THE LEUCOCYTOZOA,
A REJOINDER TO MR C. M. WENYON.
By ANNIE PORTER, B.Sc.

IN the last number of *Parasitology* (Vol. III. No. 1) Mr Wenyon has taken exception to an article of mine on the *Leucocytozoa*, published in *Science Progress*, October, 1909. It has been a matter of wonder to many protozoologists as well as to myself, why Mr Wenyon did not reply to my article in *Science Progress* itself, and thereby appeal to the readers whom he accuses me of having misled, for he states (*Parasitology*, p. 65) that my "survey of the group is inaccurate and contradictory."

The character of my article in *Science Progress* is well summed up in my concluding paragraph on p. 265. "Such is a general survey of a very interesting group, the *Leucocytozoa*, which, perhaps, is not homogeneous, as the forms in birds differ in some respects from those occurring in mammals. This survey is the first that has been attempted, and it is hoped that it may draw attention to and stimulate research upon an interesting group of parasites which are probably more numerous than we know at present. They should be searched for among reptiles, amphibia and fishes." This paragraph, along with the major part of my article, Mr Wenyon has chosen largely to ignore.

Mr Wenyon's chief accusation is that I have confused the avian *Leucocytozoa* with the haemogregariniform parasites found in the leucocytes of mammals. This is grossly inaccurate, for on p. 264 of my article, I state that "as the structure and life-history of avian *Leucocytozoa* are still subjects of controversy, and as the name *Leucocytozoon* was first applied to the parasites of birds, and Lühe seems to restrict the name thereto, the generic name *Leucocytogregarina* might be used for the highly specialised parasites of mammalian leucocytes, which have a different habitat from the strict Haemogregarines of red corpuscles." I think from the foregoing quotation, it will be quite obvious to my readers that I have not confused the avian and mammalian parasites, for both of which the name *Leucocytozoa* has been used by many authors, and it was necessary, in a somewhat popular article, to deal with all the parasites that had at any time been termed *Leucocytozoa*.

Dofflein (1909) adopts a similar view in his recent treatise on the Protozoa.

At the end of his article, after accusing me of many mistakes that I have not committed, Mr Wenyon is forced to admit that "the suggestion at the end of the review of a new generic name seems to indicate some doubt even in Miss Porter's mind as to the validity of this grouping." Needless to say, had I not been fully cognisant of differences between the avian and mammalian parasites, I should not have put forward the new descriptive name *Leucocytogregarina* for the haemogregariniform parasites of the leucocytes of mammals.

In several places in my article (*e.g.* pp. 256, 264) I state that the structure and life history of avian *Leucocytozoa* are still subjects of controversy; therefore they are open to differences of opinion. Mr Wenyon ignores this, and proceeds to use such loose statements as that on p. 64 where we read that "It is evident that Miss Porter has no knowledge of the *Leucocytozoon* of birds for her survey of the group is inaccurate and contradictory." Such dogmatic assertions regarding a worker with whom he is unacquainted personally, and of the extent of whose information he has no first-hand knowledge, need no further comment from me, except that scientific research is considered to engender a spirit of toleration accompanied by a broad outlook, hardly compatible with such assertions as the one quoted above. It may, however, give some slight satisfaction to Mr Wenyon, as well as confidence to scientific workers generally, to know that I have an intimate personal knowledge of the *Leucocytozoa* occurring in the domestic fowl, the sparrow, the lark, the Scotch grouse and the guinea fowl—some of which parasites have never been described or recorded in England till now. I fear, therefore, that in this case it is the statement of Mr Wenyon that is "inaccurate"—as well as unwarranted.

I must crave the indulgence of my readers in that I have no choice but to enter more fully into certain of the criticisms passed on the short general review that I wrote last year.

An appreciable proportion of Mr Wenyon's paper consists in the setting forth of what he is pleased to term the characters of *Leucocytozoon* and of the leucocytic parasites of mammals.

Regarding the third of these characters (this *Journal*, Vol. III. p. 65) Mr Wenyon is wrong in laying great stress on the *Leucocytozoa* of birds causing the host cell to assume a characteristic spindle form. Mathis and Léger have described *Leucocytozoon caulleryi* from the domestic fowl of Tonkin, and *L. marchouxii* from *Turtur humilis* neither of which causes any assumption of a spindle shape by the host cell.

Laveran and Lucet state that the host cell of *L. smithi*, parasitic in the turkey, may be either oval or greatly elongated. As stated above, I have seen *Leucocytozoa* living in the common fowl in England, a parasite probably the same as that of Mathis and Léger, and it does not produce spindle elongation of the host cell. The property of producing marked deformation of the host cell, then, breaks down as a character of avian *Leucocytozoa*.

Again the fourth character given is that male, female and young parasites, possibly immature gametocytes, are found in the circulating blood. I would suggest that there is another possibility—that some of these forms are not gametocytes but are schizonts. This possibility has been overlooked.

With regard to the seventh character Mr Wenyon states that the parasites never leave the host cell to move about in the blood plasma. Because Mr Wenyon has not seen this phenomenon, it does not follow that it does not occur. Further, I would ask how the primary infection of the host cell takes place and how the periodicity exhibited by *L. caulleryi* described by Mathis and Léger is to be explained, were there not free forms in the blood at some stage.

A still more astonishing character (!) is given when it is stated that the "asexual mode of reproduction is unknown." It is surprising that lack of knowledge of various phases of the life history of any organism should be quoted as evidence regarding the character of that organism. Apart from that, the statement is inaccurate, for schizogony of *Leucocytozoon lovati* of the grouse is now known. (See *Abstracts Proc. Zool. Soc.* (1910) No. 84.)

Had Mr Wenyon taken note of the sections of my article dealing with the general description, movements, comparative morphology, multiplication and reproduction, he would have acknowledged that most of his "characters," and particularly those of the leucocytic parasites of mammals, were clearly set forth therein, but without the dogmatic assertion or suggestion that they were invariable features or characters of the organisms. When Mr Wenyon can refer us all to a generally accepted complete life cycle of an avian *Leucocytozoon* as well as to an equally generally accepted complete life cycle of a mammalian *Leucocytogregarine*, then it will be time to draw up tables of differences between the various parasites under discussion. Meanwhile such character differences as those used by Mr Wenyon are very artificial, and as shown, often quite inaccurate.

A side issue is raised when the question of *Leishmania* and *Herpetomonas* is discussed by my critic. But though irrelevant to *Leucocytozoa*

it is of some service. One reason for calling the haemogregariniform parasites of leucocytes by the generic name *Leucocytozoon* was the high degree of specialisation involved in their habitat. Mr Wenyon, ignoring this specialisation, asks can mere habitat be regarded as a generic character—and himself supplies the answer in the affirmative by quoting the alternation of hosts in the case of "*Leishmania*," "a profound distinction which undoubtedly justifies its inclusion in a distinct genus" (from *Herpetomonas*). I do not wish to confuse the issue regarding *Leucocytozoa* by entering into this controversy. Capt. Patton and Mr Wenyon among others can settle that question. But the high degree of specialisation shown by the haemogregariniform parasites of mammalian leucocytes is surely more than a matter of "mere habitat."

Further, with regard to habitat being a specific character, Mr Wenyon has some slightly personal remarks, which are entirely his own, and from which I most strongly dissociate myself. He says "Miss Porter follows James and Patton, who are stated to have the advantage over Laveran and Mesnil, of first hand knowledge of the group. Apparently Miss Porter imagines that Laveran and Mesnil and possibly others have not this first hand knowledge, but I can assure her that in this she is mistaken." The inference is that of my critic and not mine; nor did I state that James and Patton had advantage over Laveran and Mesnil, but that they had first hand knowledge.

It would not be advisable to continue an argument advanced in the "Remarks on the genus *Leucocytozoon*" regarding the association in classification of the *Leucocytozoa* of birds with such parasites as those of malaria. Capt. Patton considers the avian *Leucocytozoa* to belong to the genus *Haemamoeba*, and I need only say, that in so doing, he follows Laveran in this association,—a scientist previously quoted by Mr Wenyon as an authority on the *Leucocytozoa*.

With regard to the priority of the name *Hepatozoon* of Miller, it should be stated that the first observed and type species of the haemogregariniform parasites of the leucocytes of mammals, was that parasitic in the dog, and named *Leucocytozoon canis* by James, a life cycle of which in the dog-tick has been described by Christophers. *L. canis* being the type species, then *Hepatozoon*, "a monotypic genus" (Miller), and the name of a parasite in the leucocytes of white rats, does not hold as a generic name for these leucocytic parasites of mammals, and even Mr Wenyon writes that "Miller suggests for the rat parasite the name *Hepatozoon perniciosum*." Miller did not suggest that *Hepatozoon* should be the generic name for all the *Leucocytozoa* of mammals, but even stated that as the complete life cycle of many of the parasites

had not been established, by including his parasite in any of the already existing genera, "the danger is present of giving a total misconception to the life cycle of other forms." Again, Miller's work on *Hepatozoon* has not yet been confirmed, and there is the possibility that the phases described as those of *Hepatozoon* in the mite, *Lelaps echidninus*, may be phases in the life history of a parasite natural to the mite itself. This matter I have already discussed sufficiently in *Science Progress*. Until Miller's work on *Hepatozoon* be confirmed, it seems to me well to reserve definite judgment on the sporogony of mammalian *Leucocytozoon*.

The relevancy of certain remarks in the footnote on p. 66 regarding *Piroplasma* and Trypanosomes, as well as their accuracy, may also be questioned. Two main chromatic bodies occur in a Trypanosome. The larger of these is commonly called the nucleus or trophonucleus, the smaller is generally designated the blepharoplast or kinetonucleus. The latter body is near the origin of the flagellum and is generally considered to be concerned with regulating the function of movement. Mr Wenyon calls this small body the "micronucleus¹," a term usually associated with the small mass of generative chromatin of a Ciliate such as *Paramoecium*, which body has no connection with locomotion.

Again, the law of priority is not a strict law (proved hypothesis), but a convention or rule, which, in addition to serving its primary purpose, has brought about much confusion and controversy, and must, of necessity, with increase of knowledge, be displaced. For instance, the familiar *Coccidium* is no longer designated as such owing to the strict application of the law of priority, while the equally well known *Piroplasma* should give place to *Babesia*. The vexatious use of the rule of priority has already been protested against by many of the greatest living zoologists, Lankester, Shipley, Minchin and Boulenger among others, and will probably be soon superseded by less rigid yet equally correct conventions. In the meanwhile, it is neither presumption nor crime to suggest a more reasonable nomenclature, for the world has long since dispensed with such inelastic codes as "the law of the Medes and Persians which altereth not."

¹ The reproductive character of a micronucleus has been emphasised in the classification of the Protozoa as set forth by Doflein and by Hickson. The Protozoa are ranged in two great groups:—

I. The *Plasmodromata* (Doflein, 1901), Protozoa in which the nucleus is not separated into reproductive (micronucleus) and non-reproductive (macronucleus) portions.

II. The *Heterokaryota* (Hickson, 1903), *Ciliophora* of Doflein, Protozoa possessing cilia either throughout life or only in the early stage of the life cycle, but always with a micronucleus which is entirely reproductive in function; and a macronucleus which is trophic and kinetic.

There are other remarks in Mr Wenyon's paper to which exception might easily be taken. Some of these arise from consideration of sentences in my article isolated from their context. Such a procedure is more compatible with the political arena than with scientific research. I will not weary my readers with further details.

In conclusion, I would cordially thank Mr Wenyon for the great honour he has done me in bringing into prominence my modest article on the *Leucocytozoa*, wherein I naturally considered all the parasites which had ever been so designated. The article, as Mr Wenyon knows quite well, was of a slightly popular nature, in which controversy—that adds glory to neither party—could not be set forth at length. At any rate, my article has served one purpose at which I aimed, that of drawing “attention to an interesting group of parasites.”

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Fuller references will be found at the ends of some of the papers quoted.

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