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A PROPOSAL FOR THE RESTRICTION OF THE USE OF THE TERM SUBSPECIES

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Much has been written lately concerning the danger to our present system of trinomial nomenclature arising from the indiscriminate use of the term **subspecies** when defining more or less uniform geographical populations of species of wide distribution. Not a few disparaging remarks have been made at the expense of over-zealous entomologists who have been increasing the already enormous number of "subspecies", whereas in fact they have only followed the example given by specialists in other branches of zoology, particularly, though not exclusively, those concerned with the study of higher vertebrates.

Very able criticisms of the present-day chaos in subspecific nomenclature have been offered by Mayr, Linsley, & Usinger (1953), Wilson & Brown (1954), Burt, Sibley, Bogert, Hubbell, & Clench (1954), and Edwards (1954), and remedies suggested, some of which would appear to be unjustified, as they would penalize guilty and innocent alike. Surely, to do away with trinomial nomenclature would not represent an advancement, and such action would not be required if the "subspecific house" was put in order by some action of the I.C.Z.N. before it is too late. At its Paris meeting in 1948, the International Commission separated specific and subspecific categories from categories below the rank of subspecies, but failed to give an unequivocal definition of subspecies which would safeguard the proper use of the term in future.

The old definition of a subspecies as a "geographical or host variation" is very vague, because populations of a climatically (and often also seasonally) variable species of wide distribution often present local "population characters" in accordance with prevailing external conditions, quite independent from the presence or absence of isolation which alone can maintain distinctions of a genetic (mutational) nature. Such non-isolated "regional" populations have been lately treated as subspecies, *e.g.* by TALBOT in Pieridæ and by REHN in Orthoptera, to mention only a few cases.

Such treatment of clear cases of clinal variation as subspecies does not permit exact identification of specimens from "intergradation zones" and

simply invites new would-be systematists to increase the already unwieldy number of described "subspecies" by adding still more "intermediates between intermediates", and so *ad infinitum.*

The fault, in my opinion, lies with the central authority which appears to allow too much individual interpretation to be placed upon the term **subspecies**. "Let freedom reign" may be a convenient slogan in politics, but should be used with caution in science if any degree of consequence and uniformity is to be achieved.

MCATEE'S definition of a subspecies, as quoted by DE LA TORRE BUENO in *A Glossary of Entomology* (1937), and that of FERRIS, quoted in the same work, refer unmistakably to cases of clinal variation which, as has been pointed out above, may be caused by external causes and not by genetic differences. FERRIS' statement that "the essence of subspecies is intergradation, assumed or actual", postulates the acceptance of any part of a cline as a subspecies and furthermore asks us to assume intergradation in cases where it has been found not to exist, as, for instance, in completely isolated island or high mountain populations characterized by constant distinctions of a genetic (mutational) nature.

To illustrate the case, let us consider the several clearcut and completely isolated subspecies of the African *Papilio ophidicephalus* Oberth., the nominotypical subspecies of which occurs in Tanganyika. The subspecies *chirinda* occurs from the high mountain forests of the Eastern Border of Southern Rhodesia down to the coastal forests of Portuguese East Africa and is completely constant in appearance and minor genitalic characters throughout the whole distributional area, from over 6,000 feet elevation to sea level. It is separated from the next subspecies, *entabeni,* of the Zoutpansberg range in the Northern Transvaal, by a complete barrier of lowveld thorn country over 100 miles wide, where the species can not exist because of the absence of its foodplant and adverse climatic conditions. The next two subspecies, *transvaalensis* and *ayresi,* are separated from *entabeni* and from each other by similar gaps. The coastal subspecies, *phalusco,* on the other hand, extends from the Eastern Cape forests near Kingwilliamstown to the Karkloof forest of Natal, a distance of over 300 miles, and is just as uniform in appearance as the abovementioned mountain subspecies, although its wide distributional area presents extremes of temperature and relative moisture from the cold and misty Pirie Forest in the Cape to the warm tropical climate of the Durban area. Any specimen of any of these subspecies can be identified without reference to the label, with an exactitude of 100%, as no intermediate specimens appear to exist in nature. Taking the above in consideration, it must be stated emphatically that since no intergradation appears to exist in nature in these cases, it would be erroneous and scientifically wrong to assume its existence.

It has been stated by various systematists that species (and subspecies) in nomenclature do not necessarily correspond to species (and subspecies) in nature. This statement, though unfortunately true on account of the shortcomings of our nomenclatorial practice, seems to point an accusing finger at

those who are responsible for the formulation of laws governing the application of nomenclatorial rules.

In order to give the term **subspecies** a definite and uniform meaning throughout zoological nomenclature, I would like to suggest that the I.C.Z.N. should consider amending the old definition given by the British Commission on Nomenclature, to read as follows:

"A subspecies is a population variation conditioned by geographical or host isolation".

Such a definition would automatically preclude parts of a continuous cline from being assigned subspecific names, and would also eliminate from consideration as subspecies all reversible variations caused by random changes of hosts in parasitic species, where such changes do occur.

I certainly agree with WILSON & BROWN that in cases of complete reproductional isolation of island populations, it would be sometimes difficult to ascertain, without cross-breeding experimentation, whether any of such isolated populations have already reached the specific status, that is, if they have attained complete intersterility. Far from being an impediment, this would reflect the position in nature where no clear-cut distinctions can be drawn between subspecies and closely related allopatric species, the latter being gradually derived from the former, the degree of difference being a function of the duration of complete isolation and of the mutational rate of the species in question. Moreover, the co-ordination of specific and subspecific names would reflect the actual co-ordination of species and subspecies in nature.

Obviously, a drastic step of this kind would meet with much resistance from those who are content to accept as subspecies any more or less phenotypically uniform population, but there seems no other way of avoiding complete chaos in nomenclature in the very near future.

The only difference in practice would be that, should a Latin or latinized name for a part of a cline be deemed desirable, it would be required to interpolate an abbreviation (*e.g.* "f." for "*forma*") between the specific name and the name of the population form in question in all those cases where no completely isolated subspecies appear to exist. In this way, the number of recognizable subspecies would be greatly reduced, and much unnecessary confusion avoided.

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