

## OPEN DISCUSSION

Moderator: Dr. Smith

**Dr. Fox:** I was very interested in the long list of agents to which lice have been exposed. There is only one agent in the sense of a specific strain that Dr. Jenkins did not mention and that is the E strain of *Rickettsia prowazeki*, which is safe for man but lethal for lice.

I suppose one might emphasize the ultimate absurdity of attempting biologic warfare against lice in this way by conjuring up the possibility that Weigl and some of his disciples had been equipped with a large supply of *R. prowazeki* E strain and injected it intrarectally into lice so that they would all die. They might even have immunized man against typhus in doing this, but somehow I do not see it as a very practical approach.

**Dr. Busvine:** In view of the rather negative evidence about the use of harmful microorganisms against lice, what about the inverse possibility of exterminating the beneficial microorganisms in the mycetomes? Has that been considered? I remember also that Dr. Krynski presented some evidence about the harmful effects of antibiotics on lice.

**Dr. Jenkins:** The organisms have been taken from the mycetomes, and it was shown that there was some decrease in longevity. This may be related to the vitamin production of some of the organisms in the mycetomes.

**Dr. Krynski:** In investigations with Dr. Becla, I found that some species of bacteria increased their virulence for lice after being passaged through them. This was particularly true of *Staphylococcus aureus*. It occurred only by direct passaging from insect to insect. If agar culture was introduced between them, the virulence rose very slowly. The increase in *S. aureus* virulence depended

on the strain, some becoming virulent quickly and others slowly. The most virulent for lice were not cocci but gram-negative rods, which killed lice within 24 to 48 hours. *Pseudomonas* and *Aeromonas* species appeared to be more virulent for lice than Enterobacteriaceae.

**Mr. Cole:** Dr. Busvine asked, "Has anyone tried antibiotics?" I conducted a preliminary, exploratory test several years ago with penicillin in rabbits to see if it had any deleterious effects on lice. Even though it was very crudely done, I got no promising results and so did not follow it up.

**Dr. Kostrzewski:** Dr. Jenkins enumerated several rickettsiae such as *R. quintana*, *wolhynica*, and probably *pediculi* in his second table. Since they seem to be the same agent, I wonder if it is not the proper time to reevaluate this sort of classification. The only criterion was the pathogenicity or lack of pathogenicity for man, and thus we know that the same strain in various situations may cause subclinical infection. It may produce the carrier state, which can continue for many months or even several years. Since we can culture these strains on nutrient bodies, should we not investigate their antigenic structure? That might answer the question.

**Dr. Gaon:** *Staphylococcus* was not mentioned among the pathogens for lice. I have a colony of lice of the Hamburg strain, and I remember very well that one day in 1950 when I came to the laboratory, my technicians told me that 60 per cent of the lice had died. I took the guts of the lice out, put them on blood agar, and found staphylococci. I had to throw away the whole louse colony. It was the same as an epizootic of

*Salmonella typhimurium* in mice. Therefore, I suppose it may be that some *S. aureus* strains can be very, very pathogenic for lice.

**Dr. Tarizzo:** Would it be useful to introduce *S. aureus* for the control of lice in a population that already has many infections? Wouldn't there be a risk of introducing a new infection?

**Dr. Reeves:** I have not heard anyone report looking very seriously in natural louse populations for pathogens that might control them. There was really no mention of the usual type of viral pathogens of insects that have shown promise against a number of other insect groups. Until we start looking for pathogens in natural lice with electron microscopy, fluorescence, and other sophisticated techniques, I don't think we are going to look at the whole gamut of possibilities.

I would also expand research on the microorganisms that seem normally associated with lice. If any of them are essential to the survival of lice, are there phages or other agents that might be associated with them that would adversely affect a louse population by removing what they require in the way of commensals?

Finally, I believe that the World Health Organization has held a panel meeting to advise on what position it should take concerning the release of insect pathogens that might adversely affect man. I can imagine that panel's reaction to proposals that staphylococci of any type be released into a human population.

If we could find a pathogen that really attacked lice and not humans, it might be something quite isolated geographically. If such an agent were introduced into louse populations not previously exposed to it, even in small amounts, it might spread epidemically through them. Typhus does, after all, and rather ineffectively as far as the human source for louse infection is concerned.

I hope that we might follow the same principles that have been used in the biologic

control of agricultural insects, i.e., that if we introduce a really foreign pathogen into the louse population, it might cause an epidemic. The pathogen would have to have an effective means of spreading from louse to louse, and I doubt that we know enough about life-tables and the exchange of lice between hosts to predict the success of such efforts.

**Dr. Gratz:** The WHO panel Dr. Reeves referred to was a joint Virus Unit-Vector Biology and Control group. I was away from Geneva when it met, but I believe Dr. Tarizzo took part.

**Dr. Tarizzo:** The meeting was convened last week by my colleagues responsible for the arboviruses. I attended only two sessions, but I made a point to ask some of the participants if they had any information that might be relevant to this Symposium. It might be premature to go into much detail about the meeting because the report is still in draft, but, in a nutshell, the participants concluded that no louse viruses are known. Some such viruses have apparently been sought, but none have been found. The feeling was that the priority of such research was rather low.

**Mr. Cole:** In response to Dr. Reeves' suggestion that we look for pathogens in new places, I would propose an area such as Dr. Smith mentioned, a place where the high humidity seems to result in low louse populations. Perhaps there is an associated organism in such areas that is doing the job.

**Dr. Kim:** First, a taxonomic comment. The scientific name of crab lice should be spelled *Pthirus pubis* instead of *Phthirus* or *Phthirius pubis*; this particular nomenclatural problem was pointed out by Hopkins many years ago. Then, in 1968, because of the consistent misuse of the generic names, I discussed this nomenclatural problem again in relation to *Pthirus gorillae*, which is very similar to *P. pubis* and found on the gorilla. I noted then (*J Parasitol* 54:690-95, 1968) that "the generic name *Pthirus* was decided

as an available name for this taxon by the International Commission on Zoological Nomenclature (Opinion 104, 1928). The other names *Phthirius* Burmeister and *Phthirus* Leach have been placed in the Official Index of Rejected and Invalid Generic Names in Zoology (Direction 63, 1957) and became unavailable." I would like to have you note the correct spelling of *Phthirus*.

Second, with regard to an Anoplura eradication program, I would like to make two important points. The nature of parasitism must be carefully taken into consideration because facultative parasites such as mosquitoes differ greatly from the obligate ones in their biologic, ecologic, and physiologic traits. We cannot simply apply a control technology for facultative parasites to the obligate parasite, the human louse, because we are actually dealing with an entirely different kind of system.

My other comment is related to the population dynamics of human body lice. It is obvious that we do not know very much about the population dynamics of anoplurans, and the reason for this lack of information is that we have been busy stamping out epidemics of typhus or serious louse infestation and thus have had no opportunity to study population dynamics. Whenever a louse-related problem came up we had to rush to eradicate louse infestation. My study of the population dynamics of lice on the fur seal during the last three years has indicated some interesting phenomena, such as a definite cycle in the population's development. We can probably use some of this information about the population dynamics of other lice to understand the disease louse's system, and we should also pursue work on human lice in nature to relate it to the louse control problem.

Now I come to my recommendation. We may consider the host of obligate parasites as a mobile island, and, taking into account

the theory of MacArthur and Wilson on island biogeography, we can learn a lot about louse dispersal and establishment in this sense. Through listening to your presentations on louse control practices, I have felt that we are trying to solve the louse-related problem using a monothetic approach, while population eradication should effectively be made by polythetic approaches.

When we talk about a control program for facultative parasites or pests, we are talking about population management in general. In other words, we are trying to reduce the pest population below the economical level. But when we talk about obligative parasites, we are not talking about population management in the general sense. Instead, we are seeking to eradicate the population. We must therefore realize the different nature of the pest control system.

I have seen how lousy a given human population can get. When I was in service with the Korean Army, we were required to collect at least 20 lice per day as a sanitary measure. That was the easiest way to reduce a louse population, although it was ineffective. Because of the cultural practices and economic level in Korea, lice prospered and, accordingly, each individual and family learned to deal with the louse problem. For instance, there was a customary way of reducing the louse population: mothers would literally pick the lice from their children every night after they had gone to bed.

We are not now facing the louse and typhus problems that we did in World War II and the Korean War, and thus we can study the louse-host problem in a more rational way. What I propose is that we consider a holistic or systems approach to louse eradication. Several workers have pointed out that humidity seems to have an important influence on louse population density in a given region. Someone pointed out that DDT resistance may be another factor. So we

should consider the climatic, cultural, economic, and biologic factors influencing both the host and the louse, including behavior and population dynamics of the lice. When all this information is put together, we will stand to gain momentum toward developing a model with which we can simulate a situation. This model should provide us with valid recommendations.

**Dr. Wisseman:** I would like to second Dr. Kim's comments. We have talked a little about bionomics, but we really have not talked about how lice get from one little island to another—that is, one person to another—in their dynamic pattern in large human populations. This would probably have a great deal of bearing on control measures.