the distribution of these arts the same searching technique is perceptible and the complete reconstruction of the historic event which Dr. Goldenweiser justly demands is already forthcoming. Especially is it to be observed that this is the case in the assertion of independent development in Egypt of both these practises, a proof, the possibility of which Dr. Goldenweiser apparently denies. But indeed if, as on Dr. Goldenweiser's own statement, all the proof that we have is in favor of diffusion, may we not at least with equal right transpose one of his sentences and say, "In all cases diffusion must be assumed until independent development is proved or, at least, made overwhelmingly probable"?

If such striking similarities, parallelisms, convergences in the working of the human mind really do occur, why, in the words of Mr. Means, should there be no such thing as a wheeled vehicle in all pre-Columbian America? Mr. Means's difficulties over wheels and ships are precisely those which the supporters of independent development should hasten to explain. As a matter of fact, as most recently Dr. Rivers has demonstrated, it is the useful art which frequently is lost in the spread of culture. The human mind is not the logically working instrument, leaping at once to full conception of the connection between cause and effect, between possibility and use, which we are invited to assume. In the geographical distribution of culture whatever has been merely useful tends to disappear; whatever is bound to the consciousness of the individual through some link of superstition or religion tends to be retained, though its significance may be misunderstood or indeed even reversed.

It is true, as Mr. Means hints, that so far no comprehensive and detailed analysis has been made of the physical anthropology of the American peoples comparable with that undertaken by Professor Elliot Smith and his associates upon the ancient Egyptians. It is to be hoped that we may be able to make the lack good in time. But the impress left upon the features and the impetus given to the arts and crafts alike of the ancient Egyptians by the immigration of alien peoples leads me to suspect that in the bodies of the pre-Columbian Americans themselves we may ultimately find the corroborative evidence of whence American culture came. It may well be that by this method we shall find the arrows in Dr. Elliot Smith's figure correctly placed. But even if, as in fact Professor Elliot Smith believes, inherent difficulties in the work will prevent physical anthropological studies in America from bearing the conclusive results obtained from similar researches in Egypt, the case for diffusion, contrary to Mr. Means's conception, is not thereby weakened. In the sturdy nature of its composition the culture-complex is amply strong enough to stand by itself and the possibility that some avenues of approach are closed to us does not necessarily prevent our arrival at definite conclusions along those which are plainly open. Critical ethnologists will, I am sure, judge from the facts themselves.

In conclusion, like one of your contributors, I await with impatience a further monograph from Professor Elliot Smith's fascinating and compelling pen; a monograph which I hear from other sources is to be entitled "The Ancient Mariners."

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## MOSQUITOES AND MAN AGAIN

Without continuing the discussion further than the limits of this paper, it seems advisable to state once more the contention made in my paper "Mosquitoes and Man"[1] for Mr. Jennings in his rather elaborate and erudite criticism[2] of it misses the whole point so completely as to be definitely surprising and almost amusing.

The point was not the "association" of mosquitoes with man, but that the malarial mosquito *followed* man, and while following man is included in the association with man, it is nevertheless a specific point and worthy of some attention.

[1] Science, June 2, 1916.
[2] Science, August 11, 1916.

Major Ashburn's observations were, that in a given place, men and mosquitoes being associated, on the removal of the human element the malarial mosquitoes no longer bred in that locality as before, the larvæ from being numerous became rare, almost, if not quite absent. An instance of this occurred at Miraflores, formerly a hot-bed of malaria, and where *Anopheles albimanus* bred in abundance. When, in connection with the Canal work, the inhabitants were removed, it was presently discovered that although the breeding conditions were quite as good, *A. albimanus* was no longer breeding in that locality as before, the larvæ having become *very* rare. Contrariwise, that when camps were established in new localities where malarial mosquitoes and their larvæ were rare or unknown, both adults and larvæ presently appeared in greatly increased numbers, and this was followed by a malarial outbreak among the men. Major Ashburn has records of some ninety instances where these conditions, in connection with the establishment and abandonment of construction camps, occurred, and it was on this large number of cases that he based his conclusions.

The question of an "animal barrier" is not a question of whether any given mosquito will attack a horse or a cow or a dog, but whether such animals will prove a protective barrier, against the malarial mosquitoes, for human beings living beyond. Whether disease-bearing mosquitoes will breed except near human habitations is another question, and apparently has several factors, so that it is quite possible that it can not be answered by a general statement. However it is quite certain that these mosquitoes would not have become "disease-bearing" if they had not bred near habitations and been in close touch with man.

The experience of many sanitarians has been that, under usual conditions, to keep the breeding places of malarial carriers at a distance of "four hundred yards" is sufficient to protect the inhabitants of a locality from malaria, and Watson shows that the outer coolie lines are at least the only ones attacked under these conditions. This can only mean that the malarial mosquitoes do actually breed near, and not, as Mr. Jennings suggests, "at a distance from human habitations." Also of course this implies the intimate association of malarial mosquitoes and man, and there is nothing in my paper to indicate a lack of recognition of that general condition. It called attention to an entirely new viewpoint, and one that gives a valid reason not only for the usually accepted limit of flight, but to Dr. Watson's observations concerning the outer coolie lines, and even for the long flight recorded at Ancon, while it suggests a hitherto little recognized need of the protection of human beings in the formation of new camps in heretofore uninhabited sections where no malaria has been known, or where the larvæ of malarial mosquitoes are extremely rare or unknown.

It is hardly permissible to assume ignorance, on the part of a Medical Officer and a worker in preventive medicine, of the literature and labors of many investigators whose work was based on the, at least implied, "association" of mosquitoes and man. Even the average layman knows the story of Manson's suggestion to Ross. Especially is such an assumption out of place in regard to Major Ashburn, whose work on the transmission of disease by insects, carried on in the Philippines as a member of the "Board for the Study of Tropical Diseases" is widely known and accepted as one of the authorities on the subject.

It is always better to keep "an open mind" on every subject, scientific or otherwise, and certainly to avoid unfair comments on other workers. There is work enough for all, and the various phases of the study of disease are so complicated as to give every part of the subject many sides, and many points of contact with the labors of special investigators in other branches. That the whole may develop in a well-balanced and scientifically correct fashion requires harmonious interrelation between these various workers, and a just recognition of the viewpoints of others. Mr. Jennings's connection with the work in the investigations in the Canal Zone should have broad-

ened him sufficiently to have made other attitude and action impossible. C. S. Ludlow

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Washington, D. C.,
September 29, 1916

### THE SONG OF FOWLER'S TOAD (BUFO FOWLERI PUTNAM)

In Science for September 29, Mr. H. A. Allard states that for some years he has heard at Clarendon, Va., two types of toad cries. One was uttered early in the spring, "a steady, trilling monotone," lasting "from 10 to 20 seconds," and "resembling the song of *Bufo americanus* as it is heard in New England." The other was that of Fowler's toad, "the unmistakable, weird, wailing scream which advertises its presence throughout its range." He further states that on May 2, 1916, he caught toads uttering the former note, and found them to be *Bufo fowleri*. He presented them to the National Museum, where they are under accession number 59692.

Now I have collected for some years in the region in question, as my home is in Alexandria, and I have found both *B. fowleri* and *B. americanus* fairly common, although *fowleri* seems the more abundant. I have studied the breeding habits of these toads at Haverford, Pa., where both occur very commonly and are quite distinct.

*Americanus* is one of the first Anura to appear in the spring; *fowleri* one of the last. Transformed *americanus* are sometimes met with before *fowleri* begins to sing. The note of *fowleri* there is always the short snoring scream. The note of *americanus* is always much longer, although its trill and its softness are somewhat dependent on whether the toad is on land or in the water. I have collected *fowleri* in numbers at Brevard, N. C., at an altitude of 2,200 feet. The note there was the same which I have heard at Alexandria and at Haverford.

Finally, during the first part of September, I was working in the reptile and amphibian department of the National Museum, and while looking over the catalogue I chanced to see there an entry of *B. fowleri* with the remark that the note was that of *B. americanus*. My interest aroused by this and also by the fact that they were local specimens, I looked them up and examined them. I soon came to the conclusion that they were not *fowleri* at all, but *americanus*. They were much too large for *fowleri*, and they had large warts arranged singly in spots as in *B. americanus*, instead of the small warts, three to five in a spot as in *B. fowleri*. These toads were catalogue number 59692, and were collected by Mr. Allard at Vinson Station, Va., on May 2, 1916. Mr. Allard was probably misled by the fact that they did not have the deeply spotted breast of most *americanus*, but this is not too reliable a character, as some *B. fowleri* have speckled breasts and some *B. americanus* have, as in this instance, immaculate breasts.

Thus there is no reason to believe that Fowler's toad has two distinct notes, and confidence can still be reposed in the calls of toads and frogs as differentiating characters.

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## SCIENTIFIC BOOKS

*Morphology of Invertebrate Types.* By Alexander Petrunkevitch. The Macmillan Company, New York. 1916.

Under this title Professor Petrunkevitch offers us a laboratory guide for representative invertebrate types and, in addition, material of the sort commonly found in our textbooks. "Each chapter consists of two parts: a monograph in which a description is given of the animal selected as representative of its class and instructions for the students to follow in dissection." The purpose of the former is to give the student an account of the morphology of his type form to which he may refer throughout his dissection and to give the teacher more freedom, since the lectures are thus relieved of much detail. The book is frankly morphological, as its name implies, and the author makes no apology for this; but rather contends in his preface that the student who aspires to the work of experimental zoology is often hampered by "a superficial knowledge of the structure, life and development of those very animals which in his later studies