

ON THE LIMITING OF SUBSPECIES

by WILLIAM T. M. FORBES

In his recent discussion (*Lepid. News* 9: 1; 1955), VAN SON proposes to solve the question "what is a subspecies" by excluding all the problematic cases and limiting the term to the relatively small group where physical barriers are definite, sharply marked, and can be considered complete. It is true that the persons who have put the most emphasis have been chiefly those who are working on species with interrupted distributions, but in fact the great majority of species fall into the other category, for the major part of the habitable earth is not cut into sharply defined areas but is formed of the broad continents. And even where there is probably a complete barrier, there may often be no subspecific difference. Thus the isolated colony of *Carterocephalus palæmon mandan* Edw. at McLean, N. Y., appears no different from the population further north, — at least no one has tried to make a subspecies of it; yet the latter is the eastern end of a cline which becomes true *palæmon* at its western end in Europe. And the many isolated colonies of *Euphydryas chalcedon* which HOVANITZ studied in southern California, show minute racial differences, each from each, yet as a whole form a couple of clines at an angle to each other.

If we treat cases of this sort as clines rather than isolates in spite of their actual physical isolation, we will have very few true subspecies left. And it seems to me hardly worth while to make a separate category in nomenclature for such relatively rare cases as *Papilio ophidicephalus* Oberthur.

Dr. VAN SON'S difficulty, — that the size of subspecies becomes arbitrary in the case of a species with continuous distribution, — is not at all unique at this level, but applies equally to all the units of classification save only the species; for the size of all alike is purely arbitrary, and we have had always the same difficulty in agreeing on a proper size. Thus I have seen subfamilies suggested in the Lycænidæ which I should personally consider very good subgenera, perhaps weak genera. And the *Heliconius* group is a clear concept in classification whether we include or not *Cethosia* and *Dione*, and whether we call it a family or merely an aberrant series of Argynnines.

The real problems as I see it are in quite another direction, and basically in the attitude of the Code, rather than this slight inexactitude. Firstly if subspecies are anything, they are statistical, and should be based only on substantial blocks of material, yet the Code provides they must be based on holotypes, not on blocks of cotypes; secondly in the great majority of cases they are in fact clinally connected, yet by chance the type locality may turn out to be at the blend-point of otherwise pretty well defined populations, with the result that the races may appear much less tangible than they are in nature. (Lakehurst, New Jersey, and Surinam are frequently such blend-points.) Thirdly the Code limits the machinery of the "trinomial" to subspecies as now defined, while other types of variation are in many cases definitely more important and more worthy of names. It would seem that they were originally singled out on the assumption that they

were the source of speciation. I have never seen any evidence presented to support this assumption; personally, I believe it only a minor source of new species (as against other isolating mechanisms, chiefly biological), and consider this feature of the Code a great hindrance to sound biological thinking.

I should personally suggest, then, some very different reforms: 1) recognize no holotypes of subspecies, but consider subspecific names based only on the whole block of material on which they were originally defined; 2) find some means of allowing a shift of "type locality" where the actual locality turns out to lie in a blend-zone; 3) redefine "trinomial" to include all nameable units below species and recommend the insertion of a vernacular word between the species and variety name to indicate the category of variety (as believed by the given writer); — this interpolated word not to be considered part of the name and to be changed as needed by increased knowledge.

Thus for instance we have the Green Swallowtail: *Papilio philenor*, and the lower name *hirsutus*. This latter is now considered the California race; in fact the hirsute condition is an early spring one both east and west, but it happens that early specimens are rare in the east, and the late ones rare and very local in the west. So I consider it a personal matter whether one writes "*philenor* spring form *hirsutus*" or "*philenor* race *hirsutus*".

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Bishop SKAT HOFFMEYER, of Aarhus, Denmark, lectured on biology of Danish Lepidoptera to the New Haven Entomological Society in October 1954, while visiting the United States. In the course of his engrossing talk, he told of an astonishing recent experiment by a Danish observer and kindly promised to send us the information for the *News* when it had been published. The note, by P. L. JORGENSEN, has now appeared, and the English summary sent by Bishop HOFFMEYER follows.

SUMMARY

ACANTHOPSYCHE ATRA LARVÆ HATCHED FROM BIRDS' EXCREMENTS

This paper describes a probable method of dispersal of Lepidoptera species with wingless females. It was observed that the females of *Acanthopsyche atra* L. (Psychidæ) leave their sacs a few days after pairing. They drop to the ground, where they are easily discovered by birds. Eleven of these fertilized females were fed to a captive Robin (*Erethacus rubecula*). Its fecal droppings from the following 24 hours were placed in a special cage. Once in a while they were sprinkled with water, and a fortnight later the first larvæ hatched from the droppings. In all, 30 — 40 larvæ were hatched, not a very big number, but enough to show that the species may be dispersed by birds in the way described.

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