

capable and efficient officer, if he were afforded the proper facilities for carrying out the proper functions of Government Entomologist.

F. M. WEBSTER.

SCIENTIFIC JOURNALS AND ARTICLES.

The American Naturalist for September begins with 'A Contribution to the Morphology and Development of *Corymorpha pendula* Ag.' by Albert J. May. This includes a study of the origin of the sex cells and of the phenomena associated with oögenesis. J. Arthur Harris has a paper on 'The Habits of *Cambarus*' which contains many interesting observations on the burrowing habits of some species and their 'chimney building.' Max Morse contributes the nineteenth of the 'Synopsis of North American Invertebrates,' this being devoted to the Trichodictidæ, forming a monograph of the North American species. The balance of the number comprises reviews and notes, the botanical notes being many in number.

With the October issue *The American Museum Journal* begins its appearance as a quarterly. The number is practically devoted to an account of 'The Jesup North Pacific Expedition' accompanied by maps and illustrations. The supplement forms 'Guide Leaflet No. 12,' and in its thirty-two pages W. D. Matthew describes 'The Collection of Fossil Vertebrates' which has recently been rearranged. This Leaflet contains many illustrations and a large amount of information; it should be in great demand by others than museum visitors.

SOCIETIES AND ACADEMIES.

ONONDAGA ACADEMY OF SCIENCE.

THE first meeting of the Academy since the summer vacation was called to order by the president, Dr. Kraus, in the rooms of the Historical Society in Syracuse on September 25, 1903. P. F. Schneider presented a paper on 'Mica Prospects in Northern Georgia.' He gave a description of the area in which the mica occurs, of the mica-feldspar, pegmatite dikes in which it occurs, and considered

the conditions favoring the further development, such as the water power, cost of labor, etc. He closed with a statement of the different uses of mica. Mica has been produced in limited quantities in northern Georgia in years past and Mr. Schneider concludes that the surface indications justify further development and an increased output.

T. C. HOPKINS,

Corresponding Secretary.

DISCUSSION AND CORRESPONDENCE.

THE ANIMAL PARASITE SUPPOSED TO BE THE CAUSE OF YELLOW FEVER.

My connection with the Working Party No. 1 of the Yellow Fever Institute and the basis on which I rest my claim as being the *first to have interpreted correctly and given value to the things found in the bodies of the mosquitoes infected from yellow fever patients.*

Working Party No. 1 of the Yellow Fever Institute (a bureau of the U. S. Marine Hospital Service), consisting of Dr. Herman B. Parker, P.A., surgeon and chairman, Washington, D. C., Professor G. E. Beyer, biologist at Tulane University, and Dr. O. L. Pothier, pathologist, Charity Hospital, New Orleans, reports in Bulletin No. 13 of the Institute the results of its labors in Vera Cruz during the summer of 1902.

Section 6 of this bulletin contains the description of an animal parasite which was found in the bodies of mosquitoes infected from yellow fever patients.

In the letter of transmittal the following sentence occurs:

In the proper study and classification of this new parasite the Working Party desires to express its thanks to Mr. J. C. Smith, of New Orleans, La., for valuable aid and suggestions in working out the life-history of the organism.

I claim that the above recognition is not commensurate with the services I rendered to the party. That it was not 'aid and suggestions' that the party received from me, but that it was given the pith of the whole matter included in the section entitled: 'The Contaminated *Stegomyia fasciata* and its Parasite,' as I will show further on.

The members of the party had all returned from Vera Cruz about October 1, 1902.

Professor Beyer had solicited, a number of times, my assistance in working up the material he had prepared, consisting of slides of infected mosquitoes.

On January 23, 1903—*fifteen weeks after the return* of the members from Mexico—I undertook the examination of their material. Up to this time, Professor Beyer, who was the biologist of the party, knew of no evidence of a parasite in these mosquitoes excepting some 'granular bodies,' as they were styled, which were found in the cells of the salivary glands, and which I afterwards showed the party were not 'granular bodies,' but were *linear* bodies, five or six times longer than wide—the sporozoites.

On January 30 I reported having found in the bodies of a number of the mosquitoes an animal parasite in process of sexual development. This report was accompanied by my sketch of all the processes.

According to Professor Beyer's statement to me at the time, Dr. Parker was urged to come to New Orleans in order to see the results of my investigations. The party was then ordered to convene in New Orleans. Dr. Parker arrived on or about the tenth of February, and a very short time after his arrival he and Professor Beyer called on me. They were both shown my sketch of the parasite in its various stages and given by me a history of these stages. Dr. Parker thanked me for my services and expressed his desire to see the parasite. He examined the sketch very carefully and did not say that he had ever recognized any of the stages depicted in the sketch.

On or about February 12 the members had their first session and heard my report read. At this session and the two following I was present, and, singly and alone, demonstrated step by step the parasite and its gradual development from the third to the fifteenth day after infection.

On showing the spores in the tissues surrounding the salivary glands (wandering spores), Dr. Parker remarked that he had a photograph showing this phase. He then opened a package of well-executed photographs

which he had brought from Washington, and found one showing these spores. There were about fifteen photographs in the package and after all had been examined carefully *no trace of any other phase of the parasite could be found*. As Dr. Parker knew the object of his coming to New Orleans, it is perfectly natural to conclude *that if he had had any other photographs showing the parasite, he would have brought them with him*.

At no time during these sessions did any member of the party signify by word or action that any of the phases which were being shown them had been seen before. On the contrary, the difficulty which I had in getting them to see and comprehend some of the phases was conclusive evidence that they were seeing these things for the first time.

At the conclusion of the third session I said to the members that I expected to be given full recognition for my services and this recognition must be placed *in the text treating on the parasite*, and not in the letter of transmittal.

Dr. Parker, speaking for and before the members, again thanked me for my work and promised that I should be given the recognition asked, and that it would be placed in the text treating the parasite.

At the fourth and final session—to which I was not invited and did not attend—the history of the parasite was incorporated in the final report of the working party. Dr. Parker and Professor Beyer had written this history so that it was a copy of my report, to which Dr. Pothier objected and advised that it be written in their own way so as not to appear as a duplicate of my report. This advice prevailed and the history was written as it now appears in Bulletin No. 13.

At this session Dr. Parker and Professor Beyer, notwithstanding their promise to me the day before, declined to recognize my services. Dr. Pothier was indignant and refused to sign the report unless justice were done me. A recognition was then placed *in the text treating of the parasite* and the report was signed by the three members. On the return of Dr. Parker to Washington this recognition *was suppressed*, as will be shown

further on by Surgeon-General Wyman's letter to me dated August 24.

All that I have related above concerning the four sessions can be corroborated by Dr. O. L. Pothier, who was a member of the working party and attended the sessions.

Some days after this fourth session I tried to learn from Professor Beyer what had been done in the matter of recognition of my services, but could get no satisfactory answer from him. I then told him that I intended to criticize the report when it was published, and that the party had failed to get one very important characteristic of the parasite. That characteristic is that all the gametes have a non-contractile vacuole in their anterior halves. If the reader will look over the report, he will find that this very important feature is not mentioned.

Several days after this interview with Professor Beyer, I received the following letter from Dr. Parker:

TREASURY DEPARTMENT,
MARINE-HOSPITAL SERVICE,
WASHINGTON, D. C., Mch. 6, 1903.

MR. J. C. SMITH,
New Orleans, La.

My Dear Mr. Smith: Since returning to Washington I have been intending to write to you and ask if you will transmit to me at your earliest convenience your opinion of the organism and its phases as you saw them with Dr. Beyer.

I would like this not only for my own information, but if necessary to quote you on points that at present may seem somewhat hazy.

The subject is, as you know, a rather large one, especially for those who are not familiar with more than the rudiments of this branch of the sciences. *I hope, however, to acquire enough from my friends and the books to make this organism presentable.*

Very truly yours,
(Signed) HERMAN B. PARKER,
Passed Assistant Surgeon.

(The italics are mine.)

To which the following reply was sent:

NEW ORLEANS, LA.,
March 10, 1903.

DR. HERMAN B. PARKER,
P. A. SURG. MARINE-HOSPITAL SERVICE,
WASHINGTON, D. C.

My Dear Dr. Parker: Yours of the 6th inst. to hand and from its contents I conclude something

has gone wrong * * *. It seems to me that I am not going to receive from your commission (working party), the recognition which I justly claim, so I have concluded to *reserve what I have to say about the parasite and the part I have taken to bring it to light until the report is published.* If I am mistaken as to the treatment your commission (working party) contemplates according me, I shall say no more about it, but you will have to assure me of my mistake and say just *how* I am to be recognized before I can feel satisfied to give you my written views as you request in your letter of the 6th inst.

Yours truly,
(Signed) J. C. SMITH.

To which no reply has been received.

On May 6, during the session of the American Medical Association in New Orleans, I had a conference with Surgeon-General Wyman. At this conference I related the story of my connection with the working party, which was substantially as it is written above, and said to him that I had good reasons to believe that I was not to receive the recognition I was entitled to. In response to his question as to what recognition I wanted, I handed him the following memorandum:

The commission is indebted to Mr. J. C. Smith, of New Orleans, La., for his valuable services in working out the sexual life-history of the parasite in the body of the mosquito.

This to be in the text treating on the parasite.

He said that was the first time he had heard of my connection with the working party; that the party had no right to go outside the department for assistance; that he would investigate the matter and would do justice to me.

On June 10 Dr. Wertenbaker, surgeon at the Marine Hospital in New Orleans, called on me and presented a telegram from Surgeon-General Walter Wyman which read as follows:

See J. C. Smith and get from him a signed statement of his relation to the working party and his understanding as to recognition.

On the same day Dr. Wertenbaker was given the signed statement, which contained the same items recorded above, and the fol-

lowing as 'my understanding as to recognition':

The commission is indebted to Mr. J. C. Smith, of New Orleans, La., for his valuable services in working out the sexual life-history of the parasite in the body of the mosquito.

This to be in the text treating on the parasite.

On the same date as above Dr. Wertenbaker presented a somewhat similar telegram from Surgeon-General Wyman to Dr. O. L. Pothier, whose reply was as follows:

NEW ORLEANS, June 10, 1903.

DR. WERTENBAKER,

SURGEON P. H. & M. H. SERVICE,

NEW ORLEANS, LA.

Dear Sir: In compliance with the request made through you by Surgeon-General Wyman, I wish to make the following statement:

First. When I signed the report it contained what I believed to be full recognition of the work done by Mr. J. C. Smith.

Second. The paragraph embodying this read as follows: In the proper study and classification of this new parasite the working party desires to express its thanks to Mr. J. C. Smith for valuable aid and suggestion in working out the life-history of the organism. After the words 'J. C. Smith,' it was agreed that the words 'for the demonstration and' should be embodied, but I find that these words have been left out in my copy of the report.

Third. I positively refused to sign the report unless recognition was given Mr. J. C. Smith.

As to other relevant data, I may say that Mr. J. C. Smith was present at all the meetings of the commission here in New Orleans, except the meeting preceding the signature of the report. At this meeting the discussion of the report came up and it was that night that I refused to sign the report unless Mr. Smith was given recognition, and it was the only consideration which induced me to sign the report.

We had seen, while still in Vera Cruz, two phases of the organism described in the report, one phase proved to be what is described further as the wandering spores of the parasites and the other bodies in the salivary glands, but it was not until Mr. J. C. Smith had been called by Professor Beyer, without authorization of any of the working party, so far as I know, that the whole life cycle of the organism was demonstrated.

And at the meetings of the commission here in New Orleans, Mr. Smith fully demonstrated the life cycle of the parasite.

I am fully convinced that had he not done so at the time, that the parasite would not have been demonstrated by the commission, as none of us could at that time find the different phases of the cycle without the help of Mr. Smith.

It was on this account that I refused to sign the report as no mention of Mr. Smith was made, and as I said above it was conditional on the embodiment of this recognition that I consented to sign the report. This paragraph should read as the original was intended as follows:

"In the proper study and classification of this new parasite the working party desires to express its thanks to Mr. J. C. Smith for the demonstration, and for valuable aid and suggestions in working out the life-history of the organism."

Further, I would state that it was agreed that the galley sheets were to be sent to each one of the party for correction and approval. This latter agreement was not carried out, and I have not seen proofs of the report up to this date.

In closing, I wish to state that two or three days before the receipt of the telegram convening the commission here in New Orleans, Mr. Smith sent me a full diagram of the life-cycle of the organism in question.

I think I have given all the information that I possess on this subject and hope that it may prove satisfactory.

Yours truly,

(Signed) O. L. POTHIER, M.D.

The report of the working party is dated New Orleans, February 17, 1903, but was not issued until about the fifteenth of July, at which time a copy came to my hands. Not being satisfied with the recognition in the report, nor with its position, I addressed a protest to Surgeon-General Wyman as follows:

NEW ORLEANS, August 17, 1903.

GENERAL WALTER WYMAN,

WASHINGTON, D. C.

My Dear Sir: I regret very much the necessity of entering my protest against the style and position of the recognition of my services which the Working Party No. 1 of your Yellow Fever Institute has accorded me. My request, as I had given it to you upon your solicitation, was for 'valuable services in working out the sexual life-history of the parasite in the body of the mosquito'—'this to be in the text treating on the parasite.'

In the report of the working party, recently issued, I find I am thanked for 'valuable aid and

suggestions in working out the life-history of the organism,' which may be construed as meaning very little, and this is placed in the letter of transmittal, where I consider it is buried.

When I gave into your possession the evidences on which I based my claim to recognition, you made no mention of any existing evidence that might tend to modify my claim. If any evidence came to your knowledge since our conference last May which led you to consider that I was claiming too much, I maintain that it was your duty, before taking final action, to notify me of this evidence and of its nature, in order to afford me the opportunity of rebutting it, if possible. As you have not done so, but have modified my recognition very radically, you have left but one channel open for me to obtain proper recognition for my services, and that is to appeal to the fair judgment of the scientific world.

Yours truly,

(Signed) J. C. SMITH.

To which the following reply was received:

TREASURY DEPARTMENT,
BUREAU OF PUBLIC HEALTH AND MARINE
HOSPITAL SERVICE,

WASHINGTON, D. C., August 24, 1903.

MR. J. C. SMITH,

131 CARONDELET ST., NEW ORLEANS, LA.

My Dear Sir: I have to acknowledge receipt of your letter of August 17, entering protest against the style and position of the recognition of your services which the Working Party No. 1 of the Yellow Fever Institute has accorded you in its report.

I regret that you seem to have some misapprehension concerning the matter. Before I reached New Orleans in attendance on the meeting of the American Medical Association in May last I had no knowledge whatever of your relations with the working party. I then learned for the first time that you had met with them and that a definite promise had been made to you as to recognition, and that it was believed that no such recognition had been inserted in the report.

The report was already in print and after your call upon me, on consulting an advance copy in my possession, I found that it was true that there was no recognition of you in it. I thereupon caused the issue of the publication to be suspended to permit inquiry into the matter. I found that the recognition had been promised and determined that the promise should be kept. This involved a board of inquiry and as a result the recognition was restored in the report though

it required an alteration in the printer's form to do so.

The report was then published and issued, and as published contains the recognition of your services in the very terms in which it was promised to you and agreed upon by the three members of the working party.

Respectfully

(Signed) WALTER WYMAN,
Surgeon General.

(The italics are mine.)

To which the following reply, concluding the correspondence, was sent:

NEW ORLEANS, LA.,

September 1, 1903.

GENERAL WALTER WYMAN,

WASHINGTON, D. C.

My Dear Sir: In reply to yours of August 24 I will say that on my part there is not the least 'misapprehension concerning the matter.' Whether the acknowledgment now in the report is, or is not, the same as was placed there originally and afterwards suppressed, does not concern me, as I was not afforded the opportunity of judging if it were commensurate with the services I had rendered the working party.

Further, Dr. Pothier, a member of the working party, has just assured me that when he signed the report the acknowledgment then accorded me *was placed in the text treating on the parasite*, so you will note that the suppressed acknowledgment was not even restored as it was originally.

That I had made proper effort to learn from the chairman of the working party if I were to be acknowledged for my services, and in what terms this acknowledgment was to be, you know from my reply, dated March 10, to Dr. Parker's letter dated March 6; that I failed, you also know from the fact of Dr. Parker treating my request for this information with silence.

I hold that the form of recognition which should have been considered was that which emanated from me, and which I gave you upon your own request. I also hold that if it were thought that I was claiming too much, the proper and just thing to have done was to have afforded me the opportunity of strengthening, by witnesses, what was related in my brief given to you.

In conclusion, I will say that I consider that I have been treated unfairly in this whole business, and as I am actuated by the commendable desire to be permanently connected with the results of my work on the parasite, I shall, without un-

necessary delay, appeal to all scientific men and let each judge as to the justice of my claim—as being the first to correctly interpret and give value to the things seen in the bodies of the infected mosquitoes.

Yours truly,

(Signed) J. C. SMITH.

In conclusion, I will say that I am willing to rest my case on the facts related, without analysis or argument, and leave it to the judgment of my readers if I am not entitled to the recognition I claimed at the hands of Surgeon-General Wyman and the party. That in the text treating on the parasite the following acknowledgment be placed:

“That the commission (or working party) is indebted to Mr. J. C. Smith, of New Orleans, La., for his valuable services in working out the sexual life-history of the parasite in the body of the mosquito.”

J. C. SMITH.

NEW ORLEANS, LA.,
September 24, 1903.

SOME RECENT APPLICATIONS OF THE A. O. U. CODE.

TO THE EDITOR OF SCIENCE: It is desirable that there should be public discussion of disputed cases in nomenclature, for in no other way can the weak points in codes, or in their application, be so well brought out. For this reason SCIENCE appears to be the proper place for the following criticism.

Dr. Stejneger (*Proc. U. S. Nat. Mus.*, 1903, p. 152) sets aside the specific portion of *Coronella sayi* Holbr. (Ed. 2, Vol. III., 1842), for sixty years used for Say's King-snake, now *Ophibolus getulus sayi*, for the reason that Holbrook was under the misapprehension that his species was identical with *Coluber sayi* Schlegel (1837), which is now *Pityophis sayi*, a pine snake, and included that name in his list of references. In consequence of which, a new specific name, *holbrookii* Stej. takes its place. The rule relied on for the change is Canon XXXIII. of the A. O. U. Code, which provides that ‘a specific * * * name is to be changed when it has been * * * used previously in combination with the same generic name.’ It is quite true that in this place Holbrook does cite Schlegel's name

among his references, and in that was clearly wrong. But as he puts his species in another genus, it does not appear that the application of this rule is so clear as to be compulsory. Curiously enough, however, a better ground exists upon which the vacating of Holbrook's name might be urged under the rule. Holbrook's first description of the species was under the name *Coluber sayi* Dekay, in the lately discovered fourth volume of his first edition (1840), now in the Academy's library. The only reference given here is ‘Dekay mss.’; Schlegel not being mentioned. For some unknown reason Holbrook tried to suppress this volume, and in his second edition he gave the same description and plate under the name *Coronella sayi* Schlegel.

It is of course true that by a strict construction, which is usually a narrow one, the rule quoted might be applied here, Holbrook having first used *sayi* in connection with the generic name used by Schlegel, but the fact remains that Holbrook was indisputably the first to describe publicly and name the species from original specimens, and that he subsequently placed it in a different genus where it is still retained by high authority among herpetologists, his only error being in assuming that Schlegel's species was the same as his. If, under the A. O. U. Code, there is no escape from applying the rule here, then it is one of the cases where the code conflicts with justice and common sense.

While I am on the subject I may mention also Cope's substitution (*Proc. U. S. Nat. Mus.*, 1888, p. 392) of *Natrix* Laurenti (1768) for the well-known *Tropidonotus* Kuhl (1826), on the ground that while *Natrix* was a heterogeneous collection of species, *Natrix vulgaris*, which is a *Tropidonotus*, was its type. Here we have a method of determining types which leads to the absurdity of placing a group of snakes with keeled and conspicuously rough scales in a genus whose author among its definitions expressly says ‘*Truncus glaber, nitidus.*’

ARTHUR ERWIN BROWN.

ACADEMY OF NATURAL SCIENCES,
PHILADELPHIA, August 5, 1903.