

HOW THE MALARIA SERVICE IN INDONESIA CAME INTO BEING, 1898–1948

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In the present lecture I shall have to go back to the very beginning of modern knowledge on malaria. I shall incur grave risks of telling you things you know already; as a matter of fact, it is more than a risk, it is a certainty. But I cannot help myself. All I can do is to pass over the initial stages as summarily as possible.

There are three of these stages. They can be characterized as follows:

First stage: Mosquitoes transmit malaria.

Second stage: *Anopheles* transmit malaria.

Third stage: Certain species of *Anopheles* transmit malaria.

The first stage applies to Sir Ronald Ross's initial work in Secunderabad, leading up to his fundamental (although, by itself, inconclusive) discovery of 1897. The name I have given this stage is really a misnomer. Ross was not an entomologist. He did not speak the language of the entomologists. But he knew quite well that he never succeeded in finding anything that could reasonably be regarded as a stage in the development of a malaria parasite so long as he was experimenting with his grey mosquitoes, or with his brindled mosquitoes. And he knew equally well that he saw things he had never seen in either of them, as soon as he was experimenting with his dappled-winged mosquitoes. The things he saw on the outer surface of the wall of the stomach of two of these mosquitoes he divined to be stages in the development of malaria parasites, because they carried pigment. Once he knew what to look for, he would certainly have found the complete cycle, given time and opportunity. Both were denied him. And when Sir Patrick Manson succeeded in releasing him from other duties, he got time, but no opportunity, because there was too little malaria in the station assigned to him. So he had to work with parasites of bird malaria. And Grassi got his chance, making use of Ross's inconclusive, but indispensable discovery. But that is really beside the point. The point I want to make is this: If, in 1897, one had asked Ross 'Do mosquitoes transmit malaria?', he would have answered: 'Some do, and others do not.' So the true name of the first stage ought to be: 'Some mosquitoes transmit malaria, others do not.'

The second stage is Grassi's stage. Besides confirming and extending Ross's discovery on the development of human malaria in dappled-winged mosquitoes, he gave that mosquito a Greek name, or, to put it more correctly, he found out, or Ficalbi told him, that somebody else had given it that name. Not exactly to that particular dappled-winged mosquito Ross had experimented with in India,

but to another one, belonging to the same genus—and the name of that genus is *Anopheles*.

Now, do not quote Shakespeare to disparage the value of a name, for there is a great deal in a name, if it is Greek, and nobody knows what it means. You might have noticed this fact in Indonesia, from a story Dr Terburgh told me some ten years later.

Nobody in Indonesia believed what Ross said, because nobody took any notice of that homely name ‘dappled-winged’. The idea that malaria is transmitted by mosquitoes was absurd to them. And for a very good reason. They argued like this: Malaria is a disease which does not occur everywhere. Mosquitoes do occur everywhere. Or, to put it more obscurely, and therefore more scientifically: There is no correlation between the geographical distribution of malaria and mosquitoes. So they concluded that mosquitoes had nothing to do with malaria.

But when they heard of Grassi’s statement, that malaria was transmitted by *Anopheles*, their opinion changed completely. Now they were ready to believe. They knew mosquitoes occur everywhere; they were ready to accept Grassi’s word that *Anopheles* do not.

Grassi realized the importance of geographical distribution, and in his opinion there existed an almost perfect correlation between the geographical distribution of *Anopheles* and malaria. As he expressed it himself, *Anopheles* were the ‘spies’ (‘Spione’) betraying the presence of malaria (Grassi, 1901, p. 69, third line).

I remember Grassi telling me of a journey he once made, from Rome to Munich. I never found any reference to it in literature. So I can only repeat his verbal account. Starting from the Agro Romano, full of malaria and *Anopheles*, he passed through the hills with little of either, to descend into the Tuscan Maremmas where disease and vectors were numerous again. The same alternation of conditions was encountered when passing the Apennine range and descending into the plain of Lombardy. The last of malaria and *Anopheles* was seen in the delta of the river Adda, around Colico on Lake Como. Neither of them was seen in Switzerland or the Bavarian Alps.

You know, of course, that in later years a closer inspection revealed the presence of *Anopheles* in many localities where they had been reported absent, Switzerland among them. At present we do not worry about this discrepancy. But in these early days its discovery would have been a serious setback. True; nobody doubted any longer that Plasmodia can be transmitted by certain mosquitoes. Nobody doubts in our days that the bacilli of tularaemia and the Coxiellas of Q-fever can be transmitted by ticks. But many doubt if ticks constitute the only way, or even the most important way, along which the infection spreads. The same doubt existed regarding malaria. For that reason it was of so much importance that a great authority declared that *Anopheles* indicate the presence of malaria.

Even in those days (1900–2) this assertion was challenged. But no one challenged it to such good purpose as Stephens and Christophers in India. If they had not taught us the parry as well as the thrust the mosquito theory of malaria would have fared badly.

That takes us to the third stage, which may fitly be named after these famous

research workers. The importance of their investigations is inversely related to the length of their reports which are all contained within seven small booklets: the *Reports of the Malaria Commission of the Royal Society*.

These two, together with Daniels, were sent out by the Commission to investigate malaria in Africa, India and the West Indies. I have no doubt the principal object was to test the validity of the mosquito theory.

Here I am concerned with one of their reports only, occupying eight pages and bearing the title of *Relation of Malarial Endemicity to Species of Anopheles*.

In order to establish the correlation, if any, between the geographical distribution of malaria and of *Anopheles*, and in order to test Grassi's assertion that *Anopheles* indicate the presence of malaria, they selected an area showing a variety of malaria conditions. The area stretched from Calcutta in the south right up to the Himalayas in the north. It included slightly malarious Calcutta, moderately malarious Plain of Bengal, highly malarious Duars, and non-malarious mountain areas. For the theory to prove correct there ought to be few *Anopheles* in Calcutta, a moderate quantity in the plains, a great number of them in the Duars, and none in the hills.

Except for the last one, none of these conditions was fulfilled; the facts were just the reverse of the expectations. *Anopheles* were very numerous in Calcutta, moderately so in the plains, and decidedly rare in the Duars. The facts might have fitted the theory that the presence of *Anopheles* is inimical to malaria, but they flatly contradicted Grassi's affirmation.

Here was the chance for two ardent young investigators to knock a cherished theory on the head. I do not know whether they were inclined to do so. I once asked Sir Rickard what he felt when he had got so far. He replied he did not feel anything in particular. If they experienced that temptation, they resisted it, and that made them look farther afield. Although they were no entomologists, they did not hesitate to plunge into entomological details by asking this question: 'The mosquitoes we caught in Calcutta, the Plains, and the Duars, all conformed to the type of *Anopheles*. But are they the same, all of them?' The answer to this question was definitely in the negative. The Calcutta *Anopheles* differed from that of the Duars, and the *Anopheles* of the Plains differed from both.

The next step took them to the core of the problem. How about the presence of malaria parasites in these *Anopheles*? In Calcutta, among hundreds dissected not one was found infected. In the Plains a few infected ones were found. In the Duars it took days to catch a sufficient number, but the trouble they took was well repaid; 6 % of the Duar's *Anopheles* were found infected.

Thus, for the first time in history, it was proved that it is incorrect to say that *Anopheles* transmit malaria, just as it was incorrect to say that mosquitoes transmit malaria. The correct thing to say was now proved to be: Certain species of *Anopheles* transmit malaria, and others do not.

A situation which, on first sight, had looked destructive to the mosquito theory, had turned out to be a brilliant confirmation. The correlation between the geographical distribution of *Anopheles* and malaria existed after all, so long as one takes not all *Anopheles* into account but certain species only. *Anopheles* were acting as Grassi's 'spies', not all of them, however, but certain species only.

The mosquito theory of malaria was firmly established. It was no longer a theory, it was a statement of facts.

Nevertheless, Stephens and Christophers pushed their research still further, by studying the breeding places of the various species of *Anopheles* that had come to their notice during their Indian investigations. They found that some species allow themselves a wide range in the choice of their breeding places; others are very limited in their choice, preferring one type of breeding place to all others. The importance of this discovery could not be fully realized at that time. And so it lay dormant for a few years, till it was awakened to full activity in the Malay Peninsula.

This takes me to the second part of my lecture: the mosquito facts as the basis of malaria control.

In accordance with what happened during the time that theory was becoming fact, it is possible to distinguish three stages in the development of the technique of the destruction of the vectors of malaria, viz. (1) to kill mosquitoes, (2) to kill *Anopheles*, (3) to render distinct species of *Anopheles* harmless as vectors of malaria.

There is little difference between (1) and (2), so I can take them together. You know the victories gained in malaria control by wholesale destruction of mosquitoes in Ismailia, Klang, and the Panama Canal Zone. Destruction of mosquitoes in this case means elimination of all collections of water likely to serve as breeding places, or poisoning the larvae in these collections of water. Since few collections of water are, by their nature, wholly unsuitable to serve as a breeding place to any single species of *Anopheles* (even the most fastidious), modern 'species eradication' has, in a large measure, returned to this old method of wholesale destruction of larvae, as it is aiming at the complete eradication of one species of *Anopheles*. It adds to it, however, wholesale destruction of adults in their haunts.

Sometimes wholesale destruction may prove too expensive. That was the case in Malaya.

The Ross Institute of Tropical Hygiene is *the* place in the whole world to teach you what happened in Malaya, in the first decade of this century. So I am not going to repeat to you things you undoubtedly know better than I do myself. I am referring to the history of Dr Watson (now Sir Malcolm Watson) inventing the method of malaria control to which he afterwards allowed me to give the name of 'species sanitation'. Watson never wanted to eradicate a species. All he aimed at was to render it harmless as a vector of malaria by some means or another. It was for that reason I called it species sanitation, that is, reducing the incidence of malaria by making use of the habits of one species of *Anopheles*. Later on Darling thought to improve the word by changing it into 'species control'. Thereby the emphasis shifted to the reduction of a particular species of *Anopheles*. Still later the term 'species eradication' came into use, which is species control carried to perfection. Like species control it may apply to any mosquito, even if it carries no disease germs at all, so long as it is eradicated, not merely reduced in numbers. Species sanitation, on the other hand, includes evading *A. umbrosus* by building the coolie-lines at no less than half a mile from its particular breeding places, as

well as reducing its numbers in the rubber plantations by weeding, so as to prevent drainage ditches getting into deep shade.

But I ought to ask your pardon for this unwarranted digression. Allow me to return to Sir Malcolm, in order to tell you what he did for us during his short visit to Medan, on the east coast of Sumatra, in May 1913. For that visit marks the origin of the Malaria Service in Indonesia, the history of which I selected as the subject of my lecture.

To begin with, let me explain the mental attitude of Indonesia in those days towards malaria control in relation to mosquitoes. I shall do so by quoting two passages out of a book written by our greatest authority on tropical diseases and widely used in those days (1911). The first passage bears on the identification of *Anopheles* and reads as follows:

‘The identification of mosquitoes has become so difficult that it is better to leave it alone, so long as there remains anything else to be done in this world.’

The second one bears on malaria control by anti-larval measures:

‘In no more than a few places, like Klang and Ismailia, drainage, and similar measures, have proved to the benefit of the native population. Most anti-malaria campaigns protected white people only.’

I may add that the great Robert Koch, visiting Indonesia in the early days of this century, praised the Dutch sanitary authorities for their efforts to control malaria by quinization, and expressed the little faith he had in the new-fangled methods of mosquito control (D.M.W. 1900, no. 5).

In these circumstances Kuenen, director of the laboratory for pathology in Medan (eastern Sumatra), entered my room one day in May 1913, and inquired if, as he expressed it, I could talk *Anopheles*. Without the least feeling of shame I was able to say that I could no more talk *Anopheles* than I could talk Egyptian. Neither could Kuenen. He told me he had been interviewed by a man ‘from the other side’, meaning Malaya, and I had better come and talk to him, as Kuenen could not make head or tail of what he was saying. We were both deeply immersed in the subject of dysentery amoebae, and much resented having to turn our attention to anything so uncongenial as *Anopheles*. However, the sacred duties of hospitality compelled us to do so; half-heartedly at first, with our whole heart and mind once we had succeeded in grasping Sir Malcolm’s meaning—for I need not tell you that he was our visitor.

It was rather a shock to him, I believe, to find that malaria was not a major health problem in that part of Sumatra, but Schüffner, to whom we introduced him the same afternoon, convinced him that we knew what we were talking about when we said it was not. We took him round through various parts of the province, to show him whatever small foci of malaria there were to be seen. He showed us our own *Anopheles*, adults and larvae, and introduced us to the first principles of that field of entomology. Shortly after he left, Schüffner and I gathered the first fruits of Watson’s tuition, by collecting *A. leucosphyrus* in houses in the only malarious spot in the neighbourhood, and nowhere else. Our conclusion that here was a malaria vector, premature though it was, was confirmed by Bais,

some six years later, in the same area, and, many years later, by MacArthur in Borneo.

A few weeks afterwards, de Vogel, Director of Health for the whole of Indonesia, came to us for a short visit. He was on his way to Siboga, an important seaport on the west coast of Sumatra, where a serious malaria epidemic had broken out.

We told him all about Watson's work in Malaya. I am afraid it was not all quite accurate, the information we gave him, and that I left him with the impression that *maculatus* larvae were swimming up streams like trout.

He was so much impressed by all this that he got it in his head that we knew a great deal about the subject. As a matter of fact, he himself and Schüffner were the only ones who had ever before occupied themselves with it. Of the three of us Schüffner, Kuenen and myself, I was the only one who was under de Vogel's orders, and so it was I who had to accompany him on his five days' crossing of the island of Sumatra.

Siboga is situated on a narrow coastal plain, bordered on the land side by steeply rising hills, from which numerous small streams descend to form the river at whose mouth the town is built. Still full of what Sir Malcolm had told us, I had decided that this was a place for *maculatus* malaria, even before I reached the town. The assumption that *maculatus* was breeding in the area eventually proved to be correct. Fortunately, however, my notions of where to look for the larvae of this species were decidedly incorrect. Thus, I did not find them. I called that a fortunate mistake, for, if I had found them, we should have based our plan of malaria control on the assumption that *A. maculatus* was the local vector. Even finding *A. sundaicus* breeding heavily almost within the town would not have changed our purpose. And that would have been a fatal mistake. For the epidemic was certainly caused by *sundaicus*. At that time it derived its only claim to be ranked as a vector from Christophers's investigations in the Andamans; Malayan experience did not lend much support to that claim. So *sundaicus* (or *ludlowi* as it was then called) was hardly taken seriously. However, failing to find *maculatus*, we turned our attention to *sundaicus*. It was the old story over again: man-made malaria. Man-made, in this case, not by borrow-pits, or stopping the drainage by a railway embankment, but by trying to improve health conditions by attacking mangrove swamps. Whatever they may be in other countries, in Indonesia they are the most harmless things in the world, so long as they are not interfered with by man, i.e. so long as the tide can freely enter and leave the swamp. But as soon as the tidal flow is cut off by an embankment, conditions become potentially dangerous. Danger can still be averted by quickly filling in the impounded part of the swamp. But, if that is neglected, breeding places of *sundaicus* are likely to appear within a few weeks or months, as the case may be. Sometimes impounding mangrove swamps is inevitable, e.g. for the requirements of a new seaport or a naval base. But in Siboga it was not at all inevitable, in fact quite useless. It had been done on the false assumption that the smell arising from mangrove swamps is deleterious to health; a tragic mistake which caused all the mischief it tried to prevent.

Siboga became the first example in Indonesia of malaria control consciously directed against one species of *Anopheles*. It was not a clear-cut example, because

it came to comprise a general welfare scheme, with new town quarters requiring much wider drainage than was necessary in order to deal with *sundaicus* successfully. The plan more and more developed along the lines of Ismailia and Klang, so as finally to lose its character of species sanitation.

Fortunately, in the next years, 1916–21, species sanitation came into its own in many places. Probably the most striking examples were Schüffner's malaria control in the Mandailing valley (central Sumatra) and Mangku Winoto's in the Plain of Chihea (western Java).

Many histories of malaria control refer to epidemics. It is not always possible to feel quite satisfied that the epidemic would not have gone down, even without any measures. Sometimes they subsided before the measures were properly started.

But that was not the case in the valley of Mandailing. Malaria in that area was endemic: a uniformly high spleen rate, of over 90 %, in children and adults alike; a high parasite rate, and a high parasite count, in toddlers, gradually declining with advancing years to a low level in adults; a high mortality up to the third year, and a comparative freedom from symptoms in older children and adults—except for their big spleens. That is a stable condition, lasting for years. If one succeeds in bringing about any marked change in that stage of things, such a change has an importance vastly superior to a change in epidemic conditions.

People in that valley grew rice; during the wet season only, as there was no system of irrigation. When the rice was harvested the fields remained fallow. Some fields were dry, others were like swamps.

Scattered between the rice fields were fish ponds. They received their water from rice fields and they overflowed into others. There was nothing to distinguish them from the rice fields, except their depth, and the strong and high embankments surrounding them. Nevertheless, there must have been a difference, for these fishponds were the exclusive breeding places of that same *A. sunaicus* we encountered in Siboga. Here, however, it was breeding in perfectly fresh water, far from the sea. Among the seven or eight species of *Anopheles* indigenous in that region it was the only one ever found infected to any important extent.

It was here, in the years 1916–17, that Schüffner made the attempt to control malaria by destroying adult *Anopheles*. Not by means of insecticides, but by catching by hand inside human habitations. He taught the people to recognize *Anopheles*, he taught the schoolchildren to identify *sundaicus*, *hyrcanus*, *annularis*, *aconitus* and *vagus*. On visiting a village for spleens and parasites, the first ceremony enacted was the schoolmaster, with some of his senior wranglers, appearing on the scene, carrying bamboo tubes containing freshly caught specimens of each. When seeing these exhibits, I was always reminded of the warning to doctors I quoted just now, that the identification of *Anopheles* has become so difficult that it is better to leave it alone. Here were Battak schoolboys carrying out that difficult task with a smiling face and, apparently, with the greatest facility. And Schüffner's native assistants! how quickly they reduced a pile of hundreds of mosquitoes to five or six smaller heaps of one species each, even without the use of a hand lens—and they never made a mistake. As a matter of

fact they never made mistakes in anything, except in dissections, which were quite new to them at that time. Schüffner liked my wife and me to catch them at that: 'Quite right; make them feel that they do not know everything. It will keep them in their place', he said.

In parentheses I may add that this was the time white people did everything themselves in a malaria survey. Native assistants gave assistance, nothing more. In later years the so-called 'Menteri malaria' were trained to all particulars of a survey; they carried it out independently. That greatly widened the range of activity. As it occurred long after my time, I cannot say whether the initial standard of accuracy remained unimpaired.

But, to return to Schüffner's attempt. Each village head-man of certain selected villages had to deliver the early morning's catch to the malaria laboratory, where every catch was identified and registered. Conscientious villages could be known by the majority of *sundaicus*, the principal house-haunting mosquito. Lazy villages showed more *hyrcanus*; they had found out that it is easier to make catches in the carabao sheds.

The first year (1916) it looked as if Schüffner might succeed. The next year the plan miscarried, because the Central Government stepped in by declaring this procedure illegal, being tantamount to enforced labour.

So, Schüffner had to devise other means of malaria control. He found it in the fact, revealed by that time, that *sundaicus* was, to all intents and purposes, the only carrier; and that its breeding places were confined to the fish-ponds. These fish-ponds, scattered all over the country, were the property of a relatively small number of well-to-do landowners. The fish grown in them formed no part of the diet of the common people; elimination of the fish-ponds would not adversely influence their nutrition. On the other hand eliminating the fish ponds would do away with the *sundaicus* breeding places—for the moment at least. But would the effect be permanent? Would not *sundaicus* start breeding in the rice fields, once it was deprived of its wonted breeding places? Was there not another valley, separated from the Mandailing valley by a range of mountains, where the same *sundaicus* was actually breeding in rice fields? And was there not a third valley, although much farther away, where *sundaicus* was breeding in grassy swamps, just like the fallow rice fields in Mandailing? Thus, when Government decided to buy all the fish-ponds, for the comparatively small sum of £40,000, they were likely to lose their money, according to the opinion of many people. However, they took their chance, prohibiting at the same time the laying out of new fish-ponds.

After a few years the spleen rates had dropped from over 90 to 30 %, and later on to 10 %. None of the other species of *Anopheles* had been in the least affected by the measures, among them *A. aconitus*, a well-known carrier in Java, and *A. hyrcanus*, a common vector in other parts of Sumatra, that had been found infected in Mandailing to a very small extent.

War, and the aftermath of war, prevents me from continuing the history of Mandailing, as that region became inaccessible. I am not happy about it. Even if *sundaicus* did not reappear, *hyrcanus* and *aconitus* may have played havoc with the population; I do not suppose that they were able to re-establish the highly

endemic conditions which prevailed in the time when *sundaicus* was practically the only vector. But in years of unusual prevalence either of these species may have caused an epidemic of malaria, which would have affected the adult population, deprived of their former immunity, in a manner much more serious than in the old days.

But I do not know that such a calamity happened, and so I disregard it in presenting to you malaria control in Mandailing as an example of species sanitation.

I am not in the same disadvantageous position when relating the history of Mangku Winoto's malaria control by species sanitation in the Plain of Chihea, in western Java. After the war that area remained accessible, and thus we know post-war conditions there.

In former years it was a rice-growing region wholly dependent on the rains. During the dry season the fields lay fallow or they bore dry crops. Rice was planted once a year and harvested once a year. All planting occurred at about the same time, and so did all harvesting. We do not know what malaria conditions were in these old days, but no records of unusual epidemics of fevers are extant.

The next stage in the history of this area begins with a benevolent native chief of high rank inducing the Government to grant a considerable appropriation for the construction of a proper system of irrigation, rendering the cultivation of rice independent from local rains. Now rice could be grown the whole year round, and the population availed themselves of this opportunity to the fullest extent. Each farmer planted or harvested at the time that suited him best.

In former years the whole area had a uniform colour; bright green when the rice was young, dark green when it grew older, yellow when it grew ripe. Now the fields looked chequered; bright green, dark green, and yellow patches lying side by side.

This ruthless growing of rice, rendered possible by the almost unlimited supply of water, exhausted the soil, because it was not balanced by an adequate manuring. Moreover, severe malaria epidemics began periodically to harass the population of the Chihea plain. It was in this condition of agronomical and sanitary deterioration that Mangku Winoto found them. Obviously, as a Health Officer, sanitary deterioration was his first concern.

He found malaria in epidemic form. The vector proved to be *A. aconitus*, breeding in the rice fields and drainage ditches. The largest quantity of larvae occurred in the fields where rice had been cut and the stalks trampled down, i.e. where the rice no longer afforded any shadow. Such a field no longer needs any water; still water was there because the next field, being in an earlier stage of development, had need of it. That younger field also bred *Anopheles*, not *aconitus*, however, but other species, like *vagus*, *kochii*, *hyrcanus* and *annularis*.

These findings could not fail to point Mangku's attention to the agricultural side of the problem he was facing. This led to a close collaboration with agricultural experts, whence emerged the so-called 'planting schedule'. Under this set of rules and regulations the whole of the plain was divided into two parts. On alternate

years each part was allowed a supply of water during the dry season, which enabled the farmers to grow a second crop of rice. The other part got no extra supply, and so it had to do with one crop. At the same time measures to improve conditions of the soil were carried out, and an agricultural experimental station was founded in the plain, to carry out investigations, with a view to further improvements.

Enforcing the planting schedule had two effects:

(1) It put a stop to the exhaustion of the soil. Acting together with the measures to improve the soil, it produced, after some years, harvests the double or threefold of pre-schedule days.

(2) Planting and harvesting being synchronized it was no longer necessary to keep a harvested field flooded in order to provide a neighbouring field with the water it still needed. From now onward fields turning yellow were no longer flooded. This did not completely prevent the breeding of *A. aconitus*. But it reduced it to such an extent that malaria incidence subsided to insignificant figures, and remained at that low level, although malaria epidemics continued in other parts of the country.

Then came the Japanese invasion. The policy of the invaders in all things was to convince the population that all the Dutch had done was wrong. So it was in this instance. They decreed that it was not *A. aconitus* which was the vector but *A. maculatus*. They definitely discouraged keeping up the planting schedule in the Chihea Plain. Nevertheless, the population continued to apply it, wholly on their own accord. When the Dutch returned there, in 1946, conditions were not so good as they had been in 1941. Still, the Chihea Plain was an area of moderate endemicity, with a spleen rate of round about 30 %, situated within a rice-growing province swept by savage epidemics. That result had been obtained by the people, of their own free will, adhering to an agricultural system which did not affect a single species of *Anopheles* but *A. aconitus*. It is difficult to visualize a more striking example of the effect Watson's species sanitation may have.

But I have gone much too far in time. I have still to tell you how the Malaria Service in Indonesia came into being.

Long before these and other projects of species sanitation had proved successful, or the reverse, it had become clear to the minds of all concerned in malaria exploration that an analysis of the local Anopheline fauna, including the identification of the principal local vector, was an integral part of every exploration. Without it, it was impossible to understand the local epidemiological situation. Whether this analysis could show the way to a simplified method of control was another matter; sometimes it could, quite often it could not. But even if it could not, the analysis had to be carried out.

This circumstance greatly contributed to specialization in all work concerned with malaria. It required a specially trained staff, specially trained auxiliary personnel, and special facilities for their training. The numerous reports to the Director of Health, dealing with malaria, which had accumulated in the Director's archives in the course of years, had to be collected into special archives. Mobile

teams were required for emergencies, to advise local authorities how to deal with them, both on the spur of the moment and more permanently. All this inevitably led up to the outspoken desire to have a special malaria service.

Plague, for some years, had had its own service, independent from the Health Department. In the long run this did not prove satisfactory. Moreover, it was not considered consistent with the requirements of a well-organized Public Health Department to have separate services for every preventable disease, even if these services should not exist as independent units. Thus, there was a good deal of official opposition to the plan of a Malaria Bureau, Malaria Service, or whatever name should be assigned to the special organization.

There also was a professional opposition which found voice in the Indonesian parliament as early as 1919. Older members of the profession had always looked askance at doctors busying themselves with mosquitoes. Schüffner, of course, they did not dare to criticize. I, as a non-professional intruder, was below their attention. But when my book on the Anophelines of the Malay Archipelago was published, with the clearly expressed object of putting the subject into the hands of the medical profession, things took a different aspect. Matters became worse when it became more and more clear that species sanitation was not such a simple, cut-and-dried thing as some people thought it was. You will hardly believe me when I tell you that the instruction the Director of Health gave me early in 1917 was to this effect: 'You are to find out what species of *Anopheles* occur in these islands. We know already, from experience in Malaya, which are vectors and which are not. That is no concern of yours. You simply give me lists of names of *Anopheles* in every locality you visit. That is all we require of you.'

Of course I never followed this instruction. Fortunately, after a few recriminations, they allowed me to take my own course. That course may be characterized by this principle: 'Ignore what has been found next door, it is your own house that concerns you.' In every single locality it is often possible to spot the local vector, and to deal with it separately. But in a neighbouring district the same species may not be the local vector, or, if it is, it may not be amenable to measures it responded to in the first.

'So you cannot rely on your species of *Anopheles*', said the most influential member of the profession in Batavia, 'Then what is the use of identifying them?' And when I suggested, in 1917, that there might exist composite species, including sub-species as yet unidentified, he dismissed this as 'trying to find a loop-hole to save a theory which had been untenable from the outset' (he used a more forcible expression which I cannot translate). The same opinion was expressed in the Indonesian parliament, in 1919, by a member saying: 'This, Mr Chairman, is my great objection to the policy of the Health Department: their entomological pretensions, their bickering in entomology'. Finally, zoologists in Holland were as much adverse, though on different grounds. They very much doubted the validity of the Anopheline species. They regarded them either as non-heritable modifications, or the products of Mendelian segregations. If there were valid species among them, they were sure to be so closely related that no sexual barrier existed to prevent their intermating. In 1916, our principal dipterologist seriously

objected to the name of *A. ludlowi*; he even censured the name *A. rossii* var. *ludlowi*; nothing but *A. rossii* forma *ludlowi* could be allowed, because ‘forma’ has no fixed meaning.

Perhaps the zoological objection was the most serious. It certainly took the longest in refuting, and that refutation was not done in Indonesia but in Holland. Even now it should always be borne in mind. But long before that time Rodenwaldt had succeeded in convincing the authorities that a separate malaria service could no longer be dispensed with. He became its first chief, Walch, then Susilo, and Overbeek, succeeded him; Stoker has held the appointment since 1948.

So, that is how the Malaria Service in Indonesia came into being. As with plague, Health Authorities were greatly influenced by British example. But after some initial mistakes they had the sense to see that local conditions do not set aside general rules, but may modify them almost beyond recognition. Notwithstanding the world-wide distribution of the disease, I believe Indonesia is another example confirming the view I ventured to express in 1931: ‘Malaria is a local disease to be dealt with by local efforts’ (*J. med. Ass. S. Afr.* 1931, 5, 418; *Malaria in the Netherlands*, 1938, p. 164).

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