

A PROPOSAL FOR THE UNIFORM TREATMENT OF
INFRASUBSPECIFIC VARIATION BY LEPIDOPTERISTS

John H. Masters' very thought-provoking and controversial paper under the above title (1972, *J. Lepid. Soc.* 26: 249–260) cannot be allowed to pass without comment. In the first place I consider it utterly wrong for any section of entomologists, be they lepidopterists, coleopterists, dipterists or any other, to attempt to formulate a code that would apply to their own Order only. Any such code must apply to all Orders of insects. Nor do I think it right that the requirements of the geneticists should be dismissed in such a cavalier fashion.

It would, perhaps, be most convenient if I listed my comments under the same headings as used in the original article.

INFRASUBSPECIFIC VARIATION (p. 250). Masters writes, "Other than a general agreement that infrasubspecific names should not be placed in italics. . . ." But is this true? It certainly is not for the four British entomological journals to which I subscribe, and the British Museum (Natural History) continues to print infrasubspecific names in italics in its *Bulletin (Entomology)*. Again, is it true to say, "there has been a very sharp decline in the publication of formal names to apply to infrasubspecific varieties in the last twenty years," and, "most authors are content to describe examples of infrasubspecific variations without attempting formally to name them"? The first may be partially true, probably because most of the well marked variations have already been described and named, but in my opinion, the second is not, and, in any case, what is the point of a description without attaching a name to it? Which is the more preferable title for a hypothetical article, 'The genetics of *Arctia caja* L. and its form. . . .' or 'The genetics of *Arctia caja* L. and its form as described in 1970, *Entomologist*, . . . : . . .' I know which I would prefer, and I think the majority of entomologists would agree with me. I have covered the question of Lepidopterists 'going it alone' in my introductory remarks.

POLYCHROMATIC OR POLYMORPHIC FORMS (p. 250–253). Whilst Ford's definition of polymorphism is undoubtedly scientifically correct, it does appear to reduce the proportion of the rarer to the commoner form to far below what is normally considered as polymorphism. Surely there must be a point, well illustrated by Industrial Melanism in Britain, when a form ceases to be a mere mutant and becomes polymorphic. To take the geometer *Biston betularia* L. and its black form *carbonaria* Dbl. as an example, in the late eighteenth hundreds and early in the twentieth century the black form was a great rarity, possibly so rare that it could not be maintained except by recurrent mutation, chiefly because its colour made it overconspicuous when at rest and it suffered heavily from predators. Once industrial pollution had altered the environment, the position was reversed and it was the typical speckled form that was at a disadvantage and, as a result, the black form, which was genetically dominant, rapidly increased its proportion of the total population until it is the prevalent form in many areas today.

Whilst there is some point in applying a *nomen collectivum* to all the forms in a group that are a manifestation of the same gene, it must not be forgotten that what may appear to be similar forms, even in the same species, may be the result of completely different genes. Whilst accepting the *nomen collectivum* in limited cases, I think there is still a need for a formal name for the various forms, and I also consider that the addition of the author's name is essential, not, as Mr. Masters points out, as a compliment to the author but to pinpoint the reference.

The suggestion of applying the model's name prefixed by pseudo- to the various forms of polymorphic mimics is only a partial solution of the problem. How, for example, are the four forms of *Danaus chrysippus* L., viz. *chrysippus* L., *alcippus* Cr., *dorippus* Klug and *albinus* Lanz, to be treated and what about the many examples of polymorphism in procrystic moths, such as *Achaea lienardi* Bsd., *A.*

praestans Mab., *Blenina quadripuncta* Hamps. and *Odontodes aleuca* Guen., to name only a few. Here, again, formal names seem to be the only answer.

Many aberrations in the genus *Parnassius*, the Lycaenidae and Arctiidae are almost certainly multifactorial in origin and, overlapping as they do, are probably best treated with descriptive, as opposed to formal, names. The use of descriptive terms for aberrations was probably carried to the extreme in Bright & Leeds *Monograph of the British Aberrations of the Chalk Hill Blue Butterfly, Lysandra coridon* (Poda) 1761. (Bournemouth 1938) which described some four hundred types of aberration.

MUTANT OR ABERRATIONAL FORMS (p. 253-254). The reference to the effect of cold on the pupae of *Euphydryas phaeton* (Drury) raises an interesting point. Normally the effect of unusual temperatures is an interference with the normal process of pigmentation, and Haggett (1952, *Entomologist*) has shewn that a number of the named forms of *Rhometra sacraria* L. are the result of low temperatures on the pupa, examples carrying the factor for redness producing f. *sanguinaria* Esper at slightly lower temperatures and f. *rosea* Oberthur at the lowest possible, whilst those without the factor for redness produce f. *labda* Cr. at slightly lower temperatures and f. *atrifasciaria* Stephens at the lowest possible. In other words the visible effect of the gene is enhanced by low temperatures, probably through the greater length of the pupal period. In the arctiid *Panaxia dominula* L., it has been established that there are certain genes that do not manifest themselves unless the pupa is exposed to abnormally low temperatures. Whilst I agree that purely temperature forms are not worthy of a name, I do think there is a case for naming forms which are a combination of temperature and a specific gene.

I cannot agree with Masters' statement, "Whether genetic or non-genetic in cause, aberrants are not normally an integral part of any population, each specimen is an individual without direct connection with any succeeding individual that may resemble it." This is manifestly not correct in the case of genetical aberrants, even if the gene is fully dominant and lethal when homozygous it will survive unless the heterozygotes are at such a disadvantage that all are killed by predators, and a rare and recessive gene can survive undetected for generations in heterozygotes. An illustration of this occurred here recently, three specimens of an aberration of *Charaxes brutus* Cr., lacking the chestnut component of the underside basal markings, were trapped in the same area and within a few days of each other and were fairly obviously the progeny of one female. It is only a matter of time before a pairing between two apparently normal individuals, but both heterozygous for this particular gene, occurs and the aberration re-appears.

I agree that gynandromorphs, somatic mosaics and other freaks are best left unnamed, but it must not be forgotten that many of these are genetic in origin.

SEASONAL FORMS (p. 254-255). Here is one of the few parts of the paper with which I am in partial agreement. I say 'partial' as I do not care for Masters' third, and preferred, alternative. I feel that the second is by far the best. Numerals or letters to denote seasonal forms rather break down when applied to wet and dry forms in the tropics, a wet form may occur earlier in the year in one part of a species' range and later in another.

HYBRIDS (p. 255-256). Here again I am only in partial agreement. Whilst accepting the first three classes and the method of naming them, I feel that once a stable hybrid population has established itself in nature it is far preferable to give it a name and treat it as a species, for that is undoubtedly what it will become, if not sooner then later. *Papilio kahli* Chermock & Chermock may be a fairly straightforward case, but Warren's hybrid *Pieris*, based mainly on deformed andraconia, is very much a matter of opinion and is unlikely, in my opinion, ever to be proved conclusively. After all, many so-called subspecies are probably nothing more than hybrids between two separate subspecies that have met and then become isolated.

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