Additional notes on the taxonomy of *Parakeelya* Hershk. (Montiaceae)

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ABSTRACT

Parakeelya Hershk. (Montiaceae) has become the name conserved over the older name *Rumicastrum* Ulb. for a lineage of Australian plants historically classified in *Calandrinia* Kunth. In Candollean taxonomy persistent to the late 20th Century, Australian plants were classified in a large, heterogeneous, and polyphyletic circumscription of *Calandrinia*, later referred to by the designation "*Calandrinia* s. l." Following cladistic dissection, *Calandrinia* "s. str." was restricted to a small, homogeneous, and well-supported clade of American plants. The Australian plants were referred to the formerly monotypic and poorly studied genus *Rumicastrum* Ulb., and later to *Parakeelya*, which specifically excluded the latter. Australian specialists, however, eschewed both of these names and continued to classify existing and new Australian species as *Calandrinia*. However, they never justified this usage on taxonomic evidence. In some cases, they used the name *Calandrinia* as though it applied exclusively to the Australian plants, and they never explained why *Rumicastrum* does or does not pertain to this lineage. Phylogenetics researchers later appropriated the designation "*Calandrinia* s. l." to refer to the Australian lineage plus *Calandrinia* s. str., and predicated to disprove its monophyly, which never was supported in the first place. They demonstrated that *Rumicastrum* indeed pertains to the Australian lineage, but they proposed nomenclatural conservation of *Parakeelya*. Yet, in numerous subsequent publications, they continued to use and describe new Australian species in *Calandrinia*. In the present work, I demonstrate that the application of the name *Calandrinia* to the Australian species and the designation "*Calandrinia* s. l." for this plus *Calandrinia* s. str. were *conceptually illegitimate*, because they are *conceptual homonyms* for the taxa to which these names had been applied. I discuss evidence that this usage was deliberate with the objective of preventing the correct name *Rumicastrum* from being accepted for the Australian lineage. The evidence includes but is not limited to earlier-reported irregularities in the Australian authors' proposal to conserve the name *Parakeelya*, including considerable factually incorrect or otherwise misleading information that biased in favor of the later approved conservation. I discuss this in terms of the equivalence of scientific names to scientific assertions, be they correct or erroneous, and incidentally or deliberately so.

Kew words: *Calandrinia*, *"Calandrinia s. l.*," *Parakeelya*, *Rumicastrum*, Montiaceae, Australia, taxonomy, nomenclature, ICN, conceptual illegitimacy, conceptual homonymy.

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Introduction

Hershkovitz (2023) reviewed the nomenclatural history of an Australian clade of plants whose conserved generic name is to be *Parakeelya* Hershk. The older name for this operational taxon, *Rumicastrum* Ulb., now is to be considered a synonym. The history is complicated, because both historical and later Australian literature on these species referred them to a *different* genus, *Calandrinia* Kunth.[1] Besides recombining with *Parakeelya* several outstanding names of Australian species,

[1] I am reminded of a lyric from the song "Rocky Raccoon" by Paul McCartney and John Lennon: "*…her name was McGill, and she called herself Lil, but everyone knew her as Nancy.*"

Hershkovitz (2023) criticized the Thiele et al. (2018) conservation proposal, its acceptance by the IAPT General Committee, and in general the classification of Australian species in the genus *Calandrinia* during the past three decades. The criticisms were presented in the form of itemized observations. The present work adds to, complements, and synthesizes these observations into a more coherent criticism that describes the historical classification of Australian species in *Calandrinia*, prior to and especially following Thiele et al. (2018), as *conceptually* illegitimate. Additional observations are offered that support an explanation of historical *deliberate* misuse and misrepresentation of *Calandrinia* towards the objective of avoiding the correct but undesirable generic name *Rumicastrum* Ulb. The significance vis-à-vis science and scientific taxonomy is discussed.

1. Clarification of the contemporary taxonomy of *Calandrinia* and *Calandrinia* s. lato.

Hershkovitz (2023) overlooked how Thiele et al. (2018) might give the impression that corroboration of Carolin's (1987) dissolution of Candollean *Calandrinia*, viz. "*Calandrinia* s. l." (sensu Carolin non Thiele et al.; see below), remained an incomplete and piecemeal "work in progress," and that its broader acceptance remained limited. They remarked that two of Carolin's segregate genera (*Cistanthe* Spach and *Schreiteria* Carolin) have been accepted, while a third (*Baitaria* Ruiz & Pav.) was not. These statements are misleading, because they do not capture the taxonomic complexity of Carolin's (1987, 1993) scheme, the history of its subsequent modifications, nor the theoretical difference between cladistic and typological taxa (discussed further below).

As for the purely taxonomic complexity, Carolin's (1987, 1993) circumscription of *Cistanthe* was not merely a *Calandrinia* s. l. "segregate:" it also included *Philippiamra* Kuntze (syn., *Silvaea* Phil., nom. illegit., non *Silvaea* Hook. & Arn.). Species of *Philippiamra* never were classified in *Calandrinia* or even in its tribe (e.g., McNeill, 1974). Hershkovitz (2019) resurrected *Philippiamra*, but the new circumscription includes species originally classified in *Calandrinia* s. lato…and referred to *Cistanthe* by Carolin (1987, 1993). The genus *Monocosmia* Fenzl likewise historically was excluded not only from *Calandrinia* s. l., but from whatever tribe in which the latter was classified (e.g., McNeill, 1974). Carolin (1987) found this genus to be sister to the annual species of *Calandrinia* s. str. ("*Calandrinia* s. str. sensu Carolin"), but he nonetheless retained *Monocosmia* as a distinct genus. Hershkovitz (1993a) submerged *Monocosmia* into *Calandrinia* and it has remained there in all subsequent classifications of Montiaceae.

Carolin's (1987, 1993) circumscription of *Baitaria* "s. l." included five sections from Reiche's (1897) classification of Chilean *Calandrinia* s. lato. Hershkovitz (1993a) restored *Baitaria* "s. str." (*C.* sect. *Caespitosa* Philippi; syn. *C.* sect. *Acaules* Reiche) to *Calandrinia*. The remainder of *Baitaria* "s. l." thus became *Montiopsis* Kuntze, a *Calandrinia* s. l. segregate thereafter universally accepted. Thus, Thiele et al.'s (2018) indication that *Baitaria* (sensu Carolin) later was retained in *Calandrinia* s. str. is misleading.

Finally, Thiele et al.'s (2018) characterization of Carolin's work might give the impression that his proposed taxonomy of *Calandrinia* s. l. was completely new. It was *novel*, but from a purely operational (as opposed to theoretical) standpoint, it was by no means *de novo*. Carolin's (1987) taxonomy and subsequent modifications cleanly coincided with what was then the accepted *sectional* taxonomy of *Calandrinia* s. lato. The sections were reapportioned among the cladistic genera. The Australian species were no exception: there was *no* mixing of New World and Australian endemic species in the sectional taxonomy (see also below).[2] In this sense, the supposed question of whether the Australian species

[2] See Hershkovitz (2021a) for the "exception that proves the rule" involving a *Calandrinia* section that comprised a North American *Calandrinia* species adventive in Australia plus two (presumably) Australian native species that evidently do not pertain to Montiaceae or even Portulacineae.

pertained to *Calandrinia* was a red herring. At the sectional level, they *always* were segregated from and never otherwise taxonomically grouped with *Calandrinia* s. stricto. Kelley (1973) suggested that the Australian and New World taxa of *Calandrinia* s. l. should be classified in different *subgenera*.

Hershkovitz (2019) noted that the Carolin/Hershkovitz phylogenetic classifications were not accepted initially. This owed mainly to resistance from specialists on particular genera and/or local floras, and it stemmed mainly from lack of understanding of cladistic theory and method. At that time, cladistics was relatively new to botanical taxonomy. But by the mid-1990s, even before the emergence of molecular data, the dissolution of *Calandrinia* s. l. became broadly accepted. For example, the cladistic classification of Hershkovitz (1993a) was used in Flora North America (Packer, 2004). Although this was published in 2004, the generic taxonomy was drafted nearly ten years earlier. The cladistic classification was accepted even earlier by South American specialists (e.g., Peralta, 1993, 1994). Thereafter, one or another variant of the "dissolved" taxonomy of *Calandrinia* s. l. was adopted by several independent researchers whose multiple molecular investigations corroborated in principle Carolin's (1987) thesis, viz. the polyphyly of *Calandrinia* s. l.

What Thiele et al. (2018) did not make clear was that, at that writing, generic classifications of Montiaceae *globally* adhered to a "dissolved" *Calandrinia* s. l. taxonomy.[3] More importantly, they did not make clear that, whatever variant of this taxonomy was used, all of them *unanimously* accepted the Hershkovitz (1993a) circumscription of *Calandrinia*, viz. as a small genus of New World species. And all of them *unanimously* recognized the Australian species as a *different* genus, by whatever name. Thus, Hernández-Ledesma et al. (2015) did not even *use* the terms "*Calandrinia* s. str." and "*Calandrinia* s. l.," presumably because the distinction was deemed no longer necessary or informative. It could be, however, misinformative or disinformative (Hershkovitz, 2023), as I demonstrate below.

2. Clarification of the historical origins and demise of *Calandrinia* s. l.

I turn now to the question of why the Australian species were classified in *Calandrinia* in the first place, because the answer bears directly on the premise of Thiele et al. (2018). Hershkovitz (2021b) described the taxonomic history of *Calandrinia* s. l. in the context of the familial classification. This 19–20th Century taxon was established by Candolle (1827, 1828) as a *segregate* of *Talinum* Adans. s. l.,[4] which, in turn, emerged as a segregate from *Portulaca* L. It is important to appreciate that, in this period, while conceived as "natural" in some sense, taxa were not conceived as "lineages" or *phylogenetic* clades (but see Williams & Ebach, 2020; cf. Hershkovitz, 2021c). They were conceived as *typological* entities, viz. groups of lower-ranked taxa pertaining to taxonomic neighborhoods established by the type species of the next higher ranked taxon. Taxa were circumscribed phenetically and usually on the basis of some readily-diagnosed and/or otherwise convenient and/or supposedly "important" trait or trait combination. *Segregation* of taxa in this period generally referred to "splitting," viz. subdividing a taxon at the same and recognizing two or more taxon types instead of one.

Following Candolle, both *Calandrinia* s. l. and *Talinum* s. l. expanded to include newly discovered species. Both became (even more) heterogeneous "trash can" taxa for species considered to belong to one or the other. As Hershkovitz (2021b) noted, particular Australian species had been described originally as *Talinum* species, but eventually all species were classified in *Calandrinia* s. lato. They were perceived as

[3] Another reference, Takhtajan (1997) accepted the Candollean genera of Portulacaceae s. l., but Takhtajan (2009) adopted Carolin's (1993) genera, including *Rumicastrum*.

[4] *Talinum* s. l. here refers to an emended Candollean circumscription (see Hershkovitz, 2021b), which included current *Talinum* (Talinaceae) and *Phemeranthus* Raf. (Montiaceae). But it excluded *Talinella* Baill., which now is included in *Talinum*.

belonging to the taxon *Calandrinia* s. l., but their link with *Calandrinia* s. str. was purely nomenclatural, viz. it reflected nothing more than the priority of this name for this large and heterogeneous taxon.

Reiche (1897) established a sectional taxonomy for the Chilean species of *Calandrinia* s. l. (where the overwhelming number of New World species occur), and this was extrapolated by Kelley (1973) to accommodate the non-Chilean species. Poellnitz (1934) established separate sections for the Australian endemic species. Notably, New World and Australian species were not studied comparatively until Kelley (1973), and not after that until Carolin (1987). Kelley (1973) retained the Candollean typological taxon *Calandrinia* s. l., but he suggested that the Australian taxa should be segregated as a subgenus. Carolin (1987, 1993), of course, referred the Australian taxa to *Rumicastrum* Ulb. In any case, following Poellnitz (1934), the Australian species *always* had been segregated from New World *Calandrinia* s. lato…that is, until Hancock et al. (2018; see below).

It also is worth noting that the problem historically with *Calandrinia* s. l. was not how and whether to divide it, but how and whether it was distinct from other genera (Hershkovitz, 2021b). A reference I overlooked in Hershkovitz (2021b) was Bentham (1862), who wrote:

> "*Talinum* [s. l.], *Calandrinia*, and *Claytonia* [L.; then including most of *Montia* L.] are also very closely allied to each other, being only separated by the sepals, deciduous in *Talinum*, persistent in the two others, or by the stamens, constantly 5 (one opposite each petal) in *Claytonia*, anisomerous with the petals and *usually* [italics mine] more numerous in *Talinum* and *Calandrinia*. These characters are moreover not quite constant; yet, as each group comprises a considerable number of species bearing other general resemblances to each other, we feel that it would not be safe to recommend their union into one genus without a more detailed examination of every species than can be undertaken on the present occasion."

Here, Bentham seems to have acknowledged the observations of Ferdinand Mueller, with whom he collaborated on *Flora australiensis* (Bentham, 1863), while rejecting Mueller's consequent taxonomic opinion. Indeed, Mueller merged *Calandrinia* s. l. (including the Australian species) into *Claytonia* (see Hershkovitz, 2021b). In the later 19th Century, *Lewisiopsis* Govaerts and several current species of *Lewisia* Pursh also were described as species of *Calandrinia*. Kuntze (1891), citing Mueller, merged these and also *Talinum* s. l. into *Claytonia*. Effectively, as species were added to both *Talinum* s. l. and *Calandrinia* s. l., the generic heterogeneity and polymorphism increased to the point that blurred the distinction not only between them, but between them and other genera.[5]

But the essence of Bentham's (1862) discussion demonstrates how Candollean classifications attempted to establish genera firstly according to some "major" diagnostic distinction and thereafter according to a criterion of "connectivity,"[6] viz. species within a genus resembled some other species within that genus more than species of a different genus. Since the major diagnostic distinction did not "work" in the case of these genera, more weight was placed on this "connectivity" than generic diagnostics. But interspecific similarity also ought not to "work" in otherwise heterogeneous genera, i.e., species might resemble some other species in that genus but not all or even most of them. This proved to be the case for *Calandrinia* s. l. species.

The above notwithstanding, with current *Claytonia*, *Lewisia*, *Lewisiopsis*, *Montia, Talinum*, and *Phemeranthus* excluded, the circumscription of *Calandrinia* s. l. stabilized during the 20th Century. But in

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[5] Hershkovitz (2021b) documented how this blurring already was evident in Candolle's (1828) classification, which is why at least two contemporaries rejected segregation of *Calandrinia* from *Talinum*, and J. D. Hooker repeatedly questioned it.

[6] This might be the taxonomic manifestation of the principle of "connectivity" then used to establish morphological homology, but this conjecture is beyond the scope of the present discussion.

several cases, "perfectly good"[7] species of *Calandrinia* s. l. were classified as *Talinum* s. l. and vice versa. The reason for the ambiguity, of course, was the diversity accommodated within each genus, viz. the larger the trash heap, the more trash it holds, and the more it begins to overlap with adjacent trash heaps. This ought to have been a clue that the naturalness of both genera required new scrutiny.

An obstacle to this scrutiny was the classification of both *Talinum* s. l. and *Calandrinia* s. l. in the same tribe, which, in turn, excluded genera currently congeneric with elements of *Calandrinia* s. l., such as *Monocosmia* and *Philippiamra*. The generic and tribal classification of Candollean Portulacaceae (Portulacaceae s. l.)[8] emphasized gynoecial and fruit morphology. *Talinum* s. l. and *Calandrinia* s. l. shared, among other traits, 3(-6)-carpellate gynoecia and valvate capsules. These genera thus excluded *Monocosmia*, which had a 2-carpellate valvate capsule, and *Philippiamra*, which has a 2-carpellate achene. McNeill (1974) thus classified *Talinum* s. l. and *Calandrinia* s. l. in his tribe Talineae, *Monocosmia* in his tribe Calyptridieae (Franz) McNeill, and *Philippiamra* in his tribe Portulacarieae Fenzl, along with *Portulacaria* Jacq. and *Ceraria* Pearson & Stephens, currently classified in Didiereaceae.

During the 20th Century, owing to the popularization of Darwinian theory, earlier typological higher-level taxonomy suddenly was denominated as "evolutionary taxonomy," without otherwise changing taxonomic criteria or methodology.[9] Evolutionary taxonomists merely drew presumed evolutionary arrows between the typological taxa or arranged them hierarchically in nebulous and seemingly LSD-inspired diagrams. Typological-cum-evolutionary taxa provided clues to cladistic relationships, but they also obscured them. In particular, their circumscriptions were like fortifications that obscured their para-/polyphyly with respect to excluded taxa. No degree of mathematical analytical rigor could overcome this, not in the case of interfamilial cladistic analysis (as Hershkovitz, 1989, noted for Caryophyllales), nor infrafamilial.

For example, *Monocosmia* and *Philippiamra* were separated from *Calandrinia* s. l. at the generic and *tribal* levels. *Monocosmia* was identical to sympatric annual species of *Calandrinia* s. str. in leaf morphology and anatomy, sepal morphology, and pollen morphology. It differs markedly in these traits from *Calyptridium* Nutt., with which it shares only a 2-carpellate valvate capsule. Likewise, *Philippiamra*, annual herbs endemic to the Chilean Floristic Region (Hershkovitz, 2019), was identical to certain so-endemic *Calandrinia* s. l. species in growth form, leaf morphology and anatomy and inflorescence and sepal morphology.[10] It departed markedly in these traits from *Portulacaria* and *Ceraria*, which, furthermore, are *caudiciform shrubs and trees* endemic to arid Africa. *Philippiamra* resembles these genera only in fruit type, viz. an achene. Thus, no morphological cladistic analysis of classical Portulacaceae at the classical generic level could have inferred correct phylogenetic relations.

Carolin's (1987) "new approach" recognized that *Talinum* s. l. and *Calandrinia* s. l. might not represent clades. He broke these down to the sectional level in order to realize a cladistic analysis of classical Portulacaceae. This was effective in demonstrating that the sections intercalated cladistically among other genera. Hershkovitz (1993a) revised and expanded this analysis, but Carolin's (1987) conceptual breakthrough was irreversible. Hershkovitz (1993a) and eventually all molecular and genomic (including Hancock et al., 2018) analyses found no strong evidence supporting a sister relation between the Australian species of *Calandrinia* s. l. and *any* lineage that was recognized as a genus in *any* cladistic taxonomy.

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[7] Meaning species whose close similarity to other species in the *correct* genus was overlooked, which rendered moot the generic diagnostics.
[8] Portulacaceae currently comprises only *Portulaca*.
[9] Sort of like when Twitter changed to "X."
[10] As astutely noted by Kelley (1973).

It is important to appreciate that the various cladistic taxonomies that emerged from Carolin's (1987) and subsequent analyses are not equivalent *conceptually* to the typological Candollean taxonomy. The generic taxonomies and circumscriptions cannot be compared as such. In other words, there is the Candollean taxonomy, which includes a typological but polyphyletic taxon called *Calandrinia*, later called *Calandrinia* s. l., and there are various cladistic taxonomies, in which *Calandrinia* s. l. is dissolved and does not exist. There is nothing in between.

Thiele et al. (2018) gives the impression that the Australian species *remained* classified in *Calandrinia* even as other species were "segregated." This is false. These authors…and *only* these authors[11]…continued to classify the species in *Calandrinia* without justification, cladistic or typological (see below). There is *no* cladistic taxonomy that conserves *any portion* of typological taxonomy, except by *coincidence* of names and circumscriptions.

This preceding point is not obvious, because both taxonomies are constructed in accordance with the ICN (Turland et al., 2018), which stipulates that the correct name and Type for a taxon, whether typological or cladistic, must be the oldest valid and legitimate name and Type applied to any member of that taxon. Thus, both taxonomies unavoidably share *many* generic names, including *Calandrinia*, often referred to as *Calandrinia* s. str. to distinguish it from *Calandrinia* s. lato. But the former *is not a segregate* of the latter, because segregates of taxa in cladistic and typological taxonomies, like the taxa themselves, *are not equivalent*. Typological taxa are formed on the basis of perceived affinity to presumed types and segregated according to perceived differences between presumed types. Cladistic taxa are formed on the basis of synapomorphy and segregated by dividing trees into subordinate clades.

3. Clarification of the contemporary usage of names for the Australian species of *Calandrinia* s. lato.

Hershkovitz (2023) indicated that, worldwide, the Australian species were referred overwhelmingly to as *Calandrinia* rather than *Parakeelya* or *Rumicastrum*, and that few of the 35 available *Parakeelya* combinations actually had been cited in a publication. Had Thiele et al. (2018) mentioned this, the quantitative difference been historical *Parakeelya* and *Rumicastrum* usage would have been exposed as trivial, obviating the conservation proposal itself. In the next section, I point out that considerable historical usage of *Calandrinia* by Thiele et al. (2018) and Hancock et al. (2018) authors was conceptually illegitimate.

Nonetheless, I must clarify my earlier usage remarks. Historical usage here reflects mainly two parameters: **(1)** taxonomy within Australia versus outside of Australia, viz. Australian specialists (besides Carolin) used *Calandrinia* and non-Australians used *Rumicastrum* or *Parakeelya*; and **(2)** species-level taxonomy versus generic-level taxonomy viz. usage of *Calandrinia* reflects the large number of species names and the generation of numerous species-level taxonomic, floristic, and ecological publications (generated almost exclusively in Australia) compared the few generic-level references involving only one or two generic names. Do the math. Thus, while *Calandrinia* was used overwhelmingly in publications, this was not indicative of global opinion on the generic-level taxonomy.

The "exceptions to the rule" were the various global taxonomic databases (e.g., GBIF Secretariat, 2017; POWO, 2023; WFO, 2023; Tropicos, without year) which classify taxa at *all* ranks. But because Australian specialists named new species only in *Calandrinia*, these databases were constrained

[11] I ignore here species named by Syeda Saleha Tahir, since her works based on her 1970–1980's research, which she published piecemeal belatedly.

operationally to classify *all* Australian species in *Calandrinia*, while adopting a "dissolved" *Calandrinia s. l.* classification for non-Australian species. This might give the impression that taxonomies outside of Australia accepted the artificial Australian generic classification. To most users, these databases might *appear to* and indeed *try to* be authoritative. But their function constrains them otherwise. Indeed, the world's most up-to-date and rigorously researched global floristic checklist, Govaerts (2021), *correctly* adopted *Rumicastrum* when Hershkovitz (2020a) made the combinations available. And this checklist was followed in the comparably important in POWO and GBIF classifications.

4. Conceptual legitimacy of application of the name *Calandrinia* to the Australian species

To recapitulate, on the surface, classification of the Australian species in *Calandrinia* following development and acceptance of the cladistic taxonomy appears to be a *vestige* of the Candollean typological classification. If one adheres otherwise to the Candollean classification of *Calandrinia*, this classification is legitimate. But if one otherwise adheres to a *cladistic* classification of *Calandrinia* s. l., this classification becomes *conceptually illegitimate*. These classifications are not compatible and cannot be used mix-and-match. There have been typological classifications of Portulacaceae s. l. that diagnosed and described *Calandrinia* s. l. (including the Australian species), and there have been cladistic classifications that diagnosed and described *Calandrinia* s. str. (excluding the Australian species; Carolin, 1993 emend Hershkovitz, 1993a). But, as I noted in Hershkovitz (2023), there does not exist any formal classification of Portulacaceae s. l. or Montiaceae that diagnosed and described a genus that includes the Type of *Calandrinia* s. str. and the Australian species. Thiele et al. (2018) and other publications by these and collaborating authors effectively declared the existence of such a genus. But it did not exist. They made it up in order to avoid using the name *Rumicastrum*.

In my previous criticisms of the Australian literature (Hershkovitz, 2019, 2020a, b, 2023), I did not appreciate the question of legitimacy of *application*. Legitimacy of *names* and legitimacy of application are two different things. ICN regulates only the former. If names are applied illegitimately in *taxonomic protocols*, e.g., in describing new taxa, then ICN can reject the names of those taxa. But in any other context, anybody is free to apply any taxon name they like to any taxon they like, without fear of ICN SWAT teams raiding their home just before dawn and taking them to Gitmo.

It is useful here to clarify the notions of validity and legitimacy per the ICN. Validity refers to *authenticity*. A valid name, also referred to as a *validly-published* name, conforms to ICN rules concerning orthography, syntax, diagnosis and publication. Names used in the literature that do not conform to these rules are invalid and are termed "designations." These include not only invalid names that otherwise look like valid names,[12] but also *informal* designations. These include non-Latin vernacular names, but also names that qualify a valid name in an invalid manner. Examples include the "phrase names" of the form "*Calandrinia* sp. [locality]" used in Hancock et al. (2018) and much of the literature cited here. Also included are names such as "*Calandrinia* s. l.," "*Calandrinia* s. str.,"…and "Australian *Calandrinia*." These *refer to* a valid name, but the whole name does not, because the qualifiers do not accord with ICN syntax.

[12] Hershkovitz (2020c) described and discussed a species he called *Calandrinia jompomae* Hershk. At the time, the name was invalid, because it was not published in a valid journal. Hershkovitz (2022a) published the name and diagnosis again in a valid journal, but the diagnosis was published in Spanish and not English or Latin, as required by ICN. Hershkovitz (2022b) finally correctly validated the name. Note that later valid publication renders valid the *use* of the name in earlier ones, i.e., the information reported in the earlier publications can be attributed correctly to *C. jompomae*.

Since the ICN does not recognize invalid names, it does not regulate designation format or usage. The only "rules" that apply are negative. For example, designations are not valid names, therefore are not valid synonyms, and they have no valid synonyms. Thus, the ICN-defined symbols "=" and "≡" have *no meaning* when specifying equivalency between valid names and designations. Some authors always include designations in quotation marks to avoid confusion with valid names. But, in general, names in the literature must be treated as "buyer beware" in terms of their validity.

An *illegitimate* name is something else. It is a valid name (otherwise no ICN rules would apply) that is illegitimate by virtue of superfluidity (a new name for a taxon having a valid and legitimate older name), homonymy (a newer name identical to an older name for a different taxon), or improper typification (a new name for a taxon in a sense that excludes the Type of the original name).

In this section, I consider the notion of *conceptual illegitimacy* and the *illegitimate designation*. While it is outside of the scope of ICN rules, the proliferations of designations (viz. *invalid* names), both conceptually legitimate and illegitimate, populate the literature and databases. Their conceptual illegitimacy might not be obvious and recalcitrant to identification, because the ICN does not recognize designations as valid names. This creates a sort of Catch-22 situation. Technically, one cannot formally identify a taxon named using a designation, because, per ICN, it has no Type, hence no such taxon exists. But if a taxon cannot be identified *taxonomically*, there is no point in naming it in the first place. The *operational* identification of a taxon referred to by a designation name remains challenging. More challenging is determining if the name was applied legitimately or illegitimately.

Was the application of the name *Calandrinia* to the Australian species conceptually legitimate? That depends. Under the Candollean circumscription, the typological *Calandrinia* s. l., it certainly was legitimate. Under any variant of the cladistic classification, its conceptual legitimacy depends on whether the Australian species are considered to pertain to the genus that includes the Type of *Calandrinia* s. stricto. But the application otherwise is conceptually illegitimate.

Reexamination of the earlier literature by Australian specialists J.G. West and F. Obbens does not make clear their taxonomic criterion, viz. whether they otherwise accepted the typological *Calandrinia* s. l. or the cladistic *Calandrinia* s. stricto. Obbens (2006) only explained why they rejected *Parakeelya*, and this seems have been on cladistic grounds, viz. the *possibility* that this taxon included *Rumicastrum*.[13,14, 15]

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[13] Based on details reported in Obbens (2019), Hershkovitz (2023) conjectured that, long before he and West coauthored Hancock et al. (2018) and the consequent (but *earlier* published!) Thiele et al. (2018), they were less "uncertain" of the relations of *Rumicastrum* than they indicated, e.g., in Obbens (2006). I now confirm this. Obbens (2019) discussed the close phylogenetic relationship between *Rumicastrum* and certain *Rumicastrum*-like species that he classified in *Calandrinia*. But he did not explain why the *Calandrinia* species were classified in *Calandrinia*. I had overlooked that, earlier, Obbens (2011) discussed the *Rumicastrum*-like traits of these *same* species…but here he did not mention *Rumicastrum*. Obbens (2019) *later* revealed that one species that had been collected repeatedly since the 1970s and by himself since 2004 was described in an unpublished thesis in 1979. He cited 29 collections, but noted that he had not examined two of them, because they were on loan to CANB, where West works. I also overlooked Syeda & Ashton (1989), who referred to this described but as-yet unpublished species, so the work was not completely buried in a thesis.

[14] Hershkovitz (2023) also pointed out that, despite stated awareness of the *Parakeelya*/*Rumicastrum* question and its critical importance in the context of Montiaceae taxonomy, and I add here despite the nine specimens from eight collections of *Rumicastrum* in Perth (AVH, 2023), Obbens, to my knowledge, has not once described the *characteristics* of *Rumicastrum* a propos its similarities to and differences from other Australian species classified in *Calandrinia*. This, it seems, would be a top priority for any taxonomist interested in resolving the correct name for the genus they principally studied.

[15] I add here that, given the morphological evidence, the absence of DNA data for *Rumicastrum* at this point was a red herring. Contrary to what is generally believed, molecular systematics is based only infinitesimally on DNA data and asymptomatically completely on... *morphology* or, rather, morphological correlation. In particular, the number

But he did not explain why, therefore, *Calandrinia* was applied. Other publications by these authors that likewise did not describe the circumscription of *Calandrinia* did not even mention *Rumicastrum* or *Parakeelya.* West & Chinnock (2013) referred to ongoing phylogenetic research, but did not refer per se to cladistic taxonomy. West and Obbens never justified their circumscription of *Calandrinia* on cladistic evidence or, for that matter, any evidence at all.

It seems that West and Obbens *first* defined their circumscription of *Calandrinia* as *coauthors* of Thiele et al. (2018) and Hancock et al. (2018). Here, the authors stated – *after* the fact – that *Calandrinia* "has been" conceived as an (evidently polyphyletic) genus comprising two lineages, viz. *Calandrinia* s. str. and the Australian species. But this description is problematic, because earlier work by the lead phylogeneticist in Hancock et al. (2018), viz. E.J. Edwards, had segregated the Australian species (Ogburn & Edwards, 2010, 2015). The circumscription in Thiele et al. (2018) and Hancock et al. (2018) seems to be a unique example of "taxonomic transgressive segregation," a novel circumscription emergent from hybridization of otherwise incompatible circumscriptions expressed previously by the separated authors.

Researching this further leads me to believe that West and Obbens use of *Calandrinia* reflected neither typology, nor cladistics. Conceptually, it more resembles local "folk taxonomy," the name that they learned, not unlike a vernacular name. Vernacular or "folk" names, of course, have no legitimacy per the ICN. However, the prevalent use of legitimate names in the vernacular and not necessarily the correct formal taxonomic sense does raise certain problems that the ICN deals with. The criterion for legitimate usage always is whether the Type of a name is implicitly or explicitly included in its usage. No matter how prevalently used, a name is not legitimately applied when it excludes the Type. This cannot be altered by nomenclatural conservation.

So, following Carolin (1987), West and Obbens might have been inclined to retain "familiar" usage. The plants themselves did not change, so *¿why should the name*? But West and Obbens never had to and never did "deal with" *Calandrinia* s. l. taxonomy globally. Their publications never discussed or even mentioned non-Australian taxa classified in *Calandrinia* s. l. or *Calandrinia* s. stricto. Whether their usage was typological or cladistic might not have been a concern to them, nor whether their usage included or excluded the Type.

Supporting an interpretation of "folk" usage of *Calandrinia* is Obbens (2011),[16] in which, besides describing new species as *Calandrinia* without qualification, he summarized variation in certain traits across the breadth of the Australian species. But the trait descriptions pretend to be descriptions for the *entire* genus. Here, Obbens applied the name *Calandrinia* to the collective Australian species as though these alone constitute the genus. Moreover, the circumscription of the genus was not specified, viz. the

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of sampled individuals and loci is infinitesimal compared to the number of individuals that exist. Beliefs about the relations of unsampled to sampled individuals base on morphological similarity. While this correlation is inductionist, reductionist, and otherwise epistemologically flawed in the case of organisms, it nonetheless must be accepted operationally, lest we are apt to believe that an unsampled frog is really a butterfly. In the absence of DNA data, the close morphological similarity of *Rumicastrum* to any species that had been classified in *Calandrinia* justified the inclusion of both in *Calandrinia*, lest there be no justification for the inclusion of either. Put another way, absent DNA data, it appears now that West and Obbens had far more reason to believe that *Rumicastrum* was most closely related to other Australian species than they did to believe that *any* of these species was closely related to the Type of *Calandrinia*. Yet, for taxonomic purposes, they accepted and promoted the latter belief, while rejecting the former. This observation is one of the many reasons that I have suggested that West and Obbens deliberately concealed their scientific knowledge in order to avoid the name *Rumicastrum*, and concomitantly deliberately published scientifically false taxonomic information. The corroborating evidence is outlined in this work and Hershkovitz (2023).

[16] As noted above, Obbens (2011) also cited biological information in Carolin (1993), but he ignored the taxonomy.

Candollean *Calandrinia* s. l. or just the Australian species plus *Calandrinia* s. stricto or, for that matter, *just* the Australian species. It as though the New World species and their Type are of no concern in Australia. They might as well not exist. This is essentially vernacular and not taxonomic usage of the name *Calandrinia*. While the Type of *Calandrinia* was, by omission of reference,[17] not *explicitly* excluded, its exclusion is implicit in the narrative. This usage of *Calandrinia* therefore is conceptually illegitimate. Effectively, Obbens "usurped" the name *Calandrinia* and applied it *homonymously* to refer to a *different* operational taxon, viz. one that excludes the Type of *Calandrinia*. I designate this taxon here as "fake *Calandrinia*" (~*Parakeelya*, ~*Rumicastrum*), as opposed to "real *Calandrinia*" (~*Calandrinia* s. str.).

As noted above, Obbens (2006) mentioned the *Parakeelya* and *Rumicastrum* problem, but did not explain why the Australian species described therein should be called *Calandrinia*.[18] Following Obbens (2011), Obbens published an additional eleven papers (Obbens, 2012, 2014a, b, c, 2018a, b, 2019, 2021, 2022; Obbens et al., 2017; Obbens & Barrett, 2018) that used *Calandrinia*. None referred to *Rumicastrum* or *Parakeelya* or *Calandrinia* s. str., except for Obbens (2019), which discussed species named as *Calandrinia* closely related to *R. chamaecladum* (Diels) Ulb., but did not explain why these species were classified in *Calandrinia*. Thiele et al. (2018) was not cited. In all, Obbens named 21 new Australian species as *Calandrinia*. Given: **(a)** Obbens' (2006) awareness of the generic taxonomic problem; **(b)** Obbens' (2011) citation of Carolin (1993); **(c)** Obbens' (2011) discussion of *Rumicastrum*-like species he classified as *Calandrinia*; **(d)** Obbens' (2011) descriptions of morphology of *Calandrinia* as though *Calandrinia* s. str. did not exist; **(e)** Obbens' (2018) co-authorship of Thiele et al. (2018); **(f)** Obbens' co-authorship of Hancock et al. (2018), whose results likely were known no later than January, 2017 (see below); and **(g)** Obbens' (2019) citation of Hancock et al. (2018),…it seems that Obbens' unqualified and unexplained use of *Calandrinia* in all of his publications, but especially the seven from 2017 onward, must be considered not only conceptually illegitimate, but deliberately so.

Hershkovitz (2023) criticized Hancock et al.'s (2018)[19] unqualified application of the name *Calandrinia* in the *title* of their publication "Phylogeny, evolution, and biogeographic history of

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[17] For example, qualifying the taxa as "Australian species of *Calandrinia*" presumes the existence of species that are "not Australian."

[18] As elaborated in Hershkovitz (2023), this is not a trivial matter, not from a taxonomic nor scientific perspective. Obbens (2006) and the many subsequent papers citing this view effectively have asserted that not knowing the *correct* solution to a problem justifies acceptance of an *incorrect* solution. Again, the use of scientific taxon names in a scientific publication makes a statement no less scientific or scientifically precise than that for any other entity named in that publication. A scientific taxon name is a scientific statement about the precise identity and properties of the organism and its relationship to both similarly- and differently-classified organisms. A taxon name is metaphysical, but so are such names as "succulent plant," "CAM plant," and "C4 plant." The main difference is that, unlike taxon names, the other names *have no formal Types*! They are *less* precise. And it is well-known now that there are different *kinds* and *degrees* of CAM and C4 physiology. But the fact that metaphysical entities are inherently imprecisely named does not mean that "anything goes," and that the names can be applied *in science* capriciously and without explicit scientific criterion or, worse, *conceptually illegitimately* and in a deliberately incorrect or misleading manner.

[19] As an aside, Hancock et al. (2018) reported that the previous phylogenetic analysis of Montiaceae carried out in the same lab (Ogburn & Edwards, 2015) used "only one and five [Australian] species in the three-gene and five-gene analyses, respectively." However, Ogburn & Edwards' consensus tree (2015: 187, Fig. 4) shows *seven* species labeled as *Parakeelya*. The five-gene tree includes rDNA-ITS, and there are seven corresponding sequences in GenBank. I never obtained Ogburn & Edwards' (2015) data, because the data link published in the article is broken. In any case, the larger data set refuted greater but still statistically insignificant support for monophyly of "fake *Calandrinia* s. l." (see text) in the smaller data set. Also, Hershkovitz (2023) reported erroneously that this topology was the same as that in Hancock et al. (2018). It is not. The positions of *Calandrinia* and the *Australian* clade relative to the North American clade are reversed. Combination of bad eyesight, dyslexia, and the way the taxa are arranged in the different trees.

Calandrinia." This publication did not analyze or discuss these attributes in *Calandrinia* s. str., viz. "real *Calandrinia*." I now appreciate the problem in terms of legitimacy. The name *Calandrinia*, of course, is legitimate, but its application by Hancock et al. (2018) in their title is conceptually homonymous and *illegitimate*. But, in contrast to Obbens (2011), the *text* of Hancock et al. (2018) indeed refers to the Australian species *only* by the designation "Australian *Calandrinia*." Hancock et al. (2018) might have been constrained in their usage by the fact that most species names were available only in *Calandrinia*, some in *Parakeelya*, and none in the older name *Rumicastrum*. But this constraint was "by design," reflecting the historical belligerence of one or two of authors. As I explain later, these authors could have and *should* have used *Parakeelya*.

In Hershkovitz (2023), I overlooked Hancock et al.'s (2018) use of the designation "*Calandrinia* s. l." This *does not* refer to the designation *Calandrinia* s. l. coined by Carolin (1987) to refer to the Candollean circumscription and used identically thereafter by Hershkovitz (1993a, etc.) and several other authors. *Calandrinia* s. l. in Hancock et al. (2018) is these authors' own contrivance, referring to *Calandrinia* s. str. plus *Calandrinia* as conceptually illegitimately applied to the Australian clade. It is a *homonymous designation*, viz. a later designation that circumscribes a taxon qualitatively very different from prior and accepted usage of the existing designation. The circumscription of homonymous *Calandrinia* s. l. never was proposed per se or defended taxonomically in the context of Portulacaceae s. l. or Montiaceae classification.[20] But, consequent to the authors' continuous conceptually illegitimate usage of the name *Calandrinia*, the homonymous circumscription of *Calandrinia* s. l. existed operationally in international taxonomic databases.[21] Hancock et al. (2018) thus contrived this designation for the purpose of "discovering" that the corresponding taxon that authors West and Obbens had operationally "engineered" over the years was polyphyletic. Hancock et al.'s (2018) *Calandrinia* s. l. is thus "fake *Calandrinia* s. l." and not the "real *Calandrinia* s. l." viz. the Candollean circumscription.

Also in Hershkovitz (2023), I reported that the Hancock (2017) presented the Hancock et al. (2018) research in a workshop in January, 2017. Here, the title and text refer only to *Rumicastrum* and do not mention *Calandrinia*. But I overlooked an earlier abstract from the mid-2016 Botanical Society of America meeting. Here, Hancock et al. (2016) indeed used *Calandrinia* in the title, but qualified using the designation "Australian *Calandrinia*," equating this with *Parakeelya* in the text. Here, the authors first used "fake *Calandrinia* s. l.," describing it as a genus whose polyphyly had not been established, comprising "new world" *Calandrinia* (~*Calandrinia* s. str.) plus the "old world" Australian species. It seems unlikely, however, that the word "Australian" was *inadvertently* left out of Hancock et al.'s (2018) title. Bold print. Eight authors. No way.

The same authors subsequently used the designation "Australian *Calandrinia*" in the title of Hancock et al. (2019) and 30 times in the text. But, ignoring species names, they used *Calandrinia* without qualification to refer to this clade 15 times, seven in the text and eight in the *running* title. Notably, they did not mention *Parakeelya* or *Rumicastrum* in this work, and they made no reference to *Calandrinia* s. str. except in one figure that showed this name plus Montieae as the sister group of the Australian clade. Effectively, it showed "fake *Calandrinia* s. l." as polyphyletic, but this was not stated explicitly. The 15 unqualified uses of *Calandrinia* are conceptually illegitimate, and their use easily could confuse the reader into believing that the species discussed pertain to "real *Calandrinia*." For example,

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[20] Hershkovitz (2023) suggested that West and Obbens would not have merged these two lineages had they been named differently, because the merging had no basis in character evidence, and West and Obbens otherwise never discussed *Calandrinia* s. stricto. The only reason they merged the Australian species with *Calandrinia* s. str. is because *Calandrinia* is not spelled R-U-M-I-C-A-S-T-R-U-M.

[21] I am reminded of the aphorism "*Repeat a lie often enough and it becomes the truth*," the origin of which I cannot ascertain, but it has been applied repeatedly in political propaganda.

the running title of Hancock et al. (2019) reads: "*Calandrinia* reveals lability in C_3+CAM phenotypes." This is *false*. "Real" *Calandrinia* reveals no such thing.

Holtum (2023; also an author of Hancock et al., 2018, 2019) used "Australian *Calandrinia*" seven times, in one instance equating it with *Parakeelya* or *Rumicastrum*, but three times used simply *Calandrinia*. He distinguished "Australian *Calandrinia*" from *Calandrinia* s. str. one time, equating the latter with the designation "New World *Calandrinia*." The coordinated use of the designations "Australian *Calandrinia*" and "New World *Calandrinia*" correspond to the notion of "fake *Calandrinia* s. l.," hence is conceptually illegitimate (see below). The use of *Calandrinia* unqualified refers to "fake *Calandrinia*" and not "real *Calandrinia*," hence is conceptually illegitimate. Holtum (2023) discussed Montiaceae taxonomy and phylogeny, but did not cite Hershkovitz' (2019) revision of these, which recognized *Rumicastrum* and criticized Thiele et al. (2018) and the continued use of *Calandrinia* for the Australian species. And he did not cite Hershkovitz (2020a), which also criticized Thiele et al. (2018) and recombined in *Rumicastrum* essentially all Australian species named in *Calandrinia*.

Gilman et al. (2023a; including Holtum and Edwards) cited two "Australian *Calandrinia*" binomials and used "Australian *Calandrinia*" as a generic designation twice, once with a footnote. The footnote indicates that "*Calandrinia* is nonmonophyletic…and CAM has only been observed in the clade inclusive of all Australian members of *Calandrinia* s. l." They did not mention *Rumicastrum* or *Parakeelya*. "*Calandrinia* s. l." here refers to the illegitimate "fake *Calandrinia* s. l." As far as I know, nobody *ever* has claimed that this circumscription is monophyletic. "Real *Calandrinia*," of course, is monophyletic, and Gilman et al.'s (2023a) assertion implies otherwise. This is likely to confuse readers. Remarkably, Gilman et al. (2023b; including Hancock and Edwards) not only used *Parakeelya* and *Parakeelya* combinations rather than *Calandrinia*, they *did not even mention* that this was the same lineage that they had referred to as *Calandrinia* in Gilman (2023a) and earlier publications! The post date of the manuscript suggests they were aware that *Parakeelya* was conserved. It would not surprise me to find future publications by other researchers citing Gilman (2023a) and Gilman (2023b) and stating that CAM occurs in *both Calandrinia* and *Parakeelya*.

Holtum et al. (2016; including Hancock and Edwards) used *Calandrinia* unqualified for the Australian species, in one instance equating it with *Parakeelya*, but without explanation. Winter & Holtum (2014) used *Calandrinia* unqualified for the Australian species, equating it with *Parakeelya* in one instance, but explaining the latter. Holtum et al. (2017) did the same, but mentioned also *Rumicastrum*, and also included existing *Parakeelya* combinations as synonyms for three species named in *Calandrinia*. Winter et al. (2019; including Edwards and Holtum), remarked that: "in Australia it is not uncommon to see species of *Portulaca* and *Calandrinia* growing alongside each other." Here, they referred to "fake *Calandrinia*," and not North American *Calandrinia menziesii* (Hook.) Torrey & A.Gray, which is introduced in Australia.

Holtum et al. (2021; including Hancock and Edwards) referred to "Australian and New World *Calandrinia* in the process of being split into New World (*Calandrinia* sensu stricto) and Australian entities." This designation alludes to the circumscription of Hancock et al. (2016, 2018), viz. "fake *Calandrinia* s. l.," since Holtum et al. (2021) clearly otherwise accepted the dissolved cladistic classification of the "real *Calandrinia* s. l." The "process of being split" cedes historical legitimacy to a fake taxon that *already* was "split" long before the authors contrived it. It also gives the reader a sense that taxonomic splitting is a slow and complicated "process," when all it involves is publishing recombinations. The "process" was a euphemism for the conservation proposal.

The legitimacy of the designations "Australian *Calandrinia*" and/or formal use of the name "*Calandrinia*" but with reference to *Parakeelya* and/or *Rumicastrum* and/or otherwise distinct from *Calandrinia* s. str. is problematic. If one *adhered* to the Candollean circumscription, viz. the "real

Calandrinia s. l.," the references so qualified or explained would be legitimate. Despite its contrivance, these usages might be legitimate if one adhered to the Hancock et al. (2018) circumscription, viz. "fake *Calandrinia* s. l." But at the very least, legitimacy would require explanation of the distinction between "real *Calandrinia* s. l." and "fake *Calandrinia* s. l.," because the latter is a conceptually illegitimate homonym of the former that lacked a cladistic diagnosis on morphological or genetic evidence, or any basis in the earlier typological classification.

The application of "Australian *Calandrinia*" in Hancock et al. (2019) clearly is conceptually illegitimate because, just as in Obbens (2011), the text makes no reference to *Calandrinia* s. str. except in one unexplained instance in a figure, and no reference to *Parakeelya* or *Rumicastrum*. This illegitimacy is aggravated by the 15 uses of *Calandrinia* not-so-qualified in the text and running titles. Thus, "Australian *Calandrinia*" in Hancock et al. (2019) is identical to "fake *Calandrinia*" in Obbens (2011). No reader, not even a good taxonomist unfamiliar with the complex taxonomic history of these species, could interpret this work as discussing anything other than species of some "real *Calandrinia*" that occur in Australia.

But the other applications of "Australian *Calandrinia*" might be considered conceptually illegitimate to the degree that the reader might overlook singular mentions of *Parakeelya* and/or *Rumicastrum* and/or the one or two sentences that explain why *Calandrinia* is applied. In fact, for this very reason, the ICN prohibits not only use of homonyms, but names that are likely to confuse because they *appear* homonymous (ICN Art. 53.2). This rule obviously does not apply to the use of similar *designations* or to conceptual homonyms, but it demonstrates that the ICN itself regulates names that are likely to be confused. It is self-evident that the usage described above is bound to confuse, partially for the inconsistency of usage within and among publications by the same group of authors, and partially because the use of the name *Calandrinia* in these publications drowns out the singular mentions of *Parakeelya* and/or *Rumicastrum*. For example, Pérez-López et al. (2023), citing Holtum et al. (2016), referred to the widespread distribution of *Calandrinia* in Australia. Mok et al. (2023), which I cannot access at the moment, also evidently refers to Holtum research on "*Calandrinia*."

Hershkovitz (2023) overlooked a peculiarity in the case of Holtum et al.'s (2017) use of *Calandrinia*. This work *precedes* the first publication that revealed the genomic evidence regarding *Rumicastrum*. The authors recognized that the Australian species merited segregation based on *earlier-reported* molecular and phenotypic divergence, but they maintained use of *Calandrinia* because of uncertainty of the relations of *Rumicastrum*.[22] This justification had been invoked previously (Obbens, 2006; Winter & Holtum, 2011,[23] 2014[24]). But this assertion does not fit the discovery timeline. Hancock's

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[22] "There is sufficient molecular and phenotypic support for splitting the Australian and American clades of *Calandrinia* (Carolin 1987; Hershkovitz 1993a, 1998 [sic]) but we follow the names used in the Australian Plant Census...[APC, without year]…Acceptance of *Parakeelya* awaits determination as to whether another available name, *Rumicastrum*, is congeneric with the Australian *Calandrinia* (Carolin 1987; Obbens 2006; Hernández-Ledesma et al. 2015)." Holtum et al. (2017). Note that here the authors do not consider monophyly/polyphyly per se of the two lineages as the ultimate arbiter for splitting (cf. Hershkovitz, 2023).

[23] Winter & Holtum (2011, **[?]**'s mine): "*Calandrinia*, now placed within the family Montiaceae, had previously been reduced **[?]** following subdivision into six genera: *Anacampseros* **[?]**, *Calandrinia*, *Cistanthe*, *Montiopsis*, *Schreiteria* and *Talinum* **[?]** (Carolin 1987; Hershkovitz 1991, 1993a, 1993b). The remaining **[?]** *Calandrinia* comprise two separate lineages: a well-defined lineage…native to the Americas, and a less well-defined lineage…native to Australia (Hershkovitz 1993a, 1993b; Hershkovitz and Zimmer 1997; Obbens 2006). The New World lineage has retained the name *Calandrinia*, whereas two generic names have been proposed for the Australian species, *Rumicastrum* (Carolin 1987) and *Parakeelya* (Hershkovitz 1998 [sic]). In the absence of a consensus for either name (Hershkovitz 1998 [sic]; Obbens 2006), and in the absence of a significant cladistic or genetic study that circumscribes the relatively character-diverse Australian species, here we retain the name *Calandrinia* for the Australian species." I should have emphasized in Hershkovitz (2006) that I was happy with either name, neither was *Calandrinia*, hence the "lack of consensus" was a red herring. Winter & Holtum's (2011)

(Hancock, 2017) public presentation of the Hancock et al. (2018) research on 20 January 2017 would seem to prove that Holtum et al. (2017) must have known that *Rumicastrum* pertained to the Australian clade, hence was the correct name.[25] Presumably they also knew also that the genomic data proved that "fake *Calandrinia* s. l." was not monophyletic. Notably, *subsequent* publications by these collaborators (Hancock et al., 2018; Holtum et al., 2021; West & Albrecht, 2022; Albrecht & West, 2023; Holtum, 2023) *acknowledged* relations of *Rumicastrum* and polyphyly of "fake *Calandrinia* s. l.," but they now justified continued use of *Calandrinia* for a *different* reason, viz. the pending Thiele et al. (2018) conservation proposal. This suggests that Holtum et al. (2017; including Hancock and Edwards) feigned their uncertainty on *Rumicastrum* relations in order to avoid revealing this knowledge *before* Thiele et al. (2018; including Hancock and Edwards)[26] would provide them with a *different* excuse for avoiding the name *Rumicastrum*, so that these authors could continue their conceptually illegitimate use of *Calandrinia*.

Returning to Thiele et al. (2018), the detailed usage analysis above of the literature renders clear that the polyphyletic "*Calandrinia*" they referred to in their opening sentence was *not*, as subsequent discussion might suggest, the "real *Calandrinia* s. l.," but the conceptually illegitimate homonymous "fake *Calandrinia* s. l." contrived by these very authors for the purpose of justifying continued but conceptually illegitimate usage of *Calandrinia* and avoiding acceptance of the correct name *Rumicastrum* for the Australian species.[27] The lead sentence of Thiele et al. (2018) thus should be interpreted as "*Calandrinia* Kunth…has ~~long~~ *recently* been regarded…by *us* and nobody *else*…as a genus with two centers of diversity…". With this interpretation, the subsequent discussion makes sense. Otherwise, it

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commentary, its incoherency aside, suggests that the Australian clade was retained in *Calandrinia* following segregation of other genera. As I pointed out, the Candollean and later cladistic classifications involving *Calandrinia* are completely incompatible and "all or nothing." Retention of the Australian plants in *Calandrinia* owed exclusively and entirely to West and Obbens and was incompatible with *both* the Candollean and cladistic classifications.

[24] Winter & Holtum (2014): "The Australian *Calandrinia* species are *not* [italics mine] monophyletic with the New World *Calandrinia* species (Carolin, 1993), and even though a new genus name, *Parakeelya*, has been published for the Australian clade, monophyly within it has yet to be demonstrated (Hershkovitz, 1998 [sic])." Again, nobody has ever claimed that the Australian species plus *Calandrinia* s. str. was monophyletic. And, again, any appearance to this effect was an artifact of West's and Obbens' persistent conceptually illegitimate classification of the Australian species in *Calandrinia*.

[25] As noted here and elsewhere, Hancock (2017) presented the Hancock et al. (2018) research using the name *Rumicastrum* on 20 January, 2017, and must have prepared the abstract some time before that. She would not have used this name unless she knew that *Rumicastrum* pertained to the Australian clade. [She also presented the work mid-2016 (Hancock et al., 2016; including Holtum), but here mentioned only *Parakeelya*.] Holtum et al. (2017) was submitted 12 October 2016, accepted 14 February 2017, three weeks *after* Hancock's 2017 presentation), and published 4 September 2017, 8.5 *months* after Hancock's 2017 presentation.

[26] As noted in Hershkovitz (2023), Thiele et al. (2018) cited and based itself on the conclusions of "Hancock et al. (in press)." But Thiele et al. (2018) was published only two weeks after Hancock et al. (2018) was *submitted* and three months *before* it was *accepted*. Thus, Thiele et al.'s (2018) submission apparently cited "*in press*" a work that itself *had not been submitted* for publication – *another* irregularity in this work. The Thiele et al. (2018) submission/acceptance dates are not published, but proposals are not published before the physical issue publication. I estimate that submission was at least two and as many as five months prior to publication. *More notably*, Hershkovitz (2023) and my previous references to Thiele et al. (2018) overlooked the fact that the genomic evidence for *Rumicastrum* relations was journal-published *first* somewhat stealthily in a *nomenclatural proposal*, some *six months before* the actual *research* was published! These observations add to the several "irregularities" in this short (845 word) proposal that I have described here and in Hershkovitz (2023).

[27] Hershkovitz (2023) noted that two Australian references *expected* that the Australian species would be transferred to *Rumicastrum*, viz. Short (2005) and Western Australian Herbarium (1998–). I overlooked another, Barrett & Tay (2016). This reference is fairly well cited in scientific literature. As Hershkovitz (2023) noted, Thiele et al. (2018) cherry-picked twelve references that had used *Parakeelya* and *none* of the many that used or referred to *Rumicastrum*, falsely claiming, effectively, that none of importance existed.

does not, because the cladistic classification of Portulacaceae s. l. and later Portulacineae had been accepted globally by then for two decades, and it was accepted in earlier publications by Thiele et al. (2018) authors and close collaborators. As noted above, the cladistic classification, which separated the Australian species from *Calandrinia* s. str., was *conceptually* incompatible with the Candollean classification of *Calandrinia*. Then, if Thiele et al. (2018) had reported *correct* species numbers and usage data rather than egregiously erroneous data that departed radically from that of their cited *self-authored* source and/or their other publications, the principal remaining argument for conservation of *Parakeelya* would have been its nomenclaturally *invalid* vernacular usage in Australia.[28]

5. Why *Parakeelya* could have and should have been used following Thiele et al. (2018)

As noted, several publications (fifteen, in fact) subsequent to Thiele et al. (2018) continued to conceptually illegitimately apply the name *Calandrinia* to the Australian species. Some (e.g., Hancock et al., 2018), cited the pending Thiele et al. (2018) proposal as the reason for using *Calandrinia*. Since authors of these publications were co-authors of Thiele et al. (2018), there can be no doubt as to their preference for the newer name *Parakeelya*, over which *Rumicastrum* had priority. Obviously, they would not have wanted to use *Rumicastrum*, because this would undermine their own proposal, and because avoiding the name *Rumicastrum* evidently had been the politic of the authors for a quarter century.

Possibly the authors did not appreciate that the Thiele et al. (2018) proposal effectively legitimized the use of *Parakeelya*, even with *Rumicastrum* included. Thiele et al. (2018) argued that *Parakeelya* had "been used extensively in taxonomic literature for the Australian clade, while *Rumicastrum* has not," and that conservation of *Parakeelya* would minimize nomenclatural changes. They cited ICN Art. 14.1. But they may have overlooked Art. 14A.1, a *recommendation* that, pending proposal evaluation, authors adhere to "existing usage." This is a nomenclatural "loophole" that overrides priority (Art. 11) and several other ICN articles, including Art. 14.15. The last stipulates that conservation decisions do not take effect until publication of the IAPT General Committee Report. This necessarily excepts prevalent "existing usage" proposals, per Art. 14A.1.[29]

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[28] 156 of the 845 words (18%) of Thiele et al.'s (2018) text are devoted to the vernacular usage of "parakeelya" in Australia and hence the value of conserving *Parakeelya* over the priority name. They reported not only the indigenous etymology of the name, but its history in botanical publication and as a vernacular name for all of the Australian species. They seemed to suggest subliminally that, since the vernacular name "parakeelya" actually was older than the valid name *Rumicastrum*, the later name *Parakeelya* should have priority. It seems to me odd to refer to etymology or vernacular names at all in a conservation proposal, unless this information is critical to taxonomic identification of name, as in Hershkovitz (2020d). Otherwise, since the ICN does not recognize vernacular or any nomenclaturally invalid names, Hershkovitz (2023) questioned why this argument even was permitted in the proposal text and, even more bizarrely, reiterated in the Floral Nomenclature Committee's summary of the proposal (Applequist, 2023). I add to this that, in my extensive correspondence with J. McNeill (E) in 2020, McNeill expressed his anathema towards the inclusion of invalid *scientific* binomials and their literature references in plant name indices and taxonomic synonymies. McNeill presumably edited Thiele et al.'s (2018) submission, hence I am puzzled by his editorial acceptance of a conservation argument based on vernacular usage, since vernacular names are not only invalid, they are conceptually illegitimate. And they are notoriously unreliable. GBIF currently lists "pussy paws" as the vernacular name for the genus *Cistanthe*. This undoubtedly stems from the generic common name "pussy paws" referring to *Calyptridium*, which Hershkovitz (1993a) included in *Cistanthe*. But Hershkovitz (2006) and GBIF excluded *Calyptridium* from *Cistanthe*. Yet "pussy paws" remains as the common name for *Cistanthe*. This one of countless examples of why vernacular names should not be legitimized by the IAPT. If McNeill was against even informal recognition of invalid *scientific* names in taxonomic indices and literature, why would he effectively validate and legitimize a vernacular name in a conservation proposal and cede it priority over a valid name? McNeill is a member of the IAPT General Committee that overwhelmingly supported the Thiele et al. (2018) proposal, despite its being supported by only a minority of Floral Nomenclature Committee members.

[29] Art. 14A.1, by the way, underwrites the legitimacy of the combinations published in Hershkovitz (2023).

An example of 14A.1 application is *Calandrinia* itself, which was conserved over the older but obscure name *Baitaria* Ruiz & Pav. Identity of the latter was unknown until the second half of the 19th Century (e.g., Bentham, 1862). In publications preceding the first ICN, *Baitaria* was considered a *synonym* of the universally used *Calandrinia*. *Calandrinia* later was conserved, but there was no moratorium on its continued use during the four years that the proposal was pending.[30]

While I believe that Thiele et al.'s (2018) proposal was farcical, if not fraudulent, its very publication permitted use of *Parakeelya*, notwithstanding Art. 11. ICN Art. 14A.1 not only authorized use of *Parakeelya*, it *recommended* it pending proposal resolution. Its use in Hancock et al. (2018) and subsequent publications would have been taxonomically legitimate and, more importantly, conceptually *accurate*. Subsequent conceptually illegitimate and otherwise confusing usage would have been avoided. Confusion would have been reduced even if *Parakeelya* had not been conserved, because even before its conservation, *Parakeelya* was *conceptually* synonymous with *Rumicastrum*. *Neither* name was conceptually synonymous with *Calandrinia*.

While I do not believe that conservation of *Parakeelya* was justified, somewhat ironically, its conservation would have been justifiable long *before* Hancock et al. (2018). As noted, Obbens (2006) cited uncertainty of the relations of *Rumicastrum* as the reason for operational rejection of *Parakeelya* in Australia. This predicated uncertainty itself would have justified conservation of *Parakeelya*. In particular, it could have been argued that the generic classification of a large and widespread lineage in Australia should not be contingent upon *eventual* study of a peculiar, poorly known, rare, and difficult to obtain species. I have discussed evidence that Thiele et al. (2018) authors did believe two decades earlier that *Rumicastrum* pertained to the Australian lineage, but that is beside the point. Earlier conservation of *Parakeelya* would have avoided considerable subsequent conceptually illegitimate use of the name *Calandrinia* and the associated taxonomic confusion.

6. Scientific research in the face of taxonomic change

The accuracy of my analyses of the "Australian *Calandrinia*" problem here and in Hershkovitz (2019, 2020a, b, 2023) aside, it is fair to ask whether the conservation of *Parakeelya* over *Rumicastrum* makes any difference to *science* in the long run. After all, taxonomic changes (and disagreements) are and always have been axiomatic to the advance of taxonomic research. Plant generic and especially familial taxonomy today is radically different from that used 30 years ago, prior to the molecular/genomic systematic revolution, this superimposed over the slightly older cladistic taxonomic revolution. Consequently, plant names used in all manner of scientific research 30 years ago often refer to taxa different from those in current usage. For example, Portulacaceae used to refer to a taxon that included 12–20 genera. Currently it includes only *Portulaca*. This challenges the scientific accuracy of current research that cites those older references, especially for researchers not well-trained in taxonomy and nomenclature, and even for those that are, but whose taxonomic expertise is specialized. Older references cannot update themselves. Only rare and exceptionally talented and dedicated monographers[31] take the time to reconcile usage in older literature in terms of modern taxonomy. But taxonomic monography as a discipline is practically extinct in terms of modern career and funding opportunities. The consequence is that current research is bound to be misled and even misguided from time to time by obsolete and/or conflicting taxonomic usage.

[30] The history of this conservation can be found in the Shenzhen Code Appendices (Turland et al., 2018) database at the following website by entering the taxon name and selecting the proposals/requests option: https://naturalhistory2.si.edu/botany/codes-proposals/index.cfm

[31] Like my late father, Philip Hershkovitz.

Notably, taxonomic confusion occurs even when different historical and contemporary taxonomists act with due diligence, responsibility, foresight, and, in the *first* place, awareness of the purpose of scientific taxonomy. Its purpose is global, not local, and certainly not for aesthetics. I note that the generic taxonomy of Montiaceae in Hershkovitz (2019) is rather different from that of Hershkovitz (1993a), which is different than Carolin (1987, 1993). But, right or wrong, the *reason* for each change along the way was articulated thoroughly.

For example, based on morphology, Hershkovitz (1993a) proposed the circumscription of *Calandrinia* that became accepted universally…except for two taxonomists in Australia.[32] Carolin (1993) had classified current *Calandrinia* in three separate genera. Hershkovitz (1993a) also proposed the current circumscription of *Montiopsis*. He argued, however, that *Montiopsis* shared morphological and biogeographic similarities with *Calandrinia*, so that their merging was not unreasonable. But there were no such similarities between *Calandrinia* and *Rumicastrum*, hence no reason to merge them. Hershkovitz (1993a) also expanded Carolin's (1987, 1993) circumscription of *Cistanthe* to include *Calyptridium* and also current *Lewisiopsis*.

While subsequent molecular/genomic data greatly improved phylogenetic resolution, they were not the "magic bullet" that they were purported to be. Adequate data was slow to materialize, and they ultimately showed that the current generic taxonomy was substantially accurately inferred from morphology *alone*. The first relevant molecular analysis (Hershkovitz & Zimmer, 2000) did not resolve the relations of *Rumicastrum* or *Montiopsis*. They neither supported, nor unequivocally refuted, the Hershkovitz (1993a) circumscription of *Cistanthe*. But this and Hershkovitz & Zimmer (1997) corroborated the Carolin's (1987) proposed segregation of *Phemeranthus* from *Talinum* and the closer relation of the former to Montiaceae.

Hershkovitz (2006) demonstrated polyphyly of *Cistanthe* sensu Hershkovitz (1993a), but corroborated Hershkovitz' (1993a) conclusion that *Calyptridium* was closely related to *Philippiamra*, and that *Lewisiopsis* was not a *Lewisia*.[33] The relations of *Montiopsis* and *Rumicastrum* remained unresolved. Definitive cladistic separation of *Montiopsis* from *Calandrinia* was not demonstrated until Ogburn & Edwards (2015). The relations of Australian to New World species remained unresolved by genetic data until Hancock et al. (2018).[34] But this corroborated the conclusions of Carolin (1987, 1993) and Hershkovitz (1993a) based on morphology.[35]

Thus, while the successive taxonomic changes of owing to Carolin and Hershkovitz certainly did (and still does) introduce a degree of confusion into broader taxonomic usage, the changes were

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[32] Ironically, the "birthplace" of modern Montiaceae and Portulacineae classification.

[33] The emergence of *Lenzia* and *Montiopsis* from within *Cistanthe* sensu Hershkovitz (1993a) was *not* predicted by morphology. Current *Cistanthe*, *Calyptridium*, *Thingia*, and *Philippiamra*, included in *Cistanthe* in Hershkovitz (1993a), are essentially morphologically identical vegetatively and otherwise very similar reproductively (Hershkovitz, 1991, 1993a, 2019). *Lenzia* and *Montiopsis* proved to be the "birds" and "mammals" of these "reptiles," though their morphology predicts their relation less so than real "birds" and "mammals." Meanwhile, Hershkovitz (2019) described *Lewisiopsis* as a "living fossil," pleisiomorphic, resembling *Cistanthe* not only in its morphology, but also in its rDNA and cpDNA gene sequences.

[34] As I elaborated above, morphological comparison alone *should have* predicted fairly precisely the relations of *Rumicastrum*, viz. to *known and described Rumicastrum*-like species classified in *Calandrinia.* We are left to wonder whether West and Obbens simply never made the comparison or made the comparison but failed to report on it.

[35] Hershkovitz & Zimmer (1997) would have classified *Rumicastrum* correctly if anybody besides Werner Greuter had edited this manuscript. Even so, Hershkovitz' (1998) recognition of *Parakeelya* was correct in the sense that it rendered operational the generic recognition of the Australian lineage.

conceptual advances in the correct direction. But in the process, the taxonomy has improved in the sense that the current generic taxonomy provides better predictors, for *scientific* purposes, of generic properties and evolutionary history than the replaced taxonomy.

The efforts of the Australian specialists, in contrast, particularly West and Obbens, seem more akin to taxonomic vandalism, a deliberate corruption of taxonomic standards. In the period 2006–2023, six authors of Thiele et al. (2018) and Hancock et al. (2018), viz. Thiele, West, Obbens, Hancock-plus-Edwards, and Holtum, authored or coauthored a total of some 31 publications that refer to the Australian species, extensively or incidentally. The publications collectively include some 25 new species descriptions and mention of several more by their "phrase names," comparative anatomical and morphological analysis, molecular/genomic phylogenetic and phylogeographic analysis, and ecological and physiological analysis. Ten of 31 papers were published before the phylogenomic relations of *Rumicastrum* were known (2006–2016) and 21 since (2017–2023). This quantity of publications reflected the amount of new scientific interest on these species, which was much greater than in the decades preceding.

The older papers mostly do not mention *Rumicastrum*, and the few that do state or imply that its phylogenetic relations remained unknown, which I dispute. Most but not all the later papers refer to the then-known phylogenetic relations of *Rumicastrum*. One appears to deliberately conceal this knowledge. All but one of the most recent publication, which uses the now-conserved *Parakeelya*, refer (conceptually illegitimately) to the Australian plants as *Calandrinia*, sometimes with but many without explanation, sometimes with but many without the qualification "Australian." Several refer explicitly or implicitly to the conceptually illegitimate circumscription of *Calandrinia* (viz. "fake *Calandrinia* s. l.") that the authors themselves contrived.

Do these papers reflect "diligence, responsibility, foresight, and awareness of the purpose of scientific taxonomy?" Or do they reflect extraordinary taxonomic incompetence? Or do they reflect something more sinister, the concealing and/or cover up of scientific evidence in an attempt to circumvent ICN principles for unscientific purposes? Here and in Hershkovitz (2023) I have outlined the evidence. It can be interpreted however one prefers. Regardless, this analysis demonstrates that Hershkovitz' (1999 ["1998"]) publication of *Parakeelya* was *not* the primary reason for decades of subsequent taxonomic confusion. Application of the name *Parakeelya* or *Rumicastrum* really made little difference, because it was clear that both referred to the same taxon, plus or minus one species.

The source of confusion was subsequent conceptually illegitimate application of the name *Calandrinia* to the Australian species, because this application did not reflect prior Candollean usage of this name. It reflected what turned out to be the operational establishment of a conceptually different genus *Calandrinia*, "fake *Calandrinia* s. l.," that had no basis in taxonomic evidence. Even after Hancock et al. "discovered" that "fake *Calandrinia* s. l." was polyphyletic, no later than January, 2017, the same authors continued to apply this name conceptually illegitimately in at least 21 publications to describe new species and the evolution and physiology of the Australian species, at best tantalizingly "dancing around" the names *Parakeelya* and *Rumicastrum*, one time reporting that they did not know the relations of *Rumicastrum* when it appears that they did. This is besides a nomenclatural conservation proposal laden with factual errors and otherwise misleading information biased in favor of that proposal. As I have noted, publications do not rewrite themselves. These publications are likely to confuse scientists for decades to come. "Real taxonomy" adds enough confusion to scientific research. "Fake taxonomy" is not necessary.

Hershkovitz (2020b) coined the term "fake taxonomy" in response to Obbens' (2019) naming of new Australian species as *Calandrinia* and remarking on their close cladistic relation with *Rumicastrum*, without explaining his use of the name *Calandrinia*. This, of course, was after he co-authored Thiele et al.

(2018) and Hancock et al. (2018). I also suggested that "fake taxonomy" is "fake science," and that it might cast doubt on the integrity of any and all other scientific information and conclusions offered in the same publication. But my commentary in Hershkovitz (2020b) reflected only Obbens (2019) considered in isolation, perhaps as a one-off. The current analysis, which suggests that Obbens (2019) reflects a historical systematic *pattern* of conceptually illegitimate taxonomic usage and otherwise taxonomic misinformation, gives me no reason to rescind and every reason to reaffirm my previous suggestion. How do I know, for example, if other data in these publications, ranging from taxonomic to phylogenetic to ecological to physiological, is honest and accurate or is massaged in some way to reach preconceived conclusions?

But Hershkovitz (2023) did not let the IAPT General Committee off the hook, either, for overwhelmingly approving Thiele et al. (2018), despite slightly less than lukewarm support from the Floral Nomenclature Committee. *Both* committees were derelict in their duties. The Floral Nomenclature Committee was derelict for illegitimately legitimizing local vernacular name usage as a criterion for scientific name conservation. Both committees seem to have overlooked or ignored Hershkovitz (2020a) and the acceptance of *Rumicastrum* and its combinations by Govaerts (2021) and, consequently, POWO and GBIF databases. And neither committee vetted the false species numbers and taxonomic usage data reported by Thiele et al. (2018). But I will give the committees a pass for not recognizing that *Calandrinia* in the first sentence (and word) of Thiele et al. (2018) referred to "fake *Calandrinia* s. l.," and not "real *Calandrinia* s. l.," because even I overlooked this farce in Hershkovitz (2023).

I believe that the *Parakeelya* decision somewhat delegitimizes IAPT and the ICN. Since my graduate student days 40 years ago, I have proactively defended the field of plant taxonomy in the face of critics from ecological and suborganismal biology disciplines. These critics labeled plant taxonomists, *especially* the nomenclatural specialists, as self-indulgent librarians rather than legitimate scientists, of no use to broader biology and therefore not worthy of departmental academic positions, physical space, and resources. Were they right after all? After all, if taxonomists cannot be true to the virtues and credos of taxonomy, then why should anybody else? Maybe all of science should do what Thiele et al. (2018) authors and collaborators have done, with IAPT support, and call their study organisms by whatever name that tickles their fancy.

Hershkovitz (2023) also raised the possibility that that conservation of *Parakeelya* was a "done deal," regardless of the accuracy and legitimacy of Thiele et al. (2018). Committee decisions are inherently political, not scientific. I notice now that Australian membership is "overrepresented" in the General Committee, having three members among 19 representing a total of only 12 countries. Notably, only one member represents all of Iberoamerica, and *none* represent Africa or south Asia. These territories include, I suppose, at least 75% of the angiosperm species whose names IAPT regulates. There also are no members representing the vast floristic territories of Russia and former Soviet republics. But perhaps the most interesting membership a propos the conservation of *Parakeelya* is that of Werner Greuter. As I have reported multiple times before, Greuter invited me to submit Hershkovitz & Zimmer (1997) to his journal, and promptly rejected my attempted usage of *Rumicastrum* for the single Australian sample. He insisted that a specimen of *R. chamaecladum* in B pertained to Chenopodiaceae, not Portulacaceae (s. l.). This is what prompted me to erect *Parakeelya*. So, 26 years after Hershkovitz & Zimmer (1997), the true *instigator* of this debacle, and the veritable true "father" of *Parakeelya*…acted on the committee to conserve this name. Is it possible that Greuter "set me up" in 1997? Stranger things occur in plant taxonomy politics. But it appears that Greuter's *R. chamaecladum* specimen in B must have been on loan. According to GBIF, there is no *R. chamaecladum* specimen in B or, for that matter, in any herbarium outside of Australia.

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