

up, that is, off from the chrysalis, and the pressure excited in former observations was not yet applied by the males. Apparently the males simply kept hold so as to be on hand when things developed.

Nov. 12, No. 1 is forsaken. To No. 2 four or five males cling, head down, bodies still bent up. They leave only to feed.

Nov. 13, No. 1 still forsaken, but in course of the day males flocked to it, their bodies still bent up. Were frightened away readily.

Nov. 14, No. 1 had all the attention of the males, while No. 2 remained forsaken, the bodies now touching the chrysalis, but almost midway of the abdomen of the pupa, not at the extremity.

Later. Two males had taken firm hold, as in previous observations, touching at the end of abdomen of pupa; would not let go, but had to be picked off.

Later. I found a pair in copulation on the ground. Now I examined No. 2 and found the imago nearly developed, but dead, and this explains why the chrysalis was forsaken.

During this observation I noticed that the males would alight on the chrysalis as they do on flowers, then wheel around quickly, head down, body up.

Nov. 14. My attention was attracted by a flock of six or eight *Charitonia* butterflies on the edge of woods, flying around an object which, on inspection, I found to be a chrysalis.

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## SOME REMARKS UPON THE CATOCALÆ, IN REPLY TO MR. A. R. GROTE.

BY GEO. D. HULST.

I notice in No. 9, Vol. I. of "PAPILIO," some strictures by Mr. A. R. Grote upon myself and an article written by me upon the Catocalæ of the U. S., and published by me in the Bulletin of the Brooklyn Ento. Soc., Nos. 1 and 2, Vol. III.

Following Mr. Grote in his remarks upon the article, I reply as follows:

It is substantially charged that in the article I was but the mouthpiece of Mr. Strecker; that he was the author or inspiration of the determinations made. I am able to prove by Mr. Strecker's letters to me at the time that he was not the suggester or inspirer of the article, nor the author of its ideas; and, moreover, that in one way or another he did not agree with me in my determination concerning the following Catocalæ: *Belfragiana*, *Alabamæ*, *praeclara*, *Atarah*, *abreviatella*, *Whitneyi*, *illecta*, *Bunkerii*, *Snowiana*, *perplexa*, *Meskei*, *vidua*, *Sappho*, *residua*, *Angusi* and *Mariana*—16 in all. He consequently did not so nearly agree with me as did Mr. Grote.

In giving my judgment upon the names in dispute between Messrs. Strecker and Grote, I decided upon the following evi-

dence. While these gentlemen are principals in interest in this matter, they are not principals in evidence. Priority in names is based upon priority of publication. I accepted against Mr. Strecker, Mr. Grote's admission that Part XI. of Mr. Strecker's work was distributed Nov. 10, 1874. Had there been any necessity I would have demanded evidence from the publisher Mr. Strecker employed. Mr. Grote's descriptions were published by the American Ento. Soc., of Philadelphia, through Mr. Chas. A. Blake, its curator. Mr. Strecker, hearing I was about to publish the catalogue, wrote me a long article founded on letters received by him at the time from Mr. Blake, showing from these letters that Mr. Grote's names were not published in any way before Nov. 10. I told him, by letter, I would not receive that as evidence, unless Mr. Blake gave it his personal endorsement, and thus, as the agent of the Society, gave its declaration as to the time of publication. The papers were taken to Mr. Blake, and he, on the back of Mr. Strecker's argument, wrote as follows: "After going over all the data and correspondence between Mr. Strecker and myself, and carefully comparing dates from my own postal cards and letters to him in regard to the time proofs were sent to him (Mr. Strecker), it appears that Mr. Grote could not have received his "author's proofs" of his paper bearing date September, 1874, till after November 10, 1874, as there was no final corrected proof issued on that date yet. The matter on the other side of this, I find after going over carefully with the data at hand, is perfectly correct, so that if you want to use the name of Strecker after the species of *Catocalæ* in dispute, do so.

CHAS. A. BLAKE."

After my article was published, Mr. Hy. Edwards found some letters written him by Mr. Strecker at the time, which showed that Mr. Strecker's descriptions were not as yet written early in October, 1874. Of the existence of the letters neither Mr. Grote nor myself knew at the date of my publication. But from them it becomes certain that Mr. Strecker ante-dated Part XI of his work; and the two gentlemen in Brooklyn who told me they saw his proof sheets in August, were mistaken. These gentlemen are persons whose word any one would have taken without dissent. I told Mr. Grote I would publish the fact of Mr. Strecker's ante-dating, but he demanded that with that I should say the species were his, which, of course, I could not. I offered to publish the fact in the *Bulletin* of the Brooklyn Ento. Soc.; but as the fact did not affect the priority, the editor refused to allow it as it might tend to create confusion. Mr. Grote undoubtedly had his names first in MSS. Mr. Strecker undoubtedly had his first published. The date—September, 1874—on Mr. Grote's paper is the time it was received by the American Ento. Soc., and perhaps went to the printer—but the printer was slow, and Mr. Strecker got ahead in the race. When Mr. Hy. Edwards, acting as the advocate of Mr. Grote, came with his let-

ters to a meeting of the Brooklyn Ento. Soc, (this was after my catalogue was printed) to show me how unfounded was Mr. Strecker's claim, I showed him my evidence, which astonished him. I asked him whether with the evidence I could have decided otherwise, and he answered, emphatically, "No." I am aware Mr. Edwards yet gives the names to Mr. Grote, but he does it on the basis that probably Mr. Grote's proof sheets were first run off, and anything printed, though uncorrected and undistributed, is publication. I do not look at publication in that light, and decide otherwise.

I do not prefer the name *Amestris* to the name *Anna*. Any one looking in Staudinger's Catalogue at the names of the Catocalæ of the European fauna, and over a catalogue of the names of our own species, will see that the names run almost universally in certain lines. "*Levettei*," though from the name of a gentleman of worth and ability, is not in either of those lines of names. And that name is the only one I referred to when I said Mr. Grote's names were not all Catocaline, as Mr. Grote knew when he wrote. Furthermore, *Amestris* was not named in honor of Swinburne's heroine, any more than Mr. Grote's *Anna* was named in honor of the prophetess of that name.

Concerning the position and specific worth of many so-called species, each student has a right to his own opinion. I followed in the catalogue the definitions laid down by me in the introduction, and in so doing followed Drs. Le Conte and Horn in the Coleoptera, Dr. Staudinger, in his catalogue of the European fauna, and Dr. Packard in his Geometrid Moths of N. A. And, while in some respects my opinion has changed, in the main I see no reason to doubt the substantial correctness of my catalogue.

Mr. Grote says, "it was a happy stroke and worthy of the disciple of Mr. Strecker to refer *semirelictæ* as a var. of *unijuga* and remove *pura* to some distance as a distinct species." If Mr. Grote will refer to the catalogue he will see *semirelictæ* is a var. of *Briseis*, not of *unijuga*. In the determination of *semirelictæ* I had the advantage of both the literature and type of the species and am not in the minority in my judgment of its specific value. I put *Beaniana* where, from the "literature on the subject," I thought it ought to belong, and *pura* is next. If *Beaniana* is not in its proper place, *pura* certainly is, unless, as I think now, it ought to be put before *Briseis* rather than after.

*Cælebs* is a good species. Mr. Strecker was responsible for the catalogue determination as was stated in the catalogue, and it is the only thing for which he was responsible. It is true, however, he still says, that he was misled by dark specimens of *Badia* identified by Mr. Grote as *Cælebs*. Mr. Grote, if such gross lack of appreciation be allowed me, is not absolutely beyond such mistakes. He once upon a time identified many of the Heterocera for me, receiving therefor my written and substantial thanks. Among the insects sent were two Catacolæ. One a typical spec-

imen of *Aspasia*, the other the type of *pura*, a description of which was already written. I desired thus to get Mr. Grote's unprejudiced opinion, and learn whether the former was his *Arizona*, and the latter his *semirelicta*. Mr. Grote actually sent the former back labelled "*Walshii*," and the latter "*Faustina*." Is it possible he had never "studied the literature upon the subject? Is it possible he did not know he had written a description of *Arizona*, to which he could refer? Is it possible that he did not know Mr. Strecker had written a description of *Faustina*, and had given a colored figure of it so excellent that the merest beginner could have known *pura* was not *Faustina*? Is it possible that he did not know he had the type of *semirelicta* in his cabinet, and was he ignorant of the "literature upon that subject" also? These facts throw some "light" upon Mr. Grote's claim that Mr. Strecker's *Aspasia* is his *Arizona* and that my *pura* is his *semirelicta*.

It is said I turned the sequence of the species upside down. I did, and gave as a reason for it that the yellow under winged species seem to follow more naturally after *Leucanitis* and *Parthenos*, and the dark under winged species glide more naturally into *Spintherops*. Can any one give a reason for the contrary arrangement except that Dr. Morris began it?

Mr. Grote's article and argument are largely made up of personalities. Upon these I have nothing to say. American lepidopterology has been disgraced in the eyes of the world, in the past, by such things. I will not be a willing party to the continuance of the disgrace. It is something for thought that my friend, Mr. Grote, has almost without exception had a part in all personal disputes, and has generally been plaintiff in those in which he has had part. It is certainly time the method was abated.

#### NOTE BY THE PUBLICATION COMMITTEE.

It is a subject of great regret to us that anything like personalities should have crept into the columns of "PAPILIO," but Mr. Grote's article having been printed during the absence of the editor from this city, it appeared to be only "fair play" to allow Mr. Hulst the opportunity of reply. As far as this journal is concerned, the matter will end here, and no further personal remarks will again be permitted in its pages.

## A NEW VARIETY OF CATOCALA.

BY G. H. FRENCH, Carbondale, Ill.

CATOCALA ROBINSONI. *Grote*.

Var. *Curvata*. n. var.

In this form the ground color is a little brighter gray and the markings are a little more distinct than in the usual form. The distinctive features are a prominent black basal dash extending from the base of the primaries beyond the t. a. line, and a curved shade of the same from the costa at the place of the beginning of the median shade to the outer margin below the apex. I have several specimens of *Robinsoni* in which there is a faint trace of



Hulst, George Duryea. 1881. "Some remarks upon the Catocalae, in reply to Mr. Augustus R. Grote." *Papilio* 1(11), 215–218.

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