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Response to J. C. E. Riotte's Review of the *Lymantriid Fascicle*

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The review of my lymantriid revision by the Rev. J.C.E. Riotte is not an unfavorable one, but I am glad of the opportunity to discuss a few points with which I do not entirely agree. It should be evident that the writing of a comprehensive faunal work such as *The Moths of America North of Mexico* by relatively few authors with limited technical support, if it is ever to be completed, hardly allows time to explore and evaluate such techniques as electrophoresis and scanning electron microscopy as he advocates. The techniques are good, but interpretation of the results remain questionable at this stage when we still do not know what they mean in terms of inter- or intraspecific variation in the Lepidoptera. I do not share Riotte's faith in their validity. For example, who is to say whether the variation in the reticulate pattern around the micropyle of the egg such as he illustrated (1971: 107 and elsewhere) is of specific significance? Such methods may of course yield important taxonomic evidence in a proper research context which would involve demonstrating what the variation means by examination of large numbers of specimens, but they are not practical for resolving minor problems in *Orgyia* in a work of this nature when thousands of other species remain to be treated.

The complaint that of Moths of America North of Mexico authors fascicles "rely too exclusively" on collections of the USNM is unjustified because no less than 20 collections were studied during preparation of fascicle 22.2, including that of the Los Angeles County Museum. However, I was unaware of the existence of the associated larvae that he mentions.

On the one hand Riotte approves of my conservatism (paragraph 2), but in the next paragraph says, in effect, that all those new subspecies were not worth naming because further study would probably show that some of them are really good species!

My statement, "Female genitalia not studied, was intended to mean not studied *by me.* I did see enough of them to conclude that they are not "of decisive taxonomic importance," at least not at the level where such morphological evidence is needed, i.e., for distinguishing very closely related species.

The controversy over *Orgyia wardi* Riotte persists, but after reviewing the problem again I still think that in all probability it is a mythical species. I am extremely familiar with what Riotte described as *wardi*; it is the prevalent form in Nova Scotia. The dark, blackish-headed larva characteristic of this "species" represents about 99% of the population, not only in the type-locality but probably everywhere on mainland Nova Scotia (Sable Island has a very different subspecies discovered too late to be covered in fasc. 22.2). Light, red-headed larvae tyical of *leucostigma* as it occurs from Maine southward and westward turn up in Nova Scotia relatively rarely, and the specimen I chose to illustrate on plate 7, figure 42, was reared from one of these as mentioned in the text. It would seem extremely unlikely that all six adults that I illustrated are referable to *"leucostigma"* (i.e., from pale, red-headed larvae comprising only 1% of the population) as Riotte claims. The type of *plagiata* Walker and paratypes before me of *wardi* all represent the form with a brownish submarginal band on the forewing, closely matching my figures 41 and 42. This brownish form, which may be reared from either light or dark larvae, is much commoner in Nova Scotia than elsewhere, thus providing part of the evidence that the type of *plagiata* is of Nova Scotian origin. Although Riotte considers my identification of the type of *plagiata* "absurd," even based as it was on detailed examination of the actual specimen, he does not hesitate to identify all six of my color illustrations as representing *"leucostigma"* rather than *"wardi."* I agree that they are *leucostigma* — *Orgyia leucostigma plagiata* (Walker), with *wardi* Riotte as a synonym.

Food plants mean nothing in this connection because *leucostigma* is about the closest thing to a polyphagous species that one can find. The supposed genitalic differences that Riotte figured (1971: 112) also have no significance because one can make many genitalia slides of the *leucostigma* complex and hardly find two valves alike. Neither do his figures of the female genitalia (1971: 113) show, to my eye, any clear differences. I have already commented upon "Die Mikropylrosette." I think that it will be obvious to most readers that the aedoeagus of *leucostigma* is indeed tapered to a point relative to those of other species as viewed through an ordinary dissecting microscope. Of course it could appear blunt in a SEM photograph. Riotte's larval ocelli diagrams (1971: 109) show the two largest ocelli separated in *wardi* and contiguous in *leucostigma.* I studied many larvae to check on this. Although the difference may be partly real, it is also partly and perhaps entirely an optical illusion resulting from the much greater amount of melanin in the head capsules of the dark Nova Scotian

larvae. The darkened integument encroaches closely upon the ocelli and halfway up their sides, thus exaggerating the apparent space between them and minimizing their apparent size. If there is such a difference it is certainly more subtle than his illustrations indicate and presumably subspecific.

My assignment of *O. definita kendalli* Riotte to the synonymy of *detrita* was correctly indicated as "new synonymy." Riotte did synonymize it earlier, but to *leucographa*, which is a synonym of *leucostigma*. The 1976 and 1977 papers he mentions as being omitted were received too late to be considered, but the 1973 paper on *O. gulosa* and *O. cana* was for some reason missing from my file and really was overlooked. On reviewing these papers now, however, I find that they would not have changed anything taxonomically. In the last mentioned Riotte refers to the types that he figured of *gulosa* and *cana* of Henry Edwards as "Typus" and "Holotypus" respectively. Actually they were only syntypes, each name having been based on more than one specimen without a holotype mentioned. Inasmuch as Riotte faults me for neglecting pupal characters, it might be of interest to note that in the 1973 paper cited, p. 135, he illustrates pupae of *gulosa* and *cana* in such a way that one is left to assume that the very great differences aparent between them are of specific significance; but they are of course female and male respectively. This is not explained.

In choosing between the two simultaneously published and equally available names, *leuschneri* and *rindgei*, I followed page priority in selecting *leuschneri* as the species name for the box-elder tussock moth. Unfortunately Riotte did the opposite and chose *rindgei* in his 1977 paper which I did not see before my own went to press.

The one oversight pointed out by Riotte that does cause me some embarrassment was my failure to interpret correctly the Latin word "degens" on the label pinned to the type of *O. detrita*. I should have recognized it.

Reference Cited

Riotte, J. C. E. 1971. Eine neue Art der Gattung *Orgyia* (Untergattung *Hemerocampa*) in Nordamerika (Lep.: Lymantriidae). *Entomologische Zeitschrift* 81(11): 105-115.