

2 November 1931.

Professor E.B. Poulton, F.R.S.,
Wykeham House,
Oxford.

Dear Professor Poulton:

I am very much obliged indeed for the copy of your Presidential address which you had the kindness to send me, as I was particularly glad of the opportunity to read it in full. The accounts in the newspapers were so very inadequate that I could form no idea of what a remarkable address it really was, or of how much I was missing by my absence.

Miss Tassart's plates are beautiful. Pycraft took the cases illustrated for a little popular article he writes weekly in the "Illustrated London News", but he made a mess of the example of mook cocoons in Nerazuma kolga as he so often does on evolutionary points. This was a great pity as at least one reader came to me after reading Pycraft saying what a bad example of mimicry it was. On the whole I think your address must have made several Zoologists who have been hedging for years about Natural Selection, feel rather small.

TP About the British Association in general I wonder if anything could be done to make better use of joint discussions, in which more than one section take part. They might be, and to some extent even are, much more interesting than purely sectional papers, for which indeed the British Association is not really the best medium, since publication has to be arranged separately in any case: but the joint discussions could, I believe, be exceedingly valuable, and would attract a genuinely scientific interest among non-specialists, if only they were run with a clear idea of what is wanted. And what the intelligent non-specialist wants is, I think first a lucid statement of the current scientific situation, and next the reaction of the intelligent layman, who may of course be a scientist in some other field, to the possibilities of the situation outlined. I take it that these two objects are the functions of the two parts of such meetings, the set papers by specialists and the impromptu discussion by members of the audience. This latter has, unfortunately been practically eliminated by overcrowding the programme with pre-arranged speakers, so that only a very inexperienced member would attend such a "discussion" with any intention or hope of being allowed to contribute to it.

With respect to the set papers I suggest that all that is wanted is a statement of the current state of knowledge from the very few people who have made a special study, from

one aspect or another, of the point under discussion. I mean that the opportunity for such a discussion only arises when new work in one field is thought probably to be of importance to another group of workers, or when some topic, such as the age of the earth, or the drifting of continents, is raised, upon which different sciences can supply independent testimony. If any topic is of concern, for example, to Zoologists, physiologists and psychologists we shall be lucky if one competent man can be found from each section, who has considered the point sufficiently to give a lucid account of its bearing upon his particular science, or of the relevant evidence, if any, which his science can contribute. What is disastrous is to assume, as we seem often to do, that the discussion must be opened by one or more eminent and highly distinguished psychologists, ^{etc.} who have given no particular attention to the point under discussion, merely because they are eminent and distinguished psychologists. Their place, it seems to me, if they perceive some wider bearing of the subject, which has been overlooked or inadequately stressed, is to talk in the discussion. In this way it should be possible to lighten the set part of the programme enormously keeping it to, say, three contributions of half an hour each, and to leave, ideally, as long again, though I don't expect this could be achieved at once, until the audiences have been educated up to it, for the genuine reactions or reflections

of people who are interested in new knowledge because it enables them to think more effectively.

The initial difficulty, naturally, will be to get the audiences to talk at all, as they are not used to it; though a knowledge that the time available has not been pre-empted by eminent authorities, who have agreed to take part in the discussion, would, I think, encourage them greatly; as would a request by the chairman for comments and queries not exceeding five minutes, for many hesitate to speak because they are not prepared to discuss the whole question, and the chairman can always relax the restriction for anyone who has something to say, and needs longer to get it all said. But the main point is that the audience should feel, and that it should in fact be true, that adequate time has been allotted for discussion, so I should like to instruct chairman specifically, if further discussion fails, to give their sections a short holiday, say half an hour before lunch, which do them nothing but good, and give them a chance of deciding what they really wanted to go to in the afternoon.

I take it there can be no real reform unless the secretaries organising discussions, and the chairmen in charge of them, have quite specific instructions as to how the B.A. wants them conducted. But I do not know how far it is impossible to get Howarth and the executive committee

to agree on and draw up a useful code of instructions.

To return to evolution, while I was in the States this summer I had rather numerous opportunities of seeing among Geneticists and other Biologists the beginning of a new interest in selection theory, though the amount of harm the early Geneticists have done is certainly enormous. While out there I took to collecting butterflies, partly as a good excuse for being on my own feet, instead of in someone else's car, and one experience may interest you, as one sometimes still meets with entomologists who will pooh-pooh mimetic resemblances by saying that any tolerably observant creature could tell mimic from model at a glance. Of course as an objection this only applies to Batesian mimicry, though this is not usually recognised by the critic. I was very anxious to catch the big 'Monarch' and its smaller mimic the 'Viceroy' but they only came late in the season, and it was sometime before I saw any. The first one I caught was a 'Viceroy' which I duly pinned out, and then asked an entomologist, who was working in the Lab. where they had very kindly given me bench-room, which of the two it was. His speciality was grasshoppers, and he unhesitatingly declared it to be a 'Monarch', and when I remarked that it didn't seem big enough, explained that the size was variable according to the nutritional conditions enjoyed by the larva. Of course when I had caught a few more the mistake was obvious, and we

both of us could see numerous differences by which we could have identified the first specimen. However, it was very striking that a mistake should have been made under ideal conditions of observation, on a pinned-out specimen, by a man who must have been quite practised in seeing detailed differences, merely because he had never paid particular attention to that particular distinction. Those who adduce this particular criticism seem to expect a lot of their birds. The case struck me just because it is the kind of evidence which experts in any particular group have usually put themselves out of reach of.

Thanking you again for your address.

Yours sincerely,