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PATRICK MANSON,

1844-1922.

BY

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ZONAE TORRIDAE TUTAMEN

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PATRICK MANSON, 1844-1922.

The death of Sir PATRICK MANSON, G.C.M.G., F.R.S., which, as already announced in the Society's TRANSACTIONS, occurred on Sunday, the 9th of April, has taken from the world one of its unproclaimed benefactors and from the medical profession one of its most distinguished leaders, and has bereft the Society of the most venerable of its Founders and the most illustrious of its Fellows. But, full of well-spent years, full of honour, retaining the affection of his many friends and the regard of his many colleagues and associates to the last day of his life, and dying peacefully, he may, in truth, be said to have closed his long life happily.

PATRICK MANSON was born at Old Meldrum, Aberdeenshire, on the 3rd of October, 1844. His early intention was to be an engineer, but "the divinity that shapes our ends" diverted him to Medicine, and in 1865 he graduated M.B. at his native University of Aberdeen. The following year, having proceeded to his M.D. degree, he decided to follow his calling in the Far East, and went first to Formosa, where he remained for five years, and then, in 1871, to China, where he continued—first in Amoy and afterwards in Hong Kong—for eighteen years.

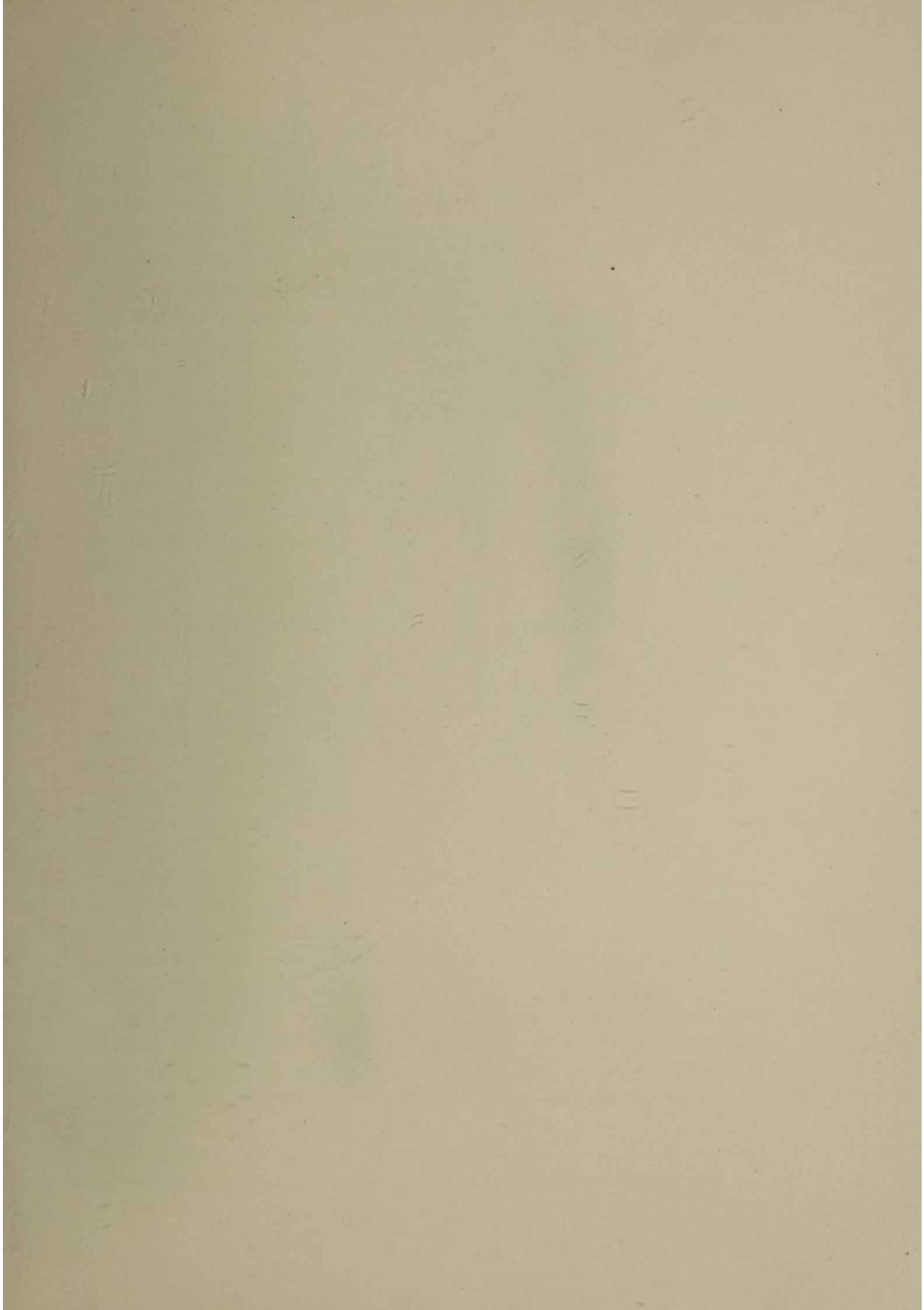
To follow MANSON's career in China with full appreciation, one has to resuscitate the standards of professional knowledge at that time

respecting the causation of what, for convenience, we speak of as "tropical diseases." In the main, these diseases were attributed, by a line of argument analogous with Touchstone's Shepherd's explanation of the cause of night—to the tropical climate. The preponderating influence of parasitic animalcules was unknown. The instrumentality of biting insects had indeed been surmised by a few intelligent men, notably, as BLANCHARD recently reminded us, by BEAUPERTHUY (1807-71); but no idea of the nature and obligation of the insect connection had yet dawned.

The ordinary man in a novel pathological atmosphere does not perhaps trouble much about causation; he is content if he can deal successfully with the practical difficulties of the moment. The man of high ability does concern himself with causation, and is happy if he can discover immediate causes. But the rare type of mind goes beyond all these into the beginnings in nature, and it is the working of a mind of this original type that was seen when MANSON in Amoy applied himself to his first great piece of work—the investigation of the causation of filariasis.

It is necessary to refer in some detail to this—the earliest—work of MANSON, because, in the first place, it is this that fixes his place in the foremost rank of medical discovery, and, secondly, because its full value seems never to have been fully appreciated; largely by reason of the fact that when, nearly twenty years afterwards, its lessons were applied in the study of the causation of malaria, it was occulted by a discovery of a more sensational kind.

MANSON had become fully informed of TIMOTHY LEWIS's observation in the blood of sufferers from chyluria of the filaria now known as *Microfilaria bancrofti*, in 1874, when he was in England on a holiday. On his return to Amoy, in 1876, he found that the parasite was extremely common in the Chinese population, and settled down to a study of its history. With the help of two Chinese assistants, whom he had trained, he made a survey of the blood of a thousand men, in the course of which he discovered that, in ordinary circumstances, the microfilariae begin to make a show in the subcutaneous blood about sunset, gradually increase in number towards midnight, and then gradually decrease until at midday it may be impossible to find them at all. This discovery of "filarial periodicity," made in 1877, was, in the course of time, published in the





PATRICK MANSON AT THE AGE OF THIRTY-ONE
1876

"Medical Reports of the China Imperial Maritime Customs" for 1881. Having satisfied himself that the microfilariae of the blood are the issue of adult filariae locked up in the lymphatics, and that they are embryos incapable of further development in the blood—discoveries in which, however, he had been anticipated by BANCROFT, of Brisbane—he argued that the train of events by which the microfilariae ultimately become adult filariae must take place in the outside world, and be begun in the body of some free-ranging blood-sucking insect, such as a mosquito, his selection of this insect being, as he remarked, particularly congruent with the phenomena of filarial periodicity. With the compliance of an infected Chinaman he put his hypothesis to the proof. He put the Chinaman to bed in a mosquito-house with plenty of mosquitoes, and in the morning he collected all the insects that had fed and stalled them separately in bottles, so that they could be examined one by one at successive intervals. The following quotations reveal the quiet fervour with which he followed, now and afterwards, the verification of his hypothesis:—

"I shall not easily forget the first mosquito I dissected. I tore off its abdomen and succeeded in expressing the blood the stomach contained. Placing this under the microscope, I was gratified to find that, so far from killing the filaria, the digestive juices of the mosquito seemed to have stimulated it to fresh activity. And now I saw a curious thing. The little sack or bag enclosing the filaria, which hitherto had muzzled it and prevented it from penetrating the wall of the blood-vessels in the human body, was broken through and discarded."

MANSON grasped the fact that, as with a great Darwinian humility he expressed it, he "had stumbled on an important fact with a distinct bearing on human pathology."

"I followed it up as best I could with the meagre appliances at my disposal, and, after many months of work, often following up false scents, I ultimately succeeded in tracing the filaria through the stomach wall into the abdominal cavity, and then into the thoracic muscles of the mosquito. I ascertained that during this passage the little parasite increased enormously in size. It developed a mouth, an alimentary canal and other organs. . . . Manifestly it was on the road to a new human host."

This great discovery was communicated to SPENCER COBBOLD, who, in turn, presented it to the Linnean Society, where it was read on the

7th March, 1878. Subsequently it was published in the Zoology section of the *Journal of the Linnean Society*, Vol. XIV., 1879, under the title, "On the Development of *Filaria sanguinis hominis*, and on the Mosquito considered as a Nurse."

It is not surprising that in the multifarious distractions of a large medical and hospital practice, MANSON did not follow the final passage of the larval filaria from the mosquito to the human host. He thought that when the infected mosquito went to water to drop its eggs it died, and that then the young filariæ escaped—as indeed young Gordiid worms escape from their insect hosts—into the water, and so reached man by that road. His discoveries, however, went quite far enough to have disclosed a "fact with a distinct bearing on human pathology," to have established a great, new, far-reaching principle of pathogenesis—the principle of *the necessary intermediation of the blood-sucking insect* in an extensive tract of tropical pathology.

In discoveries there are degrees of weight and worth. The discoveries of the highest class are those that extend the boundaries of science, increase understanding, and clearly point out new fields of enterprise. MANSON's discovery in Amoy was intrinsically one of this class, and it gains an extrinsic merit from the isolated position of the discoverer and the hindrances amid which he worked. To speak of him as "isolated" is not to forget that no man works alone; as Carlyle says, "all past inventive men work with him there"—silent, and perhaps unheeded, they escort and support him and wait upon him. And so, in turn, it may be said of every man who works out some fresh illustration of MANSON's great principle of pathogenesis, that MANSON, even if for the moment forgotten, "works with him there."

Like other revolutionary discoveries this of MANSON's was generally (if we except SPENCER COBBOLD, and ALEXANDER OGSTON, of Aberdeen, who taught it in his Class of Surgery there in 1882-83), held questionable. TIMOTHY LEWIS suspended judgment. LEUCKART was sceptical. So MANSON, in 1884, made a further communication to SPENCER COBBOLD (*Transactions of the Linnean Society*, Series 2, "Zoology," Vol. II., 1879-1888), in which, after expressing some surprise that distinguished men should doubt facts, so easy of verification, without making any attempt to investigate them, he very carefully recapitulated all his original work and described his subsequent confirmation

of it. He illustrated the story with a plate of 46 figures, showing every stage of the development of *Filaria bancrofti* within the mosquito, from the tiny embryo in the stomach to the well-developed larva—"a large and powerful animal equipped for an independent life"—in the body-cavity of the insect. He added the extremely significant facts that only one of the four Amoy species of mosquitoes was hospitable to the parasite, and that one freely hospitable only at a particular season of the year. He stated, finally, that in the course of his confirmatory work, about a thousand infected mosquitoes had been examined, and also that he had checked his observations by examination of mosquitoes fed upon uninfected Chinamen. Yet so strange and novel was his great conception that, even after this full and closely-reasoned restatement of its reality in nature, it did not receive the whole-hearted appreciation that it deserved. One can think of several reasons for this general indifference: the simplest, and perhaps the likeliest, is that filariasis does not exercise a sensational influence in Western enterprises; it does not decimate expeditionary forces, foil plans of campaign, denude garrisons, and affect military estimates; to the casual eye it does not add much to the white man's burden.

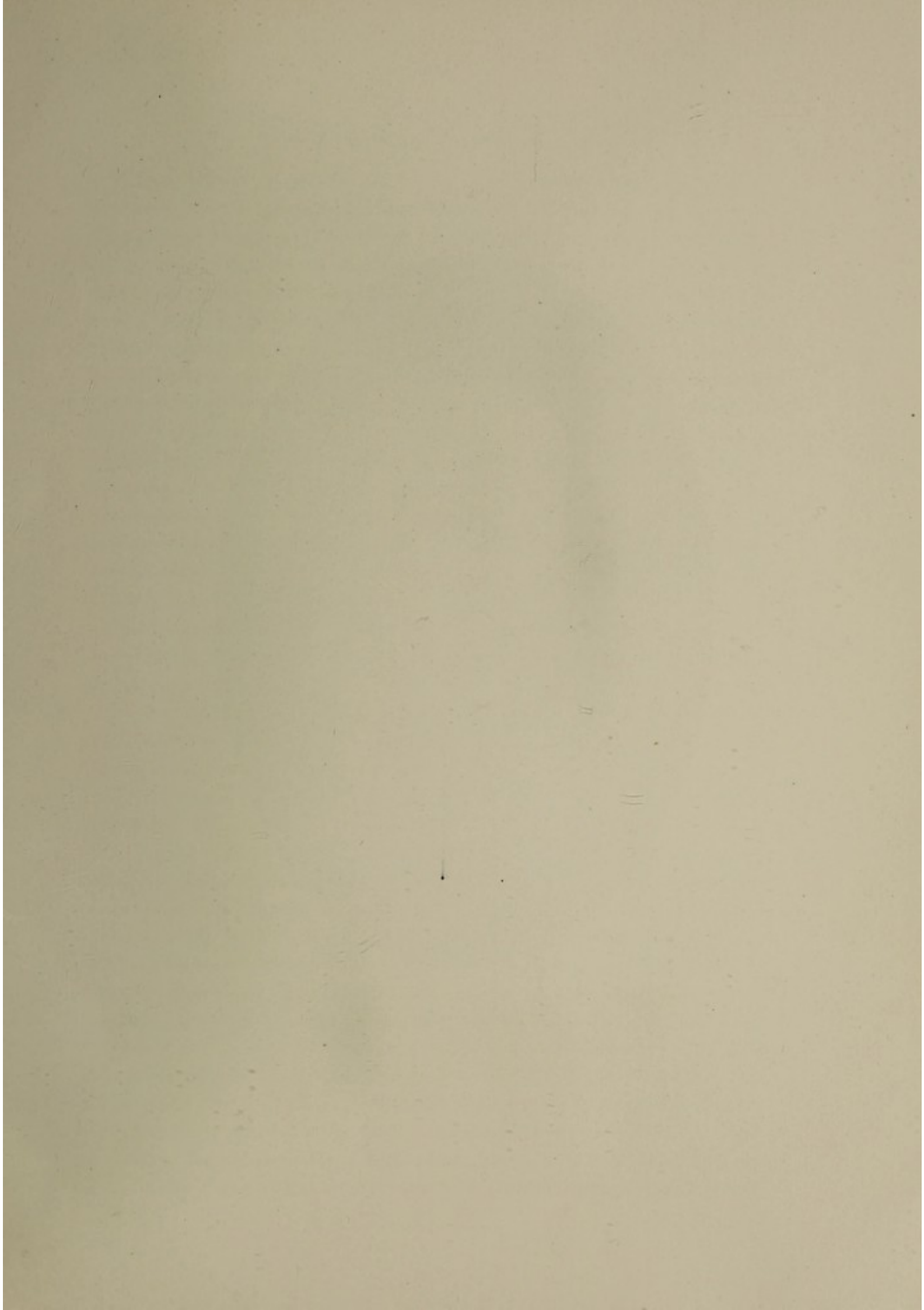
From the standpoint of the detached spectator, MANSON'S ever-memorable disclosure of the relation between the mosquito and the parasite of filariasis would appear but as an interlude in a large and successful medical practice, which comprehended also the engagements of a big hospital, and the responsibilities of a medical officer of the Chinese Maritime Customs. Among many similar side-shows, though of another rank, may be particularly specified his discovery of the eggs of a lung-fluke in the sputa of the endemic hæmoptysis of the Far East, and of the larva of a ribbonworm parasite of animals occasional in man, and his observations on filaria parasites of animals. Wide as was his range in the medico-biological province, and deep as was his craving to explore pathological causes in nature, he was constantly absorbed—and this was what raised him so far above the roll of common men—in all the practical and humane ends of his profession, in which he made many contributions to the treatment of disease, in particular the treatment of that dreadful disease sprue. But all his store of professional experience in China still speaks for itself in his classical book on *Tropical Diseases*, and in the traditions established at the London School of Tropical Medicine. It

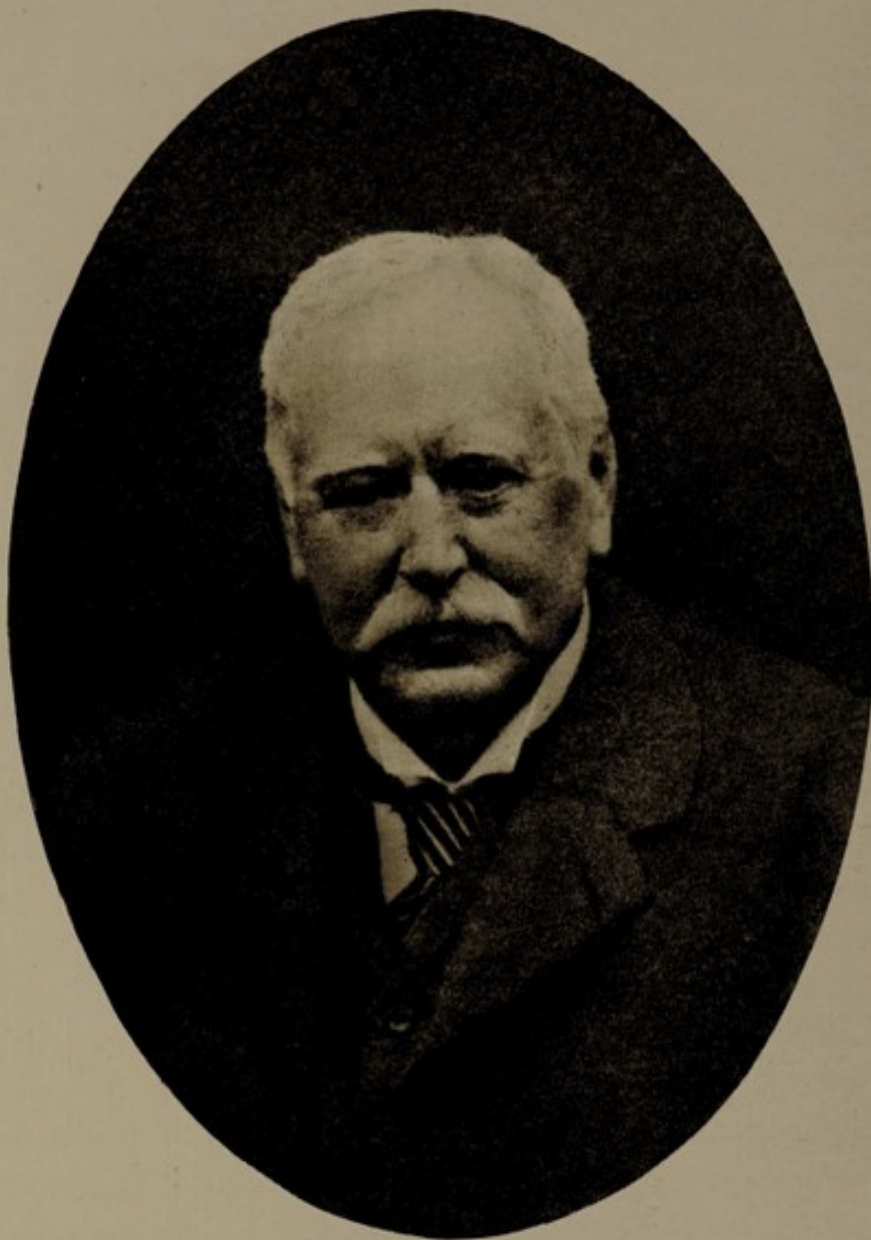
must be mentioned that when he was in Hong Kong, besides all his other work, he took a leading part in organising a medical school for the Chinese, and a pure milk supply for Europeans; for to the large heart of MANSON all the subjects of the British Empire were, in his own words (as Dr. ANDREW BALFOUR has lately reminded me), "a' John Tamson's bairns."

In 1889 MANSON left China, and in the following year he settled in Queen Anne Street as a consulting physician. Here not even the preoccupations of a London consultant could keep him from his experiments, and in several letters of this period he wrote enthusiastically of the growth and development of guinea-worm larvæ in some Cyclops that he had infected. He also kept his eye constantly on the malaria parasites, and I feel sure that had the existence of *Anopheles maculipennis* in abundance near London been as generally well known then as it is now, he would have used that knowledge to considerable effect. As it was, his contributions to the elucidation of the problem of the spread of malarial infection took a grand ratiocinative turn, effective up to the point of determinative experimental proof by others.

It is necessary to review these contributions in some detail.

MANSON, of course, had followed, with an eye of peculiar discrimination, all that had been done in malaria since LAVERAN's discovery of the parasites, and he had the inspiration to see that the flagella extruded from what (by Ross's discoveries) are now known as male gametocytes, are not, as most other observers contended they were, the prelude to death, but are a certain sign of renewed life in changed circumstances. He argued that since the gametocytes (to use the terms of Ross's subsequent discoveries) are constant and particularly resistant, they must be of vital import; that since they do not send forth their flagella until after the blood has got outside the human body, the destiny of the flagella must lie in the world outside the human body; and that since they cannot of themselves emerge from the body, they must be brought out by some outside agent. With these deep thoughts, and his experience of the microfilariæ of Amoy in his mind, it was, to him, an inevitable inference that, from its usual habits, the mosquito must be that agent. In December, 1894, he formulated this argument in the *British Medical Journal*, not as an academic exercise, but in the hope that someone in propitious circumstances would make use of it.





Palmer Mcnison.

Some people speak of MANSON'S well-grounded, closely-reasoned inference as "a mosquito-malaria theory": really it is one of those "incomplete inductions"—the fine produce of those gifted minds whose thoughts by deep and constant reflection become at last endued with prescience—which are indispensable to scientific progress; it was a working hypothesis that supplied the necessary conditions of verification, as distinct from a speculation or theory that ultimately turns out to be true in its drift. It was in the sure light of this working hypothesis that Ross embarked upon the long and laborious investigation that culminated in his splendid discovery of the sexual cycle of malaria parasites in mosquitoes, and of the exact manner in which infected mosquitoes transmit the malarial infection. Nobody has argued the difference, in this connection, between a working hypothesis and a theory more acutely than Ross, who placed his opinion of MANSON'S hypothesis on record in *Nature*, 29th March, 1900, Vol. 61, pp. 522-527, in the following among other words:—

"It is perhaps impossible for anyone, except one who has spent years in revolving the subject, to understand the full value and force of this remarkable induction. To my mind the reasoning is complete and exigent. It was from the first impossible to consider the subject in the light in which MANSON placed it, without feeling convinced that the parasite requires a suctorial insect for its further development."

"MANSON'S theory was, what I have called it, an *induction*—a chain of reasoning from which it was impossible to escape."

"I have wished to defend this work of MANSON'S because it has been much misunderstood and much misrepresented, and even (in a somewhat amusing manner) completely ignored by some who, though they once strongly opposed his theory, now, as soon as it has done its work, wish to forget it. It is true that he endeavoured to predict the history of the parasites a little too far, and that he was in error (as will presently appear) regarding the immediate nature of the motile filaments; but the centre of his theory was invaluable. I have no hesitation in saying that it was MANSON'S theory, and no other, which actually solved the problem; and, to be frank, I am equally certain that but for MANSON'S theory the problem would have remained unsolved at the present day."

MANSON did not merely set Ross's feet upon a rock by supplying the hypothesis from which Ross found it "impossible to escape"; but he

also followed, with sage and kindly counsel, the steps of Ross's advance. Some of his suggestions to Ross are now, in the full light of the latter's subsequent discovery, seen to have been off the mark; but the early exhortations to watch the behaviour of the flagellum, and the later advice to fall back on the avian infection, are sound and sage. I have read copies of fifty-five of MANSON's letters to Ross, and they confirm to the echo Ross's handsome acknowledgment (*Nature*, 29th March, 1900, p. 524) of "the continuous assistance and advice which I received both from Dr. MANSON and Dr. LAVERAN,"—a debt even more wholeheartedly expressed, in an excerpt republished in his Nobel Medical Prize Lecture for 1902 on *Researches in Malaria* (Stockholm, 1904) at p. 67, in the words: "Pour éviter tout commentaire erroné, qu'il me soit permis de déclarer ici que mes travaux ont été entièrement dirigés par MANSON." These letters of MANSON are a revelation of his heart and soul: they reveal a great master mind, rapt in the enterprise of his younger colleague; intent on everything done, and following it with shrewd counsel and suggestion; only anxious lest his colleague be hindered in the quest, or forestalled in the prize; not thinking at all of himself, but always burning for the cause, always solicitous for the acclaim and priority of the man who is doing the hard work in the field.

It has been necessary to rehearse this malaria business at some length—although both in historic precedence and in pure originality of conception, but not, indeed, in prompt artistic finish, the mosquito-malaria discovery of Ross comes long after the mosquito-filaria discovery of MANSON—because, as Ross has said, MANSON's share in it has often been misunderstood and misrepresented, especially outside the somewhat esoteric province of tropical medicine. Some misguided persons have thought that MANSON proved the mosquito-malaria hypothesis by discovering the particulars of the history of the malaria parasites outside the human body; others, not so much misguided as confused, have thought of him as one of several ingenious authors who have speculated on a causal connection between mosquitoes and malarial fevers. The true place of MANSON in the etiology of malaria is as the master mind who, from certain established facts in the initial stages of the malaria parasite outside the human body, foresaw that the *completed history* of the parasite outside the human body would furnish another particular instance of a great general principle already illustrated in his own

discovery of the extra-corporeal history of the *Filaria bancrofti* parasite.

Except as illustrating his fondness for educationally impressive enterprises of the kind, the expeditions set afoot by MANSON to show how men, by carefully shielding themselves from the night attacks of mosquitoes, can live in a highly malarious district without contracting malarial fever; and how *Anopheles* mosquitoes that have bitten malaria patients in Italy can be imported into England to bite and give malarial fever here—these expeditions do not here require much notice. They were not, and were not designed to be, an appeal to scientific men. They were an appeal, not indeed to the man in the street, but to the man in the counting-house, and to what good Adam Smith called that “cunning animal” the politician. The greatest and sincerest minds have succumbed to this kind of weakness. But unacademic appeals of this sort generally lead to trouble. In this case it led people whose definitions make no distinction between proving a hypothesis by a laborious voyage into the unknown and “proving” a schoolboy exercise in arithmetic, to make the ridiculous assertion that MANSON had “proved the mosquito-malaria theory,” and to do him wrong, being so prophetic, to offer him such show of nescience. The true inspirer of the performance is not honoured by applause due to the performers.

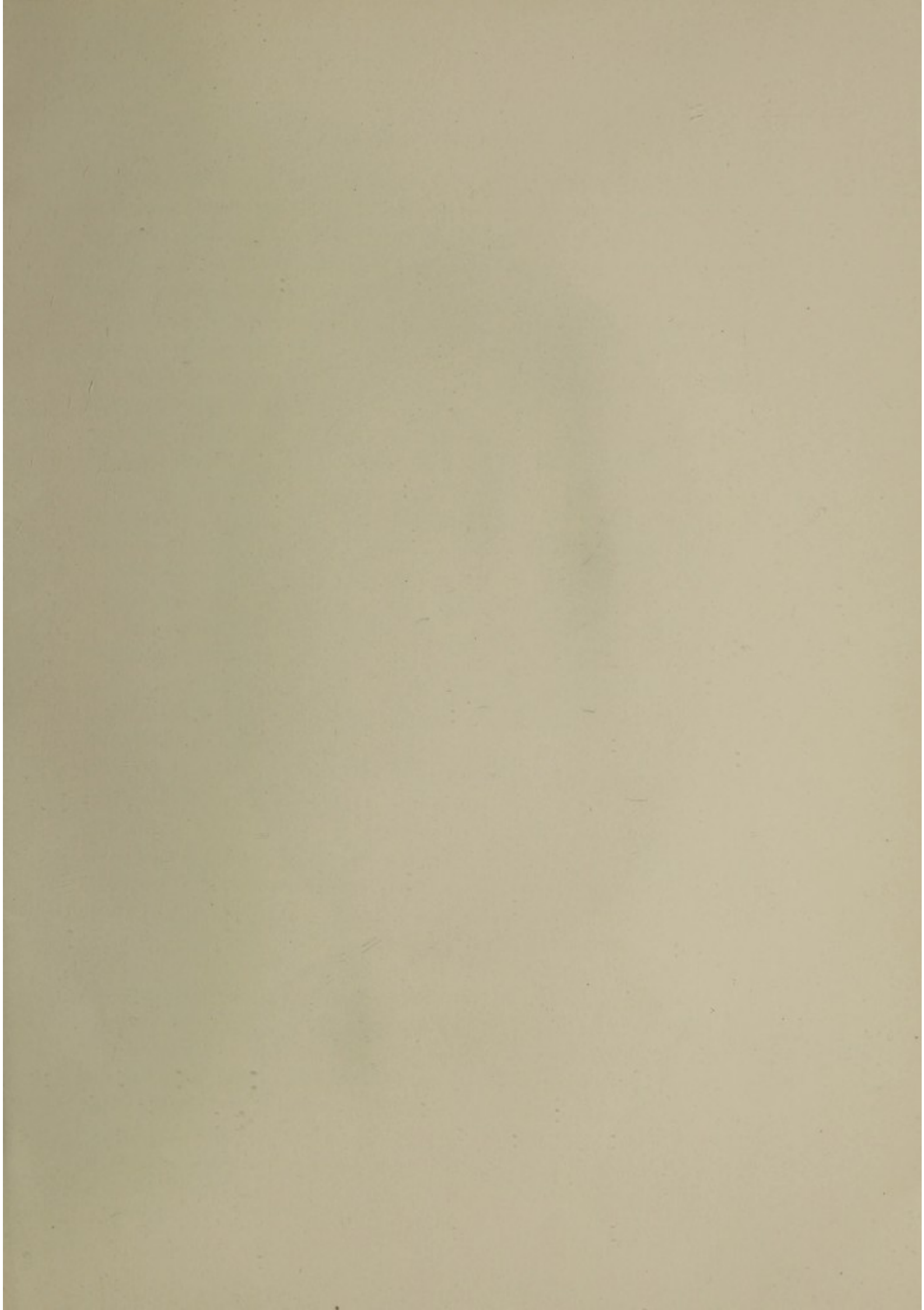
Malaria was only one of the great things that had now taken hold on MANSON. In London, as in China, he remained deeply absorbed in all the living parts of our interesting profession, and led a life so full that he wrote of himself as sometimes not getting time to sleep. In England as in China, he began to make his influence felt in the great cause of medical education, his opportunity looming when, in 1892, he was appointed to the staff of the Seamen's Hospital at Greenwich, and coming full in sight five years later when he became Medical Adviser to the Colonial Office. He started by lecturing on tropical medicine at St. George's Hospital and afterwards at Charing Cross, but this was merely a prologue.

In his early days, when he first came home from China for recreation, full of the pathological puzzles of the East, he had been disappointed at not finding in London any sort of institution where he could apply for help in dealing with the problems peculiar to tropical medicine against the time of his return to China. Thereafter it had been the dream of his life to see at work in Britain schools where the medical man, who meant to serve in the tropics, could fit himself beforehand not merely for

the clinical diagnosis and management of tropical diseases, but also for their investigation by the methods of the laboratory. At the Colonial Office, at last, he was able to impart his magnificent ideal to a statesman of vision, Joseph Chamberlain, who at once grasped its significance and adopted it. A scheme was submitted to the Seamen's Hospital Society, where there happened at the time to be two men of great insight and vigour, Mr. (afterwards Sir PERCEVAL) NAIRNE and Mr. (now Sir JAMES) MICHELLI, who understood it; and by their decisive influence and organising ability the London School of Tropical Medicine was established, in October, 1899, at the Seamen's Hospital at the Albert Dock, with MANSON at its head. It was not the very first school of tropical medicine, for the similar school at Liverpool had come into existence six months before; and it is not the last, for several similar schools are now at work in this and other countries; but of all of them MANSON may justly be regarded as the great begetting influence.

To MANSON's school in London men came, as they flocked to Drake, "for the renown of his name." To the man who looked kindly on all his fellow-subjects of whatever colour as "a' John Tamson's bairns," men came from all parts of the empire; to the man whose great discovery of the mosquito-filariasis principle had opened the way that was to end in robbing the tropics of their sting, men from the ends of the tropics came as to an oracle. Here for thirteen years, both in the lecture-room and by the bedside, MANSON poured forth his experience, not so much supplying mere professional instruction as diffusing knowledge and radiating inspiration. During those thirteen years about eighteen hundred of his younger professional brethren must have listened to him, and again and again have men told me of the spell that he exercised. As one of his hearers has written lately in the *British Medical Journal*: "He has left behind a great inspiration to all who had the good fortune to be closely associated with him. How much we owe to this great man it is difficult to estimate. His monument is in our hearts."

But his influence in the place was very far from being restricted to his own teaching and personal qualities, or to "the renown of his name." He took the keenest interest in the work of all his colleagues; but above all it was out of the depths of his experience that the whole course of study was drawn—a course based on a varied and comprehensive laboratory discipline in which medical zoology bulked large, but a laboratory





Patrick Morrison.

discipline brought in touch with practical professional ends in the lecture room and humanized at the bedside. On this fine foundation there was gradually established what I have ventured to speak of as the Mansonian tradition, a tradition that, for the varied responsibilities of tropical practice, a medical man—isolated and single-handed as so often he is—though always a clinician in his heart and a sanitarian in his broad, official outlook, should, in his attitude to pathology and etiology, be, among other things, a biologist, or, as he himself expressed it, “something of a naturalist.”

Keen though he was in providing for the requirements of post-graduate students, MANSON was equally ardent on keeping the school constantly rejuvenated by a working communion with the tropics, and to this end he was never weary in devising research expeditions to all parts of the tropical world. In proposing these expeditions he showed a catholic taste, with perhaps a slight natural predilection for the helminthological studies that had most fascinated him in early life.

But with all his immense influence in the School, there was in MANSON nothing of the hard-shell despot—not the faintest suspicion of Olympus. He would listen to suggestions from anybody, and he possessed that surest mark of a great sane mind—the ability, on occasion, to suffer fools gladly. His bearing had rather the patriarchal touch, always tempered with a humour which, like his speech, was kindly seasoned with the old-fashioned pungent savour of his native north. To see him in the lecture-room—where sometimes, as his physical infirmities grew upon him, he would have to sit—and to listen to his earnest speech, always called up in my mind the image of Chaucer’s venerable shepherd of his people—

“First he wroghte and afterward he taughte,

But in his teching discreet and benigne.”

Here, most plainly in the spirit and flesh, was a man who whatsoever he taught “first he folwed it himselve.” And in his converse with his colleagues he left the impression of a deeply reflective, far-casting, broad-minded man; large-hearted but free from sentiment: always thinking of the welfare of the School. In 1912 he retired from active participation in our work, but he remained an honoured member of the School Committee, and exercised a decisive influence on its external policy up to almost the last day of his life.

Honours, of course, came to MANSON. The greatest of them all, one might think, was when, at the International Medical Congress held in London in 1913, he was acclaimed "The Father of Modern Tropical Medicine," and presented with his medallion portrait in gold, as "a tribute of respect by his fellow-workers and admirers, and an emblem of the homage paid to the work of a great man." The work of a great man indeed. And when one considers, besides his great achievements as an investigator of nature and as a teacher, his influence as a most prescient clinician—and a clinician who had pity on his patients—his large humanity and his sympathetic and helpful attitude to his fellow-workers; when one considers how worthily he upheld the ideal of Hippocrates, one may hope that, as the Father of Tropical Medicine, his name perhaps may have some of the same lasting fragrance as that of his immortal archetype.

Another similar honour, but of a more intimate and domestic kind, was paid to him in January of this year, when, at the new home of his School, in Endsleigh Gardens, he was presented with his portrait, subscribed for by his friends and admirers in all parts of the world.

Of court and academic honours he possessed the G.C.M.G., F.R.S., F.R.C.P., Hon. LL.D. of Aberdeen, Hon. D.Sc. of Oxford, Hon. LL.D. of Cambridge, and Foreign Associate of the Paris Academy of Medicine.

He married, in 1875, Henrietta Isabella, daughter of Captain J. P. Thurburn, R.N., who brought him three sons and three daughters: all except two sons survive him.

Of the personality of a man still so green in the memory of the Fellows of this Society it is superfluous, almost presumptuous, to write; it can only be done in a brief, prospective spirit for some reader who, when all this great man's associates have passed away, may like to know how he appeared at his zenith to one who worked with him and often conversed with him.

Though deeply absorbed both in the biological principles and the practical and humanitarian aims of his profession, MANSON was not submerged in them: he was a genial, kindly person, with many enjoyable interests in life, a lover of sport and an enthusiastic fisherman. He was not afraid of a glass of port, and he liked his pipe. Though far from being a bookish man he had read much and widely, and had a mind well-stored with the treasures of his reading. Though

not much disposed to any formal philosophy he had a strong sense of philosophy and was intuitively a philosopher, a good deal in consonance with the terse, practical wisdom of Horace. He showed a philosophic discretion in his attitude to drugs, being content, as Dr. GEORGE Low tells us, with the use of specific drugs for specific purposes. Though essentially a man of thought and reflection, he was also a man of action; but, like so many men of action, in judging of other men's work he was inclined to think more of the results accomplished than of the spirit that animated the worker, and in judging of a scientific enterprise was rather disposed to overestimate its educative effect upon the unscientific mind. He was very free from sentiment: though the idea of making the tropics safe for man was rooted in his mind, he never sentimentalized about benefiting humanity; and though the London School of Tropical Medicine was the outcome of his life-long yearnings and the centre of his thoughts, he never alluded to this, nor to his own part in getting the School founded.

Finally, to estimate the position of one's contemporaries is almost an impossibility, but to endeavour to state a great man's claims to lasting remembrance is a duty.

In this endeavour, in the case of MANSON, one must consider again how largely what we speak of in a very convenient if ambiguous phrase as "tropical disease" is due to animal and particularly animalcule parasites, and to the blood-sucking arthropods by which so many of them are nursed (to use MANSON's homely phrase) and disseminated.

When MANSON started on his career the influence of blood-sucking arthropods was not indeed unsuspected, for men before him had vague ideas on the subject; but the exact nature and meaning of that influence had not been thought out, and to MANSON belongs the full credit of being the first to have tracked a specific parasite from the blood of a man into a specific blood-sucking insect, and to have demonstrated by verifiable experiment not merely what becomes of it there, but also that what happens to it there, is one of the necessary conditions of its continued existence as a pathogenic species.

That discovery, merely as a scientific achievement, laid open a large new territory to investigation, started a flood of new ideas, and thus paved the way for fresh conquests over ignorance.

Considered in its application to the practical affairs of mankind, that discovery had overflowing consequences of equal moment.

Out of it and the next discovery begotten (not made) of it by MANSON himself—namely, Ross's discovery of the extra-corporeal history of the malaria parasite—came, mainly by MANSON's influence, the first schools of what, in a convenient phrase, we may call "tropical medicine." Only a man who began his acquaintance with tropical disease in ante-Mansonian days knows what it means to a young man to go to his work in the tropics through one of these schools; whether one considers diagnosis, or prognosis, or treatment, or prevention, it means an altogether different professional outlook and competency.

Out of these discoveries, through the after-workings of the leaven of the schools of tropical medicine, has come a new conception of tropical disease, as being in considerable measure ascribable not so much to peculiarities of climate as to particular conditions of life in the tropics—where (outside big Europeanised cities) the native people live not according to the sanitary standards of the West, but very much at the mercy of half-tamed nature, and very often of her wild insect myrmidons.

Out of this new conception of "tropical disease" issues a new conception of tropical countries considerably lightened of "tropical disease," by the same indirect but orderly processes that have long set bounds to parasites and undesirable wild forms of insect and other animal life in Western Europe.

Finally, out of this conception of sanitarily (not politically) "Europeanised" tropics where "tropical disease" is thus kept at arm's-length, we find justification—perhaps the only complete justification—for the position which the white man has assumed, and still claims, in the administration of tropical countries.

Not merely by this full flood of consequence, flowing naturally and impersonally from his mosquito-filaria discovery of 1877 (and his incomplete mosquito-malaria induction of 1894), must MANSON's place in the history of medicine be calculated. In the calculation we must also include the personal element; and the telling fact in this regard is that MANSON's work was done amid all the traffic of a general medical practice, and far from any academic suggestion or influence; it was in the highest sense original—almost entirely the product of his own inspiration.

But a man may build high on his own thoughts and still may fail of greatness. How, then, shall personal greatness be appraised?

I know of no better standard than the one put by Shakespeare into the mouth of Ulysses, the proverbial far-casting hero of the wonderful Grecian dawn—

“No man is the lord of anything,
Though in and of him there be much consisting,
Till he communicate his parts to others,
. . . . who like an arch reverberate
The voice again, or, like a gate of steel
Fronting the sun, receive and render back
His figure and his heat.”

Judged by this standard, we see great parts freely communicated and reflected in the ardent devotion to education in tropical medicine, in the ready help and large sympathy for all fellow-workers, and, above all, in the Mansonian tradition of keeping medicine in constant touch with its biological foundations by direct investigation of nature herself.

Thus judged by his personal influence as by his scientific achievements we hope—we cannot do more than hope—that MANSON will appear to posterity, as he appears to us, as one of that select company of original minds, Carlyle's “inventive men,” whose mark is permanent, and—as BLANCHARD acclaimed him—the Father of the modern science of Tropical Medicine.

A. ALCOCK.



