

"STUDIES ON SCHISTOSOMIASIS IN PORTO RICO"

A REPLY

In respect to the criticisms by Dr. González Martínez in the preceding article we wish to make the following comments:

(1) Regret has already been expressed by one of us (Hoffman, this REVIEW 1928, III, 366) at the failure to give credit to Dr. González Martínez for the first observations on schistosomiasis in Porto Rico. Unfamiliarity with local medical history and the fact that the original paper of Dr. González Martínez was a privately circulated monograph, contributed to the oversight. It may be added that while both our papers were presented before the Medical Association of Porto Rico last December it was not until some weeks later that our attention was called to the error just referred to. A correction was thereupon published.

(2) Dr. González Martínez evidently believes the co-existence of *S. mansoni* and *Planorbis guadelupensis* in the same territory constitutes proof that the latter is the intermediate host of the former. We maintain that this is not true. The reports of various workers show that several species of *Planorbis* and at least one species of *Bulinus* and one of *Physopsis* may also be hosts of *S. mansoni*, and that a similar variation in host is seen in *S. hematobium* in different localities. It is known that even a particular species of *Planorbis* may vary from region to region, giving rise to varieties or sub-species. (According to Bartsch,—personal communication,—we have in Porto Rico such a sub-species.) The co-existence, just referred to, may be presumptive evidence of a host-parasite relationship, but not proof. To prove the relation infected snails should be demonstrated and the cercariae definitely identified as those of *S. mansoni*, including experimental infection of animals. Such a demonstration has been made by one of us (Hoffman).

(3) In his criticism Dr. González Martínez states for the first time, so far as we can find through a reading of his available papers, that he discovered infected snails in Porto Rico, but even now he submits no evidence that the cercariae found were those of *S. mansoni*. Marín of our laboratory has recently shown (this REVIEW, 1928, III, 397) that *P. guadelupensis* in Porto Rico is the intermediate host of at least three trematodes in addition to *S. mansoni*, and that the cercaria of one of these very closely resembles that of *S. mansoni*.

It may be added that the existence of *P. guadelupensis* in Porto Rico was noted by Martens in 1873, on whose observations, we gather, is based the statement of Dr. Iturbe, respecting Porto Rican snails, which Dr. González Martínez quotes. The quoted statement of Brumpt also apparently refers to Marten's systematic study of molluses, which was made with no reference to parasite relationships.

(4) Comment is made on our (Hoffman's) negative report as respects infected snails in the Mayagüez district. As stated in the report only one locality (Mayagüez Sugar Co.) was investigated. While "infected snails" were found the cercariae proved not to be those of *S. mansoni*, but of two other species. It is quite possible that a further investigation may show *S. mansoni* in snails of that general region, thus explaining the clinical observations and autopsy findings on cases from Mayagüez.

It will probably take several years to complete our survey of schistosomiasis in Porto Rico. That the task is not a simple one is indicated by the fact that more than twenty years after the discovery of the disease on the Island the distribution was only vaguely outlined with but few recognized foci.

(5) A discussion of the pathogenesis of the lesions found in schistosomiasis was purposely omitted from our (Lambert's) preliminary report on autopsy findings, with the idea of taking up the question in a more detailed paper to follow. This is still our intention, but we may say here that in a study of additional human autopsy material, making eighteen cases in all, we have found no support for the theory that a specific toxin is the cause, or one of the causes, of the characteristic visceral lesions, or that the worms themselves produce any serious damage. These lesions we find constantly associated with ova, the presence of which with their products of disintegration, and in certain cases, a secondary bacterial infection, seem adequate to explain the anatomical changes. Even should a specific toxin derived from the worm be found,—a demonstration not yet made,—it will remain to show its importance in the production of the significant tissue changes, chiefly in the liver and intestine, that are clearly responsible for the common clinical picture in human *S. mansoni* infections. The burden of proof in this matter is on those who maintain a double or triple etiology for these lesions.

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EDITORIAL NOTE

The Policy of this Review: In this same number we publish a contribution from Dr. González Martínez entitled: "Studies on Schistosomiasis in Porto Rico". His paper refers to those of Dr. Lambert and Dr. Hoffman which appeared in the PORTO RICO REVIEW OF PUBLIC HEALTH AND TROPICAL MEDICINE, Volume 3, No. 6. We also publish comments on Dr. González Martínez's article by Drs. Lambert and Hoffman of the School of Tropical Medicine. The editor realizes that both contributions appear in rather a controversial mood which up to now has been foreign to the policy of this REVIEW, and he wishes to state that this should not be taken as a precedent marking a change in our well-established policy the aims of which are to furnish scientific information to our readers in the most objective manner possible. In our temporary departure from the above-stated procedure we have been influenced by the fact that one of the papers giving rise to Dr. González Martínez's criticisms was written by one of our associate editors, who personally has expressed not only his willingness but his eagerness that said criticism be published.