

The International Lepidoptera Survey

newsletter

March 2000



Phyciodes batesii maconensis survey

TILS will be working for the US Forest Service again this June to assess the status of *Phyciodes batesii maconensis* in areas with known gypsy moth infestations in western North Carolina and northeast Georgia. This will help the Forest Service determine their 2001 plan of attack against the gypsy moth in these areas. The Forest Service is in the tough position of trying to eradicate these moths without harming the rare indigenous Lepidoptera in the infested areas. (*P. b. maconensis* was discovered by *TILS* member Jeff Slotten of Gainesville, Florida, and described in *TTR* 1:3.)

The purpose of this survey is to 1) determine if *P. b. maconensis* inhabits these areas, and 2) identify any suitable *maconensis* habitat in these areas. If you would like to help in this survey you should contact the survey coordinator, Ron Gatrell, at our Goose Creek, SC address very soon. *TILS* will train field personnel in the identification of the larval host, *Aster undulatus*. Participants must be experienced field workers and will be paid at least \$100 a day plus expenses. This will be hard work, long days, and involve a lot of off trail hiking into remote areas. This is not a collecting trip, however, voucher specimens must be collected. Sight records will not be accepted. Thus, anyone who is against the “killing” of butterflies will be of no help.



Neonympha helicta dadeensis update

We have received several letters recounting various interesting field observations for *Neonympha helicta helicta*, *N. h. dadeensis*, and *N. areolatus*. The following colorful field account was sent in by *TILS* member Frank Rutkowski of Jersey City, New Jersey.

Your *Neonympha* paper brought an old memory into focus. I think that *N. helicta* probably occurs on Big Pine Key, Florida. From the 1960's up to mid 1980 I made a few trips there. On 4 August 1967 George Avery took me to the Watson Hammock (an old Indian midden) on the west coast of that island. To get there we turned north off US 1 onto 940. When we parked at the edge of the piney woods and started walking west, I did not notice whether this was north or south of the road which crosses 940... the one that leads to No Name Key. On nearly bare limestone, with sparse grasses under the pines, I found a few *Calephelis virginiensis* settling on the grass and stone, and collected some. In response to my questioning, George told me that the dominant Composites there were *Flaveria linearis*, *Melanthera parvifolia*, and *Mikania bataatifolia*. However we found none of these plants then.

We walked through an area with Prickly Pears and then entered the hammock. After passing through big Strangler Figs we came into a copse of large Buttonwood festooned with a few bromeliads. The trees were so large that we seldom had to stoop. We must have been quite near the coast because we saw big Manchineel there too. (By now it was 4 PM and I was coming down with heat exhaustion, having arrived only 2 days earlier and having tried --- characteristically --- to do too much too soon.) At the edge of the hammock we suddenly came upon a wide open place with tall “sawgrass” or tussock sedge or something like. Above this, back-lit, 2 or 3 individuals of what I thought to be *N. areolata* were flying away from us but “higher, faster, and straighter than *areolata*” (in your paper). Though the behavior certainly was odd, the presence of the butterfly did not surprise me too much after finding the *Calephelis*. The satyrids probably were resting in the dappled light/shade of the hammock edge until we flushed them. I was too unwell to pursue them. These recollections are fortified by notes written the next morning.

I gave one or two of the *Calephelis* to Cyril dos Passos. The rest are in my former collection which is now at Yale Peabody Museum. These might be worth looking at. I hope that the people there are helping you in your good work. May these longwinded reminiscences be of some use.

In a subsequent letter Rutkowski wrote.

I was so interested in your *Neonympha* diagnosis that I neglected to look up *areolata* in Smith/Miller, who cited Kimball's listing of H.L. King's record from Big Pine Key...

Gatrelle was aware of the records of *areolatus* from Big Pine Key, Lignumvitae Key, and Key Largo in various publications. However, the only positive *helicta* record from the keys known to Gatrelle is Koehn's Key Largo record, which Koehn considered a stray. Gatrelle personally traversed the Keys extensively in the late 1960's and 1970's but never encountered any *Neonympha* (Gatrelle was a Florida resident in the late 60's).

Minno and Emmel figure (under the name *areolata areolata*) a *helicta dadeensis* female in their *Butterflies of the Florida Keys* (1993) from mainland Dade County. (This specimen is most probably a topotype.) The picture of this specimen is not very sharp, however the HW median and marginal lines clearly go to, and remain separate at, the costal margin – typical of *N. helicta dadeensis*. Since their book is about the butterflies of the Keys, it is unfortunate that they did not figure an *areolatus* from there.

Kimball's figure 17 on plate 1 is a typical male of *areolatus* (misidentified as a female in the caption). This specimen is from Daytona Beach on Florida's east central coast.

TILS member Alexander Grkovich of Peabody, Massachusetts sent in this interesting piece of information.

I collected a worn specimen of *Neonympha*, which strongly resembles the *helicta dadeensis* you illustrate in Figure 13, in deep grass near the edge of a lagoon at the Royal Palm Hammock, Dade County, on September 25, 1977. The butterfly was observed flying in the grasses about 18 inches above the ground, as opposed to flying above the grasses. Despite the noted differences in appearance, at the time I thought I had taken an *areolatus*. I searched the grassy area for others but did not see any. I revisited the same area during June 1993 and found to my regret that the habitat had been eliminated, as the grassy areas around the lagoon had been cleared entirely and filled in with sand.

Quite frankly, I was alarmed at the relatively low numbers of butterflies in general which we saw in southern Florida at that time. I understand that spraying for mosquitoes had been extensively undertaken in southwestern Florida in the vicinity of Naples and elsewhere, and that along with all the developments in the area appear to have taken its toll. I had hoped at the time that perhaps we had visited the area during a period when many species were in between successive broods of adults, as the number of adults of various species had been quite good during my previous visit to south Florida in December of 1990.

I have observed and collected *areolatus* at the Croatan Forest, Craven County, North Carolina [central NC coast] during late May and early June, 1977. The only specimen I have is similar to the specimen in Figure 1.



Anthocharis midea

Grkovich also sent in this valuable information on the range and flight cycle of *A. midea midea* along the central coast of North Carolina.

I have had experience in the field with *Anthocharis midea* at three localities: 1. At the Croatan National Forest, near New Bern, Craven County, North Carolina along Route 70; 2. At Jackson, Madison County, Tennessee; and 3. At the Shawnee State Forest, Route 125, Scioto County, Ohio. I am planning to search for the butterfly in Connecticut and southwestern Massachusetts this spring.

I first encountered *midea* in open southern pine forest near the picnic area of the Croatan Forest during the first and second weeks of April 1977. The species was flying in the company of *P. marcellus* and *palamedes*, *H. sosybia*, *C. gemma*, and various *Eurema*, etc. I noticed immediately the rather large size of the butterfly, the extensive orange flush in the males at the front wing apex extending to the black spot at the end of the cell, the occasional yellow flush at the front wing apex in females, and the yellow-orange hindwing flush on the dorsal surfaces of both. The female specimen which I have, resembles the specimen you illustrate in

Figure 7 of the Report, with the exception that there is yellowish gray shading at the apex instead of yellow. These observations would seem to be evidence either of the extension of subspecies *midea* up into the central North Carolina coastal area or of the existence of a cline between subspecies *midea* and *annickae* in this area. The species is double-brooded at that location with a second brood which I observed on the wing during late June and early July.

This is indeed evidence that *midea midea* extends north to the central coast of North Carolina. Further, if the occurrence of a second brood is an annual event at this location, this is of great evolutionary significance for subspecies *midea*.

The *Southern Lepidopterist News* recently published an inaccurate article (21:3, pg. 45) attributing both *A. m. midea* and *A. m. annickae* to Texas. The only subspecies found in Texas is *A. m. texana* as described in *TTR* 1:1. We strongly suspect that neither the author nor the S. Lep. editor had read dos Passos and Klots 1969 paper on *A. midea*. For if they had, this article would surely not have been written or published.

Concerning Texas *midea*, dos Passos and Klots are quoted as follows on page 3 of *TTR* 1:1. “The great phenetic similarity of the populations in Texas and in the northeast (*annickae*) is by no means evidence that they are genetically so similar that they should be considered subspecifically congruent. To do so would, in fact, contravene everything that is now known about the evolutionary differentiation of populations on the specific and subspecific level during periods of spatial isolation from each other.”

Dos Passos and Klots made it perfectly clear that all of the *midea* populations between Georgia and Texas, and from the Gulf coast to the northern Midwest limits of the species range are polytypic and NOT ASSIGNABLE TO ANY SUBSPECIES. All populations within that area are part of a vast BLEND ZONE between subspecies *texana* and *midea*. Within this geographic area, individual specimens will be found which LOOK LIKE but ARE NOT any SUBSPECIES. Gatrell’s *TTR* 1:1 article is an addendum to dos Passos and Klots 1969 paper. Which means that it can not be understood outside the context of their research. In other words, *TTR* 1:1 must be considered in conjunction with dos Passos and Klots 1969.

What Doyle found was not subspecies *midea* in east Texas, but a semi-*midea* looking (no magnification is needed to “see” the orange patch on the HW of nominate *midea* males) blend zone specimen (or population) east of the range of *A. m. texana*. Further, the specimens he referred to as Texas *midea annickae* are not *annickae* but the phenosyncronic subspecies *texana*. According to dos Passos and Klots, it is evolutionarily impossible for *midea midea* or *midea annickae* to ever be found in Texas!

Scudder’s quaiapen



Dr. Fred Rindge of the AMNH, New York, wrote Ron Gatrell concerning several aspects of *TTR* 1:10. Dr. Rindge’s comments (and Xerox copies of various papers) were very welcome and helpful. In particular, Fred was curious about the status of Scudder’s name *quaiapen* since Ron did not say anything about this name in his description of *Hesperia attalus nigrescens*. Since others undoubtedly have the same question, here are Ron’s comments on Scudder’s *quaiapen*.

The term “form” was often used by the early Lepidopterists to mean subspecies. Scudder (1889), in coining the name *quaiapen*, employs this term. This is probably why Miller and Brown in the Lep. Soc. check list place *quaiapen* as a subspecific synonym. However, it is clear from Scudder’s text that he (correctly) introduced/limited, the name *quaiapen* as/to a “dimorphic form” of the female of eastern US *attalus*. Which at the time, had not been subspecifically described!

Scudder called *attalus* (not just *quaiapen*) a “Carolinian species.” In using this phrase he was simply saying that the Southeastern US was the primary place of abundance of the species range wide (which he noted as being from Texas to Iowa to New England to Florida). He was not trying to say that North or South Carolina was a type locality. There is no type locality or type specimen for/of form *quaiapen*. Although one year older than the name *slossonae*, *quaiapen* does not have priority over *slossonae* (subspecifically) because the name was very clearly introduced as simply a normal dimorphic form of an existent subspecies. Thus, *quaiapen* is misplaced by Miller and Brown on two levels. First, it should be listed as a form not a subspecies (= ♀ f. “quaiapen”) and second, it should be listed in the synonymy of *slossonae* not *attalus*.

Form *quaiapen* does not occur in the western subspecies *H. a. attalus*. In subspecies *H. a. slossonae*, *quaiapen* is a frequent female form. In subspecies *H. a. nigrescens*, it is the only form of the female.



Mississippi Deciduphagus henrici

Ricky Patterson of Vicksburg, Mississippi sent a long series of Mississippi *Deciduphagus henrici* to Ron Gatrell for subspecific determination after the publication of new subspecies *yahwehus* in *TTR* 1:6. In the original description Gatrell stated that the range of *yahwehus* extended “..(evidently) west to Mississippi.” After examining patterson’s specimens Gatrell reports the following conclusions.

All of the specimens sent to me by Ricky Patterson were clearly subspecies *turneri*. The presence of this subspecies in most (if not all) of Mississippi is another example of how strongly the Mississippi fauna is influenced by western Lepidoptera. It now seems likely that subspecies *yahwehus* does not occur in Mississippi. We should now look for a tension zone where the descendants of the Texas and Floridian refugium have met. I expect this to occur in central coastal Alabama. It is unfortunate that I was unable to collect any of the *henrici* I observed in coastal Baldwin County, Alabama back in 1968.

The Taxonomic Report

Publication update

Several researchers have inquired about publishing their findings in Volume Two of *TTR*. So far four papers have been committed to *TTR* for 2000 but these will not be ready for publication until late in the year. There is much to be published on moths, but we have not yet had any moth research submitted to us for publication. We hope this will change in the near future.

Shortly after the publication of *TTR* 1:10 Charles Bordelon of Beaumont, Texas contacted us to encourage and assist us in the further research of the Texas population of *Poanes aaroni*. Charles is the discoverer of the Texas (Gulf Coast) segregate. Ron Gatrell will be authoring the paper. Several collectors have come forth to assist in this project. Roy Kendall has informed Charles that he hopes he and Terry Doyle can work out the life history of this segregate which is sympatric with *Euphyes bayensis* from Mississippi to Texas.

Review policy

We have made it clear from the beginning that our articles are processed via internal and/or external review. Yet our review policy continues to be misrepresented by some individuals. An example of this is the statement at the end of the *TTR* announcement in the Lep. Soc. News (41:2 pg. 53) by the editor in which he states that *TTR* “is not ... a peer-reviewed publication.” In 1:5 and 1:6 the external (Ph.D.) reviewers were clearly listed. Thus, the editor either 1) did not read the articles sent to him and misspoke or 2) based his comment on hearsay. In other issues, the fact that a reviewers name was not mentioned in an article does not mean that review did not take place.

Further, even if there was **no** review, that would not invalidate the published research under the ICZN Code. The most basic rule of science is that of the free expression of thought. Scientists have the right to make such unilaterally ridiculous contemporary statements as “the world is round, not flat.” True peer-review only comes with (much) time. We scientists have always operated in clicks. (This is usually dignified by the term “school of thought.” But it is still just a click.) To any given click, the only review that really counts is their own.

By summarily discounting our internal review process, some might render *TTR* as a partially peer-reviewed publication. However, anyone who flatly states that *TTR* is a non-refereed publication is very uninformed. To some people, nothing we have done, or will do, will be considered acceptable regardless of the method(s) of review we use. This is best evidenced by the recent rejection of *Celastrina idella* Wright and Pavulaan (1:9) by the *Committee on Scientific Names of North American Butterflies*. *Celastrina idella* Wright and Pavulaan, is as fine a piece of refereed lepidopteran science as there is.



ICZN Position

We have been in informal correspondence with the *International Commission on Zoological Nomenclature*, London, regarding some of the “unreviewed” co-authored proclamations of the *Committee on Scientific Names of North American Butterflies*. The ICZN has written us that **all** of the specific and subspecific names we have published in *The Taxonomic Report* are **available and validly published**. The ICZN has informed us that this Committee is free to put forth what ever opinions they want. However, the ICZN has also informed us that everyone else is just as free to ignore those opinions.

Further, an ICZN spokesman has informed us that our publication of *Chlosyne ismeria* as the binomial with *C. i. nycteis* as the trinomial is valid under Chapter 6 of both the old and new Code. According to article 23 of the Code, our valid publication of *ismeria* and *nycteis* as sister subspecies dictates that the names be used as *C. i. ismeria* and *C. i. nycteis* because the condition of 23:9.1.1 is not met. However, even if both 23:9.1.1 and 23:9.1.2 were met, workers would only have the option of using (23:9 Example) either the combination as *C. ismeria ismeria* or *C. nycteis ismeria* – by the rules, the suppression of *ismeria* is not an option. The *Committee on Scientific Names of North American Butterflies* does not have the authority to officially suppress **any** scientific name. Only the ICZN has that **official** authority.

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