

## Dr. Jacot as authority on the fauna of the forest soil.

By

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I am by nature and conviction a man of peaceful disposition who very rarely quarrels with my colleagues. But sometimes it would be a foolish delicacy not to react against statements and opinions, which are so far from being adequate, that they are distinctly misleading. This is my excuse for making a few remarks on Arthur Paul Jacot's paper »Evaluation of the forest floor population».<sup>1</sup>

Mr. Jacot, who has written quite a number of very good papers on one of the groups of Acarina, the Oribatids, has, so far as I know, not worked on the fauna of the forest soil. Nevertheless he has seen fit, after a short review of some papers on this topic flatly to denounce the methods hitherto used by these unfortunate authors. »The era of these former meaningless and mechanical quantitative studies should be closed, and a new one founded on sound ecological principles begun».

I freely admit that I was staggered when I read this sweeping statement. It is true that I am not included because for reasons unknown to me Jacot has studiously avoided quoting any of the two papers<sup>2</sup> I have been foolish enough to write on this theme. One of them is a paper read at the 4th International Congress of Entomology in Cornell University 1928 which he will find in the library of his own university. But although I am left out in the cold I will take up the gauntlet thrown to my colleagues, if this metaphor may be allowed, because it presupposes equality between the attacker and the defendant. When in reality there is according to Jacot not equals he is dealing with but rather he has from his superficial studies in the literature in question attained such a superior position that he is the judge, the authors being the delinquents. Such a type of investigator he considers

<sup>1</sup> Canadian Entomologist, December 1932.

<sup>2</sup> The last paper issued in June 1932 I do not blame him for not having read.

worthless. »This (the beginning of the new era) can only be carried out on the same scholastic basis and by the same type of investigators (spaced by me) that have placed the study of soil chemistry and soil protozoology on the high level which they now hold.» If this bold statement has any meaning, Jacot is also an authority on soil chemistry and soil protozoology.

Let us now see on what grounds Jacot denounces the quantitative methods hitherto employed by the investigators of the fauna of the soil.

1) »In all the quantitative studies of the forest floor sod and soil fauna the Acarina or mites have been grouped under one head as useful in reducing dead leaves and twigs to crude mineral matter, in spite of the fact that it is well known that some of the floor Acarina are eaters of mildews and moulds (minute fungi), some are predaceous, and some feed on living plant tissue».

This is not true. I have already in my first and second papers (1928) divided the Acarina into 3 groups, the Oribatidae, the Gamasidae and the Trombidiidae, which display very different feeding habits. »The Oribatidae are, so far as is known, all herbivorous, their food consisting of moss, lichens, fungi and rotten wood. The Gamasidae, on the other hand, are mostly carnivorous, although amongst the Uropodidae there are probably many herbivorous forms. The Trombidiidae have very different habits, some, as for instance the Bdellidae, Trombidiidae and Erythraeidae, being carnivorous, while others, e. g. Bryobia, are herbivorous. Our knowledge of the feeding habits of the mites is, however, on the whole as yet rather insufficient. For this reason it is not possible to divide them with any certainty into groups according to these habits, but we shall have to use their systematic units in the following discussion».

If Jacot could read the Swedish text, which I admit I have no right to expect, he would have found that I emphasised the necessity of knowing the feeding habits of all the arthropods. The quotation above shows that I was fully aware of the fact that the systematic units were not the most satisfactory basis for a discussion but they had to be used, at least provisionally.

2) Dr Jacot considers it an error to include the moss in the samples. »To include this independent faunule is to incorporate in the analysis of the litter reducing fauna, one which has only a casual relation to it». Here Jacot introduces a term »the litter reducing fauna» which is used by some of the investigators but not by all. It is evident that Jacot has not seen the floor of a forest in Northern Europe. If he had he would not have suggested anything so ridiculous as studying the fauna of the soil without taking into account the fauna of the moss and even lichen.

Surely one must study the ground as it is and there are untold square miles of forests in N. Europe where it is impossible to take a sample without finding plenty of moss. Perhaps Dr. Jacot can suggest a reliable method of separating the moss from the layer of needles covering it and tell which acarina are natives of the needles and which of the moss? Besides, is the moss exempted from the fate of all living things? Does it not grow old and die and decay, forming litter? And can anybody doubt that the Oribatids are active in the decomposition of the moss, although the procedure probably takes longer than in the case of the needle-litter?

3) Dr. Jacot then makes the important discovery that large numbers of arboreal individuals are beaten down by the rain thus temporarily enriching the fauna and upsetting the calculations of the investigators, a fact of which presumably all previous investigators were unaware. Why not tell them at the same time that quite a few arboreal insects *voluntarily* go down into the ground in order to pupate or to hibernate?

4) The climax is, however, reached when Jacot says that mass statistics on the systematic groups are of no practical value. I beg to refer him to my two first papers in which I was able to prove by the means of this worthless method: »1) that the fauna of the litter of spruce and pine needles is very poor, if formed by needles already dry when they fall to the ground. 2) that even needles may promote a rich fauna of macroarthropods, if they are not dry when they reach the ground, but are green as they are when boughs are left on cuttings. In the latter instance we find in the moss under the boughs a rich insect life, almost as rich as when leaves cover the ground because the needles decay on the ground, which involves the presence of numerous fungi and saprophytic insects and their train of followers. 3) As soon as the ground is covered by leaves the fauna becomes 70 times as rich even in the far North, and in the South of Sweden still richer.

The investigation here related amply supports the generally accepted opinion that the condition of the forest soil is improved when the coniferous and foliiferous trees are mixed and that the manuring of the forest, found to take place when boughs and twigs are left on the ground, is at least partly explained by the favourable influence due to the fauna».

Jacot is of course entitled to his opinion that these results are of no practical value. But I venture to think that his opinion is not shared by any others who have worked on this most intricate problem. The same method enabled me in June 1932 to prove that the technique hitherto employed when collecting the fauna of

the soil was very defective and gave quite misleading results; surely this was of practical value.

If dr Jacot thinks there is something new and original in his message to group the animals according to their feeding habits he is greatly mistaken. I am sure that everyone of the authors cited by him is perfectly aware of this. But, alas, we have in my country an old saying: It is far easier to say »rose» than to grow one. This proverb fits the present case exactly. Before we begin studying the habits of the different forms we must know their names, which in many instances cannot be accomplished without breeding the larval forms. Anyone who has read A. D. Michaels' magnificent work on the British Oribatids is profoundly impressed by the herculean task he performed in breeding them, ably helped therein by his wife. But his task was a simple one in comparison with that confronting the investigator who tries to unravel the interrelations between perhaps 40—50 different species inhabiting two handfulls of litter!

I conclude by quoting what I said at the Congress in Cornell University »There are so many intricate problems to be solved before we can pass from the preliminary stage of cataloguing empirical data, and an immense amount of work has to be performed before we can understand the factors controlling the phenomena which we observe. A new technique, not to say a new science, must arise. Nevertheless, let us not tarry any longer, but let us start unravelling all over the world the mysteries of the fauna of the soil».

During this work we cannot dispense with quantitative methods as we grope our way along, but surely we can do without unprovoked attacks based on insufficient knowledge of the subject.

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### Literature.

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