

**Observations on filariae / by Patrick Manson [and others] ; communicated, with an introduction, by the president.**

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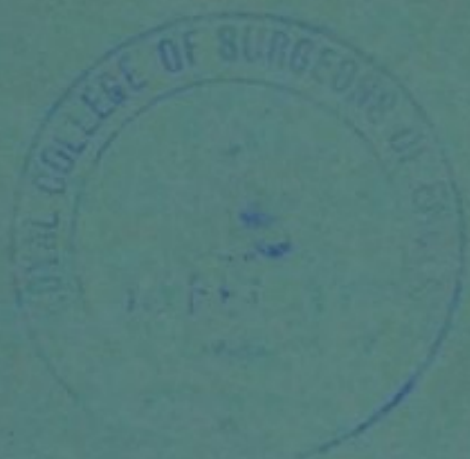
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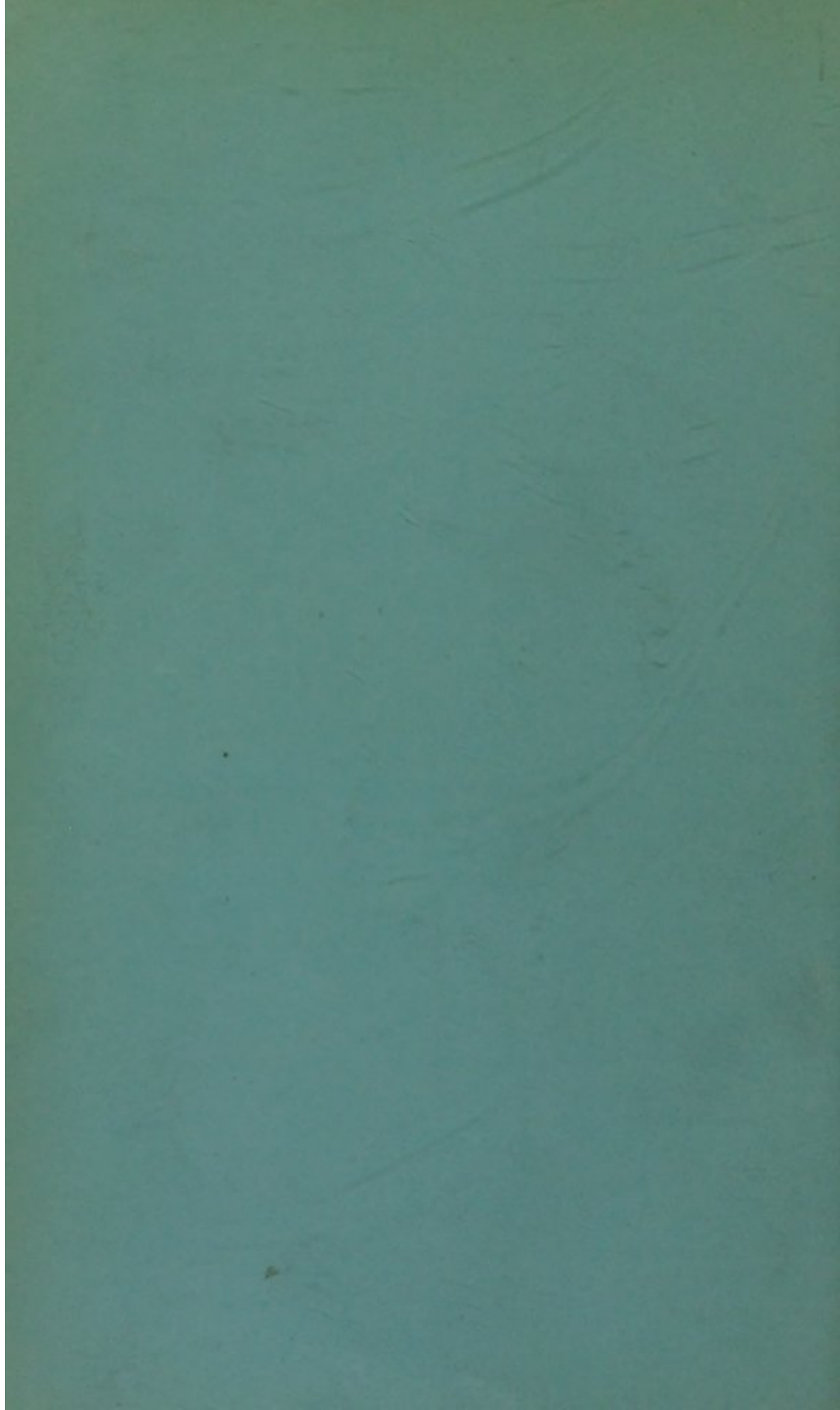
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Filariae

Feb. 1880

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## OBSERVATIONS ON FILARIÆ,

BY DRs. PATRICK MANSON, JOHN R. SOMERVILLE, JOSEPH  
BANCROFT, J. F. DA SILVA LIMA, J. L. PATERSON,  
PEDRO S. DE MAGALHAES, AND J. MORTIMER-GRANVILLE.

Communicated, with an Introduction, by the PRESIDENT.

(Read February 27, 1880.)

### INTRODUCTION.

I almost owe an apology, not only to the Club but also to the medical profession, for not earlier communicating the new and remarkable facts which I have the honour to bring under your notice this evening. I must explain that during my autumn holiday so large an amount of correspondence had accumulated that I have been unable to overtake the duty of replying to letters, of reading lengthy manuscripts, and of reporting on the various specimens of parasites that were sent during my absence.

Such rapid progress is being made by additions to our knowledge of the habits, developments, and disorders produced by the *Filariæ* and other parasitic nematodes, that in order to render Dr. Manson's researches generally intelligible it is necessary that I should state, in as few words as possible, the position at which we had arrived prior to the receipt of the particulars which the missionary physician now supplies.

The facts of filarial discovery in man originated and appeared in the following order. In its embryonal state, what is now called *Filaria sanguinis hominis* was first discovered by Dr. Wucherer on the 4th of August, 1866. He gave no name to the parasite. Two years later the same or similar larvæ were found by Dr. Salisbury in the urine,\* in a case of chyluria (1868). He thought they were a kind of *Trichinæ* (*T. cystica*). On the 22nd July, 1870, I discovered great numbers of the nematode larvæ in the excretions

\* Dr. Salisbury's figures are probably inaccurate as to size. Basing his views on the supposition that Salisbury's figures are correct, Dr. Lewis rejects the notion of the identity of the urinary worms found by Salisbury and myself.

of a little girl from Natal, who suffered from endemic hæmaturia, the disorder being occasioned by the presence of the fluke which I have called *Bilharzia hæmatobia*. Tens of thousands of eggs of this trematode passed from her daily, and as many as fifty of the larval *Filaria* were seen on one occasion mixed with the ova of *Bilharzia*. Like Wucherer, I gave the young nematode no new name. Two years later Dr. Crevaux found similar larvæ in a hæmato-chylous patient at Guadaloupe. Nothing whatever was known of the real significance of these finds until the year 1872, when Dr. Lewis announced his discovery of minute *Filaria* in the blood. He named the larval worms *Filaria sanguinis hominis*. Neither Wucherer nor myself thought it necessary to give any particular name to larvæ, which might be those of some adult *Filaria*, *Ascaris*, *Strongylus*, or other nematode genus. Dr. Salisbury, indeed, had the temerity to refer the worms to the genus *Trichina*; and Leuckart suggested that they might be young *Strongyles*. In the matter of nomenclature I thought it better to wait and see to what type the adult worm could properly be referred; for these larvæ certainly possessed no distinguishing characters of any particular genus.

Guided by certain indications which I pointed out to him, Dr. Joseph Bancroft, as he has acknowledged, sought for and discovered the adult worm on the 21st of December, 1876. He wished me to publish his very short notice of the worm, because, as he said, "I had set him on the track of the investigation." Accordingly I announced the discovery in the *Lancet* for July, 1877, and afterwards more fully described the parasite in the same journal, naming the worm *Filaria Bancrofti*. I also furnished some anatomical details. Some months after Dr. Bancroft made his discovery, Dr. Lewis also encountered the adult parasite; and losing no time in publishing his description, it appears that his diagnosis of the characters of the entozoon was actually in print before mine. Consequently, although Dr. Bancroft is the real discoverer of the sexually mature parasite, I have, on this basis, been called upon to give up the nomenclature that my description supplied. I willingly do so; with the distinct understanding, however, that such a step shall not deprive my Brisbane correspondent of the honour of priority in this matter. Dr. Bancroft's discovery dates seven months in advance of that of Dr. Lewis, whose verification occurred on the 7th of August, 1877. Two months later, in

Brazil, Dr. Silva Araujo found the adult worm on the 16th of October, 1877, and shortly afterwards Dr. F. dos Santos made a similar verification (12th November, 1877). This closes the record of the first two epochs of filarial discovery. A third epoch of discovery was opened up when, writing from Amoy, in November, 1877, Dr. Manson informed me that he had discovered *Filariæ* in the stomach of mosquitos that had gorged themselves with human blood. Dr. Bancroft, indeed, writing in the spring of the same year, had remarked to me that he fully expected that the hæmatozoa would be thus transferred, but his examinations of mosquitos up to the time of Manson's discovery had not been successful. Other discoveries and verifications rapidly followed, especially those having reference to the power of the hæmatozoa in the production of disease. On this aspect of the subject I cannot now dwell further than to say that the purely clinical questions have already been pretty fully discussed by myself in a paper communicated to the London Medical Society, and published in their recently issued "Proceedings" (Vol. iv, 1877-79, pp. 129-134).

I may add that all the various disorders produced by the *Filariæ* in question have been, as it were, rolled together into one by Dr. Bourel-Roncière, who calls the collective disease *Wucherer's helminthiasis*. Others have called the disorders *Filariasis*, but, obviously, this vague and too comprehensive sort of nomenclature cannot be allowed to stand. It is now tolerably certain that at least a dozen more or less distinct diseases are caused by the larval *Filariæ*, and amongst these are to be reckoned endemic hæmaturia, chyluria, varix, lymph scrotum, elephantiasis, lymphatic growths (*helminthoma elastica*), many other lymphoid affections, a skin disease termed *Craw-craw* (*Filariasis dermatemica*), probably leprosy, and perhaps also certain malarial fevers.

In the acquisition of these results—results which, of course, are not generally accepted, and whose full value and significance will probably not be realised for many decades or centuries to come—the justifiable method of human experiment has played a most conspicuous part. Initiated by Dr. Manson himself, he was thus enabled to trace out the higher larval stages of growth, after the hæmatozoa had passed from man to mosquito. Having caused an infected Chinese to sleep in a mosquito house, the insects were found gorged with blood the next morning. On examining the contents of their stomachs, Dr. Manson ascertained that a relatively far greater pro-

portion of the *Filariæ* existed in a drop of the sucked blood than in a drop taken from the Chinese in a direct manner. Thus, it was not unnaturally inferred that the construction of the proboscis was in some way or other expressly fitted for drawing the worm out of the capillary blood vessels. Dr. Manson examined the gorged insects at regular intervals of time, and he forwarded to me the details that were subsequently communicated to the Linnean Society, and published in their Journal (1879). He likewise reported the results in the *Custom's Gazette*; but probably the summary which I have given in my recently issued work, with illustrations reduced from Dr. Manson's original figures, sufficiently explains all that need be known on this part of the question. Thus the position stood at the commencement of last year; and the novel facts and deductions to which I now invite your attention, must be regarded as a continuation of those records. I may add that since the issue of my treatise, Dr. Lewis has published a beautifully illustrated memoir in which he not only verifies a great deal of what Dr. Manson had already observed, but also adds a multitude of interesting details. Dr. Lewis's writings, however, do not in any sense anticipate the data now brought forward. In the present communication, Dr. Manson commences by referring to the mosquitos (which I have now the pleasure to show the Club) and to a Chinese scrotum at present in my possession. He gives particulars of the remarkable case to which the scrotal preparation refers. He points to a newly observed fact that the worms enter the blood periodically. He interprets the significance of this law of periodicity in some of its more obvious bearings. He appears to have discovered the special means by which the insects contrive to fish up, as it were, the hæmatozoa. He explains the immediate consequences of this curious phenomenon. I may add, parenthetically, that this process affords a remarkable instance of so-called free parasitic ectozoa stealing true entozoa and taking them as prisoners from human hosts, to lodge them, as transported guests, in their own interior. The matter does not stop here. Dr. Manson explains how it happens that human blood does not contain *Filariæ* during the afternoon. He gives a tabular record of the results of the daily examination of the blood at different hours. He shows how the embryos, with almost military punctuality, march to their nocturnal quarters. Proofs of this extraordinary behaviour are supplied by repeated observations. He hints at the importance of

these facts in relation to the etiology of malarial fevers, and he concludes his letter—for such it only pretends to be—by stating his determination to carry on the investigation until the whole body of available facts is explored and the results, in all their professional bearings, are elucidated.

It remains for me to observe that this novelty in helminthology savours somewhat of the marvellous. When the facts come to be fully understood, I think it will appear that neither the parasitism of *Trichina*, nor that of *Echinococcus* approaches in interest that of the human *Filaria*. Probably millions of victims exist in tropical countries; many of them suffering from grave disorders consequent upon this invasion of their tissues. That in the face of such astounding facts as have of late years come to light there should be found professional men of standing averse to encouraging researches of this kind is one of those retrograde social phenomena which I greatly deplore; but where the interests of humanity are so much concerned such disparagement ought not to be allowed to check our efforts.

It must be obvious to every one that naturalists, physiologists, and medical men will separately view these discoveries from very different standpoints; and it will need some master mind to collate all the phenomena in relation to cause and effect. At present I have only had opportunity to converse at any length with one physician on the subject. In the opinion of Dr. J. Mortimer-Granville we must not look for a solution of the phenomenon of periodical local migration as arising from any special want on the part of the entozoon, but rather as arising from varying physiological conditions affecting the host. Whether this view be correct or otherwise, I think Dr. Granville's suggestions highly important; and as he has been good enough to communicate an abstract of them in writing, I append them to this introduction in the form of a separate commentary.

The communication of Dr. Somerville has more especial reference to the closely allied microscopic *Filaria* infesting the dog. The adult *Filaria immitis* engaged my attention at Edinburgh some thirty years ago; but, in recent times, Dr. Somerville was, I believe, the earliest to report on this subject in the *Customs Gazette*. He believes that the occurrence of *Filaria sanguinis hominis* at Fuchow to be comparatively rare; but, in this case, as probably obtains in many others, the negative indications may



have entirely resulted from the hours of the day selected or accidentally employed for the blood-examinations. He incidentally alludes, also, to the common eye worm of horses (*Filaria papillosa*) and some other helminthological points of general interest. It is very curious to observe Dr. Somerville's remarks respecting a case of lymph-scrotum, which he examined in company with Dr. Patrick Manson's brother (since deceased). The negative results of his search for *Filaria* is now readily explained.

Dr. Bancroft's brief communication deals with facts that verify his original opinion that the peculiar growth which he has termed *Helminthoma elastica* is really due to the presence of *Filaria sanguinis hominis*. Of more interest, however, is the circumstance that he has ascertained from actual observation that the common louse of the dog (*Trichodectes latus*) is the intermediate bearer of the larvæ of *Filaria immitis*. It will be remembered that Melnikow long ago demonstrated by experiment the intermediary functions of the louse in respect of the larvæ of a cestode (*Tænia cucumerina*); and it is therefore not a little curious that this insect should play a similar rôle in respect of a parasitic nematode.

Dr. J. F. da Silva Lima's letter specially refers to a singular affection which some persons appear to have associated with elephantiasis. He alludes to a paper by himself on *Filaria medinensis*, a translation which (from the Portuguese, by Dr. Paterson) I published in the *Veterinarian* in the spring of 1879.\* Dr. Lima encloses a paper by Dr. Paterson, which must be regarded as the sequel to his interesting paper, entitled "Facts in Filariasis," which also appeared in the *Veterinarian* (June, 1879).

As Dr. Paterson's recent memoir, however, is of considerable length and relates to a point respecting which there ought not, in my opinion, to have been any controversy at all, I shall only reproduce a portion of it. The memoir deals exclusively with the nature of the involucre of *Filaria sanguinis hominis*. Dr. Paterson appears to have thought that the envelope surrounding the microscopic *Filaria* is no genuine worm structure, but a merely adventitious layer of coagulated fibrin. Having published this view, Dr. Magalhaes, of Rio de Janeiro—from whom I have also received

\* "Remarks on the *Filaria medinensis* or Guinea worm; on the occurrence of this parasite endemically in the province of Bahia; on its entrance into the human body by drinking-water."—The *Veterinarian*, Feb.-March, 1879.

communications—seeks to overthrow this theory. In the interests of science it is desirable to publish the facts forming the basis of this unduly warm controversy, in which Dr. Magalhaes has certainly the best of the argument. So far as my own observations have gone, it seems pretty clear that neither Dr. Magalhaes, nor Dr. Paterson, nor even Dr. Lewis himself, appears to have correctly interpreted the nature of this envelope. Dr. Lewis has persisted all along in calling the structure in question “a sheath.” It seems to have escaped the knowledge of almost all observers that the so-called involucrem is neither more nor less than an ordinary skin-cast, such as invariably accompanies the moultings of nematode worms. Different larval nematodes cast their skins at different times, and this moulting is usually accompanied with an alteration in the form and structure of the larva, involving the loss of the original tail.

T. SPENCER COBBOLD.

February 26th, 1880.

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## APPENDIX.

### REMARKS OF THE LOCAL MIGRATION OF MICRO-FILARIÆ.

By J. MORTIMER-GRANVILLE, M.D.

Assuming that the change of locality is found to be habitual, and that it is not a too hasty generalisation from insufficient data, it will, I believe, be of great value to the student of rhythmical phenomena like that of “sleep.” The change of place may be fairly ascribed to change of state. Looking to the habits of life in the lowest organisms, it can scarcely be supposed that the periodicity can depend on the state or requirements of the *Filariæ*. It is not likely that the parasite needs repose, or that it resorts to special localities to feed. It seems more probable that the state of the circulating fluid determines the presence or absence of the *Filariæ* in its main current, by night and day respectively, or during the waking and sleeping states. Your verbal account of Dr. Manson’s report is not quite explicit as to whether the change of locality depends on sleep, or simply the advent of night. The alterations and alternations of condition which takes place in the blood are three (perhaps four) in number, so far as any living organism present in it is concerned.

*First*—The rapidity of the current is diminished in the recumbent posture, and (*in women and children at least*) still further during sleep. It seems probable that there may also be an integral change in the relation of the red and white corpuscles approaching that which takes place when the circulation is retarded in the web of a frog's foot. This modification of the internal ~~current~~ <sup>constitution</sup> of the main current may cause the *Filariæ* to adhere to the sides of the vessels during rapid circulation, and to fall into the stream when it moves more slowly.

*Second*—The oxygenation of the blood at night and in repose is supposed to be less complete than during the day; although some experiments made by Pettenkofer and Voit point to an opposite conclusion, and make it appear that of the total amount of oxygen taken in during twenty-four hours 67 per cent. is taken in by night, and only 33 per cent. by day; 58 per cent. of the carbonic acid eliminated during twenty-four hours being given off in the day, and 42 per cent. by night. From these results Somner drew the inference that sleep was caused by exhaustion of the reserves of oxygen during the waking state, that oxygen was stored during sleep, and that when the equilibrium was re-established by accumulation the subject awoke. This hypothesis is barely tenable, but it is worth while to mention it in this connection because it accords with the presumption that the blood is more or less abundantly charged with oxygen during sleep; and this may help to determine the location of the *Filariæ*.

*Third*—The temperature of the blood is probably lower (or it may be higher) *relatively to that of the tissues* during sleep than in the alteration <sup>nature</sup> of state consequent on the presence, or absence, of special elements derived from the food. It does not seem likely that there can be any considerable change of condition consequent upon the periodic or occasional discharge of lymph and chyle into the blood; but it may happen that when the circulation is slow and the body lies recumbent, the parasites are thrown into the main current and therefore appear in greater numbers.

It is premature to speculate on the causes of a phenomenon which may yet prove to be exceptional; but if the conclusion arrived at should be maintained, it will certainly be the duty of physiologists to pursue further into detail than they have ever yet been carried certain neglected inquiries as to the altered conditions of the blood during sleep, and when the brain and body are awake.

## LETTER BY DR. MANSON.

“ Amoy, 20th June, 1879.

“ T. Spencer Cobbold, Esq., F.R.S.

“ DEAR SIR,—Last mail brought me your kind letter of 28th April, and enclosures. I am glad your new work is about coming out, and hope my London agents have forwarded a copy long ere this time. Had I been in possession of it, I would not have troubled you with questions about *Distoma crassum*. I suspect you are very often annoyed in this way, and I am very much obliged for your courtesy in noticing my letter.

“ I will forward by this mail filaria-impregnated mosquitos. They are preserved in glycerine, and were fed on the blood of the man whose case I append. His scrotum I sent to you some time ago in charge of Dr. Holmes, a surgeon in one of Holt's steamers, who kindly promised to hand it to you. I hope you will pardon the delay in sending the mosquitos ; being in general practice here, the many interruptions this entails make work of this sort exceedingly difficult to carry out quickly.

“ I read in the *Lancet* lately an account of the discussion on a Lymph scrotum sent from India, and felt disappointed that the *Filaria* were not found. I determined to send you the first scrotum I amputated, and in which I had unquestionable evidence of *Filaria*. The scrotum I send is the result, and to complete the case I send particulars of the man's history, and the result of the examination of his blood before and after the operation. The case is one of much interest, as it exhibits, first, the transition from Lymph scrotum to Elephantiasis ; secondly, it demonstrates unmistakably that the parent worm is not necessarily present in the affected tissues themselves, though probably in close proximity. I had hoped you might find the *Filaria Bancrofti* in the scrotum, but the embryos persisting in the blood weeks after the operation show that this is unlikely. Thirdly, it illustrates well a new fact in the history of the *Filaria*—the young escape into the circulation at regular intervals of twenty-four hours, the discharge commencing soon after sunset and continuing till near midnight, from which time till the following noon their numbers gradually decrease ; by two or four o'clock till six they are nearly completely absent. This is a striking and most suggestive fact, and in connection with it one might be tempted to speculate on the causes of the periodicity of

malarial fevers. It is marvellous how Nature has adapted the habits of the *Filaria* to those of the mosquito—the embryos are in the blood just at the time the mosquito selects for feeding.

“ Another fact in adaptation you might like to know. The long lash on the tail of the embryo has a meaning in relation to its future life. I think so from the following experiment. Drop a few fibres of cotton into the fluid of a milk (filarious) hydrocele. They will subside to the bottom of the vessel very gradually. Leave them there for a few minutes, and then place them under the microscope. You will find them beset by thousands of embryos in rows and clusters, each embryo attached by its tail lash as one can attach a whip to a rope by striking it sharply with the lash. When the mosquito penetrates a blood vessel, the passing embryos lashing about, as is their habit, entangle themselves thus on the proboscis, and get sucked up. Hence the enormous numbers of embryos in the mosquito’s stomach and the secreting faculty of that insect.

“ It would be well to warn observers against concluding that a case is non-filarious from observations made during the afternoon, and that the most reliable time to make them is at night, and, if possible, they should employ a mosquito to make it for them. In consequence of my ignorance of this particular point in the history of the parasite, my statistics as to its prevalence in Amoy and neighbourhood lose much of any value they may have been supposed to possess. If I can find the time I may go over the ground again, making examinations after sunset instead of, as formerly, between 5 a.m. and 6 p.m.

“ The following are the particulars of the case belonging to the mosquitos and Lymph scrotum I send you ; I copy them verbatim from my note book :—

‘ *F. S. H.*—Lymph scrotum and Eleph. scrot. insipient. Oah. M. ; æt. 19 ; Khoan Kaw, Eong ; a rice miller. Parents dead ; no relatives with elephantoid disease as far as he knows. Eong is a small hamlet of about a hundred inhabitants in the suburbs of Khoan Kaw. Elephantiasis he has often seen in Khoan Kaw. Drinks well water stored sometimes for several days in a large jar. When 16 or 17, sometimes sick with an evanescent fever and relapsing inflammation (it may be of the testicle) of the right side of the scrotum, accompanied by enlargement of right and left groin glands, especially of right. When 15, had an abscess in left groin (scar is visible), and the same year in the right leg near the ankle ;

the whole leg was swollen ; described it as "Toa kha tang" (the expression used to designate elephantiasis). Swelling lasted for one month, and subsided with bursting of the abscess. No thickening of leg now. Scrotal inflammation and fever recurred twenty times a year. A year ago it discharged for the first time ; has run daily, with the exception of three months since that time, messing his clothes. When seen at hospital it had not been discharging for a few hours. Since then (four days ago) it has dripped constantly. In one hour I saw collected two ounces of white fluid.

"May 10th, 1879.—This morning I examined the scrotum carefully. He had it trussed up in a head cloth. On removing this a fine stream of lymph was forcibly projected as from a squirt from a point at the lower part of the scrotum. Half an ounce ran in a couple of minutes. Scrotum is as large as a small pumelo. The skin of the penis is distinctly elephantiased, and a thickening of the skin is visible and palpable over both groins, lower two inches of skin of abdomen, and over Scarpa's triangle on both sides. The upper and thigh surfaces of the scrotum are covered with a fine silky skin freely moveable over the thickened substratum ; a little lower down skin thickened and adherent as in elephantiasis ; lower still small ampullæ are visible ; lower down these become larger, and along the raphe they are the size of small beans. Pricking any of these the usual fluid escapes. This is most distinctly a case of elephantiasis scroti and lymph scrotum combined. Groin glands are large, especially the right, but they do not feel varicose. However, yesterday I pierced the right side glands with a subcutaneous syringe, and readily obtained abundance of straw fluid. In this I found *F. S. H.*, as also in the fluid from the scrotum, and a very few in the blood from finger. While writing these notes more than three ounces of fluid have distilled from his scrotum.

"Standing, a few ounces of the fluid a feeble coagulum forms, which contracts till, in eight or ten hours, it is one-sixth the bulk of the fluid. It is now tough and fibrous. A small portion was removed and placed between two glass slides and finally pressed out ; in the fluid expressed and now surrounding, and in the open meshes of the fibrine, are very many specimens of *F. S. H.* I found none in the serum the clot floated in. It would appear, therefore, that the coagulating fibrine caught the *F. S. H.*, and, contracting, carried, as in a net, all the *F. S. H.* with it, concentrating them. Some of the *Filaria* were very robust and active, others were languid, spotted, and shrivelled looking. In one such specimen the

lash was quite visible by a low power. Many short fibres  $\frac{1}{100}$ th in length were also visible—possibly the enclosing tube of collapsed embryos.

“ This morning the coagulum which had formed by last night in fluid drawn the previous forenoon, had completely disappeared; a flocculent sediment lay in the glass, and in this great abundance of *F. S. H.*

“ May 11th.—Scrotum removed under chloroform—it weighed  $1\frac{1}{2}$ lb.; there was considerable bleeding and also escape of lymph from two dilated lymphatics, one on either side just external to the cords; by pressing firmly on the enlarged inguinal glands lymph could be made to well up from these two points.

“ Scrotum placed in spirit and sent to Dr. Cobbold.

“ The patient recovered perfectly from the operation. Being anxious to ascertain if I might assure you that the scrotum sent contained the parents of the embryos I had found in the blood, I have kept this man under daily observation since the operation. But unless the examination of the blood was made during the afternoon, it has invariably been found to swarm with embryos. The inference from this is that there are parent *F. Bancrofti* in the patient still, and that probably you will fail to find any in the scrotum, though you will undoubtedly come across many embryos, especially if you search the sediment of the spirit.

“ The following is the record of the daily examination of a drop of blood placed between two slides :—

May 12	...	12 embryos.	June 1	...	6 embryos
„ 13	...	15 „	„ 2	...	11 „
„ 14	...	13 „	„ 3	...	0 „
„ 15	...	35 „	„ 4	...	0 „
„ 16	...	52 „	„ 5	...	36 „
„ 17	...	62 „	„ 6	...	6 „
„ 19	...	2 „	„ 7	...	15 „
„ 20	...	4 „	„ 8	...	6 „
„ 21	...	12 „	„ 9	...	32 „
„ 22	...	18 „	„ 10	...	12 „
„ 26	...	42 „	„ 11	...	0 „
„ 27	...	2 „	„ 12	...	0 „
„ 28	...	3 „	„ 13	...	9 „
„ 29	...	0 „	„ 14	...	56 „
„ 30	...	1 „			

“ When embryos were very few or altogether absent, the examination was made during the afternoon. To show you how punctual the embryos are in keeping their time, I had this man brought to my house, and had examinations of his blood made every four hours. At the same time I availed myself of the opportunity to feed the mosquitos I have sent you on his blood, making the case as complete as possible.

HOUR.	12 P.M.	4 A.M.	8 A.M.	12 M.	4 P.M.	8 P.M.
Monday.....	—	—	—	—	—	43
Tuesday .....	—	6	2	1	0	24
Wednesday ...	57	23	1	0	0	105
Thursday .....	21	18	0	0	0	29
Friday .....	—	15	0	0	0	29
Saturday .....	89	2	1	0	1	53
Sunday .....	41	2	0	0	0	17
Monday.....	34	5	0	0	0	14
Average .....	$48\frac{2}{5}$	$10\frac{1}{7}$	$\frac{4}{7}$	$\frac{1}{7}$	$\frac{1}{7}$	$39\frac{1}{4}$

“ The quantity of blood examined was, as nearly as possible, the same each time—a small drop, or as much as would keep well together on the point of the finger without running off.

“ One evening I watched the influx of embryos, examining the blood very frequently. At 4 p.m. I could not find one; at 6 p.m. I found one, at 7 p.m. two, at 7.30 p.m. ten, at 8 p.m. 29, at 9 p.m. 37.

“ As far as I have examined I find that in other cases the embryos observe the same periodicity. I have a gardener who comes from a filarious district. I knew his blood sometimes contained worms. It was examined on three occasions during the afternoon without finding embryos. Yesterday evening it was again examined, about 9 o'clock, and was swarming with *Filaria*. A neighbour's chair coolie at 4 p.m. had no embryos; at 9 p.m. he had 28 in one slide, and so on.



“If you think these facts of sufficient importance to make public, I would be much obliged by your doing so in any way you think best. They have not left my note-book before; I am gradually accumulating evidence to prove, I hope, to the satisfaction of such cautious sceptics as [the late] Dr. Tilbury Fox and others, that elephantiasis is a parasitic disease. I have got some strange results from tapping enlarged groin glands with the subcutaneous syringe, but until the chain is completed, either by myself or others, I will keep silent on this point.

“It seems to me that Lewis by his great discovery has opened a new field in tropical pathology. The interest and importance of *F. Bancrofti* and *F. S. H.* is by no means exhausted yet. I hear Lewis is in England now, but when he returns to India I hope he will take up the subject again. Men like myself in general practice are but poor and very slow investigators, crippled as we are with the necessity of making our daily bread.

“Pardon this long and rambling letter, and believe me,

“Yours very faithfully,

“PATRICK MANSON.”

“I hope you will find embryos in the mosquitos. I sampled them before placing them in the glycerine; but their structure is so delicate, and they are so minute, that they may be difficult to find, shrivelled up by the glycerine. I would recommend you to soak the insect about to be examined for a few minutes in water.”

#### LETTER BY DR. SOMERVILLE.

“Fuchow (China), 9th June, 1879.

“DEAR SIR,—Knowing the interest you take in these subjects, (as stated by you in the ‘Lancet’ of 5th April last), and as a small contribution to the literature of *Filaria*, I venture to send the following notes on the filaria disease in the dog. The paper which I send to you by present mail was published early in 1874 in the Customs ‘China Medical Reports,’ and it was the first, I believe (of late years, at least), to call attention to the subject in China.

“Since that time I have had many cases of the canine filaria, and several dissections, without, however, finding anything further that is new. So common is the disease here, that one can hardly cut up a native dog, or a foreign dog that has been any length of time in the country, without finding the *Filaria immitis*, in greater or lesser abundance, in the vessels or right ventricle. I have at present under

observation (with view to a *post-mortem* some day) a dog of French breed, whose blood is so full of the parasites that I often have two embryos in the field at the same time.

“With reference to the *Filaria sanguinis hominis*, I think it must be much less common here than at Amoy. I have not yet found it, and neither has my friend, Dr. Asford, of Fuchow, who has the advantage of a large native hospital, and consequently large opportunities of searching for it. The late lamented Dr. David Manson (brother of Manson, of Amoy) only found one subject (a Chinaman) of the disease in Fuchow, and there is a curious circumstance connected with this case. Dr. Manson had the man to his house to show him to me. He had “lymph scrotum,” and we first punctured one of the vesicles, and examined it, then another, but without success. We then took blood from different parts of the man’s body, and, after working at him for about two hours with a microscope each, we had to give up the search without finding a single *Filaria*. Yet only about a week ago my friend had found numerous embryos! Can it be that the mature female discharges her ova into the vessels only at certain times, and that the embryos have only a limited period for existence in the blood? I have not yet been disappointed by the dog in this way. Once get a dog who has them, and I have always, hitherto, been able to find when I wanted them.

“I was amused to notice the other day in the ‘Lancet’ (Feb. 22, 1879, p. 268) the following, with reference to one of the specimens of elephantiasis presented at the meeting of the Pathological Society by Sir Joseph Fayrer: ‘It was suggested that the numerous small semi-circular bodies, about the diameter of red blood-capsules, met with in the second case in the lymphatic channels, might be transverse sections of *Filarie*. If so, the number of these organisms present must be very large!’ I should think so! We *do* occasionally have two embryos in the field at the same time; but surely to make them anything like so numerous as this is straining the subject to an unjustifiable extent.

“In the case of the dog, it is extremely difficult to find the embryo after the blood has coagulated and movement has ceased; after, in short, the death of the *Filaria*, even when one knows it must be present in the field, and I have never yet succeeded in discovering one next morning after allowing the slide to stand over-night. It is easy enough to observe them for any period within five or six hours after the blood has been taken from the animal.

“ With regard to the association of *filaria sanguinis* and elephantiasis and lymphangitis, in the way of cause and effect, I can only send you the following facts, and they give negative results.

“ On the 2nd March of last year, Dr. Asford, of this place, asked me to be present at an operation for the removal of a large scrotal tumour—the ordinary elephantiasis Arabum. On removal, I made a careful examination of this tumour (which weighed 40 lbs.) in several different parts, and of its juices, without finding any *Filariæ*.

“ There were also, at the same time, two men in hospital with lymphangitis, and another with incipient elephantiasis of the scrotum, and in none were any *Filariæ* to be found. This, of course, is only negative evidence, but I think it is of value, *pro tanto*.

“ About the mosquito and elephantiasis, and its allied disorders, it seems to us here that if the mosquito could propagate the disease, we should all infallibly have big legs and scrotums, for we never (except in the rare instance of the presence of an epidemic) boil our drinking water, but only filter it. The Chinese, on the other hand, never on any account use water, except when boiled in the form of tea or soup. Indeed, they look with perfect horror on foreigners drinking cold water. Yet these diseases occur in natives only. I have never (during a residence of 16 years in the country) heard of a case in a foreigner in China, and Sir Joseph Fayrer mentions that he has only seen two instances of elephantiasis in the pure European in India. Surely climate and race are more likely to have to do with the causation of these diseases than the mosquito?

“ I noticed last summer a number of *Filariæ* wriggling about in the glass cistern of my wet-bulb thermometer. They resembled the *Filariæ* we sometimes find swimming in the anterior chamber of the eye of the horse in China (I operated on a pony for this some years ago), except that they were larger, some of them being about two inches long. Under the microscope they had the general appearance of the nematodes. They occurred at the same time in a flower-pot in the garden, the bottom of which had got stopped up, leaving a layer of water on the surface of the soil. They quite disappeared as the cold weather set in. What were these worms? and is it possible their embryos could have been deposited by mosquitos? They have not appeared this season as yet, but when they do I mean to study them more minutely.

“ I forgot to state, in these rather disjointed notes, a question

that continually occurs to one. Why is it that the mature *Filaria immitis* is so easy to find in the dog, and the mature human hæmatozoon so seldom discovered in man?

“ I must not trespass any more on your time, and would only say that if you find these notes of value in any way, I should be pleased you should make what use you like of them.

“ I am, dear Sir,

“ Very faithfully yours,

“ JOHN R. SOMERVILLE.”

“ Thos. Spencer Cobbold, Esq., M.D., &c., &c.”

#### LETTER FROM DR. BANCROFT.

“ Brisbane, May 19, 1879.

“ DEAR SIR,—I have not been able to find much new *re Filaria* of late, though I have worked at the subject as usual. I am sorry to say that the old volumes of the ‘Lancet’ which contain the various points of history have not been preserved.

“ I have been examining tank water at a house where cases of filaria disease reside, but have found nothing. The following matters are of interest, and suitable for publication in any way you think fit. I am much at a loss for guiding information.

“ Believe me,

“ Yours truly,

“ J. BANCROFT.”

“ P.S.—I am preparing drawings of *Trichomonas vaginæ*, which I think will interest Dr. Beale.”

“ T. S. Cobbold, Esq.”

#### (*Filaria Bancrofti*.)

There can be no doubt but that the elastic groin tumour and axillary tumour, which I ventured to call *Helminthoma elastica*, are conclusive evidence of the parasitic condition under consideration. The fluid that exudes when the tumour is tapped contains some blood, but when this has been allowed to subside for a few hours, the bulk of the superstratum has all the appearance of milk, and closely resembles chylous urine. It seems, therefore, that this elastic structure—which, in my paper in the “Pathological Society’s Transactions” of last year, is described as looking, when exposed by incision, like large everted piles—opens on some

part of the surface of the urinary tract by a lymph vesicle, causing the phenomenon of chyluria.

(*Filaria* in the Blood of the Dog.)

I have been favoured by Dr. Araujo, of Bahia, with an account of the *Filaria immitis*, as found in Brazil. I never examined the blood of dogs until April of this year, when I found the *Filaria* described by Welch in the "Lancet" of 1873, p. 337. So far, I have not enquired into the matter of the parent worm of this *Filaria*, my attention being directed to ascertain by what means the parasite may be carried from dog to dog.

On May 17 I found that the louse, *Trichodectes latus*, swallows the *Filaria* of the dog. I have seen the *Filaria* alive and very active among the blood from the stomach of the louse; in blood of fleas so far I have not seen it.

The dog *Filaria* is smaller than the human parasite, shorter by one-fourth, and about half the thickness. It has no envelope. The head is angular, that of the human subject is distinctly oval, so by this peculiarity alone the worms can be distinguished.

The dog worm is more active, and escapes quickly out of the field of the microscope, whereas the human *Filaria* will remain in the field or near by for many minutes, and in half-an-hour it will not have travelled far when under a cover glass. Beck's two-inch, with the higher eye-pieces, I find most suitable to search for *Filaria*.

LETTER BY DR. DA SILVA LIMA.

The following abstract of a letter (to myself), whilst referring to the communication of Dr. Paterson, also notices other matters having a more or less direct bearing on the subject under consideration :—

"Bahia, le 17 Juillet, 1879.

"Très honoré et savant confrère,

"Je me fais un devoir de vous remercier du bienveillant accueil que vous avez bien voulu faire à mon article sur la *Filaria Medinensis*, en le faisant paraître *in extenso* dans le *Veterinarian*; honneur qu'il ne méritait pas, et encore moins celui d'être tiré à part. Mon excellent ami, M. le Docteur Paterson me prie de vous envoyer son dernier (troisième) article sur la question de la gaine de

la *filaria sanguinis hominis*, publié en portugais dans notre *Gazeta Medica* (Maio, 1879).

“ M. le Docteur T. Hall, un confrère très distingué de cette ville est parti d'ici le 6 de ce mois, et après un court séjour au Maranhão, sa province natale, se rendra à Londres. J'ai profité de son obligeance pour vous envoyer une curiosité de pathologie exotique ; dont vous ferez ce que vous jugerez plus à propos selon l'intérêt scientifique qu'elle puisse avoir. Ce sont deux pièces pathologiques de la singulière maladie que les noirs africains appellent *Ainhum*, mot qui dans leur langue (Nagô) veut dire *scier*. C'est une constriction spontanée de la racine des petits orteils qu' à la longue finit par les amputer. Avec ces pièces le Docteur Hall vous remettra un court travail que j'ai publié le premier sur cette curieuse maladie en 1867, et une petite note sur le cas particulier d' où proviennent ces pièces. Quelques médecins de la marine française ont observé cette maladie, Collas à Pondichéry (Ind. Orient), Corre à Nossi-bé (Afrique), (*Archives de Med. Navale*, 1867 et 1879), et la considèrent, bien à tort, comme une variété de la Lèpre grecque, opinion à laquelle se range, ce me semble, votre compatriote le Docteur Tilbury Fox. Il n'est pas moins vrai, cependant, que cliniquement et histologiquement, cette affection n'a rien de semblable à aucune des manifestations de l'*Elephantiasis Græcorum*.

“ Agréez, &c.,

“ J. F. DA SILVA LIMA.”

#### ON THE INVOLUCRUM OF THE FILARIA SANGUINIS HOMINIS.

By J. L. PATERSON, M.D., Bahia.

(Abstract.)

Having stated frankly my own conviction that the so-called involucrum of the *Filaria sanguinis hominis* is not an integral part of that nematoid, but an adventitious product, a hyaline mould of the *liquor sanguinis*, through which, in process of setting, it laboriously wriggles itself, I am content to leave the further elucidation of that question to others, and, above all, to Dr. Lewis himself, and shall, with equal pleasure and loyalty receive the final verdict, whether it confute or confirm my own ideas ; and, in the present paper, would desire simply to say a few words in justification of the spirit and method pursued by me in this enquiry, both of which have been

blamed, I think, unfairly, by Dr. Magalhaes in his paper in last number of the "Gazeta Medica."

Dr. Magalhaes, in that, as in his former paper, charges me with calling in question what had been observed by others, notably by Dr. Lewis and himself, for no other or better reason than that I had not been able, as he surmises from inadequate methods, to confirm myself the truth of those observations.

Now, as I have already explained, I never called in question any observation ; what I called in question was the interpretation of an observation, plain and palpable and common to all of us.

My method for the microscopic examination of *Filaria*, Dr. Magalhaes finds antiquated. How he came to know my method is to me a mystery ; for in no communication of mine have I ever so much as alluded to that subject. When I spoke of the state of the blood on the slide at the end of five hours, I spoke of that state as the context, and the whole scope of my argument showed, not as seen in any original observations of my own, but as seen in those of Dr. Lewis, or of any one else, repeating those observations, according to the method employed and described by Dr. Lewis himself.

With Dr. Magalhaes' permission, I therefore pass on the cap of antiquity to Dr. Lewis, for whose head, and not for mine, it must have been intended. While on this subject, Dr. Magalhaes, who appears to take an unwonted interest in my scientific education, will be glad to learn that, even before his very lucid hints, I had already acquired some elementary notions of the possibility and the means of retarding or diminishing the natural plasticity of the blood, and had turned that knowledge to account so far, at least, as to prolong on the slide the life and movements of *Filaria*, and my observations on the same, to periods frequently extending beyond 24, and not rarely beyond 36 hours.

In Dr. Lewis' opinion, no difficulty ought to be encountered by any one in detecting the involucre, as he had never himself failed to do so in thousands of cases, and affirms that the only requisites for the purpose are a good microscope and properly-adjusted illumination. This assertion of Dr. Lewis I can fully bear out ; never, with all the *mal adresse* with which I am so liberally credited by Dr. Magalhaes, having once failed, if not in thousands, at least in many scores of cases, to detect what Dr. Lewis calls—and what under protest, I am content to speak of as the involucre, when, looked for at the time, and with the slide prepared as Dr. Lewis

directs. Whence, then, all this difficulty, as conjured up by Dr. Magalhaes?

Dr. Lewis, in perfect consistency with his theory, that the *Filaria sanguinis hominis* is enveloped in an extremely delicate tube, closed at both ends, within which it is capable of elongating and shortening itself, speaks of the shortening of the *Filaria*, and of the visibility of the involucrum as synonymous, as convertible expressions for one and the same phenomenon. How, indeed, could it be otherwise? The problem is a very simple one. Inside, but unattached to, a very diaphanous tube, closed at both ends, there exists a comparatively opaque body, capable of entirely filling it. Under a low microscopic power nothing is seen but the opaque body. Under even a high power, so closely fitting is the enclosing tube, and of such extreme tenuity, that it cannot be distinguished as a separate existence, so long as the opaque body continues to fill the whole of it, coming into view only when that opaque body is withdrawn.

As the tube is a short one, that withdrawal can be effected only by the shortening of the opaque body itself; which shortening is, therefore, the exact measure of the part of the tube rendered visible. This shortening, like the opaque body itself, might be seen under a power too low to bring into visible recognition the pellucid tube; but, under no possible circumstances could the empty tube become visible, and the shortening of the opaque body remain unseen. The difficulty of seeing the involucrum consists solely in its extreme tenuity. The difference in breadth of a blood corpuscle, as it squeezes itself through a space narrower than its transverse diameter, is an appreciable quantity, and is, assuredly, not the ten-thousandth part of the ordinary length of the involucrum as seen in Lewis's demonstrations, and, consequently, not the ten-thousandth part of the shortening undergone by the *Filaria*, as Lewis says, from one moment to another. Now, these two correlative quantities, the shortening of the *Filaria* and the proportional visibility of the involucrum, that, were Lewis's theory the true explanation of the phenomenon, ought to be, and are spoken of by him as being, the exact counterpart of one another, are, in the experience of every observer in Bahia, seen to be altogether independent the one of the other; or, rather, one of them is seen to be altogether non-existent. We have here, all of us, seen the so-called involucrum of all imaginable lengths, from under one-thousandth to over one half the length of the *Filaria*, and



yet we have never once, any one of us, seen the slightest lengthening or shortening of the *Filaria* itself. And Dr. Magalhaes, perfectly ready to show the involucre, confesses his inability to demonstrate the lengthening and shortening of the *Filaria*. I can, therefore, only repeat what I said in my former paper:—"Either Dr. Lewis, having a preconceived idea that the *Filaria* exists inside a closed sac, and seeing occasionally, as he supposed, half of that sac empty, inferred, but did not observe, the shortening of which he speaks—that shortening being, in fact, an inference and not an observation—or he is there in India observing a *Filaria* altogether different from ours here in Bahia."

EXTRACT FROM A PAPER BY DR. MAGALHAES, OF RIO JANEIRO, ON THE ORIGIN OF THE INVOLUCRUM OF *FILARIA SANGUINIS HOMINIS*.

"In my preceding article I had said that if Dr. Paterson and I lived in the same town, it might, perhaps, be possible for me to show him on which side lay the truth; that is, prove to him that the involucre of the *Filaria* is constituted by a true membrane, and not by coagulated fibrine. More exacting, Dr. Paterson replies that if I could ensure showing him the palpable changes in the length of the *Filaria*, he should almost feel tempted to come to Rio de Janeiro on purpose. If Dr. Paterson puts off his coming to this city till I can assure him of what he thus desires, I fear that Rio de Janeiro will not have the pleasure of welcoming such an illustrious guest; as the collaborator of the 'Gazeta-Medica' must be aware that, if in the course of innumerable repetitions of minute and prolonged observations, both in Bahia and in Rio de Janeiro, it was only on some rare occasions that I succeeded in observing and demonstrating the movements of shortening and lengthening, I could not compromise myself to show him, at any given time, such histological phenomenon, the manifestation of which I have not the power of bringing about at will.

"If in all the *Filaria* *Wuchereri* such movements were clearly manifested, then, assuredly, the observation of the sheath would not be the difficult matter it is, nor would it have escaped the search of so many observers."

FINAL NOTE BY THE PRESIDENT.

I am sorry such *savans* as Drs. Paterson and Magalhaes should

have displayed opinions so divergent as above shown. I am greatly indebted to Dr. Paterson, but I cannot abandon the views which I put forth in the Linnean Society's Journal, and which I find Dr. Magalhaes has quoted and endorsed in his paper entitled "O envolucro membranoso da *Filaria-Wuchereri*," published in the "Gazeta Medica da Bahia," for May, 1879, p. 223, et seq.—T. S. C.

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