

L. Agassiz, and which contains a few notes in his hand, not changed the name of *C. quadricornis*. He has changed his *S. Carolina* to *quinquemaculata* and says of *G. epimenis*, that "it is probably a species of *Brepha*." Therefore, *C. quadricornis* has to be retained, as Huebner had nothing in any way to do with this species.

Of *Sphinx cinerea* Huebner, Vol. II., gives only a figure with the name *Lethia chersis*, not even the locality. Therefore, Harris' name has to be restored.

Of neither of these species is any description whatsoever given by Huebner. In his Verzeich., 1815, *L. chersis* is not mentioned, therefore, the plate must have been published after this year.*

THE CAPITALIZING OF SPECIFIC NAMES.

To the Editor of "PAPILIO:"

Can the editor of "PAPILIO" inform me upon what ground and for what purpose American lepidopterists have recently adopted the vicious habit of capitalizing the specific names of their insects? The tendency among entomologists, generally, has been in the other direction of using a lower case letter, even when the species is named after some individual or, in other respects, may be looked upon as a proper name. The prime object of nomenclature being to facilitate study and thought, this last fashion has everything in its favor. The capitalizing of specific names is almost as bad and fully as unnecessary and confusing as the use of the lower-case letters which has come into vogue among some catalogues. I am led to ask the question upon reading recent articles by Mr. A. R. Grote, in which generic and specific names are often used singly and in each case capitalized, so that none but those few who are entirely familiar with the terms can know whether genera or species are referred to, except by guessing or laborious reference. If there can be any sound defence of the custom I should like to know of it.

C. V. RILEY, Washington, D. C.

CONCERNING SO-CALLED TEMPERATURE FORMS OF BUTTERFLIES.

BY ARTHUR G. BUTLER, F. L. S., F. Z. S.

I have read with much interest a paper by Dr. Hagen, "On *Papilio machaon* L., and its North American representatives, etc." and, considered from a Darwinian point of view, it is undoubtedly

* Since writing this paper I have learned from a correspondent in Europe that in Senator von Heyden's copy of Huebner, received by him from Geyer himself, this plate is marked "published 1838 by Geyer. H. A. H.

valuable, as bringing into a small compass much relating to *P. machaon* and allies that has hitherto been widely scattered, it helps to prove what we evolutionists have long believed, that *P. machaon*, *hippocrates* and allies are all descended from a common stock; but it does not and cannot prove that *P. machaon*, under any condition of temperature, would ever produce *P. hippocrates* or *P. rutulus*.

Mr. Pryer, of Japan, for whom I entertain the greatest respect as an essentially *field* naturalist, certainly makes a statement about the size of March specimens of *P. hippocrates*, which is so astounding that I can only regard it as a slip of the pen; $2\frac{1}{2}$ inches in expanse would be very small indeed for European *P. machaon*, most examples of which exceed three inches. I have examined a great number of specimens of *P. hippocrates*, taken by collectors who have lived in Japan for years, and the difference of size has only varied from $4\frac{1}{4}$ inches in the males to five inches in the largest females. I do not mean to say that you could not starve them into smaller insects, but, as a lepidopterist, who has seen probably more Japanese Lepidoptera than any man not living in Japan, I must be excused if I believe that Mr. Pryer has forgotten to double his measurement or has, at least, written a 2 for a 3 in the note upon this species.

If, however, we were unquestioningly to accept the statement that some examples of *P. hippocrates* were smaller than the European species, it would not alter the fact that the former, with its more-produced primaries, the broader dark belt of secondaries, the inner edge of which is scarcely undulated (unlike the European insect) and its usually melanistic female is confined to Japan, and *P. machaon* to Europe, and has no more claim to be called the same species than the *Gonepteryx aspasia* of Ménétriés, with its acute falcate primaries, has to be regarded as identical with the broad-winged *G. nipalensis* (a specimen of which we have from Nikko, which Pryer confounds with it.*

Does Dr. Hagen expect us to associate as conspecific all forms descended from a common ancestor? If so, all naturally constituted genera may be called species, and all species representative forms; but *cui bono*? the species will exist, by whatever terms we know them. I will not say "a rose by any other name, etc," because Dr. Walsh argued forcibly against the truth of that statement, but I will say this, that nothing will be gained by the change.

Now, as touching the species of *Terias*, associated by Pryer, and for which, as he considers that too many names have already been proposed, he suggests another, with a view to cure the evil on the homœopathic plan. It may, perhaps, surprise Dr. Hagen to

* I did, indeed, once receive a specimen of typical *G. rhamui* from Yokohama, I believe Mr. Pryer sent it in a collection which I received from Mr. Fenton; I recognized it at a glance as British and this was admitted by Fenton and (if I mistake not) by Pryer also.

hear that, of the eleven in that list, only five occur in Japan; and, therefore, that six are admitted on the authority of other entomologists, who had made no special study of the genus; of these, *T. hecabe* is a Chinese species, ranging to India and Australia. It is always of a deep chrome-yellow color in the males, the under surface is frequently immaculate and never more heavily marked than its near ally *T. mariesii*. *T. mandarina* occurs both in China and Japan, and in the latter evidently crosses with *T. anemone*, as that form does with *T. mariesii*, and here arises a distinct question. Are we to reject the specific distinction of well-marked forms, because when brought together, they will interbreed? There can be little doubt but that species, originally locally distinct, but the ranges of which have gradually been extended, are sometimes fertile inter se, and also, that hybridization between such species or "representative forms," carried on for many generations, produces a tendency to throw out gradations of form between the original parents, even in the progeny from a single batch of eggs. This is certainly the case with the silk-moths of India and Japan, which, according to the statements made by collectors, borne out by the collections sent home, are true to locality, until brought together for sale in the market, and thus subjected to the probability of hybridization.

But to return to Pryer's list, *T. sinensis* is probably *T. rahel* of Fabricius and belongs to a totally different section of the genus from *T. hecabe*. Mr. Elwes can never have read Lucas' description, or I cannot believe that even he, (notwithstanding his very broad views respecting the variation of *some* species) would have suggested the union of two forms so utterly dissimilar in all that constitutes a specific difference.

T. mariesii is, as I have said, the Japanese representative of *T. hecabe*, but not, therefore the same species. *T. anemone* is a form, perhaps, a species occurring rarely in China, and commonly in Japan. *T. connexiva* and *hybrida* are admitted hybrid varieties. *T. asiope* is a N. E. Himalayan species or, perhaps, a race of *T. hecabe*, about equally distinct to *T. mariesii*. *T. hecabeoides*, on the other hand, is a N. E. Himalayan species, readily distinguishable, both from *T. hecabe* and the whole Japanese crew, by having a brown transverse patch on the under surface of primaries towards the apex. *T. brenda* is a very distinct and purely African species, having a white female. It is entirely unlike any form ever existing in Japan, and, lastly, *T. sari* belongs to a distinct sub-section of the genus in which the under surface of the primaries is marked with a large, square, apical brown patch. Hitherto it has only come from Java, Malacca and Borneo. Its female is characterized by the obliquely cut sinuation of the external border of primaries. *T. sari* is, in fact, nearly allied to *T. silhetana*, but is not nearly related to *T. hecabe*. In conclusion, I have no hesitancy in saying that if Mr. Pryer could see the

species of *Terias* associated in his list under the name of *T. multiformis*,* figured together on one plate, he would laugh at his own credulity.

NOTES ON LEPIDOPTERA.

RARE SPHINGIDÆ. I have taken the following rare species at this place. *Arctonotus lucidus* at light, January 7 and 28, *Pterogon Clarkiæ* on flowers of wild turnip, March-April, *Sphinx Sequoiæ*, hovering over various flowers, April, *Sp. perelegans*, about various flowers, June. J. J. RIVERS, Berkeley, Cal.

WILLOW A FOOD PLANT OF PAPILIO RUTULUS. On August 29, 1879, I received from Mr. O. T. Baron, in Northern California, an egg of *Rutulus* in alcohol, which egg Mr. Baron informed me he saw the female deposit on willow; a remarkable food plant for a *Papilio* of this group. W. H. EDWARDS, Coalburgh.

DAREMMA HAGENI. In my paper in December "PAPILIO" I incorrectly gave Kansas (Snow) as a locality for *Daremma Hageni*, Gr. The material I examined came from Texas (Boll), the original habitat of the species, which has not occurred in Kansas yet. I must have misunderstood the statement made to me when I saw the specimens upon which I based my statement. The species seems to be only known from Texas, but is probably found elsewhere in the Southwest. A. R. GROTE.

BIOLOGICAL COLLECTION IN CAMBRIDGE MUSEUM. Perhaps it may be not out of place to make a general statement concerning the biological collection of the Lepidoptera in the Museum. When first arranged, about ten years ago, the number of species, more or less fully represented, exceeded 1200. The very numerous additions since this time made a new arrangement unavoidable. The Diurna now completed show 200 species. The Sphingidæ, now in way of arrangement, are surprisingly rich, and the Bombycidæ, Noctuidæ and Geometridæ even much more so, so that, probably, the final number will exceed considerably 3000 species. For the Micros the additions have been only small. Of course, anything that collectors may have to spare in the way of transformations will be thankfully accepted, in exchange or otherwise, as it is my intention to make this part of the collection as rich as possible for the benefit of students. H. A. HAGEN, Cambridge.

LOCALITIES OF BUTTERFLIES. In the Nov.-Dec., 1882, number of PAPILIO appeared descriptions of *Thanaos Tatius* and *T. Clitus*, with no mention of locality. The paper was sent on to be incorporated with the one which appeared in the October number on the new species taken by Mr. Morrison at Fort Grant and Graham Mountains, Arizona, but arrived too late. Hence the omission of the locality. Both these *Thanaos* were taken on Mt. Graham.

W. H. EDWARDS.

* Which name, having no type, can be quietly ignored.



Butler, Arthur G. 1883. "Concerning so-called temperature forms of butterflies." *Papilio* 3(3), 62–65.

View This Item Online: <https://www.biodiversitylibrary.org/item/39873>

Permalink: <https://www.biodiversitylibrary.org/partpdf/318137>

Holding Institution

Smithsonian Libraries and Archives

Sponsored by

Smithsonian

Copyright & Reuse

Copyright Status: NOT_IN_COPYRIGHT

This document was created from content at the **Biodiversity Heritage Library**, the world's largest open access digital library for biodiversity literature and archives. Visit BHL at <https://www.biodiversitylibrary.org>.