

of Yorkshire (Leeds) Corps : Surgeon Jas. S. Loe is granted the honorary rank of Surgeon-Major.

ADMIRALTY.—In accordance with the provisions of Her Majesty's Order in Council of April 1st, 1881, Fleet Surgeon Edward Mulcahy has been placed on the retired list of his rank from Feb. 10th, 1882.

### WILLS AND BEQUESTS.

THE will of David Graham Miller, M.D., Fleet Surgeon R.N., of No. 113, Camberwell New-road, Kennington, who died on November 28th last, was proved on the 23rd ult. by Mr. George Henry Burnett, one of the executors, the value of the personal estate being over £18,000. The testator leaves to his wife, Mrs. Maria Miller, his household furniture and effects and an annuity of £42 for life or widowhood ; to his son, William Graham Miller, £7000, two houses in Orkney, and a further sum of £500 to complete his education for the ministry ; to his daughter, Euphan Graham Miller, £4000 ; to his daughter Mrs. Martha Caldwell, his Queensland debentures, in addition to what he settled on her at her marriage ; and the residue of his property between his three children.

The will of Timothy Tudge, M.D., of Park Villa, Park-road, Yeovil, Somersetshire, who died on Sept. 25th last, was proved on the 2nd inst. by Mrs. Charlotte Tudge, the sole executrix, to whom he gives all his property, real and personal. The value of the personalty exceeds £1500.

The will of William Gwillim, M.D., of Burton-on-Trent, who died on Oct. 14th last, was proved on the 25th ult. by Miss Emma Waring Gwillim, the daughter and sole executrix, to whom, subject to a legacy of £100 to his married daughter, Mrs. Selina Waring Watson, he leaves all his property.

The following legacies have recently been left to hospitals and other medical charities :—Mrs. Mary Anne Smith, of 21, Russell-square, £5000 to the London Hospital ; £500 each to the Scotch Hospital, Crane-court, Fleet-street, the Hospital for Diseases of the Chest, Victoria-park, and the Royal Free Hospital, Gray's-inn-road ; £300 to the Royal Sea-bathing Infirmary, Margate ; £100 to the London Truss Society ; and £50 each to the Bloomsbury Dispensary and University College Hospital.

### THE NAPPER TESTIMONIAL FUND.

THE following circular has been issued by the Provisional Committee of the fund :—

All who have been connected with a cottage hospital will recognise the great benefit its establishment has conferred upon the public and the medical profession. The first cottage hospital was founded by Mr. Albert Napper, M.R.C.S., the Rev. Canon and the late Mrs. Sapte, at Cranleigh, in Surrey, in the year 1859. As Mr. Napper not only took part in the foundation of this hospital, but by his writings, published in THE LANCET and in *Good Words*, and circulated amongst the profession privately, and by the invaluable aid he has always readily extended to everyone who desired to gain information upon the subject, greatly fostered the movement, he is clearly entitled to claim that he is, in the best sense of the word, the founder, and in fact the father, of the cottage hospital system. In the twenty-one years which have elapsed since the Cranleigh Cottage Hospital was opened, about three hundred others have been founded in various parts of the United Kingdom. The system has extended to the colonies, and has recently become popular in America. Mr. Albert Napper has lately retired from practice, and many members of the medical profession and others interested in cottage hospitals have thought the present a fitting opportunity for initiating a movement which will commemorate Mr. Napper's work in a substantial manner. With this object a Provisional Committee, consisting mainly of the medical officers of cottage hospitals situated in the counties of Sussex, Surrey, and Kent, was appointed at a meeting of the South-Eastern Branch of the British Medical Association held in Reigate on Oct. 20th, 1881. This Committee, after due deliberation, has decided to invite the co-operation of the medical officers of every cottage hospital, and of all others who are interested in their management and progress. A sub-committee, honorary secretaries, and treasurers have also been elected *pro tem.*, with the view of enlisting the support of all who are likely to be interested in this movement.

HENRY C. BURDETT,  
CHARLES PARSONS,  
J. HERBERT STOWERS, } Hon. Secs.

23, Finsbury-circus, E.C.,  
Feb. 11th, 1882.

## Correspondence.

"Audi alteram partem."

### FILARIA SANGUINIS HOMINIS AND FEVER

To the Editor of THE LANCET.

SIR,—Dr. Carter has asked me, through THE LANCET of Oct. 22nd, 1881, if I consider that the great abundance of filariæ in the blood of the lad Tiong-Seng (details of whose case and filarial chart are published in the Journal of the Quekett Microscopical Club, vol. vi., p. 239, No. 47, July, 1881), on July 14th and 15th, had anything to do with the subsequent febrile attack. I think not. To establish a cause and effect connexion would require many more observations and a great expenditure of time and labour. It is a hint worth following up. In the meantime I look on it as a coincidence.

I would suggest to Dr. Carter that his finding in patients in Bombay filariæ during the paroxysms of elephantoid or lymphatic fever, but not in the non-febrile intervals, is explained by his having sought for them during the day only, and not during the night. Filarial periodicity is disturbed by fever. The embryos during the febrile state, and for some time after it has subsided, are thrown into the general circulation during the day. Fever subsiding, the parasites revert to their usual nocturnal habits. Dr. Carter, working probably only during the day, therefore missed them ; though had he searched the blood at night, during the non-febrile intervals, he probably would have found them.

In addition to answering Dr. Carter's letter, I would crave space for a few remarks on your report of the discussion at the Pathological Society on Dr. Mackenzie's case of filarious hæmato-chyluria.

1. Why should the disease be called "hæmato-chyluria?" The word is misleading, and evidently misled some of those who took part in the discussion. "Hæmato" implies that there was blood, and "chyluria" that there was chyle in the urine ; and that therefore the pathology of the disease must include rupture both of bloodvessels and chyle-vessels. But there is no evidence whatever that filariæ attack the bloodvessels ; and it would be difficult to understand how they could. The damage they do, as far as known, is entirely confined to the lymphatic system. The presence of red coagula appears to have been the only evidence for assuming that the urine contained blood. In chyluria, whether the lymphous or chylous admixture with the urine is red or white, the lymphatic system is alone affected. The varying proportion of red coagulum is easily explained without assuming rupture of bloodvessels. It is well known that lymph or chyle as it travels along the lymphatics, and especially towards the opening of the thoracic duct, acquires a red tinge and contains many discs shaped and coloured exactly like those of blood. Lymph proceeds in its development as it passes along the lymphatics and through the glands. Though the passage is arrested or retarded by filarial obstruction, development may still advance. A chyluria is sometimes milky, sometimes red ; discharge from a lymph-scrotum similarly is sometimes watery, sometimes milky, sometimes red, and that in the same subject. The colour of the discharge depends entirely on the part of the lymphatic system it regurgitates from and the time it has been in the vessels. It is as absurd to suppose that chyle or lymph could escape from bloodvessels as that blood could escape from lymphatics ; and it is certainly exceedingly improbable that both sets of vessels should be involved in the same way, in the same organs, and in the same individual.

2. Dr. Mackenzie remarked that filariæ were most abundant in the blood-clots. The inference from this would be that most of the filariæ came from the bloodvessels. I have shown that the clots were not blood-clots, but lymph-clots ; and why the filariæ were most abundant in these is evident. The entire mass of lymph originally passed into the urine coagulated, just as blood would do in similar circumstances. The coagulum therefore included all the filariæ, and as the lymph fibrine contracted the serum was expelled, and the corpuscles and filariæ were concentrated. Hence the dark-red colour of the clots, and the large proportion of parasites they contained.

3. Dr. Carter remarked that the parent worm is probably

the cause of lymphatic obstruction. Facts, to enumerate which would occupy too much of your space, are against this supposition. I hope next year to publish evidence which shows that lymphatic obstruction is produced by ova prematurely expelled from the uterus of the parent female filaria. These ova give rise to embolism of the lymphatic glands, where, on account of their size (1-300"  $\times$  1-500") they are arrested. In ordinary circumstances neither parent nor offspring does any damage whatever. Both are perfectly adapted to live in harmony with their human host. But when the products of conception are hurried prematurely into the lymphatics before the embryo has stretched the chorionic envelope which becomes its sheath, and acquired its slender eel-like vigorous form, so perfectly adapted for the passage of very fine vessels, then the filaria sanguinis hominis becomes a dangerous guest. I have twice removed ova from obstructed lymphatics, and Cobbold has found in urine ova very much like those of this parasite. What has been seen to happen once or twice happens oftener. Like other animals, the parent filaria aborts at times. Imagine an aborting worm pouring myriads of embolic ova into the lymphatics. Imagine the direct and remote effects of this according to the particular part of the lymphatic system she occupies, and all the varieties and seeming anomalies of Wücherer's helminthiasis receive an easy explanation. This, I am convinced, is the key to their pathology.

4. The explanation of the phenomenon of periodicity suggested by Dr. Carter is hardly compatible with the facts adduced by Dr. Mackenzie, and some I have been able to gather. Dr. Carter suggested that the overflow of chyle following absorption of food carried with it the embryo filariae into the blood. But if this is the case, why are they not found in the circulation after the morning and midday meals as well as after the evening meal? and why was periodicity not disturbed when the hours of eating were changed in Dr. Mackenzie's patient? The facts connected with filarial periodicity may be best explained by one of two suppositions: (a) The parent worm empties her uterus of mature embryos once every twenty-four hours, parturition going on from late in the afternoon till midnight, and (as a corollary) the young filariae living but a few hours in the blood, as Dr. Myers suggests; or (b) parturition is a more or less continuous process, the young being nearly constantly carried along the lymphatics and thoracic duct into the blood. In this they live for an indefinite time, circulating with it, under ordinary circumstances, during the night; but from some unknown cause, and after some unknown fashion, becoming fixed during the day. Against the assumption of intermitting quotidian reproduction are the following facts. The urine of Dr. Mackenzie's case contained filariae both during the day and during the night. Filariae were constantly, or nearly so, present in urine which I had caused a chylurious patient to pass every few hours for a week. Lymph from a lymph-scrutum, which had been running constantly for many consecutive days, contained embryos whether procured during the day or night; in this case the parent worm was found in the scrutum, so that the lymph into which she emitted her young could not have lain long in the vessels. The hypothesis of quotidian intermitting reproduction implies but a very short life to the embryos; but it is difficult to understand what purpose favourable to host or parasite could be served by this enormous daily mortality. Nature's object in making these creatures so prolific is evidently to provide as many chances as possible for the continuance of the species. Nor is it easy to understand why animalcules which can live for so many days outside the body of their host should die after so short a life in it.

The facts I have stated, and other evidence I do not bring forward at present, lead me to believe that the second hypothesis is the correct one—viz., that reproduction is continuous, and that the embryos are fixed or filtered out in some organ or tissue during the day. Dr. Carter correctly remarks that embryos are not always entirely absent from the blood-stream even during the day, occasionally one or two may be found then. These, I believe, have but recently escaped from the thoracic duct, and are carried once or twice the round of the circulation before they are brought thoroughly under the influence that has fixed the rest of the swarm. This hypothesis explains the prodigious numbers circulating during the night. I have counted as many as 200 in a slide  $\frac{1}{2}$  in. by 1 in. This would give, on the

assumption that the entire volume of this man's blood amounted to 15 lb., that the hæmatozoa were equally diffused throughout it, and that the quantity of blood examined was about half a drop, a population of upwards of 46,000,000 of embryo parasites. To give birth to this prodigious swarm every twenty-four hours is surely beyond the reproductive powers of many parents, energetic though the reproductive powers of the filariae may be. Undoubtedly, the young parasites are exposed to many dangers, and the life of each is beset with risks, mechanical and vital; these risks keep the stock under, whilst the supply keeps pace with the loss.

5. In your comments on the discussion you say that knowledge is still wanting on the question as to the possibility of asexual reproduction in the case of this parasite. We already know that the mature forms of filaria sanguinis hominis are male and female after their kind. The female has frequently been found, and Lewis has found fragments of the male. Analogy is entirely against the supposition of asexual reproduction. The sexes, though living together, are always distinct in the case of similar blood parasites; for example filaria immitis from the heart of the dog, filaria picae mediæ from the semilunar valves of the magpie, filaria corvi torquati from the pulmonary artery of the ringed crow of China—all of which have been carefully studied both in their mature and embryo forms. There is no reason to think that the human parasite differs in this respect from corresponding hæmatozoa in the lower animals.

6. I have repeated Dr. Mackenzie's experiment of keeping a filarious subject awake all night, allowing him to sleep during the day, and with the same result. After two or three days, during which egress and ingress of embryos were very irregular, periodicity became completely inverted.

Filarial periodicity may be only a pathological curio. I cannot but think, however, that it is in some way bound up with the many rhythmical phenomena, physiological and pathological, which have hitherto defied explanation; and from the ease with which it lends itself to experiment, its discovery may yet lead to useful results. Certainly the experiment of plugging the lymphatic glands by embolic ova performed for us by the parent worm is an interesting one, and shows well that one of the functions of these organs is to act as a filter. Were the ova permitted to enter the blood, consequences much graver than a chyluria or an elephantiasis would ensue.

I am, Sir, yours truly,

Amoy, Dec. 21st, 1881.

PATRICK MANSON.

## IS IODINE A DISINFECTANT?

To the Editor of THE LANCET.

SIR,—This a curious question to ask after reading Mr. Perry Marsh's letter in THE LANCET of the 4th inst. My belief is that it is not. It is an antiseptic, not a disinfectant. Like many other so-called disinfectants, such as Condry's fluid, chlorine, carbolic acid, *et hoc genus*, none of these substances are disinfectants unless used of such strength as to be destructive; moreover, the ordinary vapours of these substances not only do not destroy infection, but actually preserve it by preventing its putrefaction. They do not destroy the active properties of vaccine lymph; indeed, some years ago I read several advertisements in the medical journals, containing the following sentence, "Pure vaccine lymph preserved in carbolic acid," &c. The only real disinfectants that I am acquainted with are those which hasten putrefaction, as sulphurous acid, bisulphide of carbon, all gases that contain hydrocarbon, such as coal gas, and others of too dangerous a character to be of any practical value. These substances may be advantageously divided into three distinct classes—namely, disinfectants, that is, those substances that destroy contagion by hastening the death and putrefaction of the living germs; antiseptics, those that decompose septic or putrid animal matter by converting it into carbon, &c., and deodorisers, or those that decompose the effluvia of decaying vegetable matter. I am not aware that Dr. Richardson ever recommended iodine as a disinfectant proper, and some years ago he gave me the credit in a contemporary journal of being the first to recommend its use as an antiseptic.

*A propos* an article in the same number of THE LANCET, I can safely affirm, and many others, I am sure, will bear me out, that septic poison, as it occurs in the so-called cases of puerperal fever, may be and is immediately destroyed by