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**Searching for sense:  
varieties, “currently valid” classifications, “new” combinations &c.**

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Any branch of serious activity must be framed with a system of formal rules, procedures, definitions, or even purely customary practices, if we wish to know what exactly are we doing and how to interpret the results of our work; taxonomy is, of course, no exception. Initially such restrictions and specifications were being introduced when, with the refinement of scientific methodologies, the real need arose, but with time the situation became increasingly complicated: on the one hand some of the multitude of prescriptions and principles (*e.g.* nomenclatural regulations have swollen – not without good reason – to the size of voluminous books of generally rather juridical than scientific nature) proved incompatible with others, necessitating “codification” of (often “multistoreyed”) exceptions and other ambiguities; on the other bureaucratic lust to formalize everything, with spreading of casuistic way of thinking among scientists themselves, provoked introduction of rigid regulations where none are really needed. Thus, now we are sunk in the muddle of necessary or superfluous or even definitely harmful (often mutually contradictory) rules, recommendations, restrictions, prescriptions, imposed by more or less (sometimes not at all) legitimate bodies and published (or not...) in more or less respectable editions (from International Codes through official Opinions to editorial “standards” and “peer”-reviewers’ “mandatory suggestions”). It is not easy to orientate in this jungle, what leads to frequent misuse (and its passive acceptance by the “taxonomic community”...) of even the most fundamental regulations, with appearance of such bizarrely nonsensical proposals as that concerning “re”-introduction (KUBÁŇ 2006a, b) of the name *Lamprodila*, **allegedly** proposed **allegedly** by MOTSCHULSKY **allegedly** in 1860 **allegedly** as the “*replacement name for homonymous Lampra*”, all of these allegations being doubtful, first and last of them not even probable – see HOŁYŃSKI (2011) for details – after official suppression and placement in the “Official Index” of earlier, **validly** proposed and perfectly unambiguous *Dendrochariessa GISTL* on the pretext of conserving “current usage” to assure “stability of nomenclature”[!!!]

In the present paper I will discuss some less serious but also misleading or at least ridiculous misapprehensions, apparently based on uncritical, formalistic adherence to

superficially interpreted rules or senseless snobistic customs without reflexion on their sense. Let us see some examples.

### Variety:

The Code (ANONYM 1999) clearly distinguishes between subspecific names which are nomenclaturally available and infrasubspecific which are not, the problem of discrimination between the two categories having been resolved by ruling that

*“The rank denoted by a species-group name following a binomen is subspecific, except that*

*45.6.1. it is infrasubspecific if its author expressly gave it infrasubspecific rank, or if the content of the work unambiguously reveals that the name was proposed for an infrasubspecific entity (see also [Article 45.6.4](#));*

*45.6.2. it is deemed to be infrasubspecific if its author used one of the terms "aberration", "ab." or "morph";*

...

*45.6.3. it is deemed to be infrasubspecific if it was first published after 1960 and the author expressly used one of the terms "variety" or "form" (including use of the terms "var.", "forma", "v." and "f.");*

*45.6.4. it is **subspecific** if first published before 1961 and its author expressly used one of the terms "variety" or "form" (including use of the terms "var.", "forma", "v." and "f."), unless its author also **expressly** gave it infrasubspecific rank, or the content of the work **unambiguously** reveals that the name was proposed for an infrasubspecific entity, in which case it is infrasubspecific [see also [Art. 45.6.1](#)]" [bold face mine: RBH].*

Thus, a name introduced as a third element of a trinomen (using expressedly “subspecies” as a rank-indicator or not using any) is automatically deemed subspecific and thence available, that using “*aberratio*” or “*morpha*” are outright excluded, and only the case of “*varietas*” and – less frequently – “*forma*” must (if introduced before 1961) be more closely examined. The critical point is here the meaning of the condition “*if its author expressly gave it infrasubspecific rank, or if the content of the work unambiguously reveals that the name was proposed for an infrasubspecific entity*”. Up to *ca.* mid-XX c. taxonomists used various terms to denote the “hierarchy” of morphological (or sometimes ecological) distinctiveness (species, subspecies, race, natio, variety, subvariety, morph, aberration) as equally “valid”: some of them became considered “unavailable” only after the Modern Synthesis, when the meaning of the term “subspecies” had been restricted to **geographically** separated forms, and it was realized that individual (intrapopulational) variants are of no evolutionary (and thence taxonomic) value. But such (evolutionary populational) distinction was even formally drawn up only in the first modern edition of the Code, in 1960 – just therefore “varieties” proposed before 1960 are treated “liberally”, while those proposed thereafter are deemed unavailable outright! Ernst MAYR, arguably the most eminent zoo-taxonomist of XX Century, for decades member of the Commission, explains this very clearly:

*“For more than 150 years zoologists did not make a clear distinction between varieties that are genuine taxa (based on populations) and varieties that designate merely groups of individuals or phena. It would be ritualistic to determine the availability of infraspecific names on the basis of original terminology chosen by the author. ... In the case of infrasubspecific name given prior to 1961, corroborating evidence is to be used, such as the statement that a taxon is ‘characteristic of a particular geographical area or geological horizon’ [Art. 45d(ii)] ... It is taxonomic practice to give the benefit of doubt to authors having introduced ‘varieties’ prior to 1961.” [MAYR 1969: 362]*

This was universally accepted interpretation until HOLM & GUSSMANN (1992) suggested that the authors like “*Kerremans, Obenberger and Théry ... made a clear distinction between subspecies and variations, and their variations can therefore be interpreted as infrasubspecific throughout*”, and such erroneous (but convenient: everybody can describe under his/her own name a taxon discovered long ago by others...) empty “ritual” has unfortunately been followed by many other authors. It is true that some XIX and early XX c. authors “*made a clear distinction between subspecies and variations*”, but this distinction

meant to them mainly or exclusively various degrees of morphological (or sometimes ecological) difference: the concept of “infrasubspecific” in modern sense was – and must have been – alien to them, what makes the adherence to their original terminology purely “ritualistic”. It is not difficult to find out what kind of “*corroborating evidence*” is appropriate: the category of subspecies is defined by *separate geographical (or, in case of fossils, chronological) distribution*, so the phrase “*unambiguously reveals that the name was proposed for an infrasubspecific entity*” means nothing else than the original publication provides convincing evidence that the name refers to a form *occurring sympatrically (and synchronically)* with other conspecifics, *i.e.* – as formulated in Art. 45d(ii) of the (1960) Code and quoted by MAYR (1969) – is **not** “*characteristic of a particular geographical area or geological horizon*”; if such evidence is lacking, the name should be considered subspecific and, consequently, available. Of course, the degree and/or character of morphological or other difference, on which the “*clear distinction between subspecies and variations*” made by early authors was based, is in modern understanding perfectly irrelevant, so argumentations like

“*The content of the work unambiguously revealed that names were proposed for an infrasubspecific entity, since the taxon was briefly diagnosed only by a differences in color ... Following Article 45.6.4, such name should be deemed infrasubspecific and unavailable from the original publication*” [JENDEK 2002]

or, the opposite:

“*The contents of the work does not unambiguously reveal that the name was proposed as an infrasubspecific entity, since the taxon was diagnosed also by morphological [i.e. structural – RBH] characters. Following ICZN, Article 45.6.4, such a name should be deemed subspecific from the original publication*” [JENDEK 2003]

are based on evident misinterpretation of not only the spirit but even the letter of the Code...

On the other hand, statements like “*...contrary to its introductory statement, the zoological Code indeed partly infringes on the freedom of taxonomic thought and action in forbidding to name taxa at ranks below subspecies (e.g. variety or form)*” [RAPOSO & al. 2017] are also based on misunderstanding: nobody **forbids** to name anything, Art. 45 only specifies that such names are **not regulated** by the Code (and consequently are not “nomenclaturally available”, do not enter into synonymic or homonymic relations, &c.). This is perfectly logical consequence of the (to quote again MAYR 1969) “*clear distinction between varieties that are genuine taxa (based on populations) and varieties that designate merely groups of individuals or phena*”: the Code deals with names of **taxa**, *i.e.* distinctive [groups of] **populations**, the lowest rank of which being subspecies, while terms like “aberration”, “morph”, &c., and since 1961 also “variety” and “form”, are considered to denote **infrasubspecific** (*i.e.* – by definition – **intrapopulation**) variants. So, if somebody wishes to describe and name a distinctive **population** (even if actually **represented** by single specimen) and make the name “nomenclaturally available”, he/she should give it a rank of [sub]species – but of course if, *e.g.* in a study of variability – like that of *e.g.* MEZŐFI & KORÁNYI (2017) – names of distinctive (evtl. hierarchically arranged) individual (within-population) forms are needed, nothing and nobody prevents their creation and usage!

### “Currently standing”:

In the recent literature formulations like “classification currently in force”, or more specifically “taxon A, formerly a subgroup of B, now belongs to C...” are by no means rare. Sometimes it is only a relatively innocent mental shortcut for “the author’s opinion”, “the newest suggestion”, or “currently prevalent view”, being however frequently understood as a kind of binding dogma to be obligatorily followed by anybody, or even (especially in case of more general questions like “paraphyletic taxa are no more valid” or “morphological characters are worthless in phylogenetic reconstructions”) deliberate (executed by editors, reviewers, funding agencies: “*scientific circles do not, in fact, aim at the scientific truth, but at the situation where all their members would think the same*” – HOYLE 2001...) attempt at

monopolization of ideas and approaches (“*Roma locuta, causa finita!*”), what is already not simply *un*scientific but outright *anti*scientific attitude. In taxonomy such status of a dogma is most frequently ascribed to statements presented in various comprehensive compilatory works like reviews or catalogues (so *e.g.* one of our Colleagues recently wrote in a letter to me, that “*Vita Kuban has Tamamushia as separate genus in Palearctic Catalogue and so it is valid today*”, suggesting that other opinions – *e.g.* that *Tamamushia* KUR. is a subgenus of *Chrysodema* C.G. – automatically became “invalid today”), *i.e.* just to the type of publications where particular conclusions are least original (the author usually is not – and cannot be! – intimately acquainted with all involved taxa), least carefully studied (this would need much time while the deadlines press...) and at most superficially discussed (the volume of the publication is invariably restricted); see also the comment by Hans [MÜHLE] in the newsletter *Buprestis* 55:

*“For the Palaearctic Region there is an updated catalogue (see: KUBÁŇ 2016) containing also the Buprestidae. Again, as with the “Catalogue 2006”, I have problems with a lot of new acts... Without any further hints how to distinguish those taxa from that to which they have been synonyms, these acts are hardly convincing.*

*From my point of view a catalogue is not the place to solve taxonomical problems, particularly as long as we have no chance to understand the result of an act where the crucial part of the work was done in the background.”*

which I perfectly agree with.

However, even the thesis formulated in a publication based on original analyses of specific problems in restricted group of taxa, is no more than a view *of particular* (however eminent) *author*, and anybody can (and *should!*) refuse to follow it if *honestly* (not according to *e.g.* the journal in which it has been published, or – personal, national, or any other – sympathies or antipathies) considers it less convincing than some other hypothesis. So, there is no “currently in force” classification, no species “now belonging” to this or that genus, no name “formally synonymized” with any other: a classification *either is or is not* correct, a species *either does or does not* belong to particular genus, names *either are or are not* synonymous, and if they *are* synonyms *now*, they *were* also *previously* and *will be thereafter*, irrespectively of who does and who does not *consider* them synonymous (or even whether they are, were, or will be so *considered* at all) [in my opinion, this refers to so called “subjective synonymy” – depending upon more or less inclusive concept of the respective taxa – as well, but this opinion is true only if there exists (as I am convinced, albeit cannot prove) one single correct (adequate, of maximum information content = predictive power) detailed classification of all organisms which we should try to maximally approximate; if (as others can believe) such ideal system does not exist, then names are or are not (but *also “objectively”*: not depending upon any “authority” or “formal synonymization”!) “subjective synonyms” only in the context of the particular classification]. FRANK & SEKERKA’s (2016) conclusion:

*“HOLYŇSKI (1994, 2014) divided Chrysodema s. str. in several subgenera but did not assign species to them (with exception of type species), hence we cannot adopt his system”*

has apparently been based on similar misconception: a “genus-group” taxon is adequately defined by its *type-species and diagnostic characters*; which (if any) *other* species have (originally or subsequently, correctly or erroneously) assigned to it is absolutely *irrelevant* (such additional species can anyway be later added or removed by anybody without shaking the validity of the genus/subgenus), so subdivision of a genus into subgenera (or other similar taxonomic acts) should be adopted or not on the sole grounds of its *adequacy* (whether or not it reflects the interspecific relationships better than the alternative), not according to any formal preferences.

**“Syn. nov.”, “comb. nov.”, “stat. nov.”, &c.:**

One of the amusingly snobistic “excess of form over the essence” is the pedantic accentuation of one’s supposedly original concepts with qualification like “new” (or, in some situations, “revised”, “resurrected” &c.). It is difficult to imagine in what respect of any importance a “new” synonymy (combination, status) differs from “old” ones: if, *e.g.*, I use the name *Philocteanus (Pseudocallopiustus) resplendens (GY.)* for what other author called *Callopiustus platynotus (OBB.)*, it means neither less *nor more* than that **I, Roman HOLYŃSKI**, consider *Pseudocallopiustus OBB.* a valid synonym of *Callopiustus DEYR.*, *Chrysochroa resplendens GY.* a valid synonym of *Pseudocallopiustus platynotus OBB.*, and *Pseudocallopiustus OBB.* a subgenus of *Philocteanus DEYR.*; these **my, Roman HOLYŃSKI’s**, opinions may be right or wrong absolutely irrespective of whether the suggested synonymies, combinations or statuses are “new” [my original inventions] or “old” [accepted by some or all earlier authors], and can (should!) be followed by those who consider them correct **or rejected by those who disagree**. Analogy with “new” **names** is only superficial: the unambiguous “labelling” as “*sp. n.*”, “*gen. nov.*” &c. serves an important function: “fixation” of the name’s position in terms of priority, availability, authorship &c., whereas all those “*syn. nov.*”, “*comb. nov.*”, “*stat. nov.*”, &c. are only manifestations of empty snobistic display with **no** important information content whatsoever! And, not surprisingly, the authors probably themselves do not treat this custom too seriously: *e.g.*, in BÍLY & *al.* (2009) we find proud qualifications “*comb. nov.*” in peaceful coexistence with quotations (in lists of synonymies immediately below) of several earlier authors using the same combinations:

*Nesotrinchus australicus* (Kerremans, 1903), **comb. nov.**

*Nesotrinchus australicus*: THÉRY (1943): 644 ...; BELLAMY (1987): 101 ...; HOLYŃSKI (1993): 27.

or

*Nesotrinchus caeruleipennis* (Fairmaire, 1877), **comb. nov.**

*Nesotrinchus caeruleipennis* (Fairmaire): OBENBERGER (1936): 129 ...; THÉRY (1943): 644

and similar situations, where what has been claimed to be “new” is in fact quite old, are by no means rare.

Similarly ridiculous is the increasingly popular “hyper-exactitude” (presentation of the results – *e.g.* of statistical evaluation, measurements, geographical coordinates &c. – with precision that is evidently meaningless). So, *e.g.*, a bird (a vagrant from Central Asia) has allegedly been observed (during 5 days!) at 49<sup>0</sup>27’11.031”N, 17<sup>0</sup>17’44.214”E (the example taken from ŠÍREK & DOUPAL 2013, but I could quote hundreds of similar), *i.e.* the locality has been fixed with the precision of one thousandth arc-second; one second along a meridian or equator equals *ca.* 30 m. (along 49<sup>0</sup> parallel of course still less), one thousandth of that is *ca.* 3 cm. – whether such precision is (what I strongly doubt...) or is not **really** attainable, it is anyway glaringly meaningless in similar situations: even had the bird remained motionless throughout these 5 days, the geographical coordinates of either its bill or its tail (or both) would be different! Excessive “exactitude” is in fact misleadingly **inexact!**

**SMALL CAPS for personal names:**

The custom of writing personal family names in small caps is increasingly gaining popularity in biological (and perhaps other) literature. A very useful custom, allowing to easily find citations to literature (or other references to supporting or critical opinions) in a text and to discriminate between the family- and given-name[-s] of the respective person, what is often very difficult otherwise: for a foreigner it is by no means obvious whether a certain Chang Li Ban should be cited as CHANG L.B. or BAN C.L. or somehow else, the more so that even prevalent customs are different in various countries: in England or Poland given name is usually written first, in *e.g.* Hungary or Japan the opposite is the rule; but even a compatriote may be (and often is) not sure if to look for Janusz Bogdan on the alphabetical list under J[ANUSZ Bogdan] or under B[OGDAN Janusz]! So, the distinction of family-name

with small caps is highly recommendable but... but, as usual, the devil resides in details: in this case in **consistent** usage! Unfortunately, here also the whimsically diverse “editorial standards” have successfully transformed what was originally introduced to enhance clarity into a source of chaos and confusion: small caps are sometimes used only for authors of species names, sometimes just in such situations are not [cf. e.g. “*Nesotrinchus caeruleipennis* (Fairmaire): OBENBERGER (1936)” in the above quotation from BÍLÝ & al. (2009)]; sometimes for quotations in the main text but not in references, sometimes in the opposite way; often both family- and given name is “smallcapitalized”, in other publications neither of them – almost any imaginable combination can be encountered, what in practice deprives the usage of small caps of any meaning, and thus of any sense ... To serve their purpose, small caps – like italics, bold-face, or other forms of optical emphasis – must be applied **consistently**: only to **family-** [not to given-] names, but to **all** of them!

#### Citation of authors and years:

Throughout the XIX and XX centuries the generally accepted form of referring to a taxon was by its scientific name and (usually abbreviated) name of its author – *Chrysochroa similis* SND., *Curis C.G.*, **Julodinae** LAC. &c. However, recently some authors advocate – and many editors demand – quoting not only the author’s name in full, but also the year of description, what – especially synergic with recently fashionable multiauthority – produces often grotesquely cumbersome constructions like

*Locustella chengi* ALSTRÖM, XIA, RASMUSSEN, OLSSON, DAI, ZHAO, LEADER, CAREY, DONG, CAI, HOLT, HUNG, SONG, LIU, ZHANG & LEI, 2015

[cited after MARTENS & BAHR (2017)]. Apparently in order to alleviate the resulting inflation of the volume, another bizarre requirement is being introduced: the author[s] and date **must be cited** at first mention of a name but **must be omitted** thereafter, so that if you find a name on page, say 138 and wish to know the author, you must search through the previous 137 pages to find the first mention (to say nothing of the slovenly appearance of the page – or even list – where names with author and year alternate with those without any...)!

The requirement of citation (in text) of full author’s name and year is being usually justified by its alleged function as reference to the original description what, however, is an evident misunderstanding: it neither can effectively serve this purpose nor was this its originally intended task. As a reference to the original publication it is worthless because, on the one hand, very frequently the same author published several, sometimes tens of, papers in the same year: e.g. KERREMANS 11 in 1914 [BELLAMY 1998], OBENBERGER 42 in 1935 [JELÍNEK 1977], THÉRY 14 in 1937 [BELLAMY & DESCARPENTRIES 1990], &c.; on the other the strange custom of attributing the authorship of a name as different from that of the publication makes such “reference” simply misleading: e.g. so treated name *Acmaeodera holynskii* VOLKOVITSH 2011 would lead to the publication on... *Xantheremia niehuisi* VOLK. (VOLKOVITSH 2011), while *A. holynskii* VOLK. has been described in BÍLÝ & al. 2011!

So, what is the real need to cite the author (and sometimes year) of a name in plain texts (of course the situation with catalogues, taxonomic revisions &c. may be different)? In XVIII and early XIX centuries nothing like the code of biological nomenclature existed: FABRICIUS, DEGEER, HERBST, OLIVIER and their contemporaries applied such names to organisms (also to those already known) as they liked: sometimes accepted the name given by LINNAEUS or other earlier author, sometimes introduced a new, sometimes replaced even their own (given in an earlier publication) one with what seemed to them more appropriate. As long as the number of described organisms remained relatively small [LINNAEUS (1758) knew barely more than 4000 species of all animals, i.e. about as much as is now known in a single beetle genus *Agilus* CURT.!] such “anarchy” did not cause much trouble, but with each newly proposed name the problems became more and more evident. One of the most disturbing was

what we now term homonymy: so, *e.g.*, already GOEZE (1777) pointed to the confusion concerning (among others) the name *Buprestis*:

Buprestis GEOFFROY = Carabus L., Cicindela L.

Buprestis SKOPOLI = Chrysomela L.

Mordella SKOPOLI = Buprestis L.

Cucujus GEOFFROY = Buprestis L.

and similar situations appeared with increasing frequency, making the “bare” names (*Buprestis*, *Mordella*, *Cucujus*) effectively meaningless. To make them unambiguous, it became customary to cite the taxon name together with the name of its author: while “*Buprestis acuminata*” may be variously interpreted, “*Buprestis acuminata* DEGEER” unmistakably refers to the taxon now considered the type-species of the genus *Melanophila* ESCHSCHOLTZ; indeed, it has been quickly realized that to assure the univocal meaning the author’s name need not be quoted in full: abbreviated form *Buprestis acuminata* DEG. perfectly suffices to distinguish the species from *Buprestis acuminata* PALL.! Uncertainty remains only when – very rarely – **the same author** gave **the same name** to two (or more) **different taxa** (so, *e.g.*, *Anthaxia turcomanica* OBB. 1927 is not the same as *A. turcomanica* OBB. 1928), and **only** in such situations additional information (usually in form of the year of description) must be added (full spelling of author’s name is also in such situations obviously unhelpful and so superfluous...).

### Conclusion:

Displaying various “ornaments” to increase the superficial impression of the importance of publication and expertness of its author may be tempting or even – in the era of domination of impact factors, “peer” reviews, editorial “standards” and other formalities over the adequately assessed scientific value – felt necessary for successful career, but let us not lose the sight of the *sense* of our work: creation of make-believe appearances is not the valid purpose of serious research!

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