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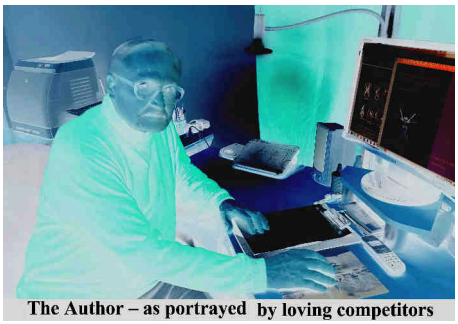


The Author – as portrayed by loving competitors

Maddison - idol who went astray ...

a background to Salicidae (Araneae) studies

ABSTRACT. The text below is addressed to a small community of taxonomists interested in Salticidae spiders, assumedly uninteresting for the majority of possible readers! Sorry!
I am interested in progress and in improvement of our research and that preferably should be done by gentle persuasions spread over years. I have been tying to act that way, without visible progress, and now, when my time is running out, I resign from courteous niceties and write openly. I quote names of colleagues as examples, but if some remarks on typical weaknesses in our profession sounds criticism-like, I wish to point that these could happen to any of us, myself as well. I try to document true episodes of recent history of arachnology, presented as precisely as I can. Whilst they concern mainly methodology of our work, but also touching inter-human relations between scientists. Apologize if one will feel offended - that certainly was not my motivation.

"do not ask for whom the bell tolls ..."

I read once anthropologists' comments why our hominid ancestors have developed so huge brain, redundant to their possible needs - it was a facility needed for their quarrels, endless politicking and intrigues. I do not know whether that view is still uncontested, but it certainly contains some truth.

Having some first 20 years experience in arachnology, I was aware that my plans to understand layout of the family Salticidae exceed possibility of a single researcher, these could materialize only if I develop team of collaborators and correspondents. In 1972 I got very limited possibility of organizing such team when become head of the Zoology Department in the minute, provincional Teachers' Training College in Siedlce, Poland. That was a newly organized institution, fighting for survival, in which every small success of my team improved survival chances of the whole College. I have freedom of doing what considered necessary and no "friend" censored me. Within a few months I selected employees (in our system these being also PhD students) willing to study Salticidae, and gradually got a circle of correspondents.
I was not good in maintaining discipline, but somehow I did not need that. We maintained mutual goodwill, both in Departmental works and in the research of team. We have progressed successfully for the next 20 years.

Gracious fate has given me many interesting correspondents, contact with whom I enjoyed very much. Majority was sending me single photographs of a jumping spiders from around the globe, which I tried honestly to answer but with little success (unless they enclosed photos of palps and internal structures of epigyne). There were other peoples sending me series of interesting photos, some of them experienced as arachnologist - very useful contacts, which taught me a lot. Some peoples collaborated with me during a few years, but were unable to make life career out of their successful beginning. The most successful was 50 years long cooperation with two of my country peoples: Wanda Wesołowska (113 publications quoted by World Spider Catalog - recognized specialist of African Salticidae) and Marek Żabka (62 publications quoted by WSC - recognized specialist of Asian and Australian jumping spiders). Fifty years memories deserve a book, which exceeds my writing possibilities - so I will direct readers to excellent publication they wrote. Two most prominent correspondents were: W. P. Maddison to whom I give my respects in this note, I will dedicate next note to D. V. Logunov.

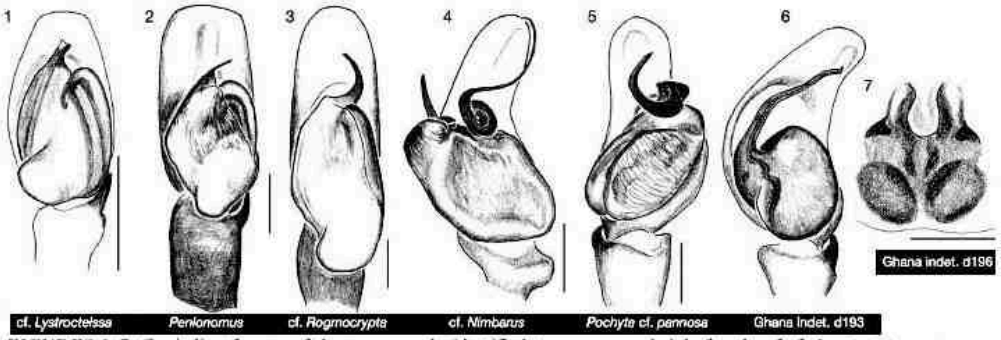
Maddison - idol who went astray ...

He begun to correspond with me in 1970ties, still as a schoolboy. I recognized his big developmental potential as a naturalist, right from the beginning, we maintained vivid correspondence for decades, decade later he become PhD student of the Harvard University and then I benefited myself quite a lot from his knowledge of new trends and methods in taxonomy (especially usage of computer databases, unknown then to us - fundamental for later works of mine and my Polish collaborators). After his PhD, he ventured into modern field of molecular research, especially Salticidae phylogeny by gene sequencing, about which I knew little. Roughly after big molecular publication of 2003, these relations begun to deteriorate, I began to feel signs of rivalry. Notably, he was incensed when I put my name as an author to my "anonymous" Salticidae database, originally stored, by his courtesy, on his server, the only place where for many years I could save it (see our correspondence in **https://salticidae.pl/2_SAMIZDAT/archiv_corresp/corresp_maddison_98-2016.pdf**, repeated also in letters of his acolyte G.B. Edwards, therein).
I begun worry in turn, when errors in synonymy of genera of Salticidae begun to be noticeable in Maddison papers after 2003, also in papers of his PhD students, co-signed by him. I have been instrumental in the pre-DNA based taxonomy - actualized meaning of species and genera, revised or described myself 854 unrecognizable species, including over 500 types and type species, documented by 3108 diagnostic plates of my original drawings. Two last decades of my work I sacrificed to work on possible reorganization of the taxonomic system of the family Salticidae worldwide. The disagreement on identification would upset that work. I begun to be worry about frequent

misidentification of genera and wrong synonymies, which could derail the progress of work. I do not know any taxonomist welcoming corrections to his work, and Maddison was not exception. There was additional factor deteriorating our cooperation. We have been working using different methodology: I feel lost reading his description based on DNA sequencing, at the same time Maddison (and his PhD students) neglected to use diagnostic documentation in my rapidly growing database. He has never explained how he managed to identify the material, but I found that he relied heavily on traditional system of Simon (1901-1903), which I abandoned 60 years earlier as not reliable. It is hard to understand to me, why publications of such brilliant arachnologist as Maddison contains so many misinterpretations (some few of them I quote below). Presumably preoccupied by more important problems, he had no patience for *minor details* and relied too much on his cooperators. I hope that having more time he will discover and describe more of important features of Salticidae.

Misidentifications of genera used for gene sequencing

by W. P. Maddison, Bodner, M. R. & Needham, K. M. (2008). *Salticid spider phylogeny revisited, with the discovery of a large Australasian clade (Araneae: Salticidae). Zootaxa 1893: 52, figs 1-7.*



FIGURES 1–7. Genitalia of some of the more poorly identified taxa sequenced. 1 Left palp of cf. *Lystrocteissa* sp. (voucher d054), which is small and narrow-bodied. 2 Left palp of *Penionomus* sp. (d122), medium sized, fairly robust, and bronze. 3 Left palp of cf. *Rogmocrypta* sp. (d205), small, with body form reminiscent of *Sitticus*. 4 Left palp of cf. *Nimbarus* sp. (d218), whose long tibial apophysis extends dorsally over the cymbium. 5 Left palp of *Pochyta* cf. *pannosa* Simon (d217), whose tegulum appears to be twisted so as to expose the basal hematadocha. 6 Left palp of Ghana indet. d193. 7 Epigynum of Ghana indet. d196, with two anteriolateral epigynal pockets.

Type species of the same genera

(after "*Monograph of the Salticidae (Araneae) of the World 1995-2012*" by J. Prószyński)

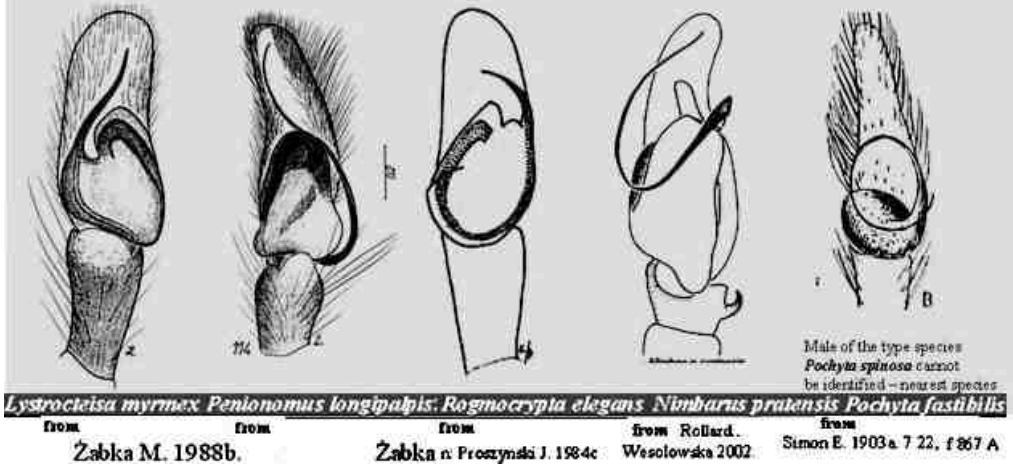
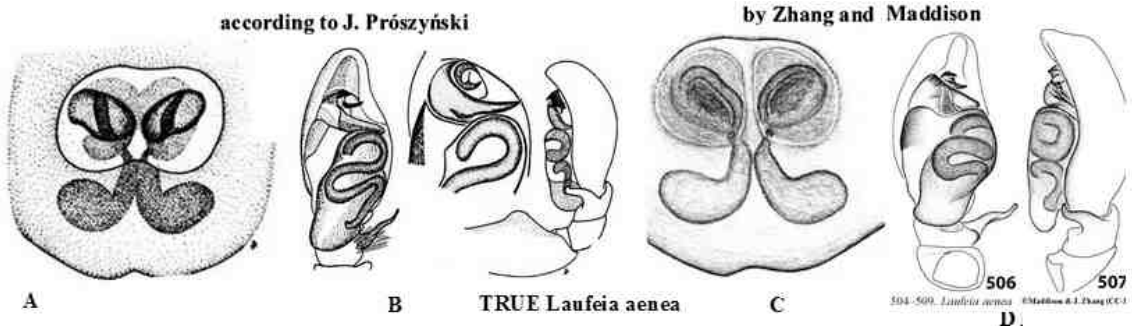
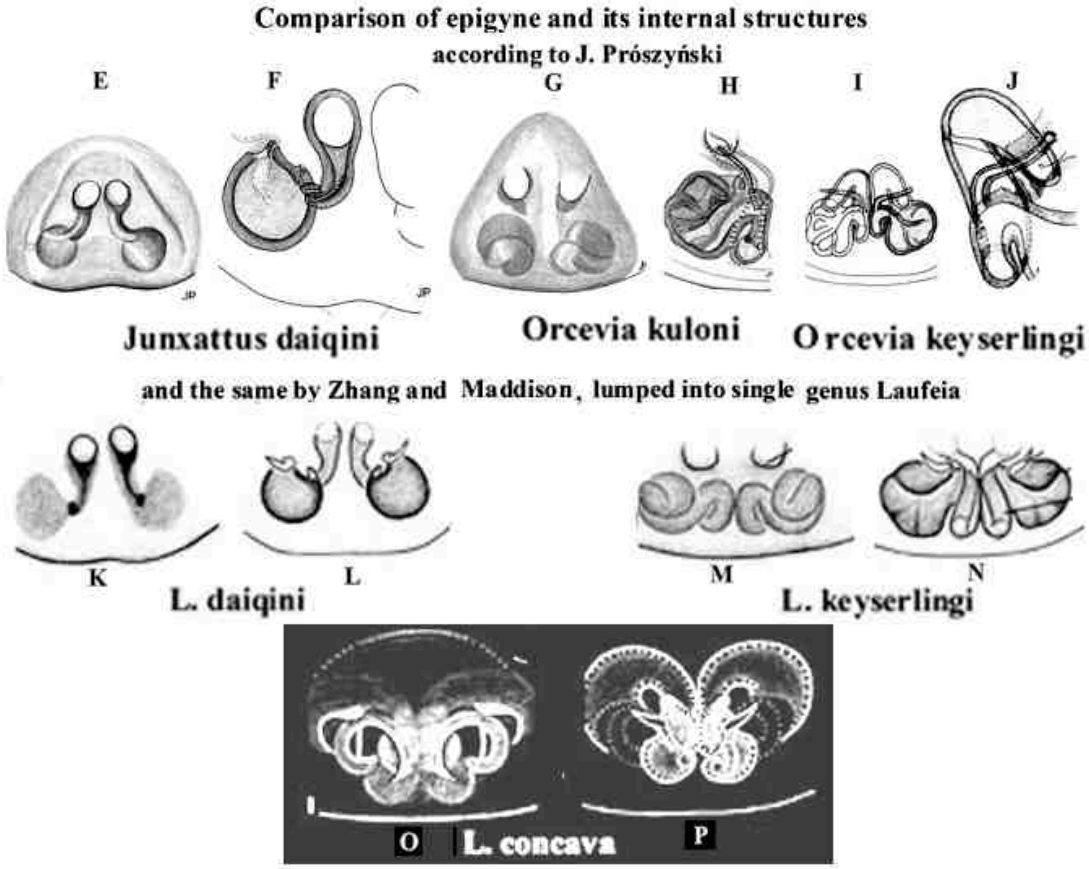


Fig. 1. It is unbelievable that the above six genera (see upper row) could be mistaken, while documentation of their name-bearers (type specimens of type species - lower row) was available within seconds in the Internet Salticidae Database. The only possible explanation is that the specimens were identified using trivial relative characters of Simon 1901-1903 by inexperienced students, and that their PhD instructor - signed also as co-author of their paper, had no time for checking correctness of the manuscript data before publishing. The sixth and seventh drawing above - the palp and epigyne of unidentified species - broke general rule of taxonomy that the scientific name is the only tool for communication, if the authors decided to publish it none the less - why they used unrecognizable drawing of epigyne?

The plate below illustrates how a very gifted, ambitious PhD student J. X.[=Junxia] Zhang, an excellent "taxonomist's material", has lost part of her scientific potential because of neglect of her PhD instructor (and co-author of resulting publication).





Figs. **2**. Drawings of *Laufeia aenea* (**A-D**) - the paratype specimens, being also type species of the genus *Laufeia,* by Prószyński (**A-B**) and by Junxia Zhang (**C-D**) have minor differences, but are pretty recognizable, however, those of *Junxattus daiqini* (**E-F**) and *Orcevia keyserlingi* (**I-J**) by Prószyński differs significantly from their much simplified equivalents "*Laufeia*" *daiqini* (K-L) and "*L." keyserlingi* (M-N) by Junxia Zhang, "*L." concava* (O-P) by the latter, is misplaced and and should be reidentified.
SOURCE: Zhang, J. X. & Maddison, W. P. (2015). Genera of euophryine jumping spiders (Araneae: Salticidae), with a combined molecular-morphological phylogeny. *Zootaxa* **3938**(1): 1-147.
Prószyński, J. (2019). Character assassination: a personal witness account with a taxonomic note on the genus *Laufeia* s. lat. (Araneae: Salticidae). *Ecologica Montenegrina* **22**: 122-123, fig. 1.

COMMENTS. "*Orcevia* Thorell (reinstated from *Laufeia*) and *Junxattus* Prószyński & Deeleeman-Reinhold are newly separated from *Laufeia* by Prószyński & Deeleeman-Reinhold (2012) based on their different genitalic structures. In the molecular phylogeny (see Chapter 2), I include the type species of *Orcevia* (*O. keyserlingi* Thorell), the type species of *Junxattus* (*J. daiqini* Prószyński & Deeleeman-Reinhold) and two *Laufeia* species newly discovered (*L. eximia* and *L. concava*, see Chapter 8 [actually misplaced! - J.P.]). They all fall in a strongly supported clade. The type species of *Laufeia*, *L. aenea* Simon was not included because no material was available for the molecular work. However, the type specimens of *L. aenea* (examined) show similarity in genitalia with *L. concava* [wrong! genus misidentified - see fig. 2 o-p above - J.P.]. For instance, they both have relatively wide bulb, small embolic disc and short embolus in the male palp, and a window structure in the epigynum [these are visible on drawings, but means little when written as words - J.P] In spite of having genitalia **more diverse** than in other euophryine genera, the species included in phylogenetic study and *L. aenea* are very similar in **body form and cheliceral teeth pattern**. **A high genitalic diversity could occur even in closely related species** [original opinion! require proof - J.P.], if for instance strong sexual selection drives rapid divergence [phantasies! - J.P.] Thus I am reluctant to follow Prószyński and Deeleeman-Reinhold's classification (2012), which will result in at least four genera for this clade with each comprising very few or even single species [typical for little known genera, more collecting often lead to discovery more species - J.P.]. Instead, I treat all of them."

.....(Excerpt from the PhD Thesis of Junxia Zhang 2012, repeated in Zhang & Maddison, 2013).

Proposals of Junxia Zhang 2012 to synonymize *Junxattus* [by the way named for her, type species *daiqini* named for Li Daiqin], *Laufeia* and *Orcevia*, endorsed later by W. P. Maddison (in their mutual publication of 2013) is unacceptable - as certified by morphological differences in spermathecae (and palps - see in relevant literature, especially Internet Salticidae databases). Body form and cheliceral teeth pattern are Simon's relational and trivial characters of doubtful value!

I was particularly shocked by an attempt to remove genus name *Rhene* in the PhD Thesis
a degree conferred to G. R. S. Ruiz under the auspices of W. P. Maddison and A. D. Brescovit

Customarily PhD Theses are expected to be error-free, as testifying adultness of a young scientists to undertake academic duties - the "Doctorandus" G. R. S Ruiz has big developmental potential, proved by his valuable and numerous publications - why he has not received proper coaching? Why referees of Ruiz's Thesis did not warned him to remove embarrassing faults?

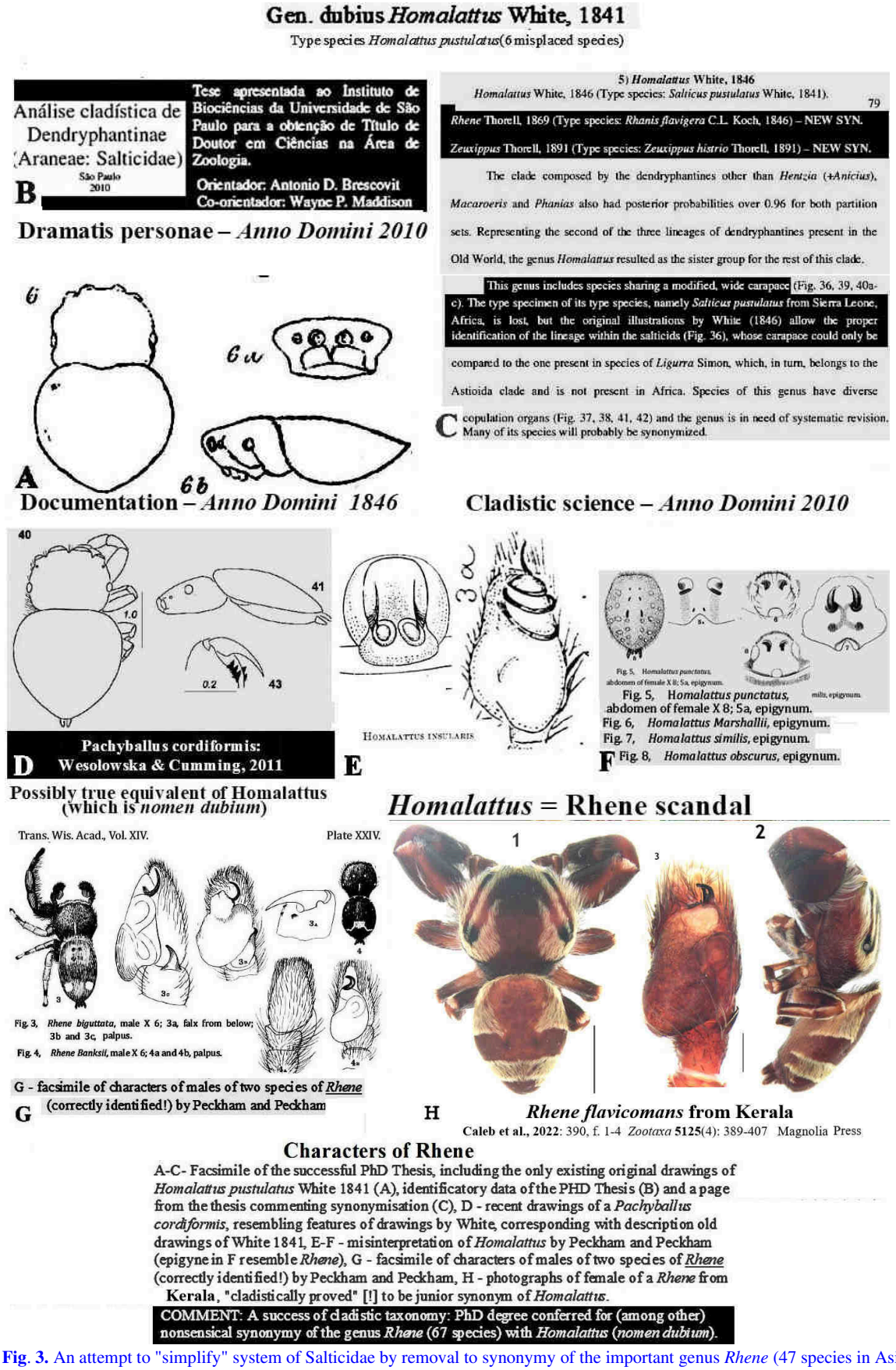


Fig. 3. An attempt to "simplify" system of Salticidae by removal to synonymy of the important genus *Rhene* (47 species in Asia and Africa) by revival of forgotten and unidentifiable genus *Homalattus* (possibly being equal, or related, to African *Pachyballus*). **A** - facsimile of the original drawings of *Homalattus pustulatus* (White, 1841), **B** - patrons of PhD degree conferred in 2010 by Sao Paulo University to G. R. S. Ruiz, **C** - facsimile of a page from Ruiz's PhD Thesis with "Redescription" o the genus *Homalattus*, **D** - 2011 diagnostic illustrations of possibly related *Pachyballus cordiformis* (by Wesołowska & Cumming, 2011: 87, f. 40-45, **E - F** - original illustrations by Peckhams' of misidentified 1902 and 1903 "*Homalattus*" species, G - correctly placed two species of *Rhene* by Peckham, 1903, **H** - color photographic documentation of *Rhene flavicomans* from Kerala, India, by Caleb et al., 2022a: 390, f. 1-4.

Success, or failure is a random event - the scandal illustrated above "never happened" - that is this PhD was conferred to G. R. S Ruiz, but the Thesis was never published in an official periodical (actually preparation for print in "*Peckhamia*" was interrupted by myself in defense of the genus name *Rhene*) so the World Spider Catalog never got wind of that. "Orientador" - professor A. Brescovit signed as a coauthor

271 papers on various spiders, mainly coauthored with his students, among these 11 on Salticidae coauthored with Ruiz, so his experience seems to equals that of Ruiz, he cosigned notorious Kropf[Nentwig?] et al. 2019 paper, furthermore is honored by membership in the WSC Expert Board (deciding on publications accepted by the WSC World Spider Catalog). Ruiz is promising author of 73 valuable publications within 20 years, mainly on Salticidae, so I appreciate him, although have criticized some weaknesses, influenced by bad company]. "Co-orientador" in this PhD affair is great arachnologist W. P. Maddison, leading authority in Salticidology. Unfortunately, co-authorship with leaders does not guarantee error-free publication, as demonstrated in another PhD students case, quoted above (Fig. 1).

Comments on excessive number of misidentifications and misinterpretations in papers of W.P. Maddison and his PhD students. I have been puzzled for many years why Maddison, with his brilliant intelligence and many talents, has committed so many errors in his professional publications. The only explanation I can think of, could be his preoccupation with more important problems (wiz. his phylogeny studies based on DNA sequencing) which resulted in relying too much on works of his PhD students, he used to endorse by cosigning their papers. I worked on my projects parallelly with my students, looking at every species and preparation under their microscopes, we discussed every conclusions they draw. But I respected their autonomy (even if different from my opinion) and never cosigned their papers. Having promising candidates, I planned their development for some five years in advance, including obligatory pre-PhD publications, PhD Theses and practices in Salticidae collections abroad. That gave excellent results in the case of Marek Żabka, cooperation with Wanda Wesołowska was shorter because she has moved to another university, but I still managed to bring half of *Heliophanus* species for her PhD Thesis from the Simon's collection. The same did for my other students, who did not completed their work. Having as much time for his student, Maddison would probably avoid troubles with identification.

Long entertained sentiments are poor reason for renaming genera

The paper on classification of *Sitticus* (Maddison et al., 2020 */- see below) is impressive collection of various experiences of its four authors. Without going into detailed analysis, I assume that general topic seems to be valuable, but the scientific level of the paper depends from resolution of its major problem - the classification, presented on the background of the literature. There is not much added to the general layout of the group of genera called by Maddison Sitticinae, worked out before, with prominent contributions of Maddison himself.

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*/ FOOTNOTE. Maddison, W. P., Maddison, D. R., Derkarabetian, S. & a: 13, and other). Sitticine[sic!] jumping spiders: phylogeny, classification, and chromosomes (Araneae, Salticidae, Sitticini). ZooKeys 925: 1-54.

I like style of Maddison's writings, but some of his assumptions do not hold the water. Lumping versus splitting of taxa could be argued endlessly, resulting in variety of names conveying roughly similar ideas. Are they worthy time spent on them? A major naming revolution is lumping of four genera into single genus *Attulus*, divided into subgenera. The same idea guided Prószyński revising (as the first) earlier existing names into single genus *Sitticus*, divided into groups of species in a series of papers from 1968 onwards (evaluated by Maddison as "**not based on a conceptual justification**"). Both solution have the same practical flaw: all their species are reported in catalogs as belonging to a single, huge genus, because WSC resigned from presenting intermediate taxonomic ranks between genera and species - arachnologist therefore simply follow names from the WSC records and do not care what both learned arachnologists wrote. In a consequence, instead of popularizing phylogeny, Maddison obliterates it in popular understanding.

Lumping of the palaearctic genera *Attulus*, *Sitticus*, *Sittiflor*, *Sittilong*, *Sittipub* and *Sittisax* is simply interpretation errors - type species of ***Attulus* - *Attus distinguendus* Simon, 1868b is not congeneric with type species of Sitticus - *Araneus terebratus* Clerck, 1757**, also is not congeneric with - ***SITTIFLOR floricola*** (C. L. Koch, 1837, that concerns also remaining genera separated from *Sitticus* by Prószyński 2017, *Sittilong*, *Sittipub* and *Sittisax)* by any identificatory standards (genitalic characters, color pattern type, environment, biological specialization) and these differences are repeatable within all species congeneric with these type species. That is proven even by Maddison's 2020: 26, figs 69-86 plate, reproduced below (**Figs 5 A-D**).
If we dismiss thoughtless juggling with forgotten synonyms, the only other "**conceptual justification**" in renaming was that worked in 1995-2016 (first published in 2017) by Prószyński, who proposed composite name of genera consisting of syllables from two words, associating with additional information (one associating with the previous, accustomed genus and the other associating with significant information, such as type species of splitted out genus, or its geographic distribution, or a honorific note, etc,). Lets' explain that on examples:
1 - association with the type species: ***Sitti**cus* s. str., ***Sitti**-flor[icola], **Sitti**-long[ipes], **Sitti**-pub[escens], **Sitti-**sax[atilis],*
***Myrma**rachne* s. str., ***Myrma**-ge, **Myrma**-gua, **Myrma**-nu, **Myrma**-pana, **Myrma**-peni, **Myrma**-plata, **Myrma**-theca,*
***MYRMA**-VOLA, **Myrm**-ele;*
2 - association with a honorific note: ***Eva-wes**[solowska], **Junx**[ia]-attus daiquini, **Logun**[ov]-yllus*;
3 - association with geographic distribution: ***Eva**-neg[evensis], **Iran**-attus.*
There are other advantages of composite names - by being so Salticidae-specific the danger of another scourge - homonyms is significantly reduced. By usage of first syllable of the previous genus name, we keep all of its species closely together in records and lists, all associating in memory of arachnologists with previous genus name, permitting to recollect previous experiences. Choice to use composite name belongs to author splitting large genus, there is no conflict with Nomenclatorical Codex.

Working the whole life with problems of classification I have developed obvious methodical principle, which Maddison seems to not understand: all changes in taxonomy of genera (synonymy, splitting, lumping, transfers) should be based on revision (or analysis) of the **whole genera**, **all relevant species** (that is why writing my largest papers took me up to 20 years each). *Attulus* s.str. is a speciose genus distributed predominantly in the whole Palaearctics. with a single species being recent immigrant to the North America, so are *Sitticus* s. str. and *Sittiflor* and *Sittipub*. Maddison on the inadequate basis of the sample of a few species occurring in Canada (at least he implies that, actually he know many other species and mixes up that knowledge in his text) attempt to regroup the whole huge genus *Attulus* s. lattisimo (=*Sitticus* s. lattisimo). Had he put in his plate all 22 species of *Attulus*, 5 of *Sitticus*, 13 of *Sittiflor*, 1 of *Sittilong*, 2 of *Sittipub* and 3 of *Sittisax* (all these names follows Prószyński 2017, for synonyms look at WSC, for pictorial diagnostic definition see Prószyński 2017: 61-66, plates 29A-J, 30A-K, another source is my Internet Salticidae Database), had he spent years collecting them from N Korea to S France, he would never lumped them into one clumsy crowd of species (see his Figs. 49-68).
Maddison should be complimented for routinous illustrating his species with photographs of live individuals, however it would be wise to present both external appearance of a specimen with documentation of its palp/internal epigyne structure (unfortunately his photographs poorly represent species they are supposed to be - compare Maddison's 2020: Figs. 49-68 with photos **5G-L** by Lissner below).

Like myself in 1953, Maddison follows example of Simon (1901-1903 - see example on Fig. 4, below) in defining genera differences by **relational characters** (wiz. proportions of legs III and IV, often barely discernible), equally applicable to many Salticidae and therefore

responsible for many mistakes, but myself I abandoned them very quickly for **absolute characters** (stained preparations of cleared epigyne, palps and color photographs of live specimens) fitting only single species, or genus each (like face photographs and papillary lines, as used in for human identification in passports).

36. *SITTICEÆ*

Je propose le nom de *Sitticus* pour le groupe d'espèces (*S. pubescens* Fabr., *terebratus* Clerck, etc.), auquel les auteurs modernes ont à tort laissé le nom d'*Attus* qui, pour Walckenaer, correspondait exactement au genre *Salticus* de Latreille, dont le type est le *S. scenicus* Clerck.

Les chélicères de ces araignées, ont, comme celles des *Chalcoscirtus*, la

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marge inférieure mutique, mais leur céphalothorax est pourvu d'une strie, leurs pattes des 2 paires postérieures sont armées d'épines beaucoup plus nombreuses et plus fortes, et celles de la 4e paire sont beaucoup plus longues que celles de la 3e paire, par tous leurs articles, même par la hanche et le trochanter.

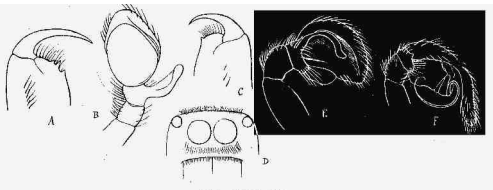


Fig. 699 à 704

A. *Sitticus pubescens* Fabr. Chélicère en dessous ♀. — B. *Id.* P. M. ♂. — C. *Yllenus arenarius* Menge. Chélicère. — D. *Id.* Face et yeux. — E. *Id.* P. M. ♂. — F. *Y. hamifer* E. Sim. P. M. ♂.

FAMILLE DES SALTICIDÆ 581

GENERA

1. Oculi antici in lineam validissime recurvam, laterales a mediis late distantes. Tibia 4i paris aculeo dorsali carens. Metatarsus cum tarso 4i paris patella cum tibia multo brevior. Tarsi antici subtus fere usque ab basin crasse scopulati. Ungues postici dentibus 10 vel 12, longis æquis et contiguis, instructi . . . **Yllenus.**

— Oculi antici in lineam rectam vel parum recurvam, laterales a mediis anguste distantes. Tibiæ posticæ aculeo dorsali fere semper instructæ. Tarsi antici apice fasciculati. Ungues postici mutici vel prope medium dentibus paucis et parvis tantum muniti. . . . . 2.

2. Cephalothorax sat longus, parte thoracica cephalica saltem 1/4 longiore. Quadrangulus parallelus vel fere parallelus. Sternum sat latum, antice parum attenuatum. . . . . **Sitticus.**

— Cephalothorax brevior, parte thoracica cephalica tantum 1/5 longiore. Quadrangulus postice quam antice paulo latior. Sternum angustius, antice acuminatum. . . . . **Attulus.**

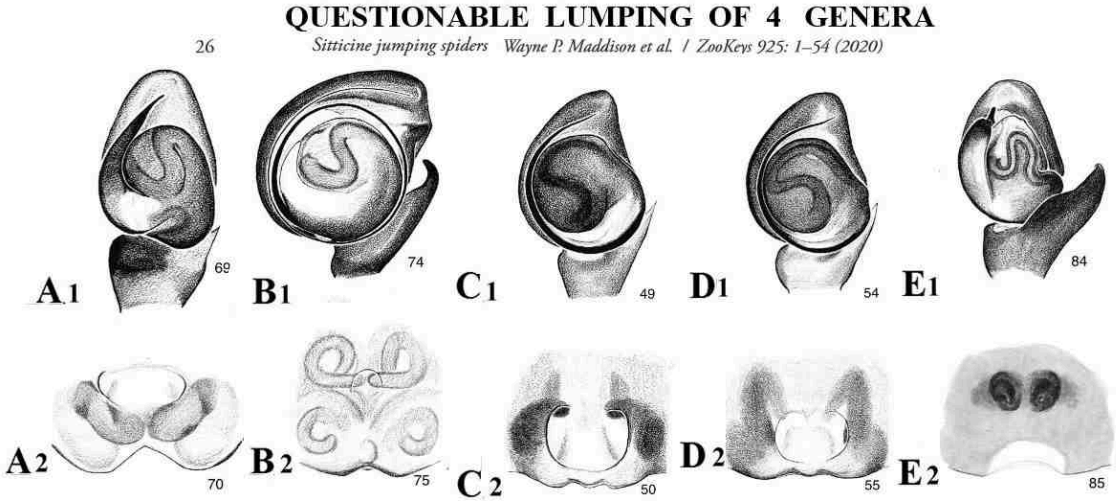
Fig. 4. Excerpts from Simon 1901: 577-581 - delimitation of groups of genera Sitticeae - basing on **relative external** body characters is futile, classification of *Yllenus* together with *Sitticus* and *Attulus* is disqualifying error, genitalic drawings serve as mere decoration without using them as absolute diagnostic characters (the same error as done by Maddison - 120 years later).

[ATTENTION: To read confortably increase magnification of the screen].

Significant contribution of Maddison to *Sitticus* (his earlier papers included) is comparison of Canadian species with South American relatives, but he is less versatile with their supposed Palaearctic allies like *Yllenus* s. l. (see above - Fig. 4) put by Simon 1901: 577-581 into the same subfamily (group of genera), clarifying of these fantasies taught me to be suspicious of any undocumented graphically inventions (compare Maddison's opinion on "**not based on a conceptual justification**" - Prószyński's statements - see below).

> Maddison gave recently the following evaluation of Prószyński 's contribution "… Prószyński, who developed our understanding of north-temperate species [of *Sitticus* Simon, 1901] in a series of papers (1968, 1971, 1973, 1980), recently (2016, 2017a) partitioned this diversity into several genera: *Sittipub* Prószyński, 2016, *Sittiflor* Prószyński, 2017, *Sittilong* Prószyński, 2017, *Sittisax* Prószyński, 2017, *Sittiab* Prószyński, 2017, *Attulus*, and *Sitticus*. Prószyński did not intend this classification to be phylogenetic, but rather "pragmatic" (Prószyński 2017b), **which is to say, not based on a conceptual justification**" ---------- (Maddison et al., 2020: 2-3). [Looks like that "**conceptual justification**" of Maddison means in fact "**undocumented fantasies**" - J. P.]

Prószyński never abandoned problems of *Sitticus*, returning to it over and over again (**1962a**, **1968c**, **1971, 1971b**, **1971e, 1973a**, **1975**, **1976, 1979, 1980, 1982**, **1983d, 1984a**, **1987**, **1987**, **1989c**, **2016**, **2017a,** the most significant works being available in the Internet: **1995-2016, 2020** - and unfinished **2022**), and finally, with 50 years experience, he proposed much more advanced concept of splitting *Sitticus* into six genera based on diversity of genitalic characters, color photographs of live specimens and their continental diversity. The differences in characters interpretation of *Sitticus/Attulus* related genera by Prószyński 2017, and *Attulus* (*sensu latissimo*) of Maddison are shown on Fig. 5A-E and can be used in laboratory identification equally well. However, for dealing with living specie in the field, the practicability of identificatory methods is tremendously different and include differences of habitats, behavior, color pattern etc.



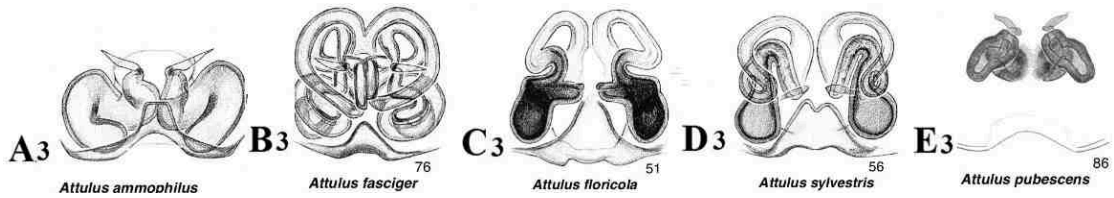


Fig. 5. A-E. Do the above structures prove single genus identity or, just opposite, genus level differences? ***Attulus ammophilus***, B - *Attulus fasciger* **- *Sitticus fasciger***, C - *Attulus floricola* = ***Sitiflor floricola***, D - *Sitticus sylvestris* = ***Sittiflor sylvestris*****,** E - *Attulus pubescens* = ***Sittipub pubescens***. SOURCE: Maddison et al., 2020 believes that all the above species represent single genus *Attulus*. Nomnclature translation from Maddison et al., 2020 = **into Prószyński 2017**.

Fig. 5. F - Evolutionary grouping of species of *Sitticus* s. l. by Prószyński 1968-1983, followed by separating groups as separate related genera in 2017.*/ As for experience of study of diversity and continental spreading of worldwide 4800 species, resulted in 2017 splitting into six related genera, differing only by names - credited [how flatteringly!] to Maddison.
I believe the photographs below, used in Prószyński's databases and other publications by kind permission of their photographers, give correct appearances of live individuals - readers are invited to compare them with Maddison's 2020: f**igs 5 M-N photographs (below)** and draw own conclusions.

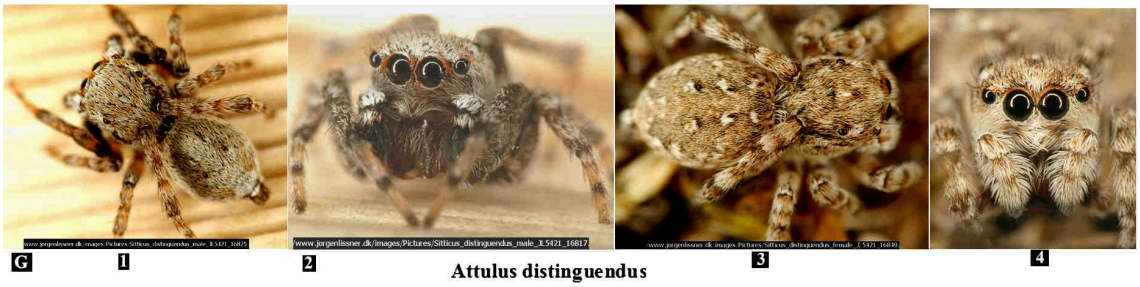


Fig. 5 G . -1-4

Fig. 5 H . 1-3

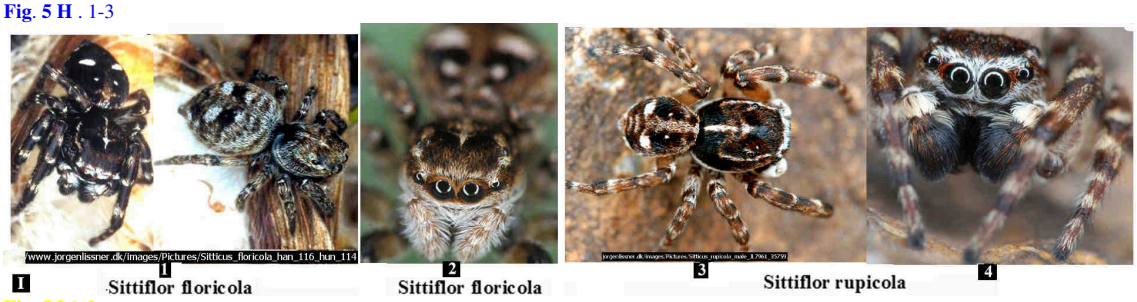


Fig. 5 I 1-3

Fig. 5J- 1-3

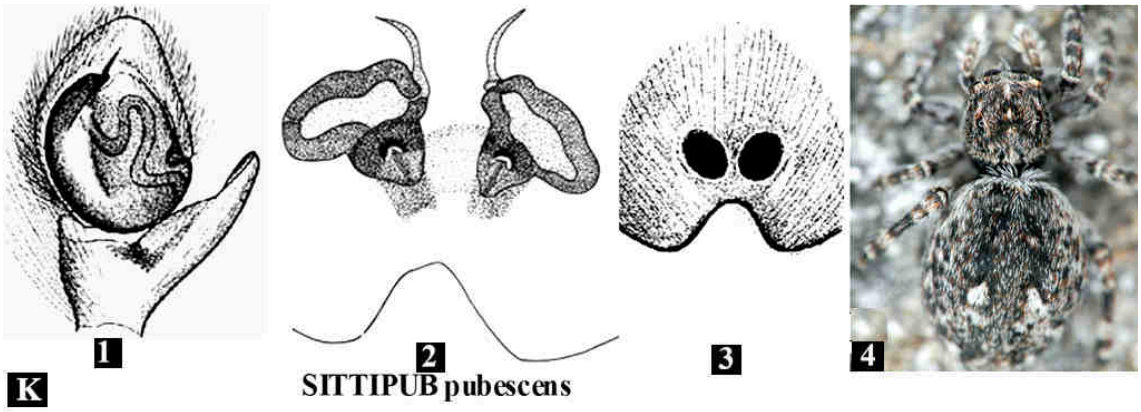


Fig. 5 K - 1-3

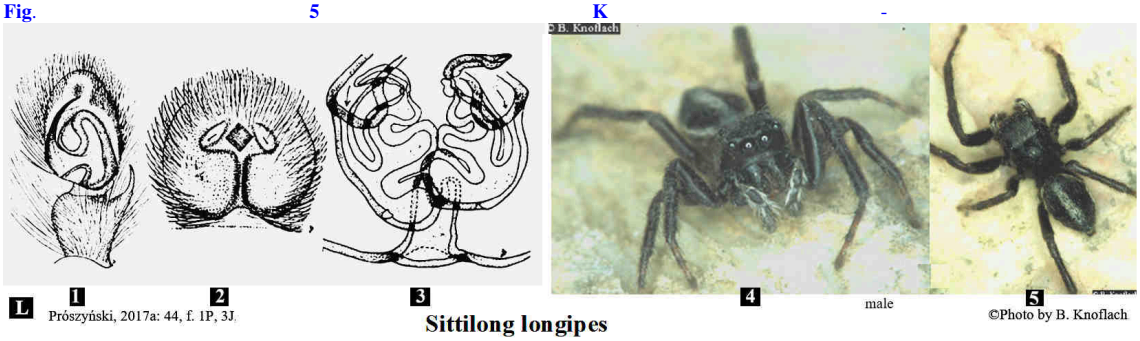


Fig. 5 L - 1-3. Below: photographs by W. P. Maddison. By courtesy.

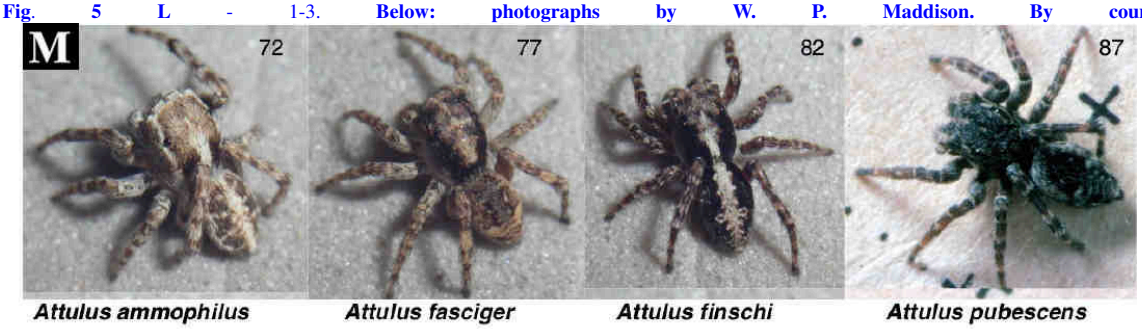


Fig. 5 M. For comparison - sample of Canadian Sitticinae. Photo W.P. Maddison.

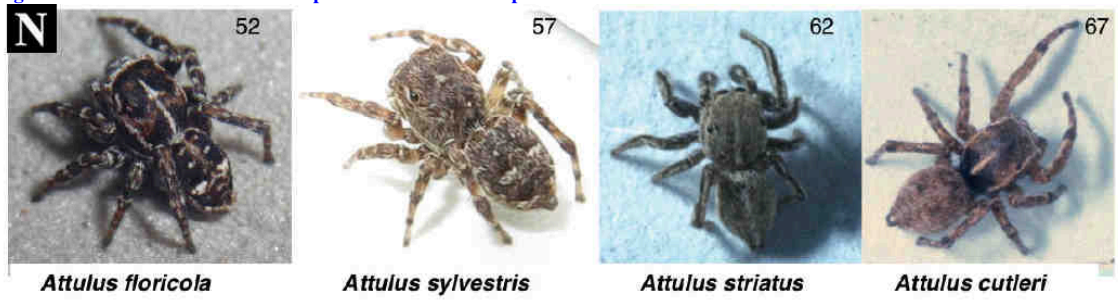


Fig. 5 N. For comparison - sample of Canadian Sitticinae. Photo W.P. Maddison.

Fig. 5 A-E . Photographs of color pattern of live ***Attulus distinguendus* (G: 1-4)** , H (1-3) **-** ***Sitticus terebratus***, I (1-2) - ***Sitiflor floricola***, I (3-4) - ***Sittiflor rupicola*,** K - ***Sittipub pubescens*** (1-3 palp and epigyne, 4 - photo of live), L - ***Sittipub pubescens*** (1-3 palp and epigyne, 4-5 photos of live). - photo of live. SOURCE: Maddison et al., 2020 believes that all the above species represent single genus *Attulus*. Nomenclature translation from Maddison et al., 2020 = **into Prószyński 2017**.

Here, below, are facsimile excerpts from Maddison et al., 2020.

"**Genus *Attulus* Simon, 1868, restored (to respect its priority over *Sitticus*)** [*Araneus terebratus* Clerck, 1757 vs. **type species *Attus distinguendus* Simon, 1868b: 540**, **nec** *Attulus helveolus* Simon, 1889 J. Prószyński] [*Calositticus* and *Hypositticus* were described as subgenera of *Sitticus*, elevated to genus rank and recognized as older synonyms of *Sittiflor* and *Sittipub* by Blick & Marusik, 2018: 237.]

Attulus Simon, 1868 (type species *Attus helveolus* Simon, 1871[not type species - J.P.]

Sitticus Simon, 1901 (type species ***Araneus terebratus* Clerck, 1757**)

Sittiflor Prószyński, 2017 (type species ***Euophrys floricola*** C.L. Koch, 1837), syn. nov."

"By considering *Attulus* as a single genus with subgenera, we also **simplify identifications** [????! - JP] by ecologists and others. A Eurasian salticid, even a juvenile, can easily be keyed to *Attulus* **based on the long fourth legs and absence of retromarginal cheliceral teeth, except only for the exclusion of *Sittisax*."**

"The three subgenera have **subtle** but mostly consistent morphological differences. *Attulus* s. str. tends to have smaller and more compact bodies, with roundish carapaces (Figs 15–38). *Sitticus* have a narrower carapace and longer legs (Figs 39–47), and (except in *A. relictarius*) a large sweeping retrolateral tibial apophysis (Figs 74, 79, 84). *Sittilong* is notable for its long first legs." [Maddison do not acknowledges author of these characters - Simon, 1901: 581 - ridiculous - but technically this is a plagiat!].

Maddison nonsensically argues superiority of the Simon's characters - "for ecologists and others ... - in the field trying to" key to Attulus ... **based on the long fourth legs and absence of retromarginal cheliceral teeth"** - instead of recognizing it at first glance (if he knew it), if he does not, then will bring it to laboratory and to look at palp or epigyne. I believe young Maddison did the same, before forgotten how he was doing that long ago.

Being thoroughly bored with nomenclatorical quarell on validity of names *Sitticus absolutus, S. callidus, S. juniperi,* versus *Attinella dorsata, A. concolor* and *A. juniperi* Prószyński accepted hesitantly (hesitation due to absence of labeled holotype - instead of using simple procedure of designating neotype, Maddison invented lost[?] label). He has also accepted Maddison's opinion (communicated in 1990ties), on position of *Attulus,* separate from *Sitticus.* These were right merits of Maddison, but joining stampede against Prószyński (after Kropf[Nentwig?] et al., 2019) Maddison moved too far in declaring that name *Sitticus* is nothing but junior synonym of *Attulus* (first version of *Sitticus* by Prószyński's 1960-1970 *à rebours*), encompassing all relevant species. Summing up these concepts, there is no much differences in organization and naming groups of species, subgenera, or genera, in understanding relationships between groups delimited by both authors.

There is difficulty with replacement of new names proposed by Prószyński 2017 by their forgotten synonyms, imposed in two papers of Blick and Marusik (2018, 2019) without courteous notification to Prószyński, normally cultivated among respectful authors. Acting in hunting emotions of ant-Prószyński crusade, initiated by Kropf [Nentwig?] 2019, in which both also participated, they did not feel obliged to behave in relation to Prószyński [who anyway "…should be ignored by the community" because "…brings nothing but chaos in salticid systematics" and "…this is nothing but scientific malpractice"... see - Kropf & twelve intellectuals, 2019]. However, at that time Prószyński has been working on proposal of the comprehensive reforming of the Salticidae naming system, related to taxonomic structure of genera, their diversity and distribution, also worked on worldwide identificatory system based on absolute diagnostic characters. Their barbarian crusading enthusiasm of "Ordnung aber muss sein" interrupted work of Prószyński for four years - is it possible to catch up on lost four years work at the age of 87?

Copernican Revolution in Salticidology

Maddison has scored a success introducing to Salticidology modern concepts of genome sequencing phylogeny and based on that new taxonomic division of the family (in fact that is eclectic combination of old Simon's 1901-1903 system, pedipalp-epigyne comparisons by Prószyński and Maddison's hypotheses on gene sequencing). The system proposed by Maddison makes appearance of something entirely new, grace introducing new units, like tribes and clade's, with slightly modified scientific names and cladistic/molecular phraseology. [**However, introduction of new formal ranks of taxa (like clades) depends from coordination in the whole zoology**], Within the last two decades editors of learned periodicals started to demand inclusion of invocations to that division in the submitted manuscripts of Salticidae papers - to which older authors yielded, fearing rejections of their manuscripts. Younger authors caught wind in sails and enjoyed being modern, so their texts are now soaked with "cladistics" - just like PhD Thesis of Ruiz, discussed above. Taxonomic division of Salticidae makes little sense without guide to identify groups of genera - new ranks of taxa looks colorful, but how to tell which is which? To answer that demand for myself, I spend decades accreting hundreds of type species revisions and testing their arrangements, so I can envy now arachnologists having a new, ready system. If it works! If I comprehend correctly, the Maddison's system serves arachnologist which already know particular genera from other sources, the task relatively easy for peoples dealing with local faunae - but how to identify genera of other, little known faunae? I note that Maddison begun nowadays to rely on characters borrowed from somewhat outdated Simon 1901-1903 (see definitions in the newest paper on *Attulus* - Maddison et al., 2020).
The speedy and easy acceptation Maddison's revolution is puzzling to me. I do not see any attempts to verify the whole concept and correctness of its particular details. **Usage of any characters as diagnostic indicators depends on their stability, verifiability, statistics of distribution, practicality of usage, compatibility with other tested characters.** I do not see confirmation of these parameters in Salticidae papers inspired by gene sequencing. Morphological characters (genitalic, general external appearance) seem to fulfill these requirements quite well at the species-genera-supra genera level, in addition color pattern of live specimens [color photography!]) are very effective for species and genera separation in some cases - will molecular data do that better? In reality Maddison's system is a compilation of old system of Simon 1901-1902, with some shifting of groups and genera, sprinkled sparsely with imprecise references to gene sequencing results, glamorized on surface with drawings of palps and internal structures of epigyne (the latter disregarded in his placement analyses), devoid of practical diagnostic part - how to recognize and place little known genera. It looks to me as theoretical lecture on phylogenetic relationships between groups of Salticidae, based mainly on guesswork, while system presented by Prószyński 2020:
(**https://salticidae.pl/offline/salticidae_genera_world_2020.pdf, https://salticidae.pl/offline/salticidae_species_attachment_2_2020.pdf**)
is designed for easy and fast identification of genera, which can also constitute morphological premises for further phylogenetic interpretations. The reaction of Prószyński to Maddison's is questioning and waiting for documentation, the reciprocal reaction of Maddison towards different views of Prószyński's is a boycott - as if they never existed (particularly importance of internal structures of epigyne).

The differences between morphological and molecular approaches **boils down to difference in methodologies of classificatory and evolutionary research**.
In classification ALL classified species should by analyzed - in a genus containing 71 species - 71 species should be studied, study of the family Salticidae containing 6000 species, out of which 4800 are recognizable, should encompass 4800 species while remaining 1200 be put aside until progress of studies permits to classify them too. *For example* - Prószyński (2016, 2017b, 2020, 2022) presented diagnostic documentation of 103 recognizable species of Myrmarachnine*/ (leaving aside 87 species as unrecognizable), 54 of other Myrmarachnine, 5 of Ligonipeinae, and 11 *of Belippo*. The methods of studies should be the same for all species, preferably conducted by the same researcher to ensure comparability of results. It is true, that equally detailed study of 4800 species is rather postulate than reality, but at least gives awareness of the true research situation. Correctness of resulting classification can be checked by their congruence with analysis of other characters, equally well chosen and studied.
Evolutionary studies are conducted on selected exemplary species, assumedly representing (often questionably) larger groups of taxa (genera, etc.). For example. Maddison's placement of the genus *Myrmarachne* (tribe) in the clade Astioida*/ **were based** originally only on a single *Myrmarachne* sp. specimens (Fig, 6A - below, Maddison, Bodner, Needeham, 2008: Appendix 1), he has later added next eight specimens (Bodner, Maddison, 2012: Table A1. - see Fig. 6B below)**/. MYRMARACHNINES are particularly difficult and make Maddison's powerless - in 2015 paper he mentions 246 species in 7 genera of Myrmarachnini but with genome sequenced only for 3 genera, and that on incidentally chosen exemplary species. Maddison used to pay lip service to Prószyński's usage of comparative morphology of palps, but maniacally pretends to not see compatible role of internal structures

of epigyne - both pillars of Prószyński's methodology. In a case of diagnostics of MYRMARACHNINES palps are too uniform to differentiate species, but spermathecae (and other details of epigyne) appears particularly practicable.

/ FOOTNOTE. Clade Astioida, **containing assemblage of dissimilar 584 species in 55 genera, is listed as new by Maddison, Bodner & Needham, 2008: 55 without diagnostic description, in fact based on Astieae Simon, 1901: 429, critically commented by Wanless 1988: 82. The 2008 paper is the one in which 7 exemplary genera sequenced were misidentified - see **Figs 1** above).

A

APPENDIX 1. Specimens newly sequenced for molecular phylogenetic analysis. Columns marked 28S, 18S, 16S, 16S-ND1, and CO1 show sequence length in pare ssion numbers. * identified by M. Zabka

Maddison, W. P., Bodner, & Needham, (2008). *Zootaxa* **1893**: 49-64.

Species	Voucher	Locality	28S	18S	16SND1	CO1
Astioida						
Myrmarachne sp. (Mal.)	d162	MALAYSIA: PAHANG	(749) EU815507		(814) EU815565	(972) EU815616

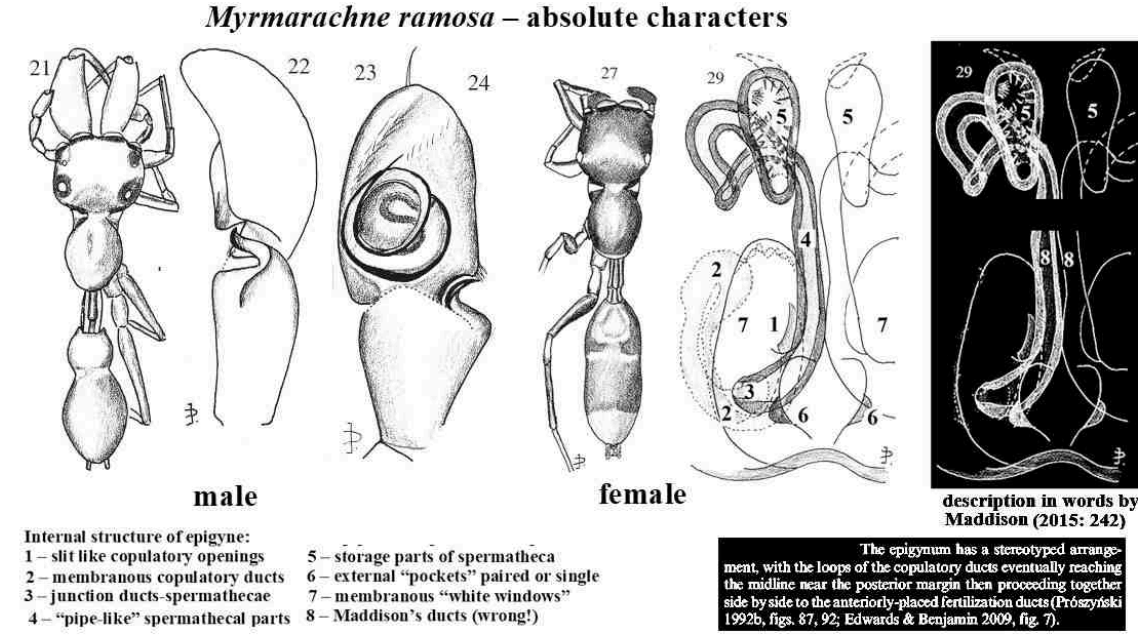
B

Table A.1 (*continued*)

Bodner, M. R. & Maddison, W. P. **(2012).** The biogeography and age of salticid spider radiations (Araneae: Salticidae). *Molecular Phylogenetics and Evolution* **65**(1): 213-240.

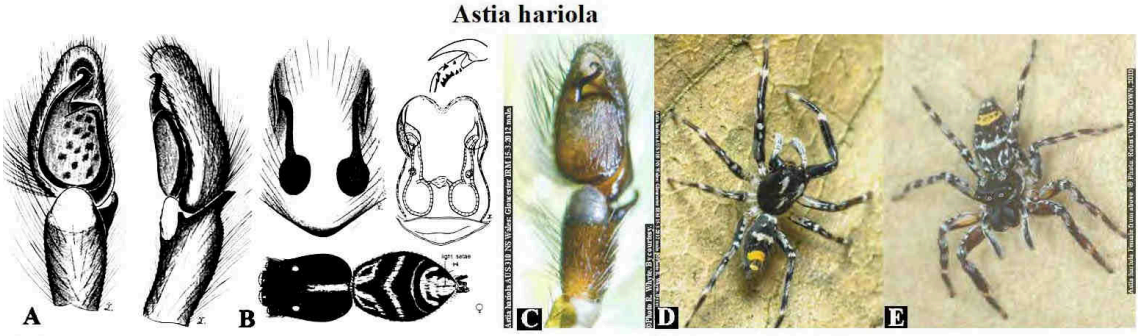
Species		Locality	28S	Actin 5C	16S/ND1	CO1
Myrmarachne assimilis Banks		Philippines: Luzon	(741)AY297284*		(582)AY296702*/ (393)AY297347*	(948)AY297412*
Myrmarachne cf. *gedongensis* Badcock	m	Malaysia: Pahang	(1044) JX145750		(899) JX145899	(969) JX145676
Myrmarachne evidens Rower	m	Gabon: Ngounié, Waka Natio Park	(1048) JX145752			(973) JX145678
Myrmarachne foenisex Simon	m	Gabon: Ngounié, Waka Natio Park	(1053) JX145753		(555) JX145901	(981) JX145679
Myrmarachne plataleoides O. P.-Cambridge	f	Singapore: Chek Jawa	(1053) JX145754		(893) JX145902	(888) JX145680
Myrmarachne sp. (tristis group)	m	South Africa: Mpumalanga Province	(1060) JX145751		(917) JX145900	(980) JX145677
Myrmarachne sp. [Malaysia]	m	Malaysia: Pahang	(749) EU815507*	(827) JX145837	(814) EU815565*	(972) EU815616*
Myrmarachne sp. [Singapore]	m	Singapore: Chek Jawa	(1052) JX145755		(895) JX145903	(968) JX145681

Figs. 6 A-B. Premise of lumping Myrmarachninae with Astioida: DNA (?) data of 9 species represented by 5 identified to genus only and 4 identified tentatively, no matched couples 7+1 specimens (**Reason for caution**: previous failure in identification of 7 genera in the 2008 paper - see Fig. 1 above)



Figs. 20–29: *Myrmarachne ramosa* Badcock. **20–25** Adult male from Genting Forest. **26–29** Adult female from Singapore. M. Edmunds & J. Prószyński 303

7A



SOURCE. : A-B - Davies Todd, Zabka 1989. Mem. Queensl. Mus., 27 (2): 208, t. 14, C-E - ©Photo R. Whyte.CE. Davies Todd, Zabka 1989. Mem. Queensl. Mus., 27 (2): 208, t. 14,©Photo R. Whyte! .

7B

Figs 7 A-B, Interesting which features used Maddison to join monotypic genus ***Astia*** (bottom) with over hundred species of ***Mymarachne*** (above). Simon himself held different opinion on relationships in Astieae, far away from Myrmarachneae as shown on the table below.

Historic Keys to the Salticid Groups by Simon's 1897-1903 translated, with an introduction and indices by H. D. Cameron and D. P. Wijesinghe **from PECKHAMIA: Volume 3 Number 1**	
SALTICIDAE PLURIDENTATI (388-390) Paths of characters separating **ASTIEAE from MYRMARACHNEAE**	
7 (6). Cephalothorax high, posterior eyes usually prominent. . . .8 8 (7). - Anterior metatarsi with at least 2-2 spines beneath. . .10 10 (8). - Fourth pair of legs much longer than the third pair . . 11 11 (10). - Anterior eye row recurved or rarely straight. Metatarsi (at least the anterior) shorter than the tibiae or at least not longer.12 12 (11). - Trochanter of the first pair of legs short, normal, tibia not inflated 13	**7** (6). Cephalothorax high, posterior eyes usually prominent . . . . .. 8 8 (7). - Anterior metatarsi with at least 2-2 spines beneath. . . . 10 10 (8). - Fourth pair of legs much longer than the third pair . . 11 11 (10). - Anterior eye row recurved or rarely straight. Metatarsi (at least the anterior) shorter than the tibiae or at least not longer. …12 12 (11). - Trochanter of the first pair of legs short, normal, tibia not inflated 13 ------ 13 (12). - Thoracic groove very minute or absent. . 14 20 (7). - Tibiae of the first pair of legs slender and cylindrical -. . **22**
13 (12). Pars thoracica impressed with a deep thoracic groove. **. 8. ASTIEAE 429 (cf Astia) [Lagnus, Astia, Helpis, Arasia, Titananus, Anaurus, Agelista, Lapsias, Charippus]**	22 (**20**). Sternum strongly attenuated in front, virtually pointed, and the coxae of the first pair of legs very close together. The upper margin of the anterior eyes form a slightly procurved, or rarely straight, line. **22. MYRMARACHNEAE 496 (cf Myrmarachne) [Panachraesta, Emertonius=? Myrmarachne, Bocus, Bizone=Bizonella=Myrmarachne]**

Relationship system of Astioida proposed by Maddison for 584 species in 55 genera

Salticoida: Astioida (584 species in 55 genera)		
Tribe Myrmarachnini (246 species in 7 genera) * - taxon with DNA sequenced	**Tribe Neonini** (27 species in 1 genus)	**Tribe Astiini** (54 species in 11 genera)
Belippo * Bocus Damoetas Judalana Ligonipes * **Myrmarachne*** Rhombonotus	Neon *	Arasia * **Astia** Astilodes Helpis * Jacksonoides * Katya Megaloastia Orthrus * Parahelpis Sondra *
Tribe Mopsini (12 species in 3 genera)	**Tribe Viciriini (other than Simaethina)** (176 species in, 20 genera)	**Tribe Viciriini: Subtribe Simaethina** (69 species in 13 genera)
	* - taxon with DNA sequenced (single species?)	

I am suspective rather than critical to relying on gene sequences as diagnostic characters, so am waiting for more information, especially that published results for Salticidae are rather personal opinions ["... our data are supported by molecular findings"] than precise facts.

Why alternate system based on verifiable "absolute" characters
(palps, internal structure of epigyne, photographs of live individuals)
failed to receive due recognition

I am asking myself this question, not explainable by any error, material or logical, over and over again. Perhaps the system is not geared to answer needs of possible users - guide to identification of some 641 genera and +6000 nominal species is useable by students of the taxonomy of the whole globe Salticidae. There are very few such persons, at present I can name five, dealing with unknown faunae of continents and archipelagoes. Average taxonomist works on local fauna (part of a continent, or a country) having now easier aids to identification of interesting species: local keys, atlases and exemplary individual publications. Trial of syntheses based on local publications, selected from the WSC records, can be less trustable, but few readers would realize that, possible doubts being irrelevant for publishers.
It may be satisfaction for an intelligent arachnologist to understand well tested system, but it has a price - a lot of individual work, inconvenience of opposing beliefs of a majority. For the arachnologists experienced in local faunae declarations on broader relationships of groups of species can be attractive as a proof of higher knowledge and broader intellectual horizons of the speaker, but creation of one, or verification of an acquired one requires really extensive knowledge of faunae above single continent level. Syntheses of relationships of whole broadly distributed genera can lead to risks of changing local names of popular species, and that is detested by local minded experts. Readers of Simon-Petrunkevich system had no choice other than repeat hearsay of that time. Starting from development of two other systems after 2003, the choice was influenced by editorial policies of learned periodicals. Suddenly system proposed by Maddison became published in variety of publications of Salticidae, not because of own initiative of authors, but under pressure of editors. I met personally two cases of such influence - en editor of my large paper on Myrmarachninae, in excellent "Arthropoda selecta", demanded endorsement of Maddison's

hypotheses in the text, I declined, the paper was never published. A colleague was wiser - receiving very gentle suggestion from "Arachnologische Mitteilungen" yielded to it, fearing "losing possibility of publishing" in the future. Editors have right to select printable text according to own evaluation, but acceptation without real consideration - that is something below my expectation. I knew also a case of rejection of deserving texts just as retaliation. So, according to old political joke "gentle pressure leads to merry enthusiasm".
I think that honest discussion of controversial hypotheses could only benefit our science. Evaluations, and possibly influencing decisions should be taken after mature considerations by competent researchers, not by secret collusion. As it happened, the collusion which yielded the notorious paper by Kropf et al., 2019, resulted in character assassination to me. Who was a "*spiritus movens*" of all of that?

For more background details read survived correspondence at:
1 -Maddison-Prószyński 1998-2009 **see: https://salticidae.pl/2_SAMIZDAT/archiv_corresp/corresp_maddison_98-2016.pdf**
fragments of correspondence with
2 - G. B. Edwards - 2014 see: **https://salticidae.pl/2_SAMIZDAT/archiv_corresp/edwards_interesting.pdf**

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Prószyński, J. (2020b). Salticidae (Araneae) genera of the world and species diversity - an atlas. Vol. 2. Attachment on Salticidae species diversity. On line: **https://salticidae.pl/offline/salticidae_species_attachment_2_2020.pdf.**