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LOS LEPIDOPTEROS ARGENTINOS: SUS PLANTAS HOSPEDADORAS Y OTROS SUSTRATOS ALIMENTICIOS. By Jose A. Pastrana. 334 pp. ISBN 987-21319-0-2. \$40 US to purchasers in Mercosur countries; \$70 to SEA members, otherwise \$80 US elsewhere. South American Biological Control Laboratory USDA-ARS and Sociedad Entomologica Argentina, Buenos Aires (from whose Web site it may be purchased). Publication date: 2004.

My favorite professor in graduate school was the late William L. (“Bill”) Brown, Jr., who had a gift for telling it like it was. He frequently admonished his students to beware of what he called “validation by frequency of citation”, the process whereby errors become institutionalized by mere repetition. It is a process that is nowhere more common or more deleterious than in the listing of host plants for phytophagous insects. Back in 1983 I published a note in a Mexican journal, identifying several such errors which had crept into the Mexican literature from ours. The worst offenders in this regard are omnium-gatherum compilations, which rarely exercise discretion in evaluating the included material and all too frequently do not trace the source. Sometimes one compilation will incorporate all the dubious material from previous ones, compounding the problem.

In 1972 Arthur Allyn arranged for the posthumous publication of Harrison M. Tietz’s card file of life-history information on North American Lepidoptera, which he had been accumulating during his decades as a faculty member at Pennsylvania State University. By the time it appeared it was 20 years out-of-date, but still valuable. Because it was well-referenced if uncritical, it at least allowed one to trace a variety of howlers that had crept into widespread usage—including some of the ones I figured in my 1983 Mexican article. I still use my well-thumbed and -annotated copy, but often wish someone would publish a detailed critical addenda and corrigenda. I am not holding my breath, and the science has moved on.

Now history has repeated itself in Argentina. Like Tietz, the late Jose Pastrana accumulated a bibliographic file over several decades, but died (at age 87) before it could be prepped for publication. The task of organizing it, standardizing the format and modernizing the taxonomy was shared by several colleagues: Karen Braun, Guillermo Logarzo, Hugo Cordo and Osvaldo DiIorio. They consulted a variety of specialists, including John Brown, Adriana Chalup, Don Davis, Fernando Navarro, Patricia Gentili, Gerardo Lamas (who reviewed the butterflies), Alma Solis and Maria Elvira Villagran. But the task clearly

overwhelmed them, and the result is very much less than satisfactory. Here is DiIorio speaking (my very rough translation):

“When the Catalog of Phytophagous Insects of Argentina was being prepared, we found the unpublished manuscript of Pastrana...How to determine which records pertained to Argentina? The manuscript mentioned the host plants of each species of Lepidoptera without any indication of localities or bibliographic references to the sources....For a certain number of plants the original source was never determined, though one could detect a certain pattern of repetition of data by Pastrana himself...In future editions or addenda we can add the missing information and corroborate the corresponding plant-insect associations.”

In other words, all the usual problems are present here, and more so than in Tietz. And as will become plain, even when sources are documented, they are often inaccessible, so that a real critical evaluation is not possible.

Undoubtedly some taxonomic groups are in better shape than others. Since I work on the Argentine Pierini and have published more life-history and biological information on this fauna than anyone else (virtually none of which is cited by Pastrana! — though my work overlapped his active years), and this group is better-known than most, I have chosen to illustrate the nature and magnitude of the problems by working through the couple of pages devoted to my own little group. My evaluations are based on my own 30 years of work in Argentina, and to save space I will not cite the various pertinent Shapiro publications. What is important is how reliable the data in Pastrana are. If what follows is at all representative...

p.201: The only given host plants of *Hypsochila wagenknechti wagenknechti* (Ureta) are “Asteraceas: *Aplopappus bailahuen*; *Senecio* sp.” The bug is a Crucifer-feeder, and this hoary error is based on old records of nectar sources, ultimately going back to Ureta himself (?).

p.203: *Tatochila autodice autodice* (Huebner): In addition to legitimate hosts (glucosinolate plants, i.e. Brassicaceae and Tropaeolaceae) many other, dubious records are cited: “Fabaceas (*Medicago sativa*) (Berg, 1895, Anonymous 1930, Lizer and Trelles 1941) (Hayward 1969, ex Joergensen, Biezanko 1959, Viana and Williner 1974); Solanaceas: *Cestrum elegans* (Biezanko 1959), *C. nocturnum* (Biezanko 1959), *C. parqui* (Giacomelli 1915, ex Burmeister 1878), (Lizer

and Trelles 1941, Biezanko 1959, Hayward 1969, Viana and Williner 1974); *C. corymbosum* (Berg 1875, Giacomelli 1915, Hayward 1969).

Tatochila autodice blanchardii Butler: Lists only Tropaeolaceae, omitting the perfectly valid records on Brassicaceae hosts.

Tatochila mercedis mercedis (Eschholtz): Oddly, the text lists the distributional records from the Province of Neuquen as “doubtful,” when hardly anything in this book is similarly qualified. But the records are accurate!

Phulia nymphula (Blanchard): “Tropaeolaceae: *Tropaeolum polyphyllum* (Reed, Hayward 1969 ex Reed).” A glucosinolate specialist but apparently confined to plants in rosette growth form, making this exuberant herb highly unlikely.

p.204: *Tatochila orthodice* (Weymer) is recorded from “Brassicaceae”: *Brassica* sp.; *Cheiranthus* (sic) *annus* (sic)(Hayward 1969), *Lobularia maritima* (Hayward 1969); Tropaeolaceae: *Tropaeolum* sp. (Hayward 1969). Despite the high degree of specificity, all of these records are wrong. The true host plant of this species remains undetermined but is almost certainly Fabaceous; it is not a feeder on glucosinolate-containing plants. And *Cheiranthus* are chemically odd, and normally avoided by Pierines.

Tatochila stigmadice (Staudinger): again listed on “Brassicaceae (Hayward 1969)”, again incorrectly.

Tatochila theodice theodice (Boisduval) is listed on Tropaeolaceae, attributed to Giacomelli (1915). It is strictly a legume feeder.

Theochila maenacte maenacte (Boisduval) is claimed to be a Brassicaceous feeder (Biezanko, Ruffinelli and Carbonell 1957; Hayward 1969, from them). It isn't. Again, its true host remains unknown but is suspected to be Fabaceous.

Most of these errors show clear trains of repetition, eventually converging to Hayward (who in his later years committed many errors, some of which I have documented elsewhere) and thence to Pastrana. There is a clear tendency to assume that “if it's a White, it eats Crucifers.” In South America this does not work. The attribution of Brassicaceous hosts to Legume feeders is actually repeated in the entry for *Colias vauthieri* (Guerin), which lists “Brassicaceae (Havrylenko 1949)!” (It also lists alfalfa, which this species does not eat, attributing the record to Crouzel and Salavin 1969.) The very persistent records of *Tatochila autodice* on the Solanaceous plant *Cestrum* are a special problem that needs to be dealt with definitively one way or the other. The chemistry is so outrageously different that the association must be viewed as highly unlikely at best.

This volume is valuable for its huge bibliography of often very obscure references, most of which, alas!, are unobtainable via interlibrary loan services within the

United States (I've tried). (The most obscure ones cited below are just as cited by Pastrana, if you feel inclined to push the envelope of your favorite retrieval system.) If you can't get them, you can just go ahead and cite them like everybody else, and keep the old errors in circulation to continue to confound those of us trying to study the interaction of coevolution and phylogeny! I consider it a sign of Divine intervention that Braby and Trueman (2006) did not consult this porqueria when they compared host relationships to molecularly-inferred Pierid phylogeny. May others with similar objectives do the same!

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