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A Comment on Friedlander's *Asterocampa* (Nymphalidae, Apaturinae): Designation of Neotypes for *A. celtis* and *A. clyton*

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A COMMENT ON FRIEDLANDER'S *ASTEROCAMPA* (NYMPHALIDAE, APATURINAE): DESIGNATION OF NEOTYPES FOR *A. CELTIS* AND *A. CLYTON*.

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ABSTRACT. Friedlander's 1986 [1987] treatment of the southeastern U.S. taxa of *Asterocampa* is examined. Neotypes of *A. celtis* and *A. clyton* are designated from Burke County, Georgia to help stabilize the status of these taxa and aid in any future research. *A. celtis* is currently known to exist within at least 40 km of *A. c. reinthali* in Georgia. Their proximity, without integration, suggests evolutionary distance, perhaps even speciation. It is proposed that *A. celtis* is descended from *A. c. alicia,* and *alicia* from a Mexican refugium. *A. clyton* and *A. c. flora* intergrade in eastern Georgia and are subspecific.

Additional key words: cryptic subspecies.

UNSTABLE TAXONOMY

Up to 1975, *Asterocampa* was usually thought of in North America as two or three species with five to seven subspecies. From 1975 (Howe *fide* Reinthal) until 1986 (Scott *fide* Friedlander) *Asterocampa* was presented in the literature as consisting of twelve North American species with only one subspecies. In 1987, Friedlander's revision of the genus left us with four species having six subspecies.

When I received number 25:4 of *The Journal of Research on Lepidoptera* in early 1988 (the date of publication is Dec. 31, 1987) devoted entirely to Friedlander's revision of *Asterocampa,* I immediately sat down to read it. I then spent the next several weeks comparing it against the literature and specimens in my collection. I, like many others, found his treatment much more acceptable than the hyper-splitting of everything into distinct species that had existed for a decade. However, it seemed to me, then and now, that his criteria for sinking taxa into subspecies or synonymy was not consistent and somewhat extreme in the opposite direction. Further, while he properly affirmed the type locality of *Asterocampa celtis* (Boisduval & Le Conte) to be Screven County in the lower coastal plain of Georgia, it seemed very risky to me that he did so without examining any topotypical specimens. Why?

All of the Georgia *A. celtis celtis* he examined were from locations relatively far from the type locality. Four locations were in the Georgia piedmont, one was montane, and two (Decatur County in southwestern GA and Chattahoochee County which borders Alabama) were in the upper coastal plain. The closest specimens he had to the type locality were from Bibb and Clarke counties, which are both over 170 km away. All of the specimens he had seen from the counties just to the east of the type locality (in both Georgia and South Carolina) were his new subspecies, *A. celtis reinthali* Friedlander, and were only 40 km from the *celtis* type locality. Without specimens, how could Friedlander *know* that what occurred in nature in Screven County was not *reinthali* too? The answer is he couldn't.

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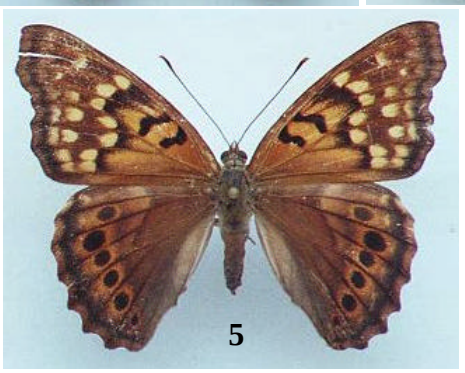

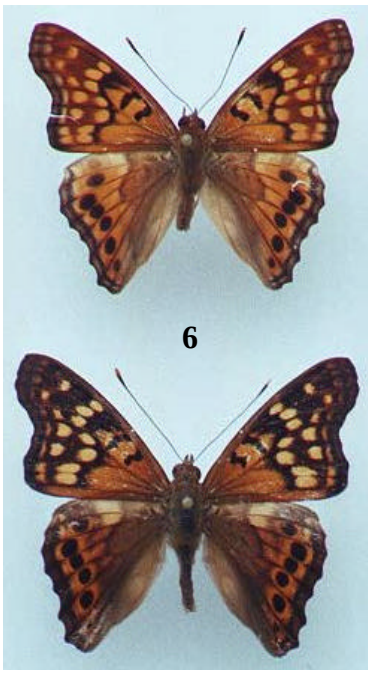

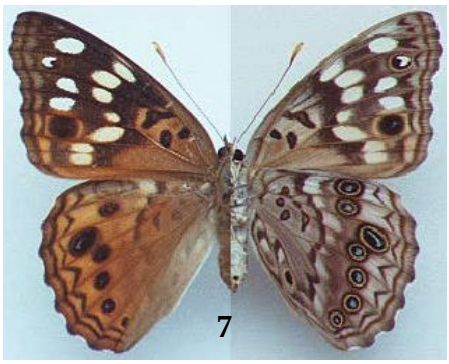

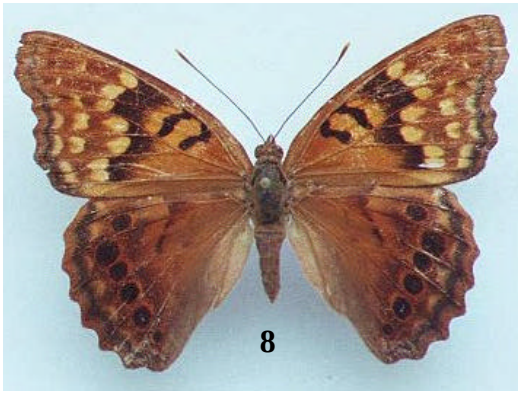

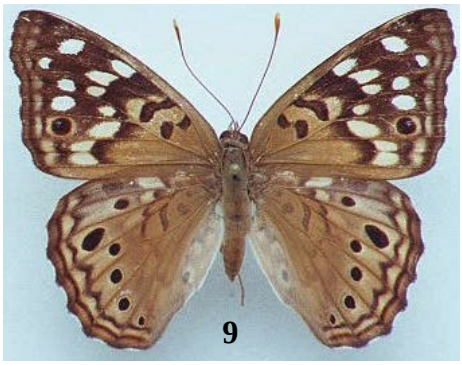

FIGS. 1-10 eastern U.S. *Asterocampa*. **1,** ♂♂ *A. celtis reinthali.* Top: 3 July 1980, Gainesville, Alachua Co., FL; bottom: 6 Sept. 1992, Goose Creek, Berkeley Co., SC (ventrals) **2,** Top: **Neotype** ♂ *Apatura celtis celtis*, 6 Aug. 1998, at Savannah River, Burke Co., GA; bottom: same as 1 bottom (ventrals). **3,** Dorsal surfaces of specimens in figure 2. **4,** Top: same as 2 top; bottom: ♂ *A. celtis celtis*, July 1960, Zering, Story Co., IA (dorsals). **5,** Topotype ♀ *A. clyton clyton,* 6 Aug. 1998, at Savannah River, Burke Co., GA. **6,** Top: ♂ *A. clyton flora*, 21 July 1992, Gainesville, Alachua Co., FL; bottom: **Neotype** ♂ *Apatura clyton clyton*, 6 Aug. 1998, at Savannah River, Burke Co., GA (dorsals). **7,** ♀ *A. celtis reinthali,* 4 Sept. 1992, Goose Creek, Berkeley Co., SC. (drosal & ventral). **8,** ♀ *A. clyton flora* (dark form), 9 Sept. 1991, Newnans Lake, Alachua Co., FL (dorsal). **9,** ♀*A. celtis reinthali*, 1 July 1980, Gainesville, Alachua Co., Fl (dorsal). **10,** ♀ *A. clyton flora* (light form) 24 July 1992, Gainesville, Alachua Co., Fl (dorsal).

Friedlander accepted Abbot's paintings as being adequate to represent the type. However, Abbot's artistic renditions can not be trusted as being so accurate, either to phenotype or place of origin, as to be equivalent to actual specimens – especially type specimens. (To me, the elevation of the artistic products of the early workers on Lepidoptera as being definitive scientific types is equivalent to accepting prehistoric cave paintings as being on a par with the fossil record.)

If it could be demonstrated, through a series of specimens from Screven or Burke counties, that what occurred at the *celtis* type locality was the *reinthali* phenotype, then *reinthali* would have to be dropped into synonymy and *celtis* would become the name applied to his new south coastal South Carolina to southern Florida subspecies. This would also mean that *A. alicia* (W. H. Edwards), as the next oldest name, would have to be applied to the inland and northern subspecies long know as *celtis*. If Friedlander was wrong about what phenotype (subspecies) existed in Burke and Screven counties, he had created a taxonomic mess. As it turns out, he was correct.

LOCAL OBSERVATIONS

I had been collecting and observing *Asterocampa celtis* and *A. clyton* (Boisduval & LeConte) in south coastal South Carolina for 18 years when Friedlander's paper came out. All I had ever encountered in this area was his *reinthali* phenotype. Dr. Richard T. Arbogast had found only *reinthali* and an occasional *A.clyton* with a *flora* (W. H. Edwards) facies in his many years as a resident of Savannah, Georgia. Charleston was further north than Screven County. In Friedlander's study, the *reinthali* he examined from coastal Georgia were four times closer to *celtis'* type locality than any *celtis* he had seen from Georgia. Thus, I felt there was a strong possibility that what occurred in Burke and Screven counties was a nondescript blend zone population, or perhaps the coastal *reinthali* – but not *celtis celtis*. So I set out to collect and rear both *A. celtis* and *A. clyton* from the south coastal area of South Carolina, and to collect topotypical specimens of *A. celtis* and *A. clyton* to compare against Friedlander's position.

From 1988 to 1995, I netted hundreds of adults and reared about 3,000 specimens of both of these species from Berkeley and Charleston counties SC. (Nearly all netted and reared specimens were released.) All local *celtis* were *reinthali* (Figs. 1-3 bottoms, & 7) and all *clyton* were *clyton clyton*. Random *reinthali* larvae were checked for antler scoli AB5. All keyed out with this and the other *reinthali* larval characters set forth by Friedlander. *Celtis laevigata* Wild. was the local host of both *A. c. reinthali* and *A. clyton.*

Of anecdotal interest, *A. c. reinthali* eclosed after midnight and before dawn. If a light was on in the room, newly emerged adults (especially females) would often come to the light. Perhaps they were fooled into thinking it was dawn and moved into the light to make themselves more available to males (this may be an explanation for Friedlander's question at the bottom of page 240). For those who find wild *Asterocampa* at light, they should note if the females are usually fresh and the males old. If so, this could indicate early morning mate locating behavior.

In August of 1998 I located sizable populations of topotypical *A. celtis* (Figs. 2-4, tops) and *A. clyton* (Fig. 5 & Fig. 6 bottom) near the Savannah River in Burke County, Georgia and Allendale County, South Carolina. All the *clyton* were *clyton clyton* and, to my surprise, all the *celtis* were identical to specimens of *celtis celtis* I had collected as a teenager in my home state of Iowa (Fig. 4 bottom)! I had expected to find a *reinthali/celtis* intermediate at the type locality. And indeed, if we were dealing with a clinal subspecies, that is what **should** have been found in this area.

These A*sterocampa* were in association with *Celtis occidentalis* var. *georgiana* (Small) Ahles. Both males and females were found of *A. clyton* but only males of *A. celtis*. Both species were very common and about two dozen of each were netted and examined. (*C. occidentalis* var. *georgiana* does not occur along the coast where *A. c. reinthali* is found. I did not observe *C. laevigata* in the area of the *A. celtis* colonies, but it probably occurs in the vicinity. To this point in time, these plant associations only indicate occurrence and do not necessarily indicate, or eliminate, regional host specificity.)

POSTULATING A CONCLUSION

Specimens, adults and immatures, from southeastern Florida and south coastal South Carolina (700 km apart) are phenotypically (genetically?) the same – *reinthali.* Specimens, at least adults, from the warm climate of east coastal Georgia and the cold climate of Iowa (1,500 km apart) are phenotypically (genetically?) so similar as to be virtually the same – *celtis.* Yet, with only one county between them (40 km), these *celtis* and *reinthali* exist in nature in eastern Georgia as very distinct phenotypes, *with no known intermediate populations or specimens from this area*. Friedlander only "presumed hybridization" in this area (page 245.)

It is strikingly odd that specimens of a single species should be so subspecifically related on both a distant *and* proximate geographical basis. Add to this: the aspect that these *reinthali* populations along the coast may be host specific to *C. laevigata,* and the Burke/Allendale county *celtis* populations may be host specific with *C. occidentalis* var. *georgiana;* the morphological differences in larval characters; and enough circumstantial evidence is accumulated to reasonably indicate speciation rather than subspeciation. This would be considered weak evidence by some, perhaps many, but we are only postulating here. We are simply giving probable cause to keep this research open until the subspeciation question can be absolutely concluded one way or the other. It should be noted that *Anthocharis midea* (Hübner) and *A. m. annickae* dos Passos & Klots function as distinct subspecies in the exact same area.

I propose that the taxon closest to Georgian *A. celtis* is the Gulf Coast endemic *A. celtis alicia* and not *A. c. reinthali.* In noting the differences between *alicia* and the Floridian population, Friedlander not only named the Florida population (*reinthali*) but sank *alicia* into *celtis* synonymy. In my opinion, this is unfortunate. However, this is consistent with the rest of his revisional rationale (pg. 232). I consider *alicia* a valid subspecies with the same basic Gulf Coast range as *Basilarchia archippus watsoni* dos Passos. At first glance, *alicia* merely looks like a very large *celtis.* And indeed their markings are about the same, with one important exception. I have only seen a few topotypical *alicia,* but even in this small series they show a strong tendency to have an additional partial dorsal FW eyespot in cell M_3. This character was not mentioned by Friedlander.

The evolutionary significance of this tendency should not be minimized. This single feature in *alicia* would seem to tie southeastern *celtis* together with the western *A. c. antonia* (W. H. Edwards) and its many allies, which are double or partially double eyespotted entities. *Alicia* is positioned, both geographically and phenotypically, as the "link" connecting western and eastern *celtis.* I agree with Friedlander, that *A. c. reinthali* is a taxon ascended from the Florida refugium. However, I see *celtis,* by way of *alicia,* as descended from the Mexican/Texas refugium. (Friedlander, in his prepublication review of this paper, agrees that this hypothesis of the origin of *celtis* is plausible and warrants further investigation.)

The *celtis – reinthali* taxonomic question seems three fold. First, while the *celtis* and *reinthali* ancestors were separated in different refugium, did they evolve into close but separate species which may now be sympatric in some part of Georgia or South Carolina? Second, are *celtis* and *reinthali* just subspecies in unusually close proximity simply because *celtis,* at the terminus of it evolutionary tract, has now abutted a long disjunct relative? If so why are they not interbreeding? Third, are they subspecies that due to evolutionary distance, say by host specificity, now cryptically act as species in this area? Until one of these, or some other option, is proved correct by collecting and/or breeding experiments, *reinthali* and *celtis* may correctly be thought of as separate species – the status they had before Friedlander's revision. The status Dr. Reinthal held to be correct. However, I am by no means embracing specific status. I am saying that the exact relationship of these two is still in question.

The situation with southeastern *A. clyton* is not complex. Occasional specimens from Savannah, Georgia I've seen in the collection of R. T. Arbogast tend to have features characteristic of both *flora* and *clyton.* Burke County *clyton* seem to be solidly *clyton.* All south coastal South Carolina *clyton* I've seen from Charleston, Colleton, Dorchester and Berkeley counties are true *clyton.* Thus, a blend zone between

clyton and *flora* can be observed in north coastal Georgia. Further, there is a dark female form of *flora* (Fig. 8) that is very similar to the light form of female *clyton* (Fig. 5).

Neotypes are definitely called for here. Accordingly I have designated a male of *A. celtis* and *A. clyton* each from Burke County, Georgia as neotypes. They have been deposited in the Carnegie Museum of Natural History, Pittsburgh, PA. They are labeled as follows: NEOTYPE *Apatura celtis,* 6 August 1998, at Savannah River, Burke County, Georgia (Figs. 2-4 top); NEOTYPE *Apatura clyton,* 6 August 1998, at Savannah River, Burke County, Georgia (Fig 6 bottom)

WESTERN SUBSPECIATION

A. leilia cocles (Lintner), *A. celtis montis* (W. H. Edwards), and *A. clyton subpallida* (Barnes & McDonnough) were considered species by most writers in the 1970's and 80's. Today, via Friedlander, they are not even considered valid subspecies. I have all the western taxa in my collection and have reared *subpallida*. They seem to be perfectly good subspecies to me. I think this bears a second look. I would hope some western researchers would revisit this situation. Bailowitz and Brock (1991) had the best opportunity to reexamine *montis* and *subpallida* but chose to strictly follow Friedlander.

I am not sure if the criteria Friedlander used in defining his western subspecies (pg. 232) was applied in the same way to his eastern subspecies. However, based on his review comments of this paper, I think many may have read a finality into his *Asterocampa* revision that he absolutely did not intend.

He and I are certainly in agreement that, "very little is written in stone about *Asterocampa.*" His lumping of western taxa was not an indication of a taxonomic simplicity or finality on his part. It was just the opposite. It was (is) the complex relationships of the western populations, coupled with his conservative approach, in the face of the unknown, that resulted in what he produced on paper. Let me relay a little of what he has written to me:

> The rest of the iceberg is those western populations I sunk under the name of *A. celtis antonia.* There are dozens of distinctive, geographically isolated *antonia*-like *A. celtis* in the West, and I don't have a clue about their phylogeny. I can guess that *A. celtis celtis* arose from their ancestors, but how are the *antonia*-like *A. celtis* related one to another? Some may be on the line to *A. celtis celtis,* some may be persistent ancestors, some may be on entirely different lines of evolution. It will become useful to have separate names for them only when we have sufficient data to do the phylogeny work. For now we have no reason to believe that *A. celtis antonia* in New Mexico is a single entity, nor do we know how *A. c. antonia* from Arizona, Colorado, Texas, Oklahoma, Mexico, etc. relate to them – better to wait so the critical research can be done, and I think it will have to be molecular. So, if you need a name, the *A. celtis antonia* population from "X" will do, but be sure to voucher your material! In this sense, vouchering is as important as naming, maybe more so, for it's clear we need genetic material vouchered, whereas defining types is still largely morphological.
>
> I sunk *subpallida* because it is the Arizona version(s) of *texana,* both of which probably are not single entities and require further investigation. I almost named subgenera for the two groupings, but of what use? They are distinctive enough to have come over from Asia separately, then separated into tropical/temperate pairs.
>
> To sum up, I guess I'm emphatic that subspecific names not be raised or given until the critical research is done to explain the phylogeny of the populations. I'm not debating the distinctiveness or isolation of the western populations – they will all eventually need formal names, if they survive extinction. But there are many, many more such populations than *subpallida, montis, cocles,* and the one *A. celtis* I left unnamed in Mexico (and *A. clyton louisa).* But if it helps to have a name [to provide protection to taxon], resurrect it now! I'd prefer to wait until the phylogeny is done right.

Two things are clear from Friedlander's remarks. First, much more research needs to be done. Second, the last word on *Asterocampa* subspeciation has not been spoken. My reason for writing this paper is to stimulate more research on the *Asterocampa.* I feel some of the named taxa were sunk prematurely. Why sink a subspecies we know will one day be validly resurrected just because it needs to be defined in a more accurate way? If some of the demoted taxa are not truly synonymous with (the same thing as) the subspecies they were placed under, then the only thing accomplished in sinking them was the replacement of an inaccurate subspeciation with an inaccurate synonymy.

If *Philotes sonorensis extinctis* Mattoni is a valid subspecies in half a canyon, and *Icaricia icariodes missionensis* (Hovanitz) on half a hill (with *I. i. pheres* (Boisduval) at the bottom of the hill), then *A. c. subpallada* and *A. c. montis* can be valid in half a state (this sentence contains hyperbole but it makes the point). As I stated, I hope someone from the area of these to-be or not-to-be Western *Asterocampa* subspecies will further research the question. It seems Tim Friedlander feels the same, though for different reasons.

ACKNOWLEDGMENTS

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