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Taxonomy: should it remain a serious branch of science or be transformed into a formal game?

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Taxonomy is frequently depreciated as „*XIX century philately*”, worthlessly pedantic pigeonholing, pointless “*counting bristles on bugs’ bums*”, &c. Hitherto such invectives are evidently no more than malicious marketing-motivated insults spread by those who would like to re-direct all available funds to their speciality, or at the very best silly coxcombrity of ignorants having nebulous idea as to what are they speaking about. However, some recent trends (e.g. increasing – and apparently supported by not only bureaucrats but also many scientists... – censorship making publication of anything not strictly following current fashions often near-impossible) and attempts to regulate every aspect of research by authoritative norms, may soon create the situation in which such scornful epithets will be justified! One of so directed proposals has been recently published by GARNETT & CHRISTIDIS (2017) in *Nature* [unfortunately, I cannot react (as would be most natural...) also in *Nature*, because in the *Comment* section “*Unsolicited contributions are not accepted*”...].

The authors evaluate the present situation, when “*As long as taxonomists follow the naming rules, they can define species however they wish*”, as “*anarchy*” and postulate that some authoritative body (they suggested the International Union of Biological Sciences) “*should create a process that does exactly what that effort [the declaration that “Nothing in this Code may be construed to restrict the freedom of taxonomic action”] avoids – restrict the freedom of taxonomic action*”!!! In particular, “*the IUBS should create a taxonomic commission to establish what rules (if any) should be applied across all life forms and, if taxon-specific definitions need to be developed, what those should be*” [“*for instance, agreed differences in calls and songs ... to delineate species of birds and primates; for fungi, genetic barcodes ... Such differences must be explicitly stated and agreed*”], then “*establish subcommittees*” which “*would review taxonomic papers for compliance with agreed standards*”, and “*judicial committee*” as “*the final arbiter ... responsible for upholding the rules*” – all that because of the alleged problems for preservation of biodiversity issuing from the different criteria applied by taxonomists to evaluate whether the particular group of

populations does or does not represent “*a lineage (an ancestral-descendant sequence of populations) evolving separately from others and with its own unitary evolutionary role and tendencies*” – SIMPSON (1961), *i.e.* qualify as a valid species. We hoped that the time when Comrade (if I remember well) ŻDANOW could have declared *ex cathedra* that the “bourgeois genetics” is incompatible with the Party line and only the progressive ideas of Academician ŁYSENKO must be followed is already gone – it is so sad to see that now two renowned scientists attempt to resurrect this style of management of science!

What GARNETT & CHRISTIDIS (2017) evaluate as “anarchy” – the researcher’s freedom to select the material for his/her study and methods of its elaboration (*e.g.* criteria to discriminate between inter- and intra-specific variability) according to what he/she considers most appropriate, interpret the results according to his/her personal views, and publish conclusions (*e.g.* classify a studied group of populations as species, subspecies or infraspecific variety) he/she had really arrived at and found adequate – is the very hallmark of serious science: some commission or committee can prescribe the exact procedure of presidential election, can decide what achievements qualify for the title of grandmaster in chess, or even which of the published synonymous taxon-names should be used as valid, but such arbitrary decisions are acceptable **only** because politics, sport, or nomenclatural regulations are **not science**: their aims are not “*systematic observation of facts and seeking to formulate the general explanatory laws and hypotheses that could be verified empirically*” (GARMONSWAY 1969) but only to solve some practical problems where the question is not “is this true?” but only “is this convenient?”. If some practical activity – like biodiversity conservation – is to be based on the results of scientific study, it must be adjusted to the conclusions arrived at by the respective scientists and not dictate them what their results should be and/or which criteria they should use to evaluate and formulate these results!

As GARNETT & CHRISTIDIS (2017) themselves admit, there *is* one, generally accepted species concept: “*a distinct evolutionary lineage*”: if so, their assertion that taxonomists “*define species however they wish*” is simply **not true**. The problem is, that the distinctness of evolutionary lineages is not directly observable or experimentally verifiable, therefore taxonomists must have invented several (the authors mention “*at least 30*”, but only very few are commonly used) indirect “proxies” to estimate the true “status” of the studied form. Thus, MAYR (1940) introduced “Biological Species Concept” (BSC: reproductive isolation is rather evident prerequisite of separate evolution), but this being applicable to only sympatric and synchronic bisexual populations, more generally relevant criterion of diagnostic morphological difference – “morphoevolutionary definition” (HOŁYŃSKI 1977, 1992, 2005), better known as “Phylogenetical Species Concept” (PSC: CRACRAFT 1983) – has been proposed [important to stress: contrary to the widespread belief, and despite their widely used names, these are **not** separate “**concepts** of species” but only criteria of approximation of the only valid “evolutionary” (SIMPSON 1961) concept: “*all modern species definitions either explicitly or implicitly equate species with segments of population level evolutionary lineages*” and so “*are special cases of the general lineage concept*” (DE QUEIROZ 1998)]. In my publications quoted above (HOŁYŃSKI 1977, 1992, 2005) I suggested to recognize, in typical situations, three “levels of approximation”: SIMPSON’S (1961) evolutionary concept as the “point of reference”, MAYR’S (1940) BSC as the most adequate whenever applicable, and my “morphoevolutionary” (\approx PSC) when reproductive isolation (or lack thereof) cannot be reliably ascertained. But even this is not the full story: the allopatry of two bird populations living on both sides of the Amazon River is not the same as in case of similarly distributed flightless terrestrial beetles; overlapping distribution of monophagous internal parasites not the same as co-occurrence of indiscriminate moths or rodents; diagnostic difference in widely variable character must not be equated with that in generally stable traits; evidence based on

unique holotype requires different evaluation than that supported by the examination of three, thirty or three hundred specimens; &c., &c., &c. – because of these and multitude of similar aspects each specific situation is different and should be more or less differently interpreted, what may be reliably done only by a taxonomist specialized in the respective group of organisms: any “standardized” rules prescribed by (however honourable and competent...) commission or subcommittee would have inevitably led to inadequate, *i.e.* erroneous conclusions and decisions!

Attempts to “standardize” systematic research by appointing one or few “decisive” characters to be obligatorily applied in solving any taxonomic problem have reappeared again and again: for some entomologists valid species is only what differs in structure of genitalia, others always look for wing venation, among ornithologists bird voices are currently in fashion; perhaps the most widespread is the reliance on DNA “barcodes” (*e.g.* PACKER & *al.* 2009); such approach – what I (HOŁYŃSKI 1993) dubbed “VIC[Very Important Character]-taxonomy” – may exceptionally, applied to some small groups of organisms, lead to acceptable conclusions, but generally must result in distorted, crippled classifications (*cf. e.g.* HOŁYŃSKI 2010)! Fortunately, heretofore such ideas were only personal, not binding to anybody, fancies of some students (or, more dangerous, editors or referees...) – GARNETT & CHRISTIDIS’ (2017) suggestion to make them obligatory would, if accepted and realized, be truly devastating!

All the above refers to *taxonomy*: a branch of science aiming to discover the *natural* system of organisms and establish the most informative “general purpose” classification. Evidently, in some cases more suitable may be different, special purpose classification (so *e.g.* ecologists, biogeographers, &c. can divide animals and plants into marine, fresh-water, semiterrestrial and terrestrial; predators, plant-eaters, scavengers and autotrophs; sedentary and migrating; autochthonous and alien; or even according to their size, colour, number of legs, DNA barcodes, or any other observable character) and of course if conservationists need such, prepared for *their specific* demands, system to decide what is and what is not worth protection, they may establish their own one which would meet their requirements. But as long as *natural taxa* are considered the most appropriate “units of conservation”, the identification of these must be left to practising specialists of particular groups – imposing on them any authoritative, “good for any occasion”, regulations would transform taxonomy from a serious, reliable, important branch of science into a formal game like playing patience and result in many so established taxa being no more than artificial constructs of no biological meaning whatsoever!

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At the end of the Cold War, Japan seemed poised to once again reject the West and seek Asian leadership. Instead, post-Cold War Japan tilted toward the United States and moved tentatively to assume "normal" international political-military responsibilities. It is as yet unclear where the Japanese will find a balance among an Asian role, the American alliance, and a "normal" international political-military role.