Systematics and Biodiversity

Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/tsab20

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To cite this article: R. I. Vane-Wright (2003): Indifferent Philosophy versus Almighty Authority: On consistency, consensus and unitary taxonomy, Systematics and Biodiversity, 1:1, 3-11

To link to this article: http://dx.doi.org/10.1017/S1477200003001063

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Indifferent Philosophy versus Almighty Authority: on consistency, consensus and unitary taxonomy

Abstract The ability of the taxonomic community to heed Charles Godfray’s wake-up call to create ‘unitary’ taxonomic systems and make them available on the internet is hampered by real difficulties over achieving taxonomic consistency, and a cultural reluctance amongst systematists to embrace consensus. This paper explores these issues by examining the taxonomic history of an African milkweed butterfly, *Amauris damocles* sensu lato. Recent differences of opinion over the classification of this insect relate to fundamental differences in the theory and practice of systematics, from creationism through authoritarianism to cladistics, differences of a kind that will forever bedevil the distributed taxonomic system. If practical unitary schemes are to emerge, then the taxonomic community will need to adopt new ways of managing and recording taxonomic change, and develop a more responsible attitude towards the needs of others who are dependent of the primary products of systematics – names, and the classification schemes they symbolize.

A foolish consistency is the hobgoblin of little minds … Ralph Waldo Emerson

An increasing number of environmental, legal and other matters now hinge on scientific names, including their status. This paper explores a seemingly minor disagreement over the taxonomic rank of some African butterflies. The wider purpose is to address the way in which taxonomists ‘do business’. My conclusion, not unlike that of Charles Godfray (2002), is simply that we do not do it well enough. To improve, we will need to establish a new and more responsible culture in which a desire for consistency and a respect for consensus are normal expectations, while at the same time being able to embrace the need for progressive change in the taxonomic system as our knowledge improves. Can taxonomy continue to be a game in which anyone can play?

Variation, names and consistency

Like birds and hawkmoths, most butterflies exhibit variation in colour pattern (or *habitus*: Kitching & Cadiou, 2000). If this occurs without accompanying changes in other characters (e.g. genital morphology, scent-organs, wing venation), the differences have generally been regarded as infraspecific. In particular, when colour changes are more or less discrete and characteristic of particular physiographic regions (e.g. islands, mountains, valleys), the fashion has been to treat these modifications as subspecific. Over the last century an elaborate trinominal classification has developed for butterflies, in an attempt to reflect this widely observed phenomenon of geographical variation.

Taxonomists, however, struggle to maintain any degree of consistency (Kitching & Cadiou, 2000) with this system of polytypic species. For example, extreme splitting into microgeographic races in certain groups, such as the apollo butterflies (*Parnassius*) is a very dubious tradition that continues unabated to this day (Glassl, 1993; Hara, 2002). Excessive fragmentation seems as unhelpful as ignoring geographic variation altogether. In another context, opinions and treatments frequently differ when observable variation is clinal. Some then regard subspecies as inappropriate (Kitching & Cadiou, 2000:29), while others may recognize transition or hybrid zones between focal centres and name them as separate subspecies. Similar difficulties arise when disjunct populations differing only in colour are widely separated by large gaps of unsuitable habitat, and with ‘polytopic subspecies’ (Mayr, 1963: not to be confused with polytypic species, i.e. those made up of two or more subspecies): disjunct but phenotypically indistinguishable populations separated geographically by subspecies that are distinct.

More uniform, one might have expected, would be the treatment of continental populations apparently distinguished only by colour, and separated by relatively narrow gaps in distribution. Even in this context, however, it has been proposed that Mayr’s ‘multidimensional’ biological species concept
Figures 1-4  Male upperside (a) *Amauris (Amaura) (damocles) lygia* Hulstaert, 1924, holotype, Democratic Republic of Congo: Bas Uele, Sassa, Colmant, 1895-96 [in MRAC Tervuren; reproduced from Berger, 1981: pl. 54, fig. 1]; (b) *Amauris (Amaura) (damocles) damocles* (Fabricius, 1793), Sierra Leone: Legwema, 16.vii.1932, J.J. Simpson, B.M. 1912-443 (Specimen Register no. BMNHE# 310441); (c) *Amauris (Amaura) (damocles) hyalites hyalites* Butler, 1874, Angola: N'Dalla Tando, 2700 ft., 5.i.1909, W.J. Ansorge, Ex Coll. Bethune-Baker, B.M. 1927-360 (BMNHE# 310440); (d) *Amauris (Amaura) (damocles) hyalites makuyuensis* Carcasson, 1964, holotype, Tanzania: Mukuyu [sic], Kigoma T.T., ii.1962, Japanese Primate Expedition (BMNHE# 310442). All specimens approximately life size.

(BSC) is not only non-operational, but also deficient. According to ornithologist Joel Cracraft (1989:41), “application of the BSC has created paraphyletic taxa and has hindered recognition of... historical patterns”.

In recent publications on milkweed butterflies, a difference has emerged regarding the status of two groups of intimately related western and central African populations of the genus *Amauris* Hübner. I first examine how this came about in detail, as I believe it exemplifies how the current process of transacting taxonomic business is fundamentally flawed. After discussion of these issues, I return to this particular case to propose a baseline consensus scheme of the sort that I believe would be necessary to initiate a unitary (Godfray, 2002) taxonomy scheme.

**Amauris (Amaura) damocles and A. (A) hyalites: two species or one?**

In the narrow sense, *Amauris damocles*¹ (F.) is restricted to an area extending from Sierra Leone and Guinea (possibly as far as The Gambia) in the west, eastwards through Ghana to Nigeria. Only one synonym is included (*A. hyalites punctata* Dufrane, 1948, from Macenta, in south-east Guinea) and this, despite being described as a subspecies of *hyalites*, has the typical *damocles* phenotype (photograph of type examined).

From 1874 to 1924, various other closely related *Amauris* butterflies were described as separate species, notably from Angola (*A. hyalites* Butler, 1874), Gabon (*A. difficilis* Aurivillius, 1891), Congo (*A. fenestrata* Aurivillius, 1907), north-central Democratic Republic of the Congo (*A. schubotzi* Schultze, 1914) and north-eastern Democratic Republic of the Congo (*A. lygia* Hulstaert, 1924) (Table 1). Talbot (1940), however, recognized only a single taxon, found from Bioko and Cameroun south to Angola and Zambia, and eastwards to the western shores of Lake Tanganyika, to include all these nominal forms (Ackerley, 1995:273). In his brief revision of *Amauris*, Talbot (op. cit.) placed this aggregate as a subspecies of *damocles*, using the oldest of the available names, *A. damocles hyalites* [as “*egialea* hyalites”]. The subspecific status of this taxon was accepted by Carcasson (1963) in his work on East African Danainae, Bernardi (1974) in his account of geographical variation and polymorphism in African butterflies, Berger (1981) in his general work on the butterflies of the Democratic Republic of the Congo (except that he regarded the rare *A. lygia* as distinct, a possibility

¹ The oldest name applicable to the populations in question is *Papilio egialea* Cramer, [1777], from West Africa. Hemming (1964) showed this name to be a primary homonym, and indicated that *Papilio damocles* Fabricius, 1793, was the oldest available replacement.
that certainly needs re-investigation), Ackery & Vane-Wright (1984) in their world overview of the Danaids, and Ackery et al. (1995) in their edited version of Carcasson's catalogue of Afrotropical butterflies (Table 1).

Well in advance of Talbot's (1940) paper, however, in the very influential *Macrolepidoptera of the World* edited by Adalbert Seitz, the then leading authority on African butterflies, P.O.C. Aurivillius, recognized within this group of populations three separate species: *A. damocles* [as egialea] and *A. hyalites* (placed with five other species as his 'egialea group sub-group 2'), and *A. fenestrata* (placed with three other species as a member of 'egialea group sub-group 1') (Aurivillius, 1911). Aurivillius' system was followed closely by Peters (1952) in his provisional checklist for African butterflies (Table 1). D'Abrera (1980), based in part on Carcasson's then unpublished draft catalogue, also recognized all three named taxa as separate species, but listed them sequentially (presumably influenced by Talbot's systematics, but without acceptance of his rankings), noting that Talbot had suggested that *A. fenestrata* "may be a form of *A. hyalites*".

More recently, d'Abrera (1997) no longer mentions *fenestrata*, while the collective entity *hyalites*, as assembled as a subspecies by Talbot and apparently not directly equivalent to any taxon recognized by Aurivillius, is formally promoted to full species rank. Most recently, Hecq (1999), seemingly unaware of d'Abrera's action, likewise formally raised *hyalites* (sensu Talbot) to full species status (Table 1). At the same time Hecq gave Schultze's taxon formal subspecies status, as *A. hyalites schubotzi* from northern Democratic Republic of the Congo and the Central African Republic; Talbot had
regarded it as a local ‘form’ of *A. damocles hyalites*, of which it may indeed be a microgeographic race.

To complete the current picture, it is also necessary to note the taxon *makuyuensis* Carcasson, from the eastern shores of Lake Tanganyika (Tanzania). This was regarded as a subspecies of *A. damocles sensu lato* by Carcasson (1964) and Ackery *et al.* (1995), but as a subspecies of *A. hyalites* by d’Abrera (1980, 1997) and, following d’Abrera, by Kielland (1990). Interestingly, this most easterly population is in some features more similar to the western *damocles* than the central *hyalites* s.s. This is evident in the greater displacement of forewing pale elements in cells M1 and M2, the slightly more gradual hindwing transition from pale to dark, and the more restricted hindwing submarginal spots, suggesting that the assemblage (*damocles* s.s. + *makuyuensis*) could be compared to a polytopic subspecies.

**Disagreement: authoritarian or authoritative?**

Why do d’Abrera and Hecq differ from Talbot and Ackery? Talbot was unable to find any morphological difference between the various populations, other than the changes in colour pattern. Talbot’s account and conclusion were written and published at the time of the full emergence of the biological species concept. His empirical observations were confirmed by Ackery & Vane-Wright (1984) who, however, were unable to find convincing evidence that this polytypic species formed a clade. They were only able to recognize *damocles* s.l. comparatively, by its possession of unique characters linking it with three other species (*A. albimaculata*, *A. nossima* and *A. ochlea*), and a lack of the characters distinguishing those three (see further below). In other words, *A. damocles* was equivalent to a paraphyletic group in the sense of Cracraft (1989), or a ‘paraspecies’ in their own terminology.

Ackery & Vane-Wright were primarily concerned with establishing a cladistic framework for the species of Danainae then recognized. Insofar as they considered geographical variation in colour pattern ‘normal’ for species involved in mimicry (either because of climatic response and/or natural selection; cf. Bernardi, 1974), they found nothing to support or challenge Talbot’s view – their findings were neutral. For Ackery & Vane-Wright (1984), *Amauris damocles* thus appeared as a paraphyletic grouping, of uncertain status. However, given that they regarded Talbot’s work as the most recent coherent overview of the whole genus, they deferred from altering what they considered to be the definitive status quo.

D’Abrera (1997), in formally promoting *A. hyalites* Butler *sensu* Talbot to full species rank, rejected the ‘lumping’ of Ackery & Vane-Wright (sic!) because ‘their systematics is cladistic’. Apparently, d’Abrera objects to any classification based on cladistics as a matter of ‘principle’, preferring instead ‘to maintain the traditional view…until fashions pass and time amplifies truth, now obscured by indifferent philosophy and inadequate metaphysical accountability’. However, as explained above, Ackery & Vane-Wright were neutral regarding the status of *A. damocles* and *A. hyalites*. Moreover, it is not clear what substantive ‘traditional view’ d’Abrera is endeavouring to maintain: certainly not that of Aurivillius (1911) or his follower Peters (1952), or even D’Abrera’s (1980) own former position (as all three works recognized *A. fenestrata* as distinct, as well as *hyalites*), or Berger (1981). Notably, d’Abrera (1997) is now silent regarding Talbot (1940), who was the real ‘lumper’ that Ackery & Vane-Wright followed. As the formal procedures of phylo- genetic systematics would have been unknown to Talbot in 1940, I presume that his non-traditional view was due to some other ‘indifferent philosophy’ rather than any premonition of cladistics.

What d’Abrera is actually talking about is, of course, taxonomic rank and the problems that invariably attend attempts to apply the polytypic species concept in a consistent way. D’Abrera accepts Talbot’s basic system (e.g. a terminal group equivalent to *A. damocles hyalites*) but not his ranking: he simply raises the subspecies as prescribed by Talbot to full species status. Given d’Abrera’s rejection of evolution as an epistemological basis for taxonomy (d’Abrera, 2001), it would not be surprising if he were opposed, in principle, to the recognition of subspecies. However, this is clearly not the case (d’Abrera, 2001:78). He has accepted thousands of subspecies in his various works, and has described many himself, including some since raised to full species by others. Thus d’Abrera’s actions do not resolve the divide between unsought authority (Aurivillius) and unlikely revolutionary (Talbot).

In this context it is interesting that d’Abrera (1997) also re-segregates the three populations of *Euploea* (Danainae) found on the Mascarene islands as separate species, rather than one polytypic species, choosing “to revert to the status quo ante Ackery & Vane-Wright”. However, in the same haste to distance himself from anything apparently to do with cladistics or those who practice it, d’Abrera once again failed to appreciate that Ackery & Vane-Wright were deferring to Talbot, and to one of the leaders of 20th century butterfly taxonomy, A.S. Corbet – a man apparently regarded with approval by D’Abrera (1982: preliminaries page xx).

Corbet (1942:266), in a preliminary paper on *Euploea*, was the first to suggest that these Mascarene butterflies “may prove to be conspecific”. In a subsequent paper by Talbot (1943), based partly on an examination of male genitalia preparations made by Corbet, this view is confirmed: “*E. euphon* (F.) [Mauritius] is regarded as being conspecific with *goudoti* [La Réunion] and *desjardinii* [Rodrigues].” The genitalia of the usual *Euploea* type, show no differences. A comparison of the wing-markings suggests close relationships, especially between *euphon* and *desjardinii*. In some specimens of *goudoti* there is an approach to the hind-wing undersides band of *euphon.* In Corbet’s (1943) important key to the crow-butterflies that followed, these three *Euploea* taxa are united as a single collective species (“*E. euphon*…Mascarene Is.”) without further comment. In this case, however, Ackery & Vane-Wright were able to confirm, with new characters, that the three form a well-marked and distinctive group, and so had no difficulty in accepting the species-level judgements of Talbot and Corbet. Even so, this issue similarly boils down not to deep questions over systematic theory, epistemology or
metaphysics but to subjective disagreements over rank: species or subspecies? The three *Euploea* taxa are fully diagnosable on pattern alone, and the ‘fact’ that together they form a well-marked clade is not fundamental to the argument concerning rank. As discussed by Kitching & Cadiou (2000), the limit of division based on observable differences has no obvious end or “stopping rule”.

Hecq (1999) claims that confusion exists in the recent literature regarding the status of various forms of *Amauris*. He cites only Ackery *et al.* (1995), and focuses entirely on *A. damocles sensu lato*. Based on the wing patterns, Hecq states that, without doubt, the difference between *A. damocles sensu stricto* and *A. hyalites* is specific. Like d’Aubrera, Hecq is arguing about rank: no new information is offered, other than a minor analysis of the already well-known differences in colour pattern (which have never been in dispute). Hecq, again like d’Aubrera, seems unaware that the “recent literature” is based on Talbot (1940), the only comprehensive analysis of the species and subspecies of *Amauris* made since Aurivillius (1911). Unlike d’Aubrera, who apparently defers to a higher authority (d’Aubrera, 2001), Hecq prefers his own authority; to him it is self-evident (“ne permet pas de laisser subsister des doutes”) that the colour pattern differences between *hyalites* and *damocles* are worthy of specific rank, while those between the *hyalites* and *schubotzi* are of subspecific rank. Although not stated in so many words, Hecq considers that those who hold a different opinion (clearly Ackery *et al.* (1995) and presumably by inference, Talbot) suffer from “confusion”.

**Taxonomy: taste or evidence?**

A problem for taxonomy, as someone once said, is that it constitutes “a game in which anyone can play”. Thus Bernard d’Aubrera (a self-confessed anti-Darwinian, who has rejected the entire evolutionary establishment as a “self-appointed and self-seeking elite” engaged “in an orgy of mutual admiration and self-congratulation”) and Monsieur Hecq, independently, and without demonstrating evidence of scholarship or knowledge of these butterflies other than their exterior colour patterns, can countermand the considered opinions of three generations of milkweed butterfly specialists. George Talbot, Steven Corbet and Phillip Ackery all wrote with acknowledged sources, and each presented new information based on detailed microscopy. They neither invoked evolution explicitly, nor engaged in self-congratulation. Of course, such professional taxonomists have no private hotline to verity, and it could very well be that d’Aubrera and Hecq have independently converged on ‘the truth’ — perhaps reflecting G. G. Simpson’s well-known dictum that taxonomy is as much an art as science.

In reality, we cannot know the true natural system, we can only estimate it from empirical data (Vane-Wright, 2001). So what observational evidence (there is no experimental evidence) stands for and against these two views about *damocles* and *hyalites*? According to Ackery & Vane-Wright, *damocles* and *hyalites* share the following apomorphous characters within *Amauris* (numbers refer to character ciphers in the argumentation scheme of Ackery & Vane-Wright, 1984): upperside of hindwing membrane hairy at base (108); wing membrane visible between androconial patch-scales (109), bursa of female genitalia with a sub-cylindrical tubular section (118), aedeagus with sinistro-lateral spicule patch near apex (127), marginal spines on male eighth tergite present in centre only (131), hindwing androconial patch-scales clubbed and thickened (132), and male hairpencils with central cone hairs (134). This complete suite of characters is found in just three other *Amauris* species. Of these, *A. albimaculata* has distinctly clubbed antennae (99), and *A. ochlea* and *A. nossima* share a unique configuration of the hindwing alar organ (139).

By addition of chemical characters derived from the butterflies’ male pheromone glands, Vane-Wright *et al.* (1992) were able to group *damocles damocles* with *ochlea* (and *nossima*) to the exclusion of *albimaculata* (presence of 11-dodecanolide and (Z)-6-nonenoic acid in *damocles damocles* and *ochlea* but not in *albimaculata*: Schulz *et al.*, 1993). This leaves *damocles* firmly grouped with these two other species, but not unequivocally united with *hyalites* by morphological peculiarities shared by both. Several potential chemical synapomorphies for *A. damocles s.l.* are evident from the work of Schulz *et al.* (1993) on *damocles damocles*: five oxidized carboxylic acids (4-hydroxybutyric, butanedioic, octadecenoic, 9-oxononanoic and nonanedioic), and the terpenoid dihydroedulan. Discovery of any of these compounds in the *hyalites* complex of populations would be of great interest. In making such an investigation, examination of *nossima*, as well as two other closely related species (*A. dannfelti* and *A. inferna*), all as yet uninvestigated chemically, would also be necessary to ensure some degree of systematic rigour.

So, on current evidence, just the wing patterns and geography are relevant to the separation or union of *damocles* and *hyalites*, as outgroup(s) to (ochlea + nossima). Interestingly, Hecq sees the colour pattern differences as clear evidence of separate status, whereas Talbot considered the peculiarities of these *Amauris* patterns as evidence of their conspecificity. If we accept Talbot’s character “hind-wing basal area with inner ochraceous suffusion” as a synapomorphy linking the various populations of *damocles* and *hyalites*, then their connection as a single lineage forming the sister-group to (ochlea + nossima) would become more compelling. The yellowish suffusion of the hindwing in *damocles s.s.* is notable, and although the ochraceous suffused area in *hyalites* is far more restricted, it appears similar to *damocles s.s.* and at the same time both seem different in this regard from other *Amauris* — but this distinction is very difficult to describe, and could be illusory. More promising would be further investigations into the pheromone characters, and the acquisition of DNA data. Even so, synapomorphy does not decide rank: two allopatric species can share a sister group relationship just as well as two subspecies (cf. Kitching & Cadiou, 2000:29). But, as stated at the outset, the convention in butterfly taxonomy has been to unite as polytypic species those terminal entities that differ only in colour pattern from place to place, and do not overlap geographically or intergrade morphologically.
Freedom or responsibility, one species or two: does it matter?

Changing fashions
This account highlights two separate problems: a general question about the way in which taxonomic decisions are made and promulgated, and the specific issue of what to do about damocles and hyalites. Taking the second point first, we could say that the butterflies spotlighted here are of minor importance; perhaps it matters little if we regard two such allopatric, continental taxa as separate species, or well-marked subspecies. However, what we choose to call the same, or not, does have practical consequences for conservation. In particular, inclusion of local areas among efficient networks for the conservation of regional species diversity is largely determined by the degree to which each component area supports different subsets of the regional species pool, or complementarity (Williams, 2001). Complementarity is determined by the taxonomic system adopted, including rank, as well as the distribution of each included taxon.

Gittleman & Pimm (1991) were of the opinion that, if distinct species are not given specific rank, “bad taxonomy can kill”. Equally, however, unjustifiable taxonomic inflation places other, more distinct taxa, at increased risk. This is an indirect effect due to reducing the effectiveness of prioritization when, as is surely the case at present, the resources available for conservation are insufficient to manage all of biological diversity. Some of the conservation problems associated with taxonomic rank are discussed by Vane-Wright (2003). However, there is little doubt that the dilemma posed by choosing between relative relationship (propinquity of descent) versus absolute difference (genetic distance) will continue to haunt this issue.

In this particular case, the solution proposed by Talbot, linking hyalites and damocles within a single, polytypic species, is typical of the conventional treatment that many, if not the majority of butterfly specialists have accepted for such cases over the last 100 years. Even though Talbot’s solution was certainly not arrived at through cladistics, d’Aubra is probably right to state that the philosophy of Ackery & Vane-Wright was indifferent, and Hecq is surely correct that they were confused: at least, they were inconsistent regarding their philosophy for recognizing higher taxa (cladistics) versus terminal taxa (what they found in the literature, unless demonstrably polyphyletic). With respect to suffering from some confusion over the ontological status of species as classes or individuals, Ackery & Vane-Wright are of course not alone (see discussion in Panchen, 1992:337–341), and these philosophical difficulties are even now not resolved to the satisfaction of all systematists (Olivier Rieppel, in litt.). However, the point here is not to retreat into pre-Darwinian taxonomy qua d’Auber, or to adopt authoritarianism qua Hecq, as rash antidotes to uncertainty. The answer, in the case of damocles and hyalites, is probably to advance further along the phylogenetic or cladistic path and, in line with the ideas of Cracraft, adopt the phylogenetic species concept for such a case: “a phylogenetic species is an irreducible (basal) cluster of organisms, diagnosably distinct from other such clusters, and within which there is a parental pattern of ancestry and descent” (Cracraft, 1989:34–35; cf. Kitching & Cadiou, 2000:29).

I have not seen any individuals of these two taxa that could not be assigned without hesitation (with the exception, perhaps, of the unique fenestralta) and, so far as known, all milkweed butterflies reproduce sexually. Not only are their wing patterns distinct, the wing shapes may also prove different — but this has yet to be assessed in any quantitative way. It is my intention to pursue the phylogenetic species line of reasoning in a future re-investigation of all Amauris. Whether or not this approach will lead to a dramatic increase in the number of full species recognized is a moot point, in practice surely dependent on the degree of strictness or laxity in making operational the criterion ‘diagnosably distinct’. Despite Cracraft’s bullish tone, it remains to be seen whether the phylogenetic species concept just moves the degree of uncertainty to a finer scale (cf. Moritz, 1994). Moritz’s expectation of ‘reciprocal monophyly’ for sister groups has simply not been demonstrable in more than a fraction of cases, at least in my experience of working with the morphospecies of conventional taxonomy.

Free-for-all, or free-to-all?
This case study provides clear evidence of the inadequate way in which formal taxonomic changes are made and recorded. Does someone compiling a list of Amauris butterflies from the Congo, for example, follow the recent major catalogue of Ackery et al. (1995) (still the only published modern catalogue for the butterflies of an entire tropical region), the current primary reference on higher classification of the group (Ackery & Vane-Wright), or the most recent species-level revision (Talbot)? Or again, do they accept the most recent illustrated continental atlas (d’Aubra), the most recent subregional atlas (Berger), or the most recent paper that specifically addresses the issue in question (Hecq), and the possible veracity of the d’Auber/Hecq position as discussed here? Also, what should be done regarding Amauris fenestralta? Such a mire of uncertainty is surely an outstanding example of the problem posed by the ‘distributed’ organization of conventional taxonomy, as discussed by Godfray (2002).

Disagreements over the rank of allopatric taxa are rarely solved by discussion. Of course, this freedom of choice, in the case of butterfly taxonomy often with unreferenced publication and unchallenged appeal to all manner of philosophical deliberations to give apparent gravitas to one’s position, has been part of the fun (and fun in science is undoubtedly important). But, in the age of informatics, we need to take a more public, or more responsible attitude to the way in which we manage our nomenclatural affairs. Anarchy may be a delight for the proponents of differing views, but it holds little attraction for the users of taxonomic information.

Having said that, I would be the last to argue against making taxonomic changes if information content and predictability can be increased as a result. Moreover, a rather good reason for the current state of ‘anarchy’ in taxonomy is that it reflects, in part, the healthy and energetic exploration of different avenues of research. But regardless of the
merits or otherwise of this particular case, making changes for the sake of it, without taking into account previous arguments and without acquiring new evidence, devalues the taxonomic system by continual introduction of unjustified and/or unjustifiable changes. To overcome these problems it has sometimes been proposed that nomenclature be fixed, or that particular works be accorded authoritative status, to stop this endless process of seemingly unnecessary or whimsical change. While others have argued strongly and in many contexts correctly against this position, the information revolution has opened new possibilities for abandoning free-for-all in favour of free-to-all (McNeely, 2002).

The internet offers the clear possibility of an alternative approach. The establishment of comprehensive internet-resident, regularly updated global lists for all manner of organisms is a goal of several emergent organizations, including GBIF (http://www.gbif.org/), Species 2000 (http://www.species2000.org/), All Species Foundation (http://www.all-species.org), DIVERSTAS (http://www.icct.org/diverstas) and the Global Taxonomy Initiative (see http://research.amnh.org/biodiversity/publ/c_gti.html), together with national coalitions such as the UK’s National Biodiversity Network (www.nbn.org.uk). The formation of an internet catalogue for butterflies, freely accessible to all, is the aim of at least one international group wishing to set up a “global butterfly information system” (GloBIS, www.ento.csiro.au/globis; Lamas et al., 2000). However, it has to be admitted that if any of these grand schemes are to be effective, both the baseline initial ‘offer’ and the procedures adopted for updating will be crucial. To be acceptable there will almost certainly need to be some form of committee structure, with all the possibilities for bureaucracy and power-seeking that so often seems to bedevil such otherwise well-meaning operations. And who is to pay for all this? The fact that these concerns and questions are difficult does not mean that such enterprises are doomed from the outset – but it does mean that a considerable degree of realism will be needed.

If successful, GloBIS will eventually offer far more than just an authoritative list, as it is planned to have linked or networked databases for images, identification systems, distribution data, maps, hostplant records, information on early stages and ecology, bibliographies and so on. The standardized, fully synonymy global list to be put at the core of this enterprise will be essential, allowing information stored under different names and according to different classifications to be inter-operable. However, even in the short term this list will give the opportunity of having an up-dateable international standard. Not that everyone will be expected to use or accept the nomenclature adopted by GloBIS at any given time, but whatever has been written about butterflies, under the host of all different names that have been applied, will in theory be tracked by this system. The goals of GloBIS are consistent with, but not so authoritarian as, the concept of a ‘unitary’ system of taxonomic organization, as proposed by Godfray (2002).

The extent to which any of these systems can be modelled successfully on the experience of ornithologists operating at national level is worthy of investigation. For example, the British Ornithologists’ Union is responsible for an ‘official’ list of British Birds, maintained by the Taxonomic Sub-committee of its Records Committee (http://www.bou.org.uk/). Regular updates are made available via their internet site, and through conventional publication (Knox et al., 2002). The experience of lepidopterists with this type of operation has so far not been very good. For example, in compiling a semi-official North American checklist, butterfly taxonomists Lee Miller & F. M. Brown (1981) adopted a ‘latest revision’ approach, and so accepted Lionel Higgins’ (1978) fragmentation of the genus Euphydryas. This led to a furious critique by no less an authority than leading ecologist and former systematist Paul Ehrlich (Ehrlich & Murphy, 1982), who was convinced that Higgins’ taxonomic proposals were a major step backward in the study of his beloved checkerspot butterflies. Such differences stem, at least in part, from fundamental individual differences in underlying philosophy of taxonomy (Vane-Wright, 1992), notably with respect to the issue of characters and their perceived aggregate or separate significance (Vane-Wright, 2001, 2003).

Returning, finally, to the particular example discussed here, GloBIS could perhaps entertain the following provisional ‘sequenced’ classification for these Amauris butterflies, to take account of the conflicting views. This would entail recognizing A. damocles as a superspecies, with two or three component semi-species: A. lygia, uncertainly placed within the superspecies (all its relevant morphological characters need checking), and A. damocles and A. hyalites, provisionally regarded as sister taxa, with the latter divisible into three subspecies, but with no interrelationships among them implied. The status of the curious A. fenestrata remains to some extent uncertain, but rather than just ignore it (as in d’Abrera, 1997) it would continue for the present to be located as a synonym (?aberration) of hyalites s.s. If we could conjure a direct link to a photographic image of the type specimen (colour lithograph in Aurivillius, 1911:pl. 25 row b), we can then begin to imagine the ultimate power and flexibility of such a system, and its potential for providing authoritative information, rather than just peddling authoritarian opinions.

Provisional classification of Amauris (Amaura) damocles superspecies

- superspecies damocles (Fabricius, 1793)
- semispecies lygia Hulstaert, 1924, incertae sedis
- semispecies damocles (Fabricius, 1793)
- semispecies hyalites Butler, 1874
- subspecies h. makuyuensis Carcasson, 1964
- subspecies h. hyalites Butler, 1874 (= fenestrata Aurivillius, 1907)
- subspecies h. schubotzi Schultze, 1914

If it is objected that the proliferation of quadri- and quinque-nominals to accommodate such a scheme is objectionable, it should be remembered that under the international code only the binomial categories are obligatory. Under such a limitation this scheme simply reduces to:
These butterflies, including the three subspecies of *A. (damocles) hyalites*, can be separated from other *Amauris* using the key of Ackery & Vane-Wright (1984:163–164), where they should run to *damocles*, either at couplet 8, or couplet 18. They can then be distinguished as follows:

1. Forewing postdiscal pale spots in cells M₁ and M₂ strongly displaced so that posterior spot lies much closer to the wing margin; entire hindwing pale discal area with an ochreous cast, with the yellowish scales at the tip of the discal cell virtually covering entire wing membrane; boundary between pale discal area of hindwing and dark marginal region indistinct, gradually changing from yellowish to brownish-yellow to brown; submarginal pale spots on hindwing upperside absent or very indistinct posterior to cell M₁ (male and female figured by d’Abrera, 1997:188) (western Africa, from Guinea to Nigeria) .......................

2. Forewing postdiscal pale spots in cells M₁ and M₂ displaced so that posterior spot lies distinctly closer to the wing margin than the anterior; white areas in forewing discal cell and cell Cu₁₄ large and confluent, separated only by darkened vein, with small adjacent white areas in cells M₁ and Cu₁₀ likewise separated by dark veins; apex of hindwing discal cell dark; submarginal pale spots on hindwing upper-side small or indistinct (holotype male figured by Berger, 1981: pl 54, fig. 1) (Democratic Republic of the Congo: Mongala and Bas-Uele) ....................

3. Forewing postdiscal pale spots in cells M₁ and M₂ very short (not longer than wide) and more strongly displaced; hindwing pale area restricted, slightly ochreous, with tip of discal cell covered by contiguous dark scales; hindwing pale/dark transition zone less sharp than typical *hyalites* (male figured by d’Abrera, 1997:188, as *makuyensis*) (north-western Tanzania) .......................

4. Hindwing pale area less extensive, with cell M₃ usually dark to base (sometimes with a small pale area at extreme base), occasional specimens having even the tip of the discal cell darkened in *h. makuyensis* (male *hyalites* figured by d’Abrera, 1997:188) (Cameroon to Democratic Republic of the Congo, Angolan and Zambia) .......................

Subspecies *hyalites hyalites* Butler

Forewing with relatively larger pale spots; hindwing also paler (especially in female, although females of a similar pale phenotype are known from as far apart as Bioko and Zambia), including pale areas at the bases of hindwing cells M₃ and Cu₁₄ (male and female figured by Hecq, 1999) (Central African Republic and adjacent area of northern Democratic Republic of the Congo) .......................

Subspecies *hyalites schubotzi* Schultz

The problem of achieving consensus

There is a problem with what I have outlined above. There is no justification for the classification that I have proposed. It stands in an analogous relationship to that of a consensus tree to the set of equally most parsimonious cladograms from which it has been derived. Almost invariably the consensus tree differs from any of the component MPCs, and is thus longer – and therefore less well supported. However, it does have heuristic value insofar as, based on one formula or another, it focuses the mind on what is universal, or not contradicted in some specified way, in the set of all shortest trees. In my opinion, if we are to move to any system approaching Godfray’s unitary ideal, then we will have to come to terms with either making arbitrary choices among competing classifications (and risking the wrath of all latter-day Paul Ehrlichs), or accept the additional burden of arriving at some compromise or consensus at the outset. From then on, one could hope to have a fully articulated set of reasons for accepting any further change to the taxonomic system, duly recorded and documented in the unitary system. In my experience, the taxonomic community does not seem to be ready to accept the idea of compromise or consensus. If so, this may scupper any real prospect of achieving a unitary approach. I hope not.

Acknowledgements

I am very grateful to Professor Michael Boppré and his colleagues Ottmar Fischer and Klaus Kiesel for making EM images of the alar and abdominal scent organs of *A. damocles* s.s. and *hyalites*, thereby demonstrating to my satisfaction that the two do not differ in these structures. The NHM Photography Studio originated the original digital images used for the figures. John Chainey drew my attention to the work of the BOU. Michael Boppré and Phillip Ackery, together with Gaden Robinson, Brian Rosen and Richard Bateman, also kindly commented on the manuscript – but this does not imply their agreement with any of the views expressed here.
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