Malária
1913
Dr. Lut’z Contentions Regarding “Forest Malaria”

Frederick Knab
Dr. Lutz’s protest against my remarks on malaria transmission by *Anopheles boliviensis* Th. (A. Lutzii Th.) is interesting and stimulating. It seems almost superfluous to say that I have the greatest admiration for the work of Dr. Lutz, as I am sure have all [those] who have become acquainted with it. Personally I have derived much inspiration from the writings of Dr. Lutz and to discredit him was far from my intention. The criticism to which he objects resulted naturally through the defense of an idea which I still believe is correct in principle. The question is a zoological one rather than a medical one, the pathogenic character of the parasite being merely an incident, and the fact that Dr. Lutz is a skilled naturalist as well as a physician leads me to hope for points of contact. I must reiterate, for the benefit of careless readers, that my remarks were, from the beginning, intended to cover only blood-parasites having life-cycles in alternate hosts and involving a blood-sucking insect. It may be here appropriately pointed out that a train of thought very similar to my own led Grassi to his great discovery, which was not, as generally formulated, that *Anopheles* transmits malaria, but that definite species of *Anopheles* do so. He conceived the idea that the transmitting blood-sucking insect and the disease must show the same distribution, and this corresponds very closely to what I have formulated. There is, therefore, really nothing new or startling in my contentions. What I do claim, however, is that this aspect should not be neglected by investigators.

I have again carefully read Dr. Lutz’s famous paper and also his present communication. From these it would appear that in the question under dispute, which involves the principle formulated by me, we agree on every point but one, namely the question of the insect host in the malaria epidemic under discussion. I am glad to admit that offering an explanation of conditions at such a distance, and concerning which I had no accurate information, was rash; but it was only a suggestion and is now withdrawn in the face of the facts brought forward by Dr. Lutz. We find ourselves on common ground in believing that the malarial outbreak observed among a large gang of workmen encamped in a previously uninhabited forest was not endemic there, but was brought into the locality by such of the workmen as already harbored the malarial parasites, and that it was transmitted

---

from man to man by a species of *Anopheles*. I am, however, unwilling to believe that a species of *Anopheles* which is peculiar to uninhabited forests and under normal conditions can not obtain human blood should forthwith become the host of a blood-parasite of man. It is true that such a case might occur and that we may have it before us in the one under discussion, but in my opinion this would be so exceptional that it would have to be very fully proven.

There is every indication that the host-relation between the malarial parasites and certain species of *Anopheles* is conditioned by a very fine physiological adjustment. Thus it has been commonly observed that of a number of species of *Anopheles* occurring in a given locality and all obtaining their blood-meals from the same source, some serve as efficient hosts for the parasites while other species simply digest the parasites with the blood. Very often the commonest *Anopheles* of a region will not be the one to serve as host. This is the case, for example, with our *Anopheles punctipennis*, and it is well established that *A. rossi* holds a similar relation in the Orient. James and Liston may be quoted to good advantage in this connection:

We have already mentioned that some species of ‘anopheles’ are better malaria-carriers than others, and apart altogether from the fact that ‘anopheles’ may be abundant in a place without there being any malaria at all, it often happens that the species which is present most abundantly is not the one which is carrying malaria at the time. It is, however, a difficult matter to estimate the relative abundance of different species in any place, for some are much more easily seen than others, and the habits which some species have of secreting themselves among the straw of a thatched roof and of resting only upon objects which are as nearly as possible the same color as they are themselves, are very important. In order to exemplify this, it seems worth while to recount an instance which happened in our experience. In the malarious village of Ennur in the Madras Presidency, *A. rossi* was so abundant that on almost every straw of the thatched roof of every house three or four specimens of this species were resting. A careful search in the ordinary way did not reveal the presence any other species, and it is certain that, had there been no other object in the search than the mere determination of the species of ‘anopheles’ present in the village, the observer would have gone away quite satisfied that *A. rossi* was alone present. But the village was an extremely malarious one, and knowing that *A. rossi* was an inefficient carrier of malaria in nature, he was unwilling to believe that no other species was present in the houses. Fixing his mind, therefore, upon the thought that he was looking for *A. culicifacies* and not *A. rossi*, he again commenced the search with great care, and was rewarded not only by detecting the presence of *A. culicifacies*, but by catching a sufficient number of this species during several days’ work, to prove that it was the species responsible for the prevalence of malaria in the place and not very much more abundant species *A. rossi*.1

---

1 A monograph of the anopheles mosquitoes of India (first edit.), p.53-4, 1904. [A.N.]
Now we find that upon a short visit of investigation Dr. Lutz found a single species of _Anopheles_ in the locality and at once concluded that this was responsible for the malarial outbreak. “Ich war sofort überzeugt, die gesuchte Mückenart gefunden zu haben, obgleich damals über die Charaktere der Malariaüberträger noch nicht bekannt war. Als bald darauf erkannt wurde, das dieselben unter den _Anopheles_-arten zu suchen seien, sah ich mit Befriedigung, das die neue Art ein _Anopheles war_.”² He assumed that no other species of _Anopheles_ could be present in the locality because it appeared to him that there were no suitable breeding-places other than the bromeliads. In fact I have found three species of _Anopheles_ breeding in small pools in the bed of a mountain stream, where the topographic conditions must correspond very closely with those outlined by Dr. Lutz, and two of these species (A. _argyritarsis_ and A. _eiseni_ ) occur also in southern Brazil. I have repeated these observations on two visits to Cordoba, Mexico (June, 1905, and December, 1907, to April, 1908), and found the larvae on these occasions in a canyon which is virtually scoured out by mountain torrent after every heavy rain. Similar observations are at hand from the rapid streams in Panama. Furthermore it may be pointed out that during a five month stay at Cordoba I did not capture a single adult _Anopheles_, and had I not collected the larvae I should have been led to conclude that no _Anopheles_ occurred in that locality. It may therefore be pardonable if I express my incredulity that no other than the bromelicolous _Anopheles_ were present in the locality described by Dr. Lutz. Aside from this possible or even probable presence of other _Anopheles_, which admittedly might be in such small numbers as to be a negligible factor, there may be still other sources of error. The question naturally arises: How completely and for how long a period were the workmen confined to the forest habitat? Did they not, singly or in small parties, take holidays outside that zone or make nocturnal visits to taverns and pleasure resorts beyond its confines? From what we know of the habits of _Homo_ in general we have a right to suspect this! In short, the claim that a wholly “wild” species of _Anopheles_ should become an efficient host of a human malarial parasite seems to me so improbable that no evidence other than the demonstration of the parasites in the salivary glands of the mosquito will induce me to accept it.

Lest I be accused of ignorance of the literature, I must state that I have examined a paper by Galli-Valerio in which that author claims to have found oocysts of the malaria parasites in the stomach-walls of a specimen of _Anopheles boliviensis_.³ I am unable to accept Galli-Valerio’s determination. The specimens were brought to him from the State of Paraná by a friend and were in very bad condition (“_qui malheureusement étaient dans mauvais état de conservation_”). Even a very close student of American mosquitoes might hesitate to positively identify such specimens, and I am not aware that Galli-Valerio had previously given the least attention to American mosquitoes!

² _CentralBlatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten_, v.33, n.4, p.283, 1903. [A.N.]
³ “Notes de parasitologie. Sur la présence d’ocystes chez _Anopheles lutzii_, Theobald”, _CentralBlatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten_, 1 Abt. Orig., v.35, p.85, 1904. [A.N.]
As to my statements concerning the association of Anopheles albimanus with man, it may be that I have been too positive. But the available observations, and new ones that have come to hand since my papers were written, seem to show that my contention has some foundation in fact. Naturally the association is not an intimate one, such as it is the case of Aedes calepus and Culex quinquefasciatus, and this indicated in the beginning. The reason is obvious enough in the long period during which the malarial parasites are present in the human circulation. We now know that Anopheles albimanus will fly long distances from breeding-places to obtain blood and fly back again to lay eggs, just as do certain species in India which are known to hold a similar relation there. Recently veritable migrations of this nature have been observed. In them the Anopheles albimanus were not carried involuntarily by the win, but were governed in their movements by the food-supply for the adults and by the available breeding-facilities. Finally there would seem to be a variation in the ratio of Anopheles albimanus to other species of Anopheles, according to the size of the settlements (within certain limits!) and the consequent available food-supply in men and animals. I do not think that this difference is explainable by topographic conditions, but believe that where conditions are otherwise favorable it is governed by the consideration just mentioned.

Finally it must be admitted that, in formulating what I believe to be an important principle, I have been somewhat dogmatic. But the nature of the subject, and, except in the case of the most pronounced examples, the dearth of observations bearing upon it, have made this necessary. If I have caused students to think, to criticize, and perhaps to investigate from a new point of view, I shall feel that I have done something worth while.

Dr. Dyar said: To state the matter concisely, certain workmen engaged in railroad construction in a wild, weeded country were afflicted with malaria. Dr. Lutz, investigating the outbreak, found no other Anopheles present but the species breeding in the epiphytic bromeliads in the forest, A. lutzii Theob. He concluded that they were responsible for the outbreak of the disease among the workmen.

At the time of this investigation, it was not known to what a degree of specialization the malarial relation had established itself. It was thought that malaria in man was to be considered as conveyed by Anopheles as against other mosquitoes. Lutz’s conclusion was, therefore, at the time a natural and plausible one. But we now know that the malarial relation is a highly specialized one. Each kind of malaria is conveyed usually by but one or two species of Anopheles in a locality. Often we have in a given locality several species of Anopheles present, only one of which is capable of carrying the form of malaria prevalent there. Mr. Knab has pointed out that for such a delicate relation to have established itself, an habitual association of the vertebrate host and mosquito host must have preceded — in other words domestic or semidomestic Anopheles only will be found to be malaria carriers. This view renders Dr. Lutz’s conclusions less plausible than when viewed in the former light, and, in conversation with Mr. Knab, we had concluded that Dr. Lutz’s explanation was probably erroneous. Certainly, in view of recent discoveries,

---

Dr. Lutz’s explanation is at least unlikely and unusual and can be accepted only after strict proof, which has not been adduced.

It seems to me that there are three possible theories to account for the outbreak of malaria observed by Dr. Lutz. First, that the true carrier was overlooked. Second, that the disease was spread by *Anopheles lutzii* from a latent case among the workmen, and, after the incubation period, first in the mosquito, then in the man, appeared generally among the men as a result of the bites of the infected *lutzii*. Third, that there existed a form of malaria among wild animals in the forest, conveyed by *A. lutzii,* and that man is subject to this disease when specially exposed by residence in the forest and so bitten by the mosquitoes already infected from the wild animals.

In regard to the first alternative, it is difficult to discuss possible sources of error at this distance from the facts, distance both of time and space. It is however true that the ordinary malaria carrier may be overlooked, especially if one is possessed by an original idea or theory one wishes to establish. The men may not have been as strictly confined to the camp as supposed, and there are a thousand and one possible chances of error, any one of which may have been operative. Personally I believe that Dr. Lutz was the victim of some error of this kind.  

In regard to the second alternative, the chance that wild species of *Anopheles,* never before having carried human malaria, should be in a condition to do so when malaria cases are presented, seems remote. The condition is possible, but unlikely, and should only be accepted after rigorous proof. The ordinary malarial parasite should be proved to develop in *Anopheles lutzii.*

The third alternative is no more than an interesting possibility. No material organisms are know to inhabit wild animals and be transferable to man, though it seems possible that there might be parasites of monkeys, conveyed by forest *Anopheles,* and man perhaps susceptible to them. If such a relation exists, it could be demonstrated by suitable study, but I think we are not entitled to invoke it as an explanation of the present case merely on a possibility.

As between the second and third alternatives, there should be at once evident a difference in the time of appearance of the disease after the men were encamped in the forest. In the second case the disease would appear much later than in the third. But the first alternative might show either a long or short period, according to what the actual mode of infection was, whether by the infection of local overlooked *Anopheles* from latent cases among the men themselves or by infection of the men individually outside the camp by already infected mosquitoes.

But whatever explanation be the true one, the burden of proof rests upon the investigator, in this case Dr. Lutz, and we have he right to expect that proof should be complete or to reject the explanation offered.

---

5 It seems to me probable that some ordinary *Anopheles,* like *A. albimanus* or *argyritarsis,* was really present, but overlooked by Dr. Lutz. It appears from his article that he was too much impressed by the apparent lack of ground breeding-places. He found species of *lantinosema* and others present, which are exclusively ground-pool breeders, but he classifies them as occasionally breeding in bromeliads. Apparently he accounts for their presence by their supposed faculty of occasionally so breeding; but this is surely an error. Where *lantinosema* could be present there must certainly have been abundant opportunity for the breeding of ordinary *Anopheles.* [A.N.]