

## Polemics. Полемика

### A joint refutation: Correcting and amplifying the published record

#### Совместное опровержение: Исправляя и усиливая опубликованные данные

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Recently published statements by Brewer, Sierwald & Bond [2012] in the online outlet PLoS ONE tend to misrepresent aspects of two of our diplopod contributions, and we thus feel compelled to post a refutation in the formal published record. As an online outlet, PLoS ONE is instantly available through a simple mouse click, and although Arthropoda Selecta is less readily accessible, it is incomparably better reputed for its diplopodological output.

Two years ago we coauthored a biogeographic work [Shelley, Golovatch, 2011] that culminated in hypotheses on the origin and early evolution of Diplopoda and its ordinal and higher taxa per concepts at that time. They were based on fossil and paleogeographic evidence, with indigenous distributions and geologically dated tectonic events documented therein. Nearly three years in planning and preparation, the only illustrations were 50 distribution maps and seven reconstructions depicting stages in our hypothesis of how these poorly vagile soil organisms arose from a single origination event to become a dominant, mega-diverse class that now blankets the temperate and tropical zones of the world. The hypotheses have predictive capability, a form of a test, positing, for example, more “Gondwanan”/southern-based orders than “Laurasian”/northern ones, and this indeed is the case (seven primarily Gondwanan vs. four exclusively Laurasian).

Nowhere do we claim that our work is perfect or constitutes the final word on these topics, but it does represent a sincere attempt to address an uninvestigated aspect of diplopodology and glean potential insights into the organisms. We consciously intended a purely geographic treatment as opposed to phylogenetic, anatomical, physiological, ecological, ethological, or anything else, therefore mapping ordinal and higher-level indigenous distributions and analyzing resultant geographic patterns. There are no numerical data, and we state clearly in paragraph one, “We therefore adopt a novel perspective by treating millipeds as geographic entities and departing from taxonomy, systematics, and cladistics in the strict senses....” In paragraph four we

add, “our map groupings are also purely geographic; they are not intended to be phylogenetic and should not be misconstrued as such.” In a recent contribution on Callipodida, Stoev & Enghoff [2011] acknowledge our “explicitly not phylogeny-based narrative,” but Brewer et al. offer a different assessment. They correctly state, “A recent work by Shelley and Golovatch posits a number of hypotheses regarding the timing of divergence and biogeography of the major diplopod groups” but then dismiss it with “However, the work lacks any statistical or phylogenetic analyses,” thereby mentioning our methodological limitations without the balance of our explicit statements acknowledging the same. They also fail to mention that the work deliberately contained only narrative and maps and that it therefore lacked numerical data. Without even oblique allusions to our openly stated caveats, we feel that the latter statement downplays our effort, thereby reducing its value to other researchers. Brewer et al. then provide a useful listing of computer software programs that facilitate divergence datings, characterizing them as “means to **objectively** (emphases ours) assess these issues.” In the partial context of the preceding sentence, readers could interpret this statement as implying that our study was subjective or biased, which is not the case. The authors might also have fairly acknowledged the time factor. We state in paragraph five that our study took >2½ years and making it phylogenetic or anything else would have extended it for years. We are substantially older than those authors; tomorrow is not guaranteed to anyone; and as “senior citizens” in our countries, we wanted to finish and publish it in our lifetimes. Carrying unpublished insight to the grave benefits no-one. Diplopodology experienced such irretrievable losses in the recent deaths of C.A.W. Jeekel and R.L. Hoffman, and does not need a refrain.

Since Brewer et al. did not mention positives, we can only assume that they found none and did not consider our study useful or worthwhile. Readers are always free to like or dislike and accept or reject others’ research in a collegial manner, but we consider the

omissions of our openly stated caveats in an online, open-access outlet to be an egregious misrepresentation of our effort. Nothing is provided to dissuade readers from erroneously concluding that we did not bother to analyze numerical data, that we simply ignored phylogeny, or that our study was subjective. Our data were the multitude of localities in two centuries of published literature. Recent statements characterizing milliped distributions as poorly known are incorrect; a lot was actually known and data existed, but it had never been properly assembled. We did this and without a hint of subjectivity, painstakingly plotted localities on maps to assess distributions and view emerging patterns. The distributions we depict are undeniably real. As we state “up front” our scope, purpose, intent, and limitations, we take exception to even unintended implications otherwise.

Brewer et al. did not mention it, so we emphasize that our study, the first of its kind on Diplopoda, provided the **first** solidly based hypotheses on originations and timings, thus providing a foundational base line for future refinements, and we even state as much in our concluding paragraph. By no means is our effort the final word, but it does open a new arena for research. Our biological training came long before the advent of computers, much less statistically based software, and we lack the mathematical, statistical, and computer skills to utilize the latter. However, documented distributions and tectonic events are beyond dispute, and we think that combining these advanced diplopodology by providing hypotheses on which persons who possess such skills can build.

In 2007, RMS contributed an invited paper on Diplopoda to the Linnaean Tercentenary issue of *Zootaxa*. Though missing a contribution on molecular taxonomy, the volume contained 21 papers on specific taxa plus ones on general topics (Tree of Life, NSF PEET program, museum collections and research, and invertebrate taxonomy and developmental biology). On p. 11 Brewer et al. cite RMS’ article: “...it is stated that millipedes **will soon be able to discard the monicker of ‘poorly known’**” (emphases ours). This remark counters the letter, spirit, and intent of what RMS actually said and is thus attributable solely to Brewer et al. themselves. The actual statement was, “...the day when the Diplopoda **can discard** the sobriquet of ‘poorly known’ **looms on the horizon**” (emphases ours). The terminal phraseology, a relatively common “figure-of-speech” referencing a once invisible goal, perhaps considered unattainable, becoming a plausible reality, was selected because it is positive, upbeat, and reflects the scope of the undeniable post-1971 advancements in diplopod taxonomy. Figures-of-speech are not meant to be stretched, dissected, or taken literally, and it is neither semantics nor “hair-splitting” to note the fundamental difference between the conditional “can” (an event will happen if conditions are met) and the absolute “will” or “will soon be able to” (an event will happen regardless of conditions). The horizon does not

get closer unless man continues advancing in that direction, hence the deliberate phrasing “can discard” instead of “will discard.” After setting up this fictitious “straw man,” Brewer et al. proceed to knock it down four paragraphs later in a “symbolic victory”: “Contrary to the **conclusions drawn by Shelley** (emphases ours), millipede taxonomy is still, methodologically speaking, in its infancy...” However, RMS drew no such conclusions nor made statements indicative of such. The authors use their own phrase, “will soon be able to discard,” as justification for explanatory paragraphs of their position and a “symbolic victory” over a claim that never existed in the first place. The wording “...it is stated that millipedes will soon be able to discard...” is solely that of Brewer et al., who necessarily own it.

We believe it timely to issue a friendly communication reminder: adult-to-adult, first person messages (I/we think..., In my/our opinion...) are preferable over parent-to-child, authoritarian, “should” transactions, implying a moral obligation. Because they are neutral and among equal adults, first person messages are more readily “**heard**” by recipients; the latter tend to confer inferiority or even subordination on the recipient(s) and superiority on the writer(s). All degree-carrying, professional diplopodologists are adults who are not **required** to do anything; single author publications and collaborations advance the field, but few subordinations exist where a professional works under another who holds superiority in an institutional hierarchy. We submit that the only “**should**” in diplopodology is that workers **should** seek to advance the field by conducting the type of research that they want and like to do, possess the skills to do, and which is compatible with their institutional missions and acceptable to those who truly hold hierarchical superiority. It makes little sense for workers who lack mathematical, statistical, and computer skills, to attempt to use the programs that Brewer et al. state “**should**” (emphases ours) be employed in subsequent investigations.” In a molecularly-based taxonomic study, Marek & Bond [2006: 718] went so far as to state, “Overturning, or ignoring our classification system on the basis of unsupported opinion or deference to authority **should be unacceptable to the systematics and diplopod community as a whole**” (emphases ours). Such statements seem tantamount to “territorial claim staking.” In a science where researchers have historically been “few and far between,” it is both senseless and self-destructive to even suggest restricting fields to particular lines of research or workers who can conduct them. We believe that there is no “one way” and no “right/wrong way” to study diplopods; every avenue warrants pursuit to potentially reveal secrets that other avenues may at least partly obscure. We contend that Diplopodology **should** be a “broad tent,” where **all** knowledge and insight are welcomed by everyone.

As far as modern biological techniques are concerned our skills are limited indeed; beyond comput-

ers, we also antedate molecular taxonomy, and RMS lacks taxonomic training altogether. Except for mentoring in the 1970s by R.L. Hoffman, he is entirely self-educated in taxonomy, systematics, and diplopodology. Having conducted one study, we are intrigued by the insights deriving from the simple geographic mappings that no-one had previously attempted or perhaps even thought of. Because diplopods are poorly vagile organisms that can neither run, fly, swim, nor float, they could only reach distant land masses in the past when collisions occurred or by riding tectonic “ferries.” Consequently, with knowledge of indigenous distributions and the datings of tectonic events by geologists and geographers, it is possible to estimate general origination dates and regions for diplopod taxa based on sound, solid evidence. This had not previously been done. Should this activity be abandoned because it is neither statistical nor phylogenetic, in essence because of its simplicity? Are there “degree of difficulty” and technological thresholds that diplopod studies must meet or surpass to be considered acceptable and worthwhile nowadays? If so, should workers who cannot meet these thresholds but still wish to share their knowledge and insights not do so or even leave Diplopodology altogether? We think not, and geography requires only a pile of blank maps instead of costly equipment; hence it is compatible with our depleted institutional budgets and does not require months of labor on grant proposals. We also conduct faunistics and anatomically-based taxonomy, mostly at the alpha-level because of existing states of knowledge. We shall continue all of these, and a geographic assessment of Polydesmida is even in progress. To colleagues who do not like or are opposed to geography, faunistics, and/or anatomically-based taxonomy, we respectfully recommend that you avoid read-

ing our future contributions because the contents will hold little appeal. To those who do read them, we respectfully request that we be quoted exactly to eliminate inaccurate paraphrasings, that critiques report all qualifications that we ourselves provide, and that special efforts be made to place our works in the desired contexts to avoid raising unintended aspersions in the minds of uninformed readers. In particular, we ask that care be taken to accurately report the letter, spirit, and intent of our works in this era of rapid, open-access, and readily available publications.

Thank you.

## References

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