

REBUTTAL TO TERRELL'S TAXONOMIC NOTES OF TURNER'S
TREATMENT OF TEXAN AND MEXICAN *HEDYOTIS*

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ABSTRACT

A vigorous rebuttal to Terrell's evaluation of my treatment of Texan and Mexican *Hedyotis* is presented. It is suggested that populational work will prove pivotal in judging between the merits of the two contrasting nomenclatural systems, this to be performed by unbiased field workers using more sophisticated techniques than those employed by the contestants concerned.

KEY WORDS: Rubiaceae, *Hedyotis*, *Houstonia*, México, Texas, Terrell, Turner

Terrell (1996b) provided a "critique" of my taxonomic treatment of the *Hedyotis nigricans* (Lam.) Fosberg complex (Turner 1995a), and yet other elements of *Hedyotis* occurring in Texas and México (Turner 1995b, 1995c, 1995d). This after an introductory defense of his acceptance of *Hedyotis*, *Houstonia*, and *Oldenlandia*, which classification I did not follow, preferring instead to follow that of Fosberg (1937) and Shinnars (1949), if not other workers, who view *Hedyotis* in the broad sense, treating the several generic segregates as but infrageneric categories. The informed reader will understand that either *Hedyotis* (s.l.) or *Hedyotis* (s.s.) is equally acceptable, unless it can be shown that the former is patently polyphyletic or perhaps paraphyletic, which to my knowledge has not been demonstrated.

The only substantive comment made by Terrell in regard to my paper is that he called to the fore an error or lapse in my key to the varieties of *Hedyotis acerosa* A. Gray in which I inexplicably substituted the name var. "*fasciculata*" for the intended var. *acerosa*. But this is no big deal: any reader could have detected the lapse, and made allowances accordingly.

What Terrell fails to comment upon adequately in his paper is the considerable intergradation between *Hedyotis acerosa* var. *acerosa* and *H. a.* var. *polypremoides* (A. Gray) W.H. Lewis in west central Texas. This was commented upon and mapped in detail by me (cf. Figure 1), but these were crudely remapped and treated by Terrell

as if the taxa were nonintergrading *sympatric* subspecies, although he noted that there was intergradation in regions of overlap, as I also surmised. Since Terrell claims to have done field work over the range of *H. acerosa*, as I myself have, he must know that in the region of intergradation, the populations vary, some having specimens mostly resembling var. *acerosa*, some with specimens mostly resembling var. *polypremoides*. It matters not if Terrell wishes to call these two intergrading taxa subspecies: the latter categories can each house a morphogeographical variety, much as a monotypic subgenus can house a single species. He might *think* that the use of the terms "subspecies" and "variety" are mutually exclusive, but I do not read the *International Code of Botanical Nomenclature* in this fashion, nor should Terrell.

Terrell states (1979, but see his contrary views as expressed in 1996 as noted below) that "the differences between these two entities are on a higher level than the usual variety. In addition, the geographical separation in New Mexico and adjacent lands is clearly marked, despite intergradation in western Texas and northern Mexico." I never denied that the two taxa might not belong to different subspecies: I merely treated the two at the varietal level consistent with the treatment accorded most intergrading infraspecific categorical units by most modern workers (e.g., Cronquist, numerous publications; Turner 1956; etc., cf. Kapadia 1963 for a reasonable review). I recognized four morphogeographical elements under the fabric of *Hedyotis acerosa*, providing a key to these and maps for each. That Terrell might not *think* these populational units worthy of recognition is fine with me, but he has not offered any real data to disprove their reality.

Terrell contends that "the type specimen of *Hedyotis acerosa*, Wright 237 (see Terrell 1996a), was collected in 'Western Texas to El Paso, New Mexico' in 1849 . . ." but he fails to note that this name needs lectotypification, as I clearly pointed out, and that Wright 237 was collected in what is now Kinney or Val Verde counties, Texas (east of the Pecos River) during June of 1849, and Terrell (were he to have looked this up in Wright's published field notes), need not ascribe its type locality as somewhere between "western Texas" and "El Paso, New Mexico [sic]," the latter region a rather meaningless locale, geographically speaking.

Further, commenting upon the veracity of my var. *potosina* B.L. Turner, he contends that the pulvinate low plants from southernmost Coahuila and San Luis Potosí, México, having elongate corolla tubes, are "part of a cline that northward has taller plants with coarser leaves and longer internodes. In southern Texas [a lapse here, he should have said in western or trans-Pecos Texas!], there are collections that are somewhat transitional, with rather fine leaves and small stature." I take the few sheets he cited in defense of this statement to be depauperate or otherwise atypical plants of var. *acerosa*, as occurs in populations everywhere, be these remarkably variable or remarkably invariable. In truth, there are no *populations* in north central México or western Texas which resemble the *populations* of var. *potosina* called to the fore in my paper, nor is there a cline of *populations* between these in the region concerned. I have traveled over this terrain many years now and would have been happy to find such, but none was observed. In fact, var. *potosina* is probably better marked than var. *polypremoides*, which Terrell accords subspecific status, and I was surprised to see that Terrell did not "elevate" or position var. *potosina* in its own subspecies, as he did var. *polypremoides*; certainly, the latter shows much greater "clinal" intergradation over a broader area than does var. *potosina*, as is clear from both of our distributional maps of the former complex. A similar comment could also

be made about his reluctant acceptance of my var. *gypsophila* B.L. Turner, this being much better marked than var. *polypremoides*. Terrell (1996b) also glibly passed over my somewhat more weakly circumscribed var. *tamaulipana* B.L. Turner, contending that, while well isolated and possessed of a differing calyx and corolla, it was not worthy of recognition because it somewhat resembled a collection of var. *polypremoides* from Jeff Davis Co. in trans-Pecos Texas (*Rollins & Chambers 2759* [US]), as if a single atypical element from the mile-high volcanic outcrops of the Davis Mountains might mitigate the localized variation found in the populational units of the much lower calcareous outcrops of the Tamaulipan shrublands of northeastern México.

More disturbing, to me at least, has been Terrell's confusing presentation of the infraspecific variation found in *Hedyotis acerosa*. Thus, Terrell (1979), originally recognized two subspecies in this taxon, but in his 1996a revision he stated that "The variation [in *H. acerosa*] seems a continuum; there are no apparent discontinuities, and I now recognize only observable species." Pray tell: so why reinvent the subspecies so as to castigate my recognition of these at the varietal level?

Terrell (1996b) also waxes in a grumbling fashion about my recognition of *Hedyotis palmeri* (A. Gray) W.H. Lewis var. *muzquizana* B.L. Turner, which is clearly a morphogeographical populational unit, but must we believe his statement that "My presently limited sample leaves me [Terrell] reluctant to accept the existing morphological data as conclusive concerning *muzquizana*, pending further collecting of it." In my opinion, there are sufficient collections of the taxon (LL,TEX) to venture the name I have proposed, although Terrell might mean *he* prefers to collect this himself before accepting the putative taxon.

It should also be noted that Terrell (1996b) sweeps under the fabric of *Hedyotis nigricans* var. *nigricans* my proposed varieties, *H. n.* var. *austrotexana* B.L. Turner and *H. n.* var. *papillacea* B.L. Turner, claiming the characters separating these "to be minor." But, no more minor than the varieties *H. n.* var. *floridana* Standl. and *H. n.* var. *pulvinata*, both weakly differentiated endemics of Florida, which Terrell accepted. Indeed, had Terrell *not* recognized the latter two morphogeographical units as worthy of nomenclatural status, I perhaps would not have provided formal varietal status to the two Texas populational systems. Thus, Terrell set the minimal standards for varietal recognition within *H. nigricans* in his 1986 paper (Sida 11:471-481).

Finally, it seems worth noting that Terrell passes over my proposed *Hedyotis pooleana* B.L. Turner (Turner 1995d), claiming this to be but part of the variation of his concept of *H. mullerae* Fosberg, a species of north central México. I was disappointed that he deigned even to examine the only specimen of *H. pooleana* known to me, the holotype (TEX). The differences between my proposed *H. pooleana* and his *H. mullerae* are certainly as great as the differences between *Houstonia butterwickiae* Terrell (a localized taxon in close proximity to *Hedyotis pooleana*) and the widespread *Hedyotis nigricans*. But he who erects a taxon likes to stand by it, myself included.

In the final analysis, any two differing systematic treatments are likely to be tested by field workers, who will attest to their populational validity, or by DNA workers using restriction site analysis, or some such, the latter presumably gathered and analyzed without bias. I sincerely believe that my classification of *Hedyotis*, vis-à-vis

that of Terrell, will meet the tests imposed; perhaps Terrell feels the same about his classification.

Field workers in the region concerned, myself included, should make a concerted effort to examine and comment upon *populational* units of *Hedyotis*. Indeed, such observations and extrapolations from these led to my particular treatment. I do not doubt that Terrell has done considerable field work in Texas and perhaps north central México, but this has not been especially obvious to me in the collections he has assembled and distributed, nor is this obvious from the information presented in his critique of my own work.

There is a truism in systematic botany, or should be: other things being equal (*i.e.*, brains and experience), the systematist most likely to know best the specific and infraspecific boundaries of a given group is that worker having the most field experience with the taxa concerned. I care not to judge the merits of the two antagonists in the present controversy, but I do find the competing hypotheses stimulating; hopefully some younger worker with more field experience and better sampling techniques will ultimately resolve the systematic problems posed in the present paper.

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