

Meetings of the Entomological and Botanical Clubs of the American Association will be held throughout the week.

On the whole the coming meeting of the American Association at Washington promises to be one of the most profitable and enjoyable of any ever held under the auspices of that most successful body.

CORRESPONDENCE.

THOUGHTS ON SPECIES.

Sir,—In considering the value of the structure of genitalia in determining species, we come in direct collision with the old unsettled question, "What constitutes a species?" I believe it to be a simple one, when we remember that there are two ways of using the term, the natural and the artificial. All are agreed that there is no such a thing as genera in nature, and I am fully convinced that there is such a thing as species in nature, which is satisfactorily demonstrated by cross breeding; and that the artificial method of defining species is merely a matter of individual opinion, as to how much difference should be considered enough to make a species. Does the fact stated by Prof. Smith, that "in an entire genus all the species will be practically alike," prove that the character has failed? May it not rather go to show that genus to be composed of just so many artificial species, of one natural species, which I am quite satisfied may often be the case, and, therefore, be a valuable proof of its success?

There are no two opinions of the advantage to be obtained from a reliable test of species. Prof. Smith expresses confidence in the genitalia up to a certain point; given his experience, we might have the same. One of his published investigations convinced me that there must be a great deal in it.

In *Entomologica Americana* for August, 1890, dealing with the *Scopelosomas*, he says, "*Moffatiana* is closely allied in colour and maculation to *Grafiana*, so closely, indeed, that for a long time they were considered identical. * * * The genitalia of *Moffatiana* are of an entirely different type from the other species of the group. * * * This strong difference in species otherwise so nearly allied is remarkable." As I know the history of how they came to be separated, I will give it as confirmatory evidence to the value of that method.

In that famous entomological year, 1877, I took *Scopelosomas* for the first time; they were in great abundance. From the latter part of September to the first day of December I took about 800 moths, mostly *Scopelosoma* and *Lithophane*, the bulk of the *Scops.* being of that form now known as *Moffatiana*. I had noticed a difference in the depth of shading in the yellow ones, but thought it the result of age and exposure.

In November I visited Mr. Grote in Buffalo, taking with me representatives of my recent captures, and received from him over a dozen names of *Scops.* and *Liths.*, and amongst them *S. Graefiana*. In following years I observed that the yellowish form was just as fresh as the reddish one, and that in some localities one would greatly outnumber the other, and I began to suspect that we might have in these forms different moths. About this time Roland Thaxter, who is now, I understand, entitled to the prefix of Dr., opened communication with me, with a view to exchange; to him I expressed my suspicion, and sent to him an example of the light form as being least abundant with me, and received the reply, that he saw no difference in it from those he took. I then sent him the reddish form; he expressed delight, never having seen the same before, and enquired if Mr. Grote had seen it. I told him that I had got the name from just such specimens.

I supplied him with a good series, and he went into communication with Mr. Grote about it, and it seems with some difficulty succeeded in persuading Mr. Grote that it was deserving of a separate name. And now Prof. Smith, by the examination of the genitalia, finds them widely apart. I, by observing their habits, had suspected this might be the case, but could not prove it, whilst from appearance alone Mr. Grote had failed even to suspect it.

As resemblance is not always proof that they are one, so the lack of it is not a demonstration that they are separate. In the early part of 1890 I had an opportunity of examining an extensive series of *Lithophanes* in the collection of Capt. Geddes, Toronto. I could arrange in line 30 or 40 *Disposita*, *Petulca*, *Ferrealis*, *Signosa*, *Bethunci*, in such a way as to make it appear impossible to tell where the separation should be made. What verdict would the genitalia give in this case? I would expect it to be in favour of their being artificial species of one natural species; yet it may not, but suppose it did? let no one think that I would favour the obliterating of a single name.

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