

## How not to conduct a scientific debate: a counterpoint to the recent critique of the “pragmatic classification” of jumping spiders (Arthropoda: Arachnida: Araneae: Salticidae)

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In a recent Correspondence in *Zootaxa* (Kropf et al. 2019), a group of thirteen well-known arachnologists, representing almost the entire administrative team of the World Spider Catalog (WSC; Nentwig et al. 2015), provided a strongly worded critique of Jerzy Prószyński’s recently published and highly controversial taxonomic interventions in the classification of jumping spiders (e.g., Prószyński 2016, 2017a, 2017b, 2018), attempting to clarify and justify the official position of the WSC regarding his views and methodology.

Given the central place of the WSC as an exemplary service for the arachnological community, it seems worthwhile having a closer look at their argument. One of the main benefits provided by the WSC is that it enables the documentation of a lively, dynamic taxonomic debate, covering a wide variety of alternative hypotheses and controversial viewpoints, while still maintaining nomenclatural stability. There are quite rational and compelling reasons for the WSC not to implement each and every nomenclatorial proposal immediately and uncritically, quite independent of the personality of the proposer and without a value judgment regarding the scientific quality of the work. Scientific names in zoology have two fundamental functions, as unique and generally accepted identifiers of taxa, and as a hierarchical representation of a zoological system, i.e. a specific and testable scientific hypothesis. These two functions are rarely fully compatible. A catalogue such as the WSC, acting as a point of reference far beyond the community of practicing taxonomists, naturally has to emphasize stability, even if this means that not every newly suggested scientific hypothesis can be immediately reflected in its structure. For example, not every proposed regrouping at the family or genus level needs to be reflected in the catalog arrangements of species as soon as it is published. But this doesn’t mean that a catalogue should feel compelled to take sides in a scientific debate, and every precaution should be taken to make sure that its reluctance to implement a new taxonomic hypothesis cannot be construed as a scientific value judgement. Documentation in the WSC should be obligatory, but implementation can and should happen prudently and reluctantly.

How do Kropf and colleagues position themselves relative to this fundamental principle? In their concluding sentence they write: “we will reserve the right not to implement all of the suggested changes for the structure of the [World Spider] Catalog to promote taxonomic stability in the Salticidae.” This seems to be a self-evident commitment, and who would ever have disputed their right to do so? But what are the reasons for singling out an individual author for public criticism? Doesn’t this create a certain risk that this might appear as editorial arbitrariness? It obviously can and should not be the intention of the catalogue to take on the role of an arachnological vice squad, judging the value of taxonomic publications and punishing

transgressions by non-implementation. However, this is exactly the impression created by form and content of this correspondence: whoever runs afoul of the community consensus faces public ostracism by the WSC and exile from the arachnological community. Is this really the intention of the authors? One should have thought that vigorous disagreements are an indication of a healthy and progressing field of science, and it would be a disaster if a misunderstanding about this important point would be used to suppress the debate or even to prevent the publication of alternative taxonomic proposals for the sake of a misguided desire for stability.

The warning that Prószyński's proposal will cause "confusion and chaos in future salticid systematics" seems much exaggerated. Do the authors really want to suggest that spider taxonomists are too naïve and submissive to authority to come to their own conclusions? I doubt that there are any practicing arachnologists who would be prepared to accept Prószyński unexplained (and often inexplicable) taxonomic proposals without making their own assessment of the complete evidence.

To accuse Prószyński of a "disregard of ICZN rules" because he argues that long-winded textual descriptions of subtle morphological characters are all but useless, and would better be replaced by improved (photo)graphic documentation of diagnostic differences, requires a rather creative interpretation of his argument: Prószyński's assessment has to be read in the context of his preceding fifty-five-year track record in jumping spider taxonomy (starting with Prószyński 1962), which, like the vast majority of taxonomic publications on Salticidae until very recently, entirely lacks *any* photographic illustrations and often does not even include informative habitus drawings. Against this background, his vigorous argument for more and better illustrations becomes quite understandable. Kropf et al. construe this argument to imply that Prószyński refuses verbal descriptions entirely, without any evidence that this is indeed the case; in the incriminated papers, at least, he seems to be providing a short, but formally more than sufficient verbal diagnosis for each taxon (and Kropf et al. acknowledge this fact).

Prószyński is also accused of a "disregard of modern scientific methods". His ignorance and rejection of molecular genetics (and cladistics!) he shares with a surprisingly large number of arachnologists of his generation; singling him out for personal reproval appears quite unfair and unjustified. Even less comprehensible is the criticism that Prószyński rejects "morphometrics" – there is no evidence in his published writings that he is guilty as charged. He simply presents the results of a personal cost–benefit analysis, coming to the conclusion that the routine measurements of body proportions reported in the taxonomic literature are too tedious and meaningless to justify the considerable effort involved in acquiring them. This is a reasonable (albeit debatable) position. The morphometric studies referenced by Kropf and colleagues require considerably larger series of specimens and standardized measurements across multiple species, preferably by a single author – the authors provide no evidence that the measurements routinely included in species descriptions are useful for this purpose, and especially among the morphologically quite conservative jumping spiders this is highly doubtful. This does not mean that morphometrics could not be useful for distinguishing closely related "cryptic" species, but so would be controlled mating and breeding experiments, and comprehensive molecular genotyping, and the authors would certainly not suggest that these are made an obligatory part of all taxonomic work on jumping spiders.

Contrary to the assertions of Kropf et al., Prószyński is not committing any "basic errors" here, he is not breaking the rules of the game – he is playing his own game, outside the established scientific discourse. Once this is fully understood, many of the concerns voiced by the WSC authors are easily resolved.

For example, the authors observe a "disregard of the need to explain evidence" in Prószyński's work. This is quite true: Prószyński quite obviously doesn't see a need to explain or justify his taxonomic decisions, and he refuses to engage in a meaningful academic debate. For any passionate scientist, this attitude is difficult to understand, extremely frustrating, even infuriating. But of course Prószyński is fully entitled to his position; and his ideas, the legacy and summary of a lifetime of experience, can still be considered an inspiration and provocation for future work, if only by highlighting that, despite major progress (Maddison 2015, Maddison et al. 2017), we still are far from a full understanding of salticid phylogeny. One might wish for something more substantial, more explicitly reasoned, more in line with accepted practices, but there is no need and no way to force an author to deliver on these requests.

Most importantly, by explicitly placing himself outside the academic discourse (and outside the rules of the ICZN), Prószyński's actions become far less dangerous and harmful than Kropf and colleagues seem to fear. In contrast to recent high-profile cases of "taxonomic vandalism" (Kaiser et al. 2013), Prószyński is *not* exploiting and abusing the rules of the ICZN to cause damage to the system of zoological nomenclature. Instead, he goes out of his way to make sure that such damage is impossible. This is particularly clear in the

case of the “suprageneric names ending with –INES”, which the authors fear will “bring nothing but chaos in salticid systematics”. This concern appears completely unjustified. In all his publications (in the sense of ICZN Article 8), Prószyński makes it very clear that his personal nomenclature for genus groups is informal, applied outside the remit of the Code (he refers to them as “informal groups of genera” and “provisional groups”, Prószyński 2017:1); and he consistently uses all capitals and invalid suffixes (-INES/-OIDA) to ensure that the informal nature of these names is unambiguously emphasized (“To indicate differences with formal subfamilies of canonical authors, names of groups of genera are written in CAPITAL LETTERS and distinguished by ending -ES, instead of formal –INAE”, Prószyński 2017:9). He couldn’t be clearer that these are not intended as part of the official arachnological nomenclature. Very similar informal names ending in “-ines” are used, e.g., in a recent high-profile phylogenetic analysis of the spider family Araneidae (Scharff et al. 2019). Kropf et al. seem to underestimate the competence of spider taxonomists and practicing arachnologists when they fear that anyone could be confused by the creation of these names.

The risk is further minimized by the fact that Prószyński proposes his system explicitly and unashamedly as a “pragmatic classification”, by which he means that it is no attempt to represent a “natural system” (“It disregards temporarily hypothetical interpretations of relationships and ancestry”, Prószyński 2017:6 and “[its] purpose [is] limited to assisting in identification”, Prószyński 2018:133). While Kropf et al. attribute the view that the “‘pragmatic classification’ should also shed light on the relationships of species and genera” to “Prószyński 2018”, this attribution is certainly not correct. Again, while Prószyński’s decision to construct a personal non-phylogenetic classification is not a choice that many practicing taxonomists would share, it is a perfectly legitimate personal viewpoint – and it ensures that Prószyński’s informal genus groups will be only of the most limited interest for future taxonomists, except perhaps as condensed summaries of taxonomic intuitions acquired over a lifetime of work in the field, against which to test modern classifications. Prószyński makes it abundantly clear that these names are merely an aid to communication about an informal identification tool, analogous to using named branches in a species identification key. The suggestion that Prószyński’s genus-group names should be “considered as incorrect spelling of” available names under the ICZN is counterproductive: it violates both the Code and the explicit intentions of Prószyński, and would indeed create chaos where there was none before.

So, where do we go from here? It is clear that the WSC can only continue to fulfil its hugely important role with the limited resources available, if its administrators are relieved of the extraneous task of acting as arbiters in highly acrimonious taxonomic debates. To achieve this, it seems essential that the WSC team establishes transparent and accountable procedures for the implementation (or not) of newly proposed changes to the arachnological system. Many country check lists and field guides tend to take a very conservative approach to nomenclature, without implying a personal judgement of the merits of individual publications. Usually, they nevertheless document new proposals for future reference. A similar approach could form one of the guiding principles in a general working procedure for the WSC, which would help it in its function as a point of reference for a broad range of communities, from ecologists and behavioural scientists, to clinicians and policy makers, far beyond the relatively small group of active spider taxonomists. For example, there is nothing to stop the WSC from instituting a general obligatory delay before implementing any wide-ranging rearrangements at the family and genus level, based on specific objective criteria. This could be done by an entirely formal assessment, analogous to assessing whether a taxonomic act fulfils the criteria for a valid publication according to the ICZN, which also does not require a scientific judgement.

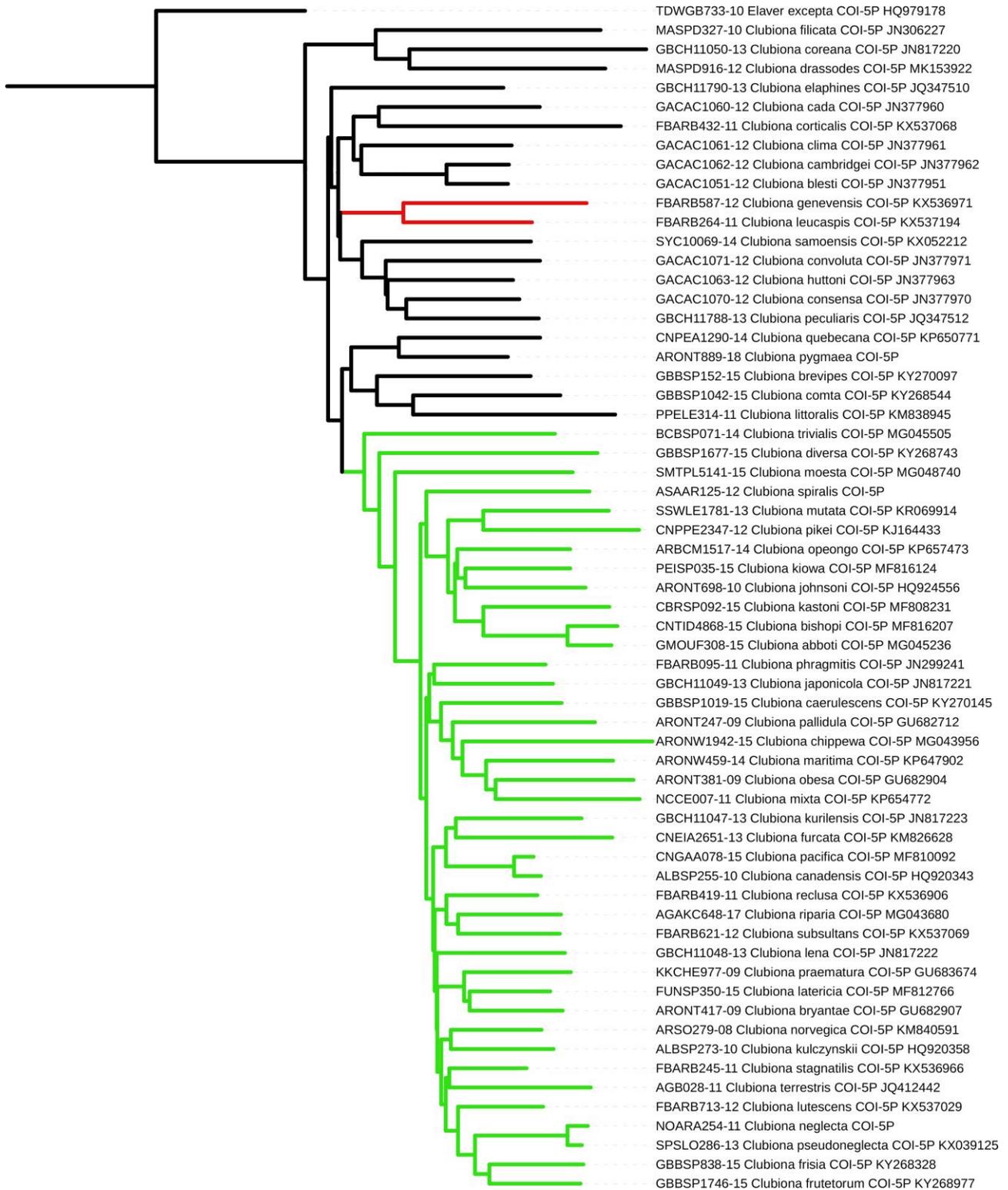
That the current policies employed by the WSC are unsatisfactory is evident from even a cursory analysis of the (relatively few) examples of non-implementation in the present version of the catalog. This does not mean that any of the decisions made by the WSC team are unreasonable – in fact, an even more conservative approach would often seem advisable –, but there is a notable lack of consistency and scientific rigor in the arguments provided. For example, numerous rearrangements at the family level proposed by Murphy & Roberts (2015) are not implemented in the WSC, with the explanation that they are “not sufficiently justified”. The proposed rearrangements would indeed be very disruptive and often in contradiction of other widely accepted proposals; they also are not supported by any attempt at a cladistic argument. Their non-implementation would therefore seem to be prudent enough – but without a clear definition of what would constitute “sufficient justification” of a taxonomic decision, it creates the impression of a certain degree of arbitrariness.

Another example illustrates even more clearly that an attempt by the WSC to pre-empt a public scientific debate is likely to be futile: the genus *Cedicus* was transferred to the family Desidae by Lehtinen (1967), by Murphy & Roberts (2015; sub Amphinectidae, which the catalog currently considers a junior synonym of Desidae), and by Zamani & Marusik (2017), who provide the most detailed morphological justification for this placement so far. The catalog does not implement this transfer and maintains *Cedicus* (and related genera) in the Cybaeidae, “because it does not fit with the geographical range given by Wheeler et al., 2017: 608 and the subfamilies defined there” – a truly puzzling argument, as Wheeler et al. do not include *Cedicus* or its relatives in their analysis, do not discuss this case, and use a much wider concept of Desidae s. lat. than any of the previous authors. Moreover, even if the family as defined by Wheeler has its main biodiversity in Australia, New Zealand and South America, as *Desis* itself is correctly described as having a “worldwide” distribution, the zoogeographic argument appears to be rather weak. Perhaps it would be more productive if the WSC policy included, for example, a statement that “large-scale transfers at the family level will be documented in the catalog, but will only be considered for full implementation after a 2-year grace period, during which additional supporting or refuting evidence might become available.” This would also have minimized the temporary instability following the recent abundance of major phylogenetic analyses of large parts of spider diversity.

Inconsistencies are even more apparent in decisions at the genus level. For example, Wunderlich’s (1995) separation of *Artanes* from *Philodromus* is rejected because “this unjustified elevation of an automorphic species group would render *Philodromus* paraphyletic”. This is certainly a valid argument (see the similar case of *Arboricaria* / *Micaria* in Breitling 2017), but subsequently the much more far-reaching sub-division of *Philodromus* by Wunderlich (2012), which potentially would have overcome the problem of paraphyly, is equally rejected as “based neither on cladistic analysis nor an adequate taxon sample”. This is contradictory, but would still be acceptable as merely indicating a change in policy. However, it becomes hard to defend when later two of Wunderlich’s proposed splits (of *Pulchellodromus* and *Rhysodromus*) are implemented after all, although they still lack cladistic justification and, especially in the case of *Pulchellodromus*, most certainly lead to aggravated paraphyly of the remaining *Philodromus* s. str. (Breitling 2019a).

Similar inconsistency in the WSC approach is evident in the case of Wunderlich’s (2011) subdivision of *Clubiona* into multiple genera. This is rejected (rather dismissively) as “not based on a thorough, global analysis”. It is unclear why the catalog would need to include such a judgemental statement at all, given that Mikhailov (2012) had already formally synonymized the genera proposed by Wunderlich (2011). It becomes even more problematic when one of the genera proposed by Wunderlich is subsequently resurrected by Marusik & Omelko (2018) with a slightly altered composition, and this change is uncritically implemented in the catalog. This not only creates immediate problems of paraphyly for the remaining *Clubiona* s. str., it also implies a double standard, as the underlying analysis is also far from global in scale and lacks a cladistic analysis: most importantly, the authors did not examine the supposed Southeast-Asian members of the genus and their geographic neighbours. As a result, *Porrhoclubiona* sensu Marusik & Omelko (2018), i.e. combining the Palaearctic *genevensis* group (Bosmans et al. 2017) and the Southeast-Asian *pteronetoides* group (Deeleman-Reinhold 2001), is very likely to be a polyphyletic assemblage.

The members of the *pteronetoides* group are not only zoogeographically unlikely relatives of the *genevensis* group, they also lack key synapomorphies of *Porrhoclubiona* as defined by Marusik & Omelko (2018). For instance, “[f]emales of *Porrhoclubiona* differ from these of *Clubiona* by the shape of receptacles: round sclerotised (or primary, Sr) and round hyaline (or secondary, Hr) receptacles (vs. both pairs of receptacles elongate)”, but the members of the *pteronetoides* group have some of the most elongate receptacula of all *Clubiona* species. Moreover, the males “possibly lack modified setae on the cymbium”, while “*Porrhoclubiona* differs from all other clubionids by having modified setae on the cymbium”. Given the close functional link and co-evolution of male and female genitalia (Wiehle 1965, Huber 1995), the major differences in the epigynal structures cast serious doubt on the idea that the superficial similarities of the male pedipalps are synapomorphic. Additionally, the males’ chelicerae are not “strongly protruding” and their pedipalps don’t seem to have a distinct “prolateral tibial apophysis, thus lacking another two of the supposed synapomorphies of *Porrhoclubiona* sensu Marusik & Omelko. Finally, the members of *pteronetoides* group are pale or greenish foliage dwellers, quite different in habitus and ecology from the brown, ground-living, distinctly patterned members of the *genevensis* group.



**Figure 1. Neighbor-joining tree of selected barcode sequences of *Clubiona* species from the BOLD database. The Holarctic core *Clubiona* s. str. is indicated in green, the *genevensis* group in red. The key features, as discussed in the text, are also found in maximum parsimony and maximum likelihood trees of the same sequences.**

It is almost certain that a redelimited *Porroclubiona* s. str., i.e. including only the Palearctic *genevensis* group, is monophyletic, and it may even be the sister group of the remaining Holarctic *Clubiona* s. str. However, members of the group have variously been included in other species groups or subgenera of *Clubiona*, including *Microclubiona* (Sterghiu 1985), the *filicata* group (Zhang & Hu 1989), and the *comta*

group (subgenus *Hyloclubiona*; Mikhailov 1995), indicating that its phylogenetic affinities are not easily resolved. A preliminary analysis of publicly available barcode sequences, analogous to that in Breitling (2019b), does not yet allow a resolution of the internal structure of *Clubiona* s. lat. (**Figure 1**). While it is clear that there is a strongly supported Holarctic core *Clubiona* s.str. (barcoded members *C. pallidula* [type species], *C. abboti*, *C. bishopi*, *C. bryantae*, *C. caerulescens*, *C. canadensis*, *C. chippewa*, *C. diversa*, *C. furcata*, *C. frisia*, *C. frutetorum*, *C. japonicola*, *C. johnsoni*, *C. kastoni*, *C. kiowa*, *C. kulczynskii*, *C. kurilensis*, *C. latericia*, *C. lena*, *C. lutescens*, *C. maritima*, *C. mixta*, *C. moesta*, *C. mutata*, *C. neglecta*, *C. norvegica*, *C. obesa*, *C. opeongo*, *C. pacifica*, *C. phragmitis*, *C. pikei*, *C. praematura*, *C. pseudoneglecta*, *C. reclusa*, *C. riparia*, *C. spiralis*, *C. stagnatilis*, *C. subsultans*, *C. terrestris*, *C. trivialis*), as well as a number of consistent species groups, e.g., *C. comta+littoralis+brevipes*, *filicata+drassodes+coreana*, or *pygmaea+quebecana*, the *genevensis* group (barcoded members: *C. genevensis* and *C. leucaspis*) is not consistently associated with any of them, and their composition is not easily reconciled with the traditional species groups of Mikhailov (1995) and subsequent authors. Moreover, species from outside the Holarctic region are only very partially represented (but seem to form a number of relatively consistent species groups by themselves, well separated from core *Clubiona*). The difficulty of delimiting the group and identifying its phylogenetic relationships indicates that for consistency the genus *Porrhoclubiona* is for the time being better considered a junior synonym of *Clubiona* (**syn. conf.**), pending a global cladistic analysis of the entire genus, as are the equally well justified (sub)genera *Paraclubiona* (type species *C. corticalis*), *Hyloclubiona* (type species *C. comta*) and *Breviclubiona* (type species *C. brevipes*).

The WSC team cannot hope to resolve this kind of scientific complexities as part of its cataloguing routine, but it can strive to develop a clear, unambiguous and pragmatic approach that would allow handling difficult cases consistently and without descent into personal acrimony. It is to be hoped that this contribution will help stimulating the wider debate required for establishing consensus on the necessary procedures.

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