

SMITHSONIAN INSTITUTION  
UNITED STATES NATIONAL MUSEUM  
WASHINGTON, D. C.

Bucaramanga, Colombia, June 19, 1949.

F.L. Werneck,  
Instituto Oswaldo Cruz,  
Rio de Janeiro, Brasil,

My Dear Doctor Werneck:

I have just returned to Bucaramanga after six months spent in the field, collecting birds for the Museum. The work was done in the northwestern portion of Colombia, all in low, hot country, and by the time I had finished I was quite ready to take a short vacation and rest.

The trip was very successful from an Ornithological standpoint, about 2,000 specimens being taken including almost 400 species, many of rare and interesting. Unfortunately not much time could be spent in the collection of Mallophaga, although I did get some material which may prove interesting. I find that most of the passerine birds harbor few or no Mallophaga, and what they do have are uninteresting, belonging usually to Philoaterus, or closely allied genera.

I received your kind favor of January 29th., forwarded to me in the field, but waited until my return before answering so that I could check on the papers which had arrived in Bucaramanga, since no second class mail was forwarded.

I am happy to report the receipt of the following separates from you which arrived during my absence:

- Os Malófagos de Mamíferos, - Part I, 1948.
- "Neohaematopinus longus" N.SP. - 1948
- Os Malófagos de "Cervus elaphus", "Dama Dama", y Capreolus capreolus", - 1947
- Um Novo Malófago de Antilope, - 1946
- Notas sobre o genero Enderleinellus, - 1948

Thanks very much for your courtesy and thoughtfulness. I especially appreciate your sending me the monograph, and trust that the remainder will appear at an early date.

Although I have never published anything, or made much of a study of the Mallophaga of mammals, I am, nevertheless always glad to have the literature, to which I have occasion to refer quite often.

With best wishes for your good health and continued success, I am

Yours very sincerely,

  
M.A. Carriker, Jr.

P.S. Address for airmail: Carrera 21 No. 33-69, Bucaramanga, Colombia  
" for ordinary mail: Apartado No. 181 " "

M. A. CARRIKER JR.  
FIELD REPRESENTATIVE  
U. S. NATIONAL MUSEUM  
WASHINGTON, D. C.  
U. S. A.

Popayan, Colombia, January 15, 1953.

Fabio Leoni Werneck,  
Caixa Postal 926,  
Rio de Janeiro, Brasil,

Dear Doctor Werneck:

Just a few lines to let you know that I am finally settled, and am again able to continue my work on the Mallophaga. I have retired from active field work and will devote most of my time to my bird lice.

I have just finished checking over very carefully Hopkins & Clay's new List of the Mallophaga of the World. It is, I am sure, a very fine piece of work, and something very badly needed, and the authors are to be congratulated on it. However, I must say that I think that they have been much too conservative in their treatment of the genera, relegating to synonymy a good many perfectly good genera, not only of my own, but of other authors.

I certainly agree with them in their treatment of most of Eichler's genera and most of Keler's, but I think that they are serving no good purpose in lumping a heterogeneous lot of species together and calling it a genus. It is absolutely impossible to characterize some of the genera as they have

now left them. Neither have they been consistent, retaining some genera which are closer to others than some of those which they have synonymized.

It seems to me, after all is said, that systematic classification is merely a means to an end, and an aid in separating the many zoological forms so that they may be recognized. A genus is a purely artificial group in most cases, while their philogenetical relationships are not always clear, and in many cases never can be clear, since so many previously existing forms have become extinct. So why bother in trying to solve so many hypothetical questions. What are, or are not, generic characters, always will be more or less an individual opinion. Everyone's opinion is as good as the other man's, provid-

ing he has sufficient knowledge of the subject. There always have been "lumpers" and "splitters", both of species and genera, and there always will be more. Keler and Eichler, and to a lesser extent Conci, may be classed as "splitters", while now Hopkins and Clay have reversed their former attitude and have become "lumpers". The same thing has been happening with the birds.

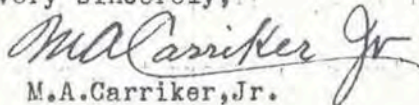
Another unfortunate thing with the new List, is that they have listed the forms of lice all as species, ignoring all subspecific rank, so that unless one has the original description it is impossible to tell what are the relationships whether species or subspecific.

Personally, I cannot follow them in some of their decisions, and do not intend to do so, and I am sure that there are other authors that will think as I do. I should very much like to have your opinion on this matter. I do not like to use "subgenera", and I do not think it is necessary. There are always some species which are very difficult to allocate, having a mixture of characters belonging to two well-recognized genera, but that cannot be helped, and they must be recognized, as such.

Although I have not been able to do any work on mammal Mallophaga, I am always interested in your very fine work with them, and I trust that you may continue it.

Wishing you every success, and hoping to hear from you, I am,

Yours very sincerely,



M.A. Carriker, Jr.

Address for letters:

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~~Apartado No. No.~~

C

Popayan, Colombia, February 23, 1953.

Dr. Fabio Leoni Werneck,  
Rio de Janeiro,  
Brazil,

Dear Doctor Werneck:

Your kind favor of January 28th. arrived while I was spending a couple of weeks in the country, and was received upon my return some ten days ago.

I was very glad to hear from you and enjoyed very much your remarks on the systematics of Mallophaga. After reading your letter I read your remarks in your Monograph on the Mallophaga of mammals, under Trichodectes, and your ideas on this subject seems to be very similiar to my own.

You know, the human mind tends to run in cycles, from one extreme to the opposite, in matters of government, politics, social problems, religion and science, especially in the systematics of Natural Science. I have lived long enough to see the pendulum of change swing back and forth. You know how the Mallophaga were classified in 1900, when Kellogg and Osborn were going strong, and that is the year in which I began my own studies, and had the temerity to describe two new genera from the Tinamous (Ornicholax and Kelloggia). The trinomial system for birds was just coming into use by american Ornithologists, while the British still clung to the ideas of Sharpe, Sclater and Salvin. But the pendulum was then swinging to the right and not long afterwards Ridgway began making new genera for every species of Columba, and Oberholser began his hair-splitting of species. Now, it has swung to the left. Peters has tried to place all of our hawks in Buteo and the ducks in Anas and the Goatsuckers in Caprimulgus.

In the Mallophaga the same thing is taking place. We have had had the breaking up of the old cumbersome genera, which Eichler, Keler and Conci seem to have carried to an unwarranted extreme, while I myself may have had some tendency in that direction. Now Hopkins and Clay, who were the first ones to begin breaking up the old genera, are swinging back to the opposite extreme. It is a great pity that they cannot keep to the middle and not run to such extremes.

I admit, the same as you, that the separation of species into genera is an extremely difficult problem, if properly done. However, it seems to me that instead of running off at extreme tangents relating to ancestral types, ancestral characters, evolutionary characters and adoptive characters, that we should use a little more "common sense" in the matter. At best, the genus is an artificial group, a convenience in the classification of innumerable specific forms. It seems to me that a genus should have some character, or characters, which are outstanding and which would enable one to recognize it easily, such as the head spines in Menacanthus; the "aster" of abdominal spines in Myrsidea, dimorphic antennae, etc. etc. Any group of species which contains so many different types that it cannot be characterized certainly cannot be called a genus, and serves no useful purpose. There are always aberrant species, and there are always species which seem to form connecting links between two genera, but it does not seem to me that because this is true, that those two genera must necessarily be combined. I think that such aberrant species should be placed in the genus which they most nearly resemble, and mention made of their conflicting characters. There will always be some monotypic genera, but there should not be too many of such. Undoubtably many species and genera of Mallophaga which formerly existed have become extinct, either due to the disappearance of their host or some other reasons which we do not know. Therefore there must be some "relict" species still existing, possibly all that remains of their particular type of louse, and such species must necessarily be placed in monotypic genera. When I described the genus Trichodomedia (Goniidine forms found on american Cracidae and Odontophorus) I had a wealth of material, and all of the species placed in that genus formed an unusually compact, homogenic group. Yet, in spite of this Clay and Hopkins insist on merging this genus with Chelopistes which contains species from all manner of Old World Galliformes and which can easily be separated from Trichodomedia. Clay first erected a genus for

the whole group which she called Virgula (but antedated by Chelopistes Keler) and used as the genotype the Goniodyne louse of the American Turkey, a bird very distinct from the Cracidae, and I do not consider that the species ~~x~~ from it, used by Clay as the genotype of Virgula to be conspecific with the species I placed under Trichodomeia.

The Turkey belongs to a separate family (Meleagridae) and may well be the last of a group of birds which were common in some prehistoric age, and there is no particular reason why it should not have a louse generically distinct from the Cracidae or the old World Galliformes.

There are a number of other workers in Mallophaga which think as I do in this matter, Emerson, for one, and I believe that Guimaraes also leans towards my way of thinking.

As for Clay and Hopkins, especially Clay, the end is not yet in sight. In a letter recently received from Clay she says: "I am once more trying to make some sense out of the Ischnoceran genera, but find it very difficult. We shall have to sink many more I think before we are finished, and in the end approach the condition of the earlier authors." This attitude on her part I simply cannot understand. It is like wanting to return to the "horse and buggy" days.

Well, I shall not tire you further with my remarks. At least you probably get some idea of what I am trying to express. I do not want to swing with the pendulum to any extreme, I only want to use a little common sense, together with the fundamentals of scientific systematics, and I think it too bad that we have to put up with so much cluttering up of the synonymy of the Mallophaga. Also, I think it a great misfortune that Clay and Hopkins, after having done such a tremendous amount of very useful work, should now swing so far to the opposite point from which they originally started.

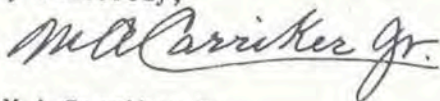
Another very great error committed in the new List (in my opinion) is the listing of all of the Mallophaga under binomials, when so many of them were originally described as trinomials. It confuses the whole picture and causes one additional work in looking up the original name. I cannot understand why they did this. Certainly it could not have been just to save a few pounds sterling on the cost of publication.

Regarding your papers, the last I received was the first volume of the Monograph published in 1948. I have nothing of yours published after that, and would certainly be glad to receive any others, if still available.

I hope to soon begin publishing papers frequently, just as rapidly as I can prepare the drawings and MS. The drawings take the most time, but I have nobody to assist me in that work. Fortunately I am able to do them quite rapidly, from long practice.

Hoping to hear from you from time to time, and wishing you all success in your work, I am,

Yours very sincerely,



M.A. Carriker, Jr.

Permanent address:

Apartado Nacional No. 82,  
Popayan.

P.S. I was pleased to learn that you had been in Popayan, and that you had been so favorably impressed by it. I have been here three months now, and like it better all the time, and am well satisfied with my choice.

Popayan, Colombia, May 16, 1953.

Dr. Fabio Leoni Werneck,  
Caixa Postal No. 926,  
Rio de Janeiro, D.F., Brasil,

Dear Doctor Werneck:

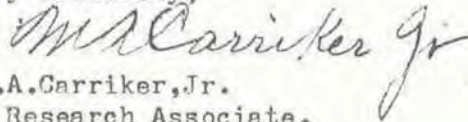
Just a line to let you know that I received your last report on the Mallophaga of the Mammals (Part II). Thanks very much for same. It is a magnificent piece of work, and you may well be proud of it. It is a great pity that there are not more men who are doing such quality of work as yours.

At the moment I am working on Tinamou lice. I have not published anything on this group since 1944, and have a lot of undescribed forms, from hosts which I had not previously taken.

Could you tell me whether or not Dr. Guimaraes is still in Sao Paulo. I have written him twice since February and have had no reply. I was very anxious to get some information from him about the scientific names of three species of Tinamous from which I have lice (Brazilian species), but the only names I have are the local names in Portuguese. Perhaps you might know what they are. They are the following:

"Jao"; Inhambu; and Surunina. "Inhambu" is from Matto Grosso; "Jao" is from Espiritus Santo and "Surunina" is from Espiritus Santo. The first two also from Goiaz. With best wishes for your continued good health and success, I am,

Yours very sincerely,



M.A. Carriker, Jr.  
Research Associate.