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NEWS

Conference on Anopheline Biology and Malaria Eradication (May 21-23, 1969)

Sponsored by the US Army Medical Research and Development Command and the Armed Forces Pest Control Board this Conference was held in the Walter Reed Army Medical Center, Washington, D. C. Col. R. M. Altman was the moderator and Professor J. E. Scanlon and Dr. R. A. Ward were Program Coordinators. The Conference was opened and the participants welcomed by Col. A. J. Colyer, Executive Officer, WRAMC and Col. L. P. Frick, Special Assistant to the Director, WRAIR.

Briefly the specific aim of the Conference was to take stock of the present situation and make recommendations for the future. To this end several well-known entomologists were invited to attend and present papers on specific aspects as follows:

- Global Review - L. J. Bruce-Chwatt, read by George Davidson.
 Problems Facing Vector Control - J. Hamon, Vector Ecology. H. F. Schoof, Physiological Resistance.
 Systematics of Malaria Vectors - J. A. Reid. Systematics with special reference to Southeast Asia, read by J. E. Scanlon.
 M. Coluzzi. Problem of sibling species.
 Methodology of Control - M. S. Mulla. Measures against immature stages.
 C. N. Smith. Repellent Development.
 Application of New Procedures to Control - J. B. Kitzmiller. Genetic Control.
 G. B. Craig. Sex and Accessory Hormones.
 H. C. Chapman. Animal and Virus Parasites.
 D. W. Roberts. Fungal Parasites.
 Assessment of Control and Eradication - M. T. Gillies. Measurement of Populations.
 C. Garrett-Jones. Epidemiological Entomology.
 G. Conway. Computer Simulation.

In addition to the items mentioned above Drs. D. Clyde, D. Micks and G. Davidson, on invitation by the moderator, addressed the Conference on drug resistance, chemical taxonomy and a recent experiment on genetic control of A. gambiae in upper Volta, respectively.

Of over 50 observers who were invited to attend and take part in the discussions only about 25 were present at any one time and their participation in the discussions was minimal.

On the final afternoon a small committee appointed by the moderator drew up a set of recommendations which were accepted with acclaim. These plus the contributions made by the participants will be published by the Entomological Society of America later this year as one of their Miscellaneous Publications.

It has been felt for some time that an impasse had been reached in the progress of the world wide malaria eradication campaign. Prof. Bruce-Chwatt presented a brief history of the subject and said that prior to the advent of the residual insecticides the decrease in prevalence of malaria was based "primarily on the use of larvicides and on naturalistic control." The great campaign in Zululand and Natal with indoor pyrethrum spraying from 1932-1945 which saw an end of the devastating epidemics and made a big dent in the endemic zone is not mentioned. The fact that this pioneering and highly successful work was based on the results of the study of vector behaviour makes it even more memorable.

With the advent of DDT and other residuals some spectacular results were reported. The first African Conference on Malaria in Kampala in 1950 supplied the initial stimulus for world-wide malaria eradication, the 8th World Health Assembly adopted it officially and the 6th WHO Expert Committee on Malaria, fortified by, and laced with, Macdonald's epidemiological theories, gave it what Bruce-Chwatt so aptly calls its "book of words." However, it gradually became evident that all was not going well with the Malaria Eradication program. Trouble, like the "cassoulet de la mere Clemence," had been brewing for some time and by 1962 at the WHO Third Africa Malaria Conference it was already fully and officially realized that Malaria Eradication had failed in Tropical Africa, at least, and by 1968 the Global Campaign had not only slowed down but had actually suffered some reverses.

Bruce-Chwatt and Hamon et al dealt in some detail with the factors involved in this halt in the progress of malaria eradication. It was clearly shown that there was no single cause for failure - vector exophily, exophagy, physiological and behaviouristic resistance to insecticides, operational defects and unfavourable human ecology - all played a part in varying proportions in the countries surveyed. A most impressive list of vectors which are now showing resistance to residuals was produced by Schoof; no less than 12 are resistant to both DDT and Dieldrin and 22 to Dieldrin alone. This should be enough to shake the confidence and is, more often than not, held to be the most important cause of failure in ME. But when the situation is analyzed, as by Bruce-Chwatt, it soon becomes evident that in nine of the some 30 odd countries examined resistance has not hampered the achievement of, or progress towards, eradication. In a few countries insecticide induced or imposed exophily is a handicap. On the whole, however, it appears that it is only when these two factors, namely resistance and exophily are combined with other disadvantages such as operational defects, unfavourable human ecology, lack of adequate finances and trained staff, that the campaign breaks down. In other words international expertise is not enough if the requisite national support is lacking. This is now abundantly clear and whereas malaria eradication was formerly conceived as an independent activity divorced from any other national health service, they have now been remarried and the international warcry is the "Pre-eradication Programme." Even this

is now somewhat muted for we read in an Official WHO Manual the following: "Pre-eradication Programmes which cannot move to eradication programmes within the foreseeable future are more in the nature of control programmes." (WHO Manual of Epidemiology and Epidemiological Services in Malaria Programmes - R. H. Black, 1968). That the word "control" should now be heard within the "palace walls" is certainly a sign of changing philosophies and times.

It was said a long time ago "Name mine enemy and I shall destroy him" and it seems somewhat strange that it should still be necessary to have to stress the need for accurate identification of vectors. Readers of the "Newsletter" will be happy to hear that the subject received full treatment and was covered by a resounding final recommendation. There were two papers which dealt with this subject, one by Reid on microtaxonomy and the other by Coluzzi on the problem of sibling species with special reference to A. gambiae. In the absence, so far, of any morphological characters for separating the siblings A, B and C of gambiae, rather laborious cross-breeding methods or cytotoxic techniques involving larval salivary gland chromosomes, have had to be employed. These are hardly to be regarded as readily available and practical methods for field studies. The recent work of Coluzzi, however, has shown that the polytene chromosomes of the adult ovarian nurse cells probably are a reliable character for separating the siblings. This seems to throw the door wide open and we can look forward to workers in and associated with Africa making still further progress in this and other fields which have so far been almost exclusively theirs.

Once you can recognize your mosquito, the next step in the ladder, is the investigation of its biology, ethology and ecology and here, of course, man is part of the mosquito's ecology. It is the whole of this ambience which allows or does not allow the establishment of malaria in a community. The problem now is to measure all the factors that go to make up this ambience and this is a truly formidable task at present mainly because we don't know how to do it. Changes in the character of the ambience can produce spectacular results and one need only look at the splendid outcome brought on by such measures as "bonification" in Italy, and economic development in the country I know best, S. Africa, to realize the power of this method. Both of these countries, of course, lie on the fringes of the distribution of malaria but they cannot be disregarded because of this, at least, they serve as examples to illustrate the importance of the environment.

At this meeting we were only called upon to deal with the entomology and both Bruce-Chwatt and Hamon et al elucidated the complicated situation with clarity and brevity. It is quite evident that the whole object of an entomological attack in malaria control or eradication is to break or minimize the contact between vector and man. This is possible either by (1) eradicating the vector, and there are examples of this or (2) making the contact between primed vector and man so tenuous that transmission either ceases or persists at such a low level as to make the disease unimportant.

Now this being the case it behooves us to gather meaningful data for the parameters which determine the amount and intensity of this contact and secondly to assess them. Here we have an exposition by Gillies who has spent many years in this field in what many believe to be the home of malaria, namely, Africa (Bruce-Chwatt, 1965 Paleogenetics and Paleo-Epidemiology of Primate Malaria - Bull. Wld. Hlth. Org. 32, 363). It was shown that nearly every phase of the procedures usually adopted is subject to error and liable

to misinterpretation and one can reel them off, 1) degree of vector contact with man, 2) blood feeding preferences, 3) behaviour under different environmental conditions, 4) density either relative or absolute, 5) longevity and so on. Of course, in the absence of reliable data it is not possible to calculate the Garrett-Jones indices, by which it is hoped to measure diminishing transmission. It does seem quite incredible that except where the vector was eradicated we have no ready, reliable and practical means either entomologically or parasitologically for evaluating the status of transmission at any given moment and at short intervals. Which, of course, is what the malariologist wants. In other words the malariologist works in the dark, more or less, until years later when after having examined mountains of blood slides a tentative diagnosis of the interruption of transmission is made.

Once upon a time one did not mention anti-larval measures in the halls of eradication. One went for the adult mosquito, and according to the book cut short its life with residual insecticides until this was shorter than the extrinsic incubation period of the parasite and transmission ceased! This could be proved by elegant mathematical models and formulae. This attitude persisted throughout and there was little or no thought given to an attack against larvae - in fact there was opposition because it was said to bring on resistance much faster, a contention for which there is actually no factual evidence. Since it was not necessary to eradicate the vector all one had to do was to keep track of its life expectancy and recent Russian discoveries - relating to malaria in temperate zones - in age-grading techniques appeared to provide the answers. Alas! for one reason or another, especially stressed by both Gillies and Hamon et al this has not proved to be the case. In spite of a great deal of work, thought and ingenious manipulation of data there is still no unequivocal field evidence that our present methods of measuring longevity can and does give reliable clues to the status of transmission in the tropics, and this is where malaria is beating us.

It was therefore with some interest and anticipation that one listened to Mulla discussing the very valuable and fundamental work that he and his colleagues are doing at Riverside. The special merit of this work is that it is aimed to prevent the destruction of non-target organisms, a formidable but laudable task. The progress that had been made came as a revelation to many of those present. I gathered from this, and other writings on the wall, that antilarval methods will probably be revived to good effect provided, as someone said, "good sense prevails." It is, of course, well known that WHO through Vector Biology and Control has for many years been supporting a program for the evaluation and testing of new insecticides. This program involves larvicides as well and some pretty potent chemicals have emerged from this well-organized, far reaching and important effort.

The progress as shown by Chapman and Roberts, that has been made in the study of mosquito parasites has been revealing and now for the first time it seems well worthwhile expending further efforts in this connection especially when so-called "integrated-control," which means fighting with all your armour, is becoming the watch word.

There is, of course, the ago old custom of hanging a bag containing a clove of garlic round one's neck to keep unpleasant beasties and poltergeists at bay. The habit has survived to modern times, comparatively speaking, because I walked around with such a bag as a child. The question of repellents

either for tropical application or systemic use is being rigorously investigated by Smith and his team. It seems that the ones tested so far have not led to any real breakthroughs and so far in the words of Smith "The prospect of developing a systemic repellent seems remote." It seems that no further progress can be expected from the present class of compounds and that a program such as the WHO one on insecticides is required to speed things up.

The aspect dealt with by Kitzmiller is strictly "a la mode" these days and will no doubt interest many. There are two examples of genetic control of field populations that are generally trotted out and exhibited to the admiring and thirsting throng. The first is the eradication campaign against the screw-worm which is always heralded as a resounding success. Indeed someone at this meeting said that the screw-worm had been eradicated! A few statistics from the recent report of the USDA may have a sobering effect in this connection. After six years of eradication, which started in 1962, the 1968 season was the worst for outbreaks in the USA. During 1968 alone 2 billion 942 million 889 thousand sterile males were released in the USA and 4 billion 221 million 947 thousand over Mexico. In spite of all this an even worse outbreak is forecast for 1969. I do not draw attention to this very admirable and worthwhile experiment in order to criticize it or to stop the competition for "wild females," an activity which I fully endorse, but to illustrate the fact that genetic control is not just the simple answer that many people have been looking for. Nor is it the grand panacea that will relieve us and our masters of all bothersome details, expense and hard work. This was well illustrated by several speakers. The sole experiment with mosquitoes that I know of related to that carried out by Laven in a small isolated village in Burma. There an incompatible strain of Culex quinquefasciatus was released and the local population was apparently eradicated. This was a small scale experiment and I don't know if it was followed up. Whether the technical difficulties and expense involved in a large scheme of this sort are feasible and practicable remains to be seen. Let us hope it is not undertaken until every facet is thoroughly exposed, recorded and assessed. At this meeting Davidson very kindly gave us his experiences with A. gambiae in an experiment in Upper Volta. It is much too soon to pass any kind of judgment except that every probe leads to information which is what is wanted.

The method obviously has many advantages over more conventional ones but much further work certainly remains to be done before we can let mis-directed enthusiasm run amuck.

I shall leave the item on computer simulations by Conway severely alone because of ignorance of the subject but his presentation has again served to show how small our knowledge of mosquito population dynamics is. This is the age of the computer and, as usual with a new tool, it has served to reveal our basic ignorance just as DDT did in the past. This is happening all the time. Actually I often wondered what the devil we, and I am speaking for my generation of medical entomologists, have been doing all this time.

When one considers the difficulties that mosquito control and eradication is faced with these days - and the recent collapse of the large and expensive

Aedes aegypti eradication campaign bears witness here - then every straw must be grasped at and every avenue explored. Craig gave much food for thought in his excellent discourse on the new vistas, the exciting new world represented by the fascinating, if somewhat Machiavellian procedures inherent in hormone control. The elixir of eternal youth appears to be within the grasp of the insect world. What about us, George?

I don't suppose that there is anyone who will not have noted that most of the real progress in the understanding of the entomology of malaria has been made by the well established, funded and staffed project with an assured future. Most of us have experience of the ill-prepared, hit and miss type of undertaking which rushes out to Bongo-Bongo, does its thing and then retreats to a more salubrious climate to lick its wounds and prepare for the next predoomed assault. The itch to go out and try something to see if it might work is, we hope, on the way out but it is a longtime dying! Unfortunately entomology is still the Cinderella and in spite of minimal progress in the costly search for new drugs and other panacea the situation is hardly likely to change until Prince Charming gets a good push in the back. If this Conference has lent any kind of weight to this effort it will have been well worth it.

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ACTIVITIES

The Classification Society

The Classification Society, founded in Great Britain in 1964, has as its main purpose the promotion of cooperation and interchange of views and information among those interested in the principles and practice of pattern recognition and classification in any discipline that uses them. As a result, its membership includes anthropologists, biologists, computer and information specialists, geologists, librarians, linguists, psychologists, soil scientists and others.

The Society seeks to provide unique services to its members. These include symposia on classification that are not discipline-constrained and a project under consideration that will result in a bibliography of articles dealing with the theoretical and applied aspects of classification. Supplements to the original bibliography would be issued periodically.

Business of the Society is conducted by a Committee elected by the membership. The Society recently organized into two branches, The European Branch and The North American Branch. Other branches will be organized as the need arises. Current membership numbers around 300, divided equally between the two branches. Annual dues are US \$3.00 and entitle members to receive copies of the Bulletin of the Society, which contains contributions of both a formal and informal nature. Membership applications may be obtained from The Secretary, Doctor Theodore J. Crovello, Department of Biology, The University of Notre Dame, Notre Dame, Indiana 46556, or from Doctor A. J. Willmott, Department of Computation, University of York, Heslington, York, England.