29	99.1	62	99
14	21	98.3	9	99	0	99	0	99.1	0	98.6	0	99	0	99.6	5	99	80	99.3	170	99.4

It will be seen that the records amply bear out Dr. P. MANSON'S statements. The embryos appeared regularly between 6 and 8 P.M., generally a little after 6. In the 14 observations made at 6 P.M. there were 10 blank searches, and 4 in which embryos were present. By 6.45 P.M. they had begun to appear regularly, although still in small numbers, and it was not until 7.15 P.M. that they had become numerous. By midnight they would seem to have attained their

maximum, and from that hour gradual decrease set in. In the morning they would also appear to retire between the hours of 6 and 8, which gives a period of 12 hours during which they disport themselves. As Dr. P. MANSON has already pointed out, this is just the time when their liberators are in active search for food, suggesting one of those remarkable but never-failing adjustments met with all through nature.

Only on 3 occasions out of the 14 have I been able to see any embryos in the interval from about 8.30 A.M. or 9 A.M. to 12 noon, though I have diligently searched for them, sometimes charging four or five slides. All these appearances were put in at the noon examination, and on no occasion was more than one embryo present.

The temperature was taken *before* drawing the blood, so as to obviate any risk of its being affected by the operation, if perchance this could be looked for.*

It will be noticed that at the hour when the embryos return the temperature rises slightly, more than perhaps can be attributed to ordinary evening elevation. As soon as they had appeared in marked number (8 P.M.), the temperature did not go, save on one occasion (2nd day) above 99°.4, and on 3 (11th, 12th, and 13th days) stood below 99°; at 10 P.M. it again rose, however, as four times the thermometer registered 100°, and at no time fell below 99°. At 12 midnight, when the embryos were in greatest number, the mercury appears to have fallen somewhat, as though it registered 100° once (1st day), it was 8 times at between 99° and 99°.5, twice showing 99°.1. Four times it fell to between 98°.4 and 99° (2nd, 6th, 8th, and 12th days). By 4 A.M. there is a considerable fall, 98°.2 to 98°.5 being recorded 8 times, with the embryos in ample numbers. At 6 A.M. there is a slight tendency to rise, and from 8 o'clock onwards this is well marked.

On the whole, therefore, it would appear from this case that the temperature bears no very marked relation to the number or activity of the embryos; still some influence seems shadowed forth. Of course, with observations taken from one case, and only occasionally compared with two others, these temperature results are at present of little or no scientific weight.

I have examined To AH on several occasions during the time he was under my observation, and I have found that his temperature was, as a rule, somewhat higher than normal, and tended to vary considerably in the 24 hours; this, too, at times when he had had no attack of "ague" for a considerable period, and said he felt quite well. I therefore merely offer these temperature notes for what they are worth, trusting they may tempt some one more advantageously placed for seeing a number of filariated persons to take regular observations, and prove or disprove what I can now only surmise as possible.

In as far as the observations on embryos and the periodicity of their advent or exit goes, this table may be more valuable, as it is an addition to observations taken by others, and in another locality, further borne out as it is to some extent by what I have been able to learn from the other cases, in as far as their complaisance would permit.

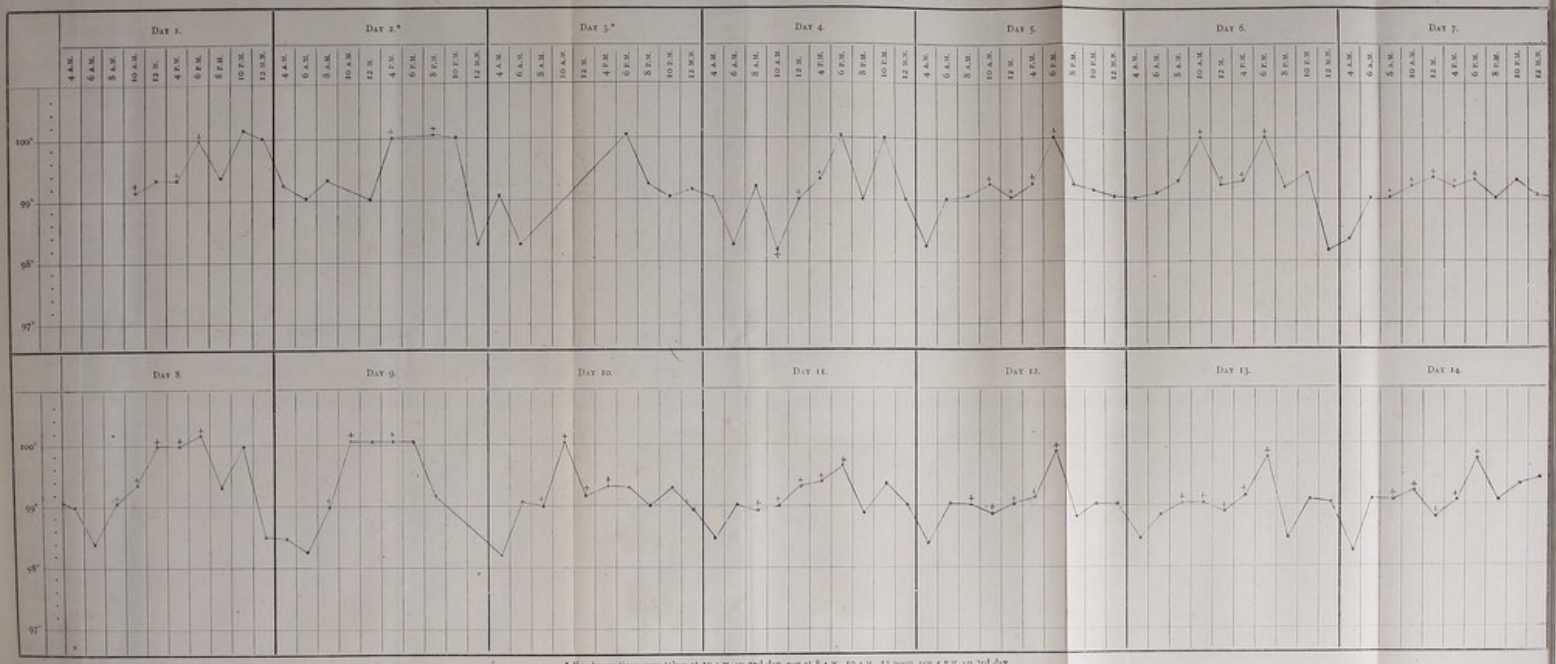
I next come to speak of the results of my observations on what at the beginning of this paper I put as the third object for investigation, viz., "To account, if possible, for the

* *See* Chart 1, opposite.

CHART 1.

Showing To Ab's Temperature (under Tongue) for a period of 14 Days consecutively.

(Hours at which Embryos were absent from Blood marked thus +.)



* No observations were taken at 10 A.M. on 2nd day, nor at 8 A.M., 10 A.M., 12 noon, nor 4 P.M. on 3rd day.

disappearance of the embryos at certain hours, discovering, if feasible, whether this was final as regarded the swarm, or whether they lay dormant and adherent during certain periods in the lungs or other organs of the body."

In order to do this I got To Ah once more to submit himself to examination for a series of seven days, commencing my examination at 6.15 A.M., and taking observations at least every quarter of an hour until the embryos had disappeared.

In the following tables I give the results, with a record of simultaneous temperature. From what Dr. P. Manson has written I gather that he is inclined to think the embryos do not periodically dissolve in the blood, but that they probably congregate in some organ (possibly the lungs), and there remain until the time arrives for their wanderings and withdrawal by mosquitos. In favour of this view he gives the results of postmortem examinations of dogs, and, finding a great congregation of embryos in the lungs, he suggests—if analogy is thought to have any bearing on the matter—that this supports the conjecture he has advanced.

I own that I am not as much influenced by this as Dr. Manson would seem to be, for (as affecting the value of the experiment) I am inclined to lay considerable weight on the fact that the embryos are never absent from the blood of infected dogs, but, on the contrary, are at all times circulating in considerable numbers. For this reason it appears to me that at a postmortem examination, where the blood is in a state of stagnation, and a drop is deliberately taken from any great hæmic collection, one would naturally expect to find the embryos thus secured in directly greater proportion to the mass of blood from which they are then drawn than in the case of those escaping from a minute puncture in the smallest vessels, with the current rapidly sweeping past the opening. Reasoning likewise from analogy, I should rather assume that were it possible to get a postmortem on a man suddenly killed during the night, when the embryos are in full vigour, we might expect to find the greatest concentration in a drop taken from the largest mass of blood. But I should hesitate before deducing the theory that where embryos are then found most numerous, there they rest at certain periods during the life of the host.

I am, however, unable to think that the two cases are sufficiently analogous, or even approximately so, as, unless we had a case where the embryos were during a certain time absent from the circulation, and in those hours found them concentrated in some large centre, we could not fairly look on the observation as in any way suggestive of the conduct of the embryos when in a condition the reverse of that solely presented by the subject under observation. Again, I am inclined to doubt whether the relatively few mosquitos which can get at either dogs or men during the limited time available for their operations could make any very appreciable diminution in the myriads of embryos which must be existent in the body; and unless the parent worm either breeds but once, or breeds only at excessively long intervals, it would seem that some more rapid mode of disposing of the offspring than that offered by the mosquito would be required, in order to avoid the choking up of the vessels by the blood-displacing embryos. Judging, however, from analogy as to the generative power of the parent worm or worms (for it must be remembered there may be many), one is justified in

assuming the produce to be frequent and enormous, more so than thousands of mosquitos, even if constantly at work, could manage to keep within bounds. Further, arguing on the same basis, may we not assume that nature has provided by this very excess for the security of propagation, which would be hazarded were there only a proportionate number of embryos to that of mosquitos available, herself providing means for the absorption of the residuum in time to make way for a fresh supply, which one of her equally imperative ordinances is sure to bring forth? Again, if those generated had to wait their turn in the lottery of selection, would this not seem to necessitate a condition of unusual and indefinitely limited arrest of development in the young, and of function in the parent? It is to be further noted that from both the poisons used by Dr. MANSON in destroying the dogs, lung engorgement is seen after death, and this would, I think, readily suggest why (the largest collection of blood remaining in these organs) the embryos should be in excess there, while, coupled with this, it is not improbable that, under the circumstances, there should be a strong local attraction for the embryos.

As may be supposed, I hesitate considerably in offering opposition to the views of so able and painstaking an observer as Dr. MANSON; and in this case, if his observations had been the subject of my dissent, I should have been far more diffident in venturing to join issue with him. Here, however, I merely query the conclusions he draws from certain data, as to my mind they do not seem quite to follow the premises; and though in suggesting the possibility of diurnal solution as the end of such of the embryos as do not come within mosquito range, I advance certain observations of my own, still, as far as I know, they extend in a different direction from that previously explored, and, of course, are in turn open to be verified or shown inconstant by the subsequent investigations of those who may take the trouble to check me.

So much for theory. I will now submit the only experiments I have been able to make in reference to the matter; and, knowing how inadequate they are, both in number and nature, I can only offer them as first steps in that research for which much ampler opportunities must be afforded.

If the embryo be constantly watched after withdrawal from the body, the primary symptoms of debility visible will be its tendency to stretch itself out. At first the oval which it forms when in a state of vigour and activity will be seen to become more and more perceptibly elongated. The almost indistinguishable movements will appear more isolated, and as weakness increases, these change into a semi-undulatory action, which at the last gradually ceases from the centre outwards, until the feeble motions of head or lash are all that remain to denote vitality. The lash becomes more distinct, and the integument puts on a somewhat shrivelled appearance. When death takes place, the embryo is seen to be extended often at full length, and generally so to a great extent. After practice it is easy to classify the embryos under the different stages from perfect vigour through extreme debility to death, and this experience may be readily gained by preserving the slides and watching the movements until life has ceased. In order to carry out the experiments I proceed to detail, it will be better to familiarise oneself with these appearances, and keep constantly refreshing the memory by comparing the embryos recently abstracted with those which by dint of keeping have been

reduced to various states of debility. At least, I found this method of procedure useful, and it appeared to me likely to secure most exactitude.

As will be seen from Tables 2 and 3, I commenced my observations at those hours in the morning and evening when I knew by experience the embryos would be retiring and reappearing, on each occasion examining the slide immediately on preparation, and frequently both between the intervals and afterwards (they were, of course, all labelled); and when I could persuade my subject to let me, which was not without difficulty, I tapped at periods other than and between those recorded, with a view of checking the latter.

TABLE 2.

ORNING RECORD of OBSERVATIONS on BLOOD taken from TO AH (GIGMAN), commencing at 6.15 A.M. and continue until the EMBRYOS had disappeared; and of TEMPERATURE of his MOUTH.

6.15 A.M.		6.30 A.M.		6.45 A.M.		7 A.M.		7.15 A.M.		7.30 A.M.		7.45 A.M.		8 A.M.		8.15 A.M.		8.30 A.M.		8.45 A.M.		9 A.M.		9.15 A.M.		9.30 A
No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.
	°		°		°		°		°		°		°		°		°		°		°		°		°	
18	98.3	12	98.4	13	98.5	3	98.4	0	99.1	0	99	0	...	0	...	0	...	0	...	0	...	0	...	...	...	...
18	98.3	10	99.1	0	99.1	4	99.2	5	99.1	4	99.1	4	99.1	0	99.1	0	99.1	0	...	0	...	0	...	...	...	...
44	98.4	9	98.4	16	99	9	98.4	26	98.2	...	...	5	98.2	...	...	7	99.2	3	99.2	2	99.2	0	99	...	...	...
10	98.5	52	98.6	9	97.4	16	97	31	97	3	97	2	97	6	97.2	11	97.4	20	97.3	6	97.4	3	97.1	0	97.2	0
17	98.5	8	98.4	3	98	2	97.9	7	97	1	97.8	1	97.8	1	97.8	1	97.1	3	97.8	0	...	0	...	...	...	...
29	98.3	10	98.1	6	98.3	0	98	1	98	4	98	0	98	1	98.2	0	98.4	0	...	0	...	0	...	...	...	...
36	98.4	11	98.3	4	98.3	0	98	3	97.8	0	97.9	0	98	0	98	0	98.8	0	98.5	0	98.3	0	98.5	...	...	...

TABLE 3.

EVENING RECORD of OBSERVATIONS on BLOOD taken from TO AH (GIGMAN), commencing at 6.30 P.M. and continued until the EMBRYOS had reappeared in NUMBERS; and of TEMPERATURE of his MOUTH.

DAYS.	6.30 P.M.		6.45 P.M.		7 P.M.		7.15 P.M.		7.30 P.M.	
	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.	No. of Embryos.	Temperature.
		°		°		°		°		°
1	0	...	0	...	4	99.2	14	99.2	17	99.1
2	0	100	1	100.1	5	100	24	99.4	40	99.3
3	0	100	1	100	4	99.4	30	99.5	25	99
4	0	99.8	2	100.1	6	99.3	20	99.2	25	99
5	1	99.8	4	100.2	7	99.8	15	99.3	30	99.5
6	0	99.6	2	100	5	100.1	25	99.8	27	99.4

I believe, however, that examinations made every quarter of an hour, with the occasional checks I have indicated, are sufficient for the purpose, and therefore, to avoid confusion, I have contented myself with merely noting these.

The following are the notes I made, on each occasion specified, of the conditions of the embryos then seen. I would state that on two days I was fortunate enough to secure the presence and assistance of Dr. JOHN DUDLEY, R.N.; that days 1, 2, 3 and 4 were consecutive, but 5, 6 and 7 were taken with intervals between; that where morning and evening observations are recorded they were made on the same day; and that I have, whenever I could persuade the man to allow me, and his duties would permit, tried to verify my former experiments; but that the notes of those given are selected because they were the only ones I have so far been able to get continuously, and besides, the appearances were so very similar as to render further record unnecessary.

First Day, 6.15 A.M.: 18 *Embryos on Slide.*—1st to 5th normal; 6th to 10th languid, stretched out; 10th to 14th normal; 15th weak, stretched out, body bent in distinct folds; 16th very much stretched out, weak, only lower two-thirds of body appears to be moving; 18th normal.

6.30 A.M.: 12 *Embryos.*—1st to 4th normal; 5th stretched out, decidedly weak; 6th and 7th stretched out, somewhat languid, and pauses between contractions marked; 8th stretched out, languid; 9th to 11th normal; 12th very weak, scarcely moving, shrivelled appearance.

6.45 A.M.: 13 *Embryos.*—1st to 7th apparently normal; 8th stretched out, languid, folds in body well marked and retained during appreciable pause; 9th very weak, stretched round in circle, head only moving occasionally; 10th brisker but decidedly weak; 11th and 12th normal; 13th stretched out, lash distinct and moving vigorously, upper part of body much less vigorous.

7 A.M.: 3 *Embryos.*—1st very weak, stretched out, what little motion there is undulatory; 2nd more vigorous, but stretched out; 3rd normal.

Second Day, 7.15 A.M.: 5 *Embryos.*—1st and 2nd extended, body movements not as vigorous as usual, and more distinctly localised; 3rd, very slight motion at lower extremities, getting less and less under observation, body puckered and shrivelled; 4th, movement only in head, stretched-out body, shrivelled appearance; 5th, body stretched to full length, slightest movement of lash, which ceased under observation.

7.30 A.M.: 4 *Embryos.*—1st, semi-convulsive jerks of body, the bends taking place at each movement being distinctly separated by inactive portions of body and apparently deeply indented, as though the body was more impressible, retaining the mark until the next movement took place; half stretched out; 2nd apparently as vigorous and well as usual; 3rd, movements much more sluggish, folds very slowly straightened out at each jerk, elongating itself, movements apparently involving same bends and folds of body every time; 4th much more vigorous in action, but with marked tendency to elongation, and the pauses between motions well marked.

7.45 A.M.: 4 *Embryos.*—1st very feeble in its action generally; 2nd extended and markedly feeble, same signs of irregularity in action observed; 3rd lively, and apparently as usual; 4th very feeble, much stretched out, action spasmodic, with marked pauses.

Third Day, 6 A.M.: 29 *Embryos.*—Many lively and apparently well, some very languid, scarcely moving, stretched out, with head and tail touching so as to form a large circle, apparently just dying.

6.15 A.M.: 44 *Embryos.*—Numbers of them were as usual, but others very much debilitated, body stretched out, and movements decidedly languid.

6.30 A.M.: 9 *Embryos.*—1st languid, tendency to elongate and form circle by approximation of tail and head, folds and pauses marked, shrivelled; 2nd very similar in condition; 3rd still more feeble, folds in body very marked, giving an appearance as though body would break at each movement; 4th and 5th

stretched out and moving with extreme languor; 6th *quite dead.* (N.B.—This is the first time I have come across a dead embryo in a drop of blood freshly drawn. The slide had as usual been carefully washed and dried prior to use.)

6.45 A.M.: 16 *Embryos.*—1st very languid, stretched out; 2nd and 3rd quite stretched out, only very slight movement in head, both almost dead; 4th and 5th ditto, ditto, creased appearance very marked. The remainder were more or less debilitated and stretched out, with the exception of two, which were quite vigorous and apparently well.

7 A.M.: 9 *Embryos.*—1st lively and normal, though slightly inclined to stretch out; 2nd stretched out in semi-circular form, decidedly weak, movement more distinctly undulatory; 3rd languid, stretched out; 4th and 5th stretched out, somewhat weak in action; 6th very feeble, movement slow and intermittent, made apparently with great effort; 7th still less active, seemingly nearly dead; 8th and 9th normal.

7.15 A.M.: 26 *Embryos.*—1st to 4th nearly normal; 5th stretched out, much more languid; 6th stretched out, very languid; 7th stretched out, with undulatory movement; 8th, movements feeble, one extremity (oral) motionless, apparently paralysed, stretched out.

7.45 A.M.: 5 *Embryos.*—1st feeble, stretched out; 2nd ditto, very weak; 3rd normal; 4th slight motion, semi-paralysed appearance; 5th normal.

8.15 A.M.: 7 *Embryos.*—1st active, normal; 2nd feeble, stretched out, undulatory movement; 3rd feeble, stretched out; 4th stretched out, activity not however affected; 5th stretched out, rather feeble, and tendency to drag lower half of body; 6th curled up, scarcely moving; 7th normal.

8.30 A.M.: 3 *Embryos.*—1st normal; 2nd stretched out, movements feeble and intermittent, with distinct pauses; 3rd normal.

8.45 A.M.: 2 *Embryos.*—1st normal; 2nd more stretched, movements less vigorous.

Fourth Day, 6.15 A.M.: 10 *Embryos.*—1st very languid and stretched round in wide oval shape, movements spasmodic; 2nd very feeble, only moving after long pauses and only at either extremity, the central portion of body being motionless; 3rd barely moving, and then only with a weak undulatory motion, stretched out; 4th stretched out, only movement in head, very spasmodic and irregular; 5th normal; 6th much extended, slow, undulatory movement running spasmodically along the whole body, beginning at tail, pauses very marked; 7th quite extended, though movement is brisker and more regular than last; 8th and 9th stretched out, slow, feeble, convulsive movement of head and tail; 10th stretched out, very feeble.

6.30 A.M.: 52 *Embryos.*—1st stretched out, action much more languid than normal, with appreciable pauses; 2nd and 3rd nearly normal, stretched out; 4th curled up, very feeble; 5th same as 3rd; 7th to 9th stretched out, very weak; 10th to 12th feeble, with spasmodic action, exhibiting distinct pauses; 13th stretched out, brisker than last, but pauses very marked; 14th stretched out and very feeble; 15th and 16th extended, feebly moving from head downwards in slow wave-like manner, getting feebler under observation; 17th stretched out, motions spasmodic, with marked pauses; 18th stretched out, otherwise normal; 19th formed in wide ring, very feeble movement of tail detected at long intervals; 20th much brisker, stretched out; 21st and 22nd, nearly normal, stretched out; 23rd motions spasmodic, slow, feeble; 24th and 25th feeble, stretched out; 26th much extended, very feeble; 27th ditto, but more brisk; 28th normal; 29th to 31st, very feeble, motion spasmodic and slow; 32nd and 33rd very feeble, stretched out; 34th and 35th normal; 36th to 40th more languid than normal, stretched out; 40th to 45th normal; 46th and 47th stretched out, very weak; 48th to 50th stretched out, nearly normal; 51st stretched out very weak, motion spasmodic and slow, with long pauses; 52nd normal.

6.45 A.M.: 9 *Embryos.*—1st and 2nd quite dead; 3rd stretched out, very feeble; 4th to 9th nearly normal.

7 A.M.: 16 *Embryos.*—1st to 4th stretched out, nearly normal; 5th and 6th stretched out, very feeble; 7th normal; 8th to 11th stretched out, feeble; 12th to 15th stretched out, very feeble; 16th stretched out, movement barely discernible.

7.15 A.M.: 31 *Embryos.*—1st stretched out, otherwise nearly normal; 2nd stretched out, very feeble; 5th and 6th stretched out and feeble; 7th stretched out, very feeble, motion spasmodic and slow; 8th and 9th stretched out, feeble; 10th normal; 11th and 12th stretched out, but motions lively; 13th to 15th stretched out, motions slow and spasmodic, altogether very feeble; 16th normal; 17th very feeble, head only moving; 18th stretched out, lower one-third of body motionless, giving paralysed appearance, movements of other portion spasmodic and slow, stopping suddenly at the quiescent part; 19th stretched out full length, slow convulsive movements just discernible; 20th stretched out, feeble, with slight spasmodic motion; 21st stretched out, nearly normal; 22nd and 23rd normal; 24th stretched out, feeble; 25th stretched out, very feeble; 26th stretched out, feeble; 27th normal; 28th very feeble, formed in large ring; 29th normal; 30th stretched out, feeble; 31st stretched out, very feeble.

7.30 A.M.: 3 *Embryos.*—1st and 2nd stretched out, very feeble; 3rd nearly dead, stretched out, and motion, when visible, confined to lash.

7.45 A.M.: 2 *Embryos.*—Both very feeble, one much in same condition as No. 3 on last slide.

8 A.M.: 6 *Embryos.*—1st and 2nd very feeble, scarcely moving, well extended; 3rd to 6th very feeble, though less nearly moribund than first two seen.

8.15 A.M.: 11 *Embryos.*—1st to 4th normal; 5th to 11th all in different stages of debility, stretched out.

8.30 A.M.: 20 *Embryos.*—1st normal; 2nd stretched out, feeble; 3rd to 7th normal; others more or less feeble (except 18th and 19th, which were normal), stretched out.

8.45 A.M.: 6 *Embryos.*—1st and 2nd very feeble, barely moving, stretched out; 3rd to 6th stretched out, feeble movements, though brisker than 1st and 2nd, still materially affected.

9 A.M.: 3 *Embryos.*—1st and 2nd feeble, stretched out; 3rd nearly dead, movement only observed after long intervals.

Fifth Day, 7 A.M.: 2 *Embryos.*—1st stretched out, motions slow, spasmodic and irregular; 2nd very feeble, stretched out, folds or bends in body well marked and permanent, almost dead, movements only discernible at long intervals, body appears shrivelled.

7.15 A.M.: 7 *Embryos.*—1st stretched out, feeble, movements irregular, with distinct pauses; 2nd stretched out, semi-irregular, spasmodic motions; 3rd very feeble, stretched out; 4th stretched round in wide circle, only slightest visible movement, nearly dead; 5th stretched out, slow undulating movement at long intervals, very feeble; 6th stretched out, very slight movement in head only; 7th appears quite motionless and dead, but after long observation slight movement detected in lash.

7.30 A.M.: 1 *Embryo.*—Very feeble, only slightest movement in head.

7.45 A.M.: 1 *Embryo.*—Stretched out, very feeble, slight spasmodic movement.

8 A.M.: 1 *Embryo.*—Very feeble, irregular movement at intervals; stretched out.

8.15 A.M.: 1 *Embryo.*—Same as one on last slide.

8.30 A.M.: 3 *Embryos.*—1st stretched out, very feeble; 2nd stretched out, brisker than last, but still decidedly enfeebled; 3rd stretched out, very feeble, movement irregular, spasmodic, folds or bends well marked.

Sixth Day, 7 A.M.: *No Embryos.*

7.15 A.M.: 1 *Embryo.*—Quite dead, and shrivelled.

7.30 A.M.: 4 *Embryos.*—1st very feeble, stretched out; 2nd dead and shrivelled, lower one-third of body was very transparent, and with difficulty made out; 3rd stretched out, very feeble, body corrugated; 4th stretched out, very feeble, slight movement, chiefly confined to lash.

7.45 A.M.: *No Embryos.*

8 A.M.: 1 *Embryo.*—Almost dead, stretched out, with flattened appearance.

Seventh Day, 6.30 A.M.: 11 *Embryos.*—1st to 5th stretched out, very feeble; 6th apparently quite dead, extended in circular form; 7th and 8th normal; 9th to 11th stretched out, and all more or less feeble.

6.45 A.M.: 4 *Embryos.*—1st and 2nd stretched out, very feeble, scarcely moving, and what motion there is, confined to extremities.

7.15 A.M.: 3 *Embryos.*—1st normal; 2nd stretched out and much less vigorous, motion irregular and chiefly confined to lower extremity; 3rd coiled in two loops, very feeble, irregular movement, with marked pauses, evidently in last stage of debility.

The foregoing are the notes taken immediately on each slide being charged, and from the appearances therein described I think it is evident that some decided change takes place in the condition of the embryos just prior to their retirement from the circulation. The contrast with the vigorous, rapidly-moving organism seen at night was very marked. Then, the movements are almost too rapid to be distinguished; now, in the majority of cases, irregular and with distinct pauses. Then, the stretching out or extension of the body being but temporary, and apparently only with a view to alteration of position; now, is permanent, and obviously the result of weakness.

It may be suggested that these are only the premonitory signs of approaching and temporary lethargy, and that those embryos which seem to be defunct are not so in reality, but in a state wherein they remain until the mosquito calls them forth in renewed vigour for their nocturnal rambles. I am inclined to doubt that in this state any mere power of adhesion would enable them to resist the force of the blood current. In fact, if we do suppose them to congregate in any central part during the hours of their absence from the circulation, they would need to expend much energy in order to maintain their position. It would be necessary for them to work at full speed against the tide, just as the typhoon-pressed vessel is sometimes able to preserve her place only by "steaming to her anchors."

Again, I think if torpor were the state indicated by the appearances I have described, the departure of the embryos would be more simultaneous, instead of being comparatively so gradual as we find it.

Of course, in the present state of our information all must be more or less surmise; still I am prompted to think, from what I have seen, that creatures exhibiting such signs of physical debility just prior to their withdrawal from the blood are in reality approaching a condition which ends with their existence.

I have not thought it necessary to describe in detail each evening observation, for all the embryos seemed to be vigorous and healthy, exhibiting a marked contrast in condition to those extracted in the morning. As it is of great importance, in order to get reliable observations, that extreme precautions be taken to avoid injuring the embryos, it may not be out of place to allude to the consequences of carelessness in withdrawing the blood or applying the covering slide. The blood drawn should flow or spring freely from the puncture, without any extraneous aid save that afforded by the slightly constricting band previously applied to the finger. When squeezing or rubbing of the part, in order to force out more blood, was attempted, I found that the serum was separated in the drop before it became large enough for transfer to the slide, and that the embryos were invariably rendered languid in proportion to the distance to which they were removed from the corpuscles. On the slide, towards the edges, it will sometimes be found that a band of serum is formed almost free from corpuscles, or with these in diminished

number. Any embryos found here, or that may work themselves into these limits, are decidedly debilitated or become rapidly so on arrival therein. By watching the entrance of an embryo into the serous area, and contrasting its condition after deprivation of corpuscular contact with that previously observed, the effect will be more readily seen. For these and mechanical reasons, should the drop extracted be insufficiently copious to allow of the blood equally disseminating itself over the whole surface immediately covered by the upper glass, and pressure other than the weight of the covering glass be used to effect this, then the embryos will be weakened and the preparation rendered unsuitable for the purposes in view. Of course, where the mere presence of embryos is all that is desired to be proved, these precautions are unnecessary, although if it be desired to ascertain the numerical relation of embryos to as nearly certain a quantity of blood as can be estimated short of actual measurement, then I think it will be found best to so make the puncture that, as far as one can judge, the same amount of blood springs from the orifice each time. I make my puncture in the middle of the second joint of any of the fingers with a No. 5 sewing needle, and always feel the point against the bone. Done rapidly, this does not appear to cause either increased pain at the time nor irritation afterwards; and for the constriction of the finger I have found a divided india-rubber letter-band bound lightly round the first joint quite sufficient.

Knowing these risks, and with them fully before my mind, I observed the greatest care in preparing the slides which formed the subjects of my investigation into the relative condition of the embryos extracted in the morning and evening, and I can only say that if error has crept in, I have done what in me lies to avoid it; but at the same time, where so much depends on one's ability to appreciate and estimate signs almost entirely consisting of comparative degrees of motion, a single observer may easily be misled or mistaken. To obviate this risk as far as possible, I have sought the assistance of others, and thus far I have met with no difference of opinion.

The experiments first made 15 months ago were undertaken, as previously mentioned, in the presence and with the assistance of Dr. JOHN DUDLEY, R.N., then of H.B.M.S. *Mosquito*; by a coincidence, my last observations were made a few days ago with the assistance of Dr. MCKINLAY, R.N., of the same ship, while at the same time I was fortunate enough to secure the presence and assistance of Dr. PETER ANDERSON, of the English Presbyterian Mission. To these two latter gentlemen I briefly explained my suspicions, and asked them to examine the blood in the morning and evening. I explained and demonstrated at the same time the consequences of carelessness in preparing the slides, and left them to make their own observations. They authorise me to state that, as far as this case goes (and of course none of us can go farther), they agree with the statements I have made. I have also submitted this paper to them, and they continue their support to the descriptions I have given in it of the behaviour of embryos after nocturnal and morning extraction.

In order to secure greater accuracy, I had previously instituted another series of experiments, and although I was not here able to secure the control afforded by simultaneous observations made by other professional men, still I was during the first three days fortunate enough to obtain the presence and assistance of one whose powers of observation have been considerably quickened by his studies in the field of geology and kindred sciences. I am happy

to say that this gentleman also confirms my conclusions. I allude to the Rev. DAVID SMITH, of the English Presbyterian Mission, who became greatly interested in the subject, and through whose intervention I hope to obtain specimens of mosquitos from different parts of the country, as well as further information concerning the presence or absence of elephantiasis and allied diseases in the various districts he may visit in pursuit of his calling.

In order to test the longevity of the embryos after withdrawal from the body, we prepared slides, with all precautions, at 9.30 P.M. and from 7.45 A.M. With a view of preventing desiccation, the slides were carefully oiled for a space of about a quarter of an inch round their rims; blood was then drawn in the manner before described, the covering glass (of similar size and thickness to that on which the blood lay) was then carefully adjusted, and only those preparations were selected on which the blood and its corpuscles were, as nearly as we could judge, equally disseminated. In order to ensure greater opportunities for contrast, blood was extracted at 7.45 A.M. and 8 A.M., these being the morning hours which reference to Table 2 seemed to suggest as being the most suitable. Each embryo was carefully observed and its condition noted, and the following results were obtained. I have arranged the notes in parallel columns for facility of comparison.

MORNING SLIDES.

First Day:—1st Observation, 7.45 A.M.: 12 *Embryos.*

1*st* stretched out, spasmodic action, extremities more active than centre, very languid.

2*nd* stretched out, extremities active, generally languid.

3*rd* stretched out, almost dead, extremities slightly moving with slow undulatory motion.

4*th* stretched out, extremities very active, motion spasmodic.

5*th* stretched out, general movement, but less vigorous than normal, action spasmodic, with distinct pauses.

6*th* stretched out, very weak, action spasmodic.

7*th* more active, stretched out, pauses very marked.

8*th* very languid, two-thirds of body gyrating slowly round passive upper third, which is extended.

9*th* very nearly normal in action, slightly stretched out.

10*th* very languid, about two-thirds of body moving spasmodically, stretched out.

EVENING SLIDES.

1*st Day*, 9.30 P.M.: 31 *Embryos.*

Every one appeared to be vigorous and moving with great rapidity, presenting a "star-like appearance;" motion so rapid as to be quite undistinguishable; no stretching out or other sign of debility to be seen in any of the embryos coming under observation.

MORNING SLIDES.

11*th* stretched out, spasmodic, interrupted action, quite languid, remaining passive for some time.

12*th* stretched out, less languid than last, though evidently weak.

8 A.M. : 7 *Embryos.*

1*st* stretched out, very languid, one portion gyrating slowly round passive part.

2*nd* languid, gyrating, spasmodic action, stretched out.

3*rd* stretched out, very languid.

4*th* stretched out in long semi-oval, very feeble.

5*th* stretched out, spasmodic action, very languid, motion confined to extremities.

6*th* more active than last, stretched out.

7*th* stretched out, nearly normal action, though pauses are discernible.

2nd Observation ; Preparation 12 *hours old.* 7·45 A.M. *Slide, only* 8 *Embryos now visible.*

1*st* quite dead.

2*nd* stretched out, languidly moving.

3*rd* much the same as last.

4*th* ditto, ditto.

5*th* more vigorous than others.

6*th* stretched out, but very vigorous.

7*th* stretched out, languid.

8*th* stretched out, much more feeble, what action there is, spasmodic.

8 A.M. *Slide, only* 5 *Embryos now visible.*

1*st* upper two-thirds of body curled, and slowly moving on lower one-third, very feeble.

2*nd* ditto, ditto.

3*rd* stretched out, weak undulatory motion.

4*th* stretched out, more vigorous.

5*th* stretched out, very weak.

Second Day :—3rd Observation at 9.30 P.M.; *Preparation* 36 *hours old.* 7·45 A.M. *Slide,* 8 *Embryos only visible.*

1*st and* 2*nd* dead.

3*rd* wriggling itself in knots, then passive for two or three seconds, convulsively freeing itself, pausing a like time, and then repeating the process.

EVENING SLIDES.

Second Day :—2nd Observation at 9.30 P.M.; *Preparation* 24 *hours old.* 9.30 P.M. *Slide,* 31 *Embryos visible.*

All more stretched out than on previous evening, and the movements less vigorous, especially about oral extremity.

Two embryos seemed less active than rest, but otherwise no very marked signs of debility.

MORNING SLIDES.

4th stretched out, more vigorous, spasmodic action, with long pauses.

5th stretched out, much attenuated, resting by head and tail, very feebly swaying rest of body.

6th head moving languidly to and fro, lower part of body convulsively starting occasionally, but no general movement, very feeble.

7th moving languidly, with undulatory motion along body, distinct pauses, stretched out to full length.

8th quite dead, attenuated, transparent, and shrivelled up.

8 A.M. *Slide*, 36 *hours old;* 5 *Embryos visible.*

1st and 2nd dead.

3rd very feeble motion in oral extremity, otherwise passive.

4th ditto, ditto.

5th very languid movement in both extremities.

Third Day:—4th Observation at 9.30 P.M.; *Preparation* 60 *hours old.* 7.45 A.M. *Slide*, 5 *Embryos visible.*

1st only upper third of body present, this very transparent and attenuated.

2nd lower one-third gyrating slowly and convulsively round rest of body, which is perfectly passive.

3rd similar appearance, though the active portion is more vigorous.

4th stretched out, very feeble undulatory motion visible at long intervals.

5th stretched out, slightly more vigorous.

No traces of others on slide.

Fourth Day:—5th Observation at 9.30 P.M.; *Preparation* 84 *hours old.* 7.45 A.M. *Slide*, 2 *Embryos visible.*

1st very nearly dead, motion very feeble.

2nd quite dead.

No others nor traces of others visible on slide.

8 A.M. *Slide*, 2 *Embryos visible.*

1st very feebly moving.

2nd only about two-thirds of body present, shrivelled up and attenuated.

No others nor traces of others to be seen.

EVENING SLIDES.

Third Day:—3rd Observation at 9.30 P.M.; *Preparation* 48 *hours old.* 9.30 P.M. *Slide*, 31 *Embryos present.*

All more or less stretched out, and motion more undulatory. Lash in one or two instances becoming visible. One very feeble, though movement was general as to the body.

Fourth Day:—4th Observation at 9.30 P.M.; *Preparation* 72 *hours old.* 9.30 P.M. *Slide*, 30 *Embryos visible.*

All more languid than last night, but still movements in most cases very lively.

Three embryos appeared specially weak, and central portion of body nearly passive, but the general uniformity of condition was very striking.

MORNING SLIDES.

Fifth Day:—6th Observation at 9.30 P.M.; *Preparation* 108 *hours old.* 7.45 A.M. *Slide,* 1 *Embryo visible.*

After long watching the oral extremity is seen to move in a barely perceptible manner. No others nor traces of others to be seen.

Twelve hours afterwards both slides examined, and no traces of embryos could be observed on either. The examination of each slide took about 40 minutes, and it was closely inspected several times on each occasion.

EVENING SLIDES.

Fifth Day:—5th Observation at 9.30 P.M.; *Preparation* 96 *hours old.* 9.30 P.M. *Slide,* 25 *Embryos visible.*

General languor marked, and motion more generally confined to extremities.

14*th and* 18*th* were very weak, and had to be watched for some time before motion other than in oral extremity could be detected.

Sixth Day:—6th Observation at 9.30 P.M.; *Preparation* 120 *hours old.* 9.30 P.M. *Slide,* 20 *Embryos visible.*

Twelve embryos much stretched out, undulatory motion running along body, pauses distinct.

13*th to* 16*th* much more vigorous.

17*th and* 18*th* quite dead, attenuated.

19*th* movement like the first embryo observed.

20*th* only two-thirds of body present, very transparent. No traces of others visible.

Seventh Day:—7th Observation at 9.30 P.M.; *Preparation* 144 *hours old.* 9.30 P.M. *Slide,* 12 *Embryos visible.*

1*st to* 8*th* movement decidedly feeble and spasmodic.

9*th to* 12*th* quite dead, transparent, two-thirds of another embryo was visible, it was much attenuated. No traces of others visible.

Eighth Day:—8th Observation at 9.30 P.M. *Preparation* 168 *hours old.* 9.30 P.M. *Slide,* 4 *Embryos visible.*

1*st* very feeble motion of both extremities, central part of body passive.

2*nd* quite dead.

3*rd* very languid movement in lower extremity, which had to be watched for some time before it was detected.

4*th* quite dead. There were traces of two or three others, or what appeared to be portions of others.

An examination made 12 hours afterwards failed to detect any living embryos, and only one dead one was seen, with traces of two others. The slide was examined for about 40 minutes each day.

It will thus be seen that, 12 hours after extraction, out of the 12 embryos originally contained on the 7.45 A.M. slide, 4 had disappeared, and there was 1 dead embryo in the field. With the 8 A.M. slide, after 12 hours, 2 had disappeared; 24 hours after this—that is, 36 hours after withdrawal,—2 dead embryos were visible, but there was no diminution in the number on the 7.45 A.M. slide.

In 60 hours only 5 embryos, or rather $4\frac{1}{3}$, were to be seen on the 7.45 A.M. slide, making a total loss in the time of $7\frac{2}{3}$. The 8 A.M. slide does not appear to have been examined at this time, for I have no note of it; but in 84 hours only 2 embryos were visible on the 7.45 A.M. slide, 1 of which was quite dead, making a loss in this time of 10.

On the 8 A.M. slide there were also only 2 embryos visible, or rather $1\frac{2}{3}$, and in 108 hours but 1 embryo *in articulo mortis* was to be seen on the 7.45 A.M. slide.

With the evening slide I found that up to 24 hours the numbers remained undiminished and the condition satisfactory, though certainly less vigour was displayed than on the first observation, and it was not until 72 hours after withdrawal that the first embryo disappeared, and very decided signs of debility were present. In 96 hours langour became very marked, but still only 6 embryos had disappeared; and in 120 hours 11 had vanished, and the dead and dying were met with for the first time on the field. In 144 hours but 12 remained, and again dead embryos were to be seen. At the eighth observation—that is, 168 hours after withdrawal,—only 4 embryos were to be detected on the slide, 2 of which were quite dead and the others nearly so. 12 hours afterwards all had disappeared. In a word, the embryos withdrawn in the morning and treated in an exactly similar manner to those withdrawn in the evening were all more or less weak when first extracted, and had all died or disappeared two or three hours after the termination of the fourth day; whereas those withdrawn in the evening had not all disappeared or died until an hour or two past the seventh day, besides being very much more vigorous at the outset, and preserving their vivacity for a considerably longer period. It may be asked why, if the embryos die every 24 hours in the host, do they live so much longer when liberated? In reply to this I would suggest that up to a certain time the act of withdrawal is a compliance with the natural requirements of the parasite, and that whereas it is not impossible that continuance in the circulation of the host beyond the time allotted to them may set up influences designedly calculated to bring about a rapid fatality, and thus clear the way for the new swarm, on the other hand, some time might be necessary before the inadequacies of the artificial state made themselves felt; and thus, in lieu of sudden and general destruction, we have the gradual decadence of strength and vigour exhibited by the embryos preserved on the slides.

As to the cause of disappearance, I do not feel sufficiently informed to venture more than a surmise. I took every precaution to avoid missing embryos in my various searches, and I think the condition of the portions of embryos seen lends weight to the supposition that solution may be the final process by which removal is effected; at least, it would appear from what I saw that this was the most reasonable and probable method of accounting for what is no doubt a very remarkable and puzzling phenomenon. It may be (and on this account I am most anxious to speak with the greatest diffidence and caution) that all I observed is peculiar

to the solitary case on which I was able to experiment, and it may be found that with different subjects different results are obtained; this time and future investigation alone can determine.

As some experiments I made by applying various matters to the blood, in order to observe the effect on the filariæ, may be of some interest apart from the immediate object for which they were undertaken, I will here describe them.

To the preparation I added a drop of water, and from this the most speedy effects were visible. The blood corpuscles were washed away from the embryos, and the latter, absorbing the fluid, became dilated and enfeebled, and rapidly died. The great effort of the embryo seems to be to get in contact with the red corpuscles, and as soon as it becomes affected, this anxiety is more marked.

To another preparation I added a few fine crystals of arsenious acid. The embryos immediately began to stretch out, both they and the corpuscles becoming very transparent. The embryos moved in a feeble, jerky manner, and in 38 minutes the first death was noted. It was astonishing, however, what a comparatively large quantity of the drug it took to bring about this result.

To a third slide I added salicylic acid, when again extension and enfeeblement began immediately, and gradually increased until eight hours afterwards, when the first dead embryo was seen.

To a fourth drop I applied santonine, and although the embryos immediately began to extend their bodies and show symptoms of debility, the effect of this medicine was very much less marked than that of either arsenic or salicylic acid.

Quinine (I used the bisulphate on account of its solubility) had a rapid effect in reducing the embryos to the last stages of weakness; indeed, apparently a more speedy one in this respect than the arsenic, but I was not able to be sure of the death of an embryo until five or six hours had elapsed. When To Ah was taking quinine (and so it was with the other two patients), I always noticed that the embryos were much less lively and healthy looking, and for this reason I was particular in seeing that he had taken none for some time before I made the observations set forth in Tables 2 and 3.

As the question is not, however, how to destroy the blood embryos, but rather how to get rid of their parental source, these therapeutical observations do not point to much that could not have been previously surmised, save that it seems likely that before an effective result could be obtained from the use of drugs, the blood would need to be so saturated that the remedies would probably act towards both host and parasite in a manner the impartiality of which would defeat the end desired.

It now seems tolerably certain that the *locale* of the mature worm is in the lymphatic system, and generally in the more superficial glands, so that the exact habits of the parasite, and the situations most frequently selected being first ascertained, it may be found that help lies more in the surgeon's knife than in the physician's medicaments; though, of course, with so indefinite a range, it is also highly probable that more than partial relief, in so far as the presence of embryos in the blood is concerned, cannot be very sanguinely anticipated.

With dogs, though worms are found in other parts of the vascular system, still the greater number abide in the heart, and undoubtedly from that position effect all their mischief. In man, the home of the parasite appears to be less desperately localised; and if it should happen that the favoured and most important lodging is one accessible from the surface, the human sufferer will not only have much of that despair alleviated, which observations on canine subjects might tend to justify, but may hope that those parasites which have taken up their abode in other and deeper situations may, like the extra-cardiac filariæ in the dog, continue their existence without imperilling or materially inconveniencing that on which their own depends.

Alas! in China, where postmortems are so strictly prohibited, much progress in solving the remedial problem cannot be hoped for; but, perchance, clues obtained here from the living may lead to satisfactory results in lands where the pathologist is looked on with less horror and detestation than he excites in this country. I hope very shortly to recommence my experiments on monkeys with the genuine filaria-nurturing mosquito, and should success crown my efforts to infect them, doubtless much useful and interesting information may be obtained. I also hope at no distant date to be able to submit descriptions and measurements of the mosquitos found in this island, with a view of aiding in determining the peculiarities, if any, of those species which are to be dreaded, as compared with those which, as far as the diseases under notice are concerned, need not be regarded with such pathological interest.

CHART 2.

TEMPERATURES in MORNING, while EMBRYOS were disappearing from TO AH'S BLOOD.

(Hours at which Embryos disappeared marked thus +.)

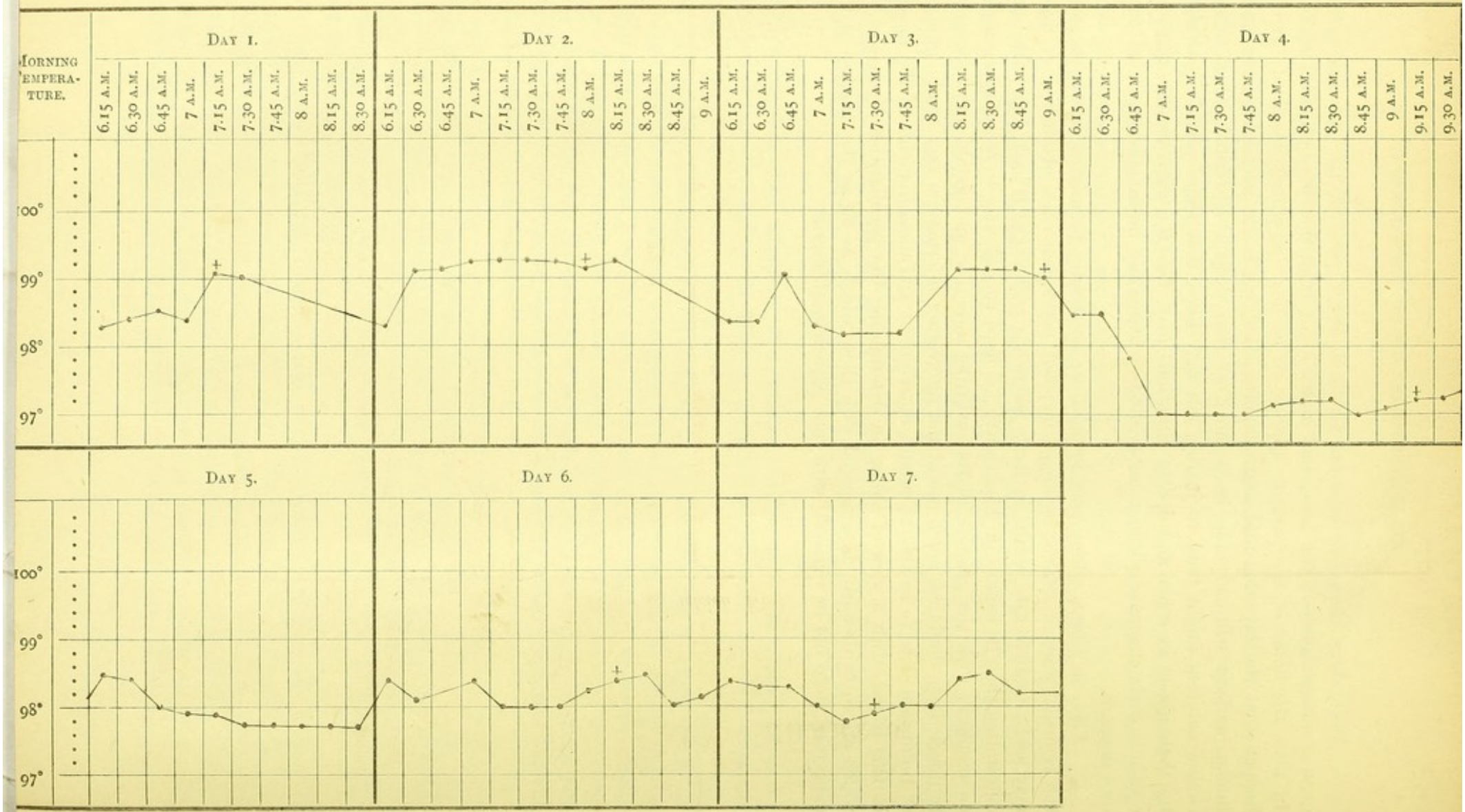


CHART 3.

TEMPERATURES in EVENING, while EMBRYOS were returning to TO AH'S BLOOD.

(Hours at which Embryos reappear marked thus +.)