

How to avoid unprofitable debate on systematics among taxonomists

By

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Abstract. The authors express their opinion that the main source of unprofitable debate on systematical problems among taxonomists is the inadequate distinction between taxonomy and systematics. After defining "taxonomy", "systematics" and biological "classification" of systematics (vs. taxonomical "arrangements"), some types of the fruitless debate are mentioned. It is argued that the simplest possible taxonomical arrangements (where the genus is simply the lowest named taxonomic category, the names of taxa are character categories etc.) may be the most suitable for devising useful arrangements for users. Though the urgent tasks of zootaxonomy may force us to work in this way, several concurrent arrangements of an animal group should not necessarily be a cause for debate.

The authors of this paper are very different kinds of men. There is an age-difference of about one-and-half generations between them and the senior author specializes in mites, the junior in dipterans. They share, however, a common concern over the present crisis of their field of speciality, zootaxonomy. Currently various members of the not too populous company of zootaxonomists, working with inadequate funds and in scattered locations, engage themselves in futile debates on systematics which are irrelevant to their own problems and are a waste of time, thinking and paper. We think that if these problems have brought the attention of an acarologist and a dipterist to the same questions, they are of a nature general enough which should be the concern of every taxonomist. We are aware of other even more important aspects of the current crisis of zootaxonomy. We wish to pay more attention in forthcoming papers to the problems of modernizing nomenclature, to funding and strategic goals of taxonomic research, and the acceptance of taxonomic identification as intellectual achievement.

The crux of the problem defined in the title of this paper lies in the fact that zoosystematics and zootaxonomy are two fundamentally different fields of science and the lack of distinction has been the cause for many misinterpretations. The junior author recently published a brief overview of the concepts

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of taxonomy, systematics and phylogenetic classification (PAPP, 1986). According to him, taxonomy is concerned with the study of the manifestation at the individual (population) level of the diversity of living organisms. The level of study is not necessarily idiobiological, rather it focuses attention on problems at the infraindividual level or at the borderline between idiobiological and supra-individual levels. This implies that studies are carried out in some manner on single (animal) specimens although they are always closely related to the infra- and supra-individual levels. The duties of a taxonomist are to collect and analyse primary and basic information on morphological, bionomic and perhaps biochemical characters. Taxonomy deals with the end products of phylogeny, i.e. with the populations and individuals representing species which are the result of biological evolution. Thus, although taxonomy is not a phylogenetic branch of science, it cannot do without a sound basis of evolutionary perspective. PAPP (1986) emphasized the general "information-producing" role of taxonomy with which it serves all other branches of biological sciences, and summarized this point briefly, if not precisely enough, as follows: "considering the results of taxonomy, it is but a general auxiliary field of science". With the above-said in mind, taxonomy is concerned with phenological studies.

Systematics studies the causes and modes of the process during which the diversity of life developed, explores the relationships between the various forms (species, monophyletic groups, etc.) which are components of the diversity (canalized manifestations of living matter) brought about by evolution. Systematics draws from the results of taxonomy, evolutionary biology, population genetics, palaeontology and various branches of infraindividual and synbiological levels of study. Systematics is an evolutionary, historical science, yet at the same time its task is to classify organisms in a phylogenetic system. We accept SIMPSON's concept that systematics is the science of biological sciences, which, however, is only valid if taxonomy is considered an independent branch of science. Systematics is a causal field of science, and is not only concerned with the phenomena discovered by taxonomy.

In close relationship with the above is that classification necessarily has a different meaning in zoology than in formal logic. We propose that classification be considered that branch and family of methods of systematics which divides the phylogenetic tree and phylogenetic relationships, i.e. the reconstruction of evolution, into taxa. This concept concurs with that of CHARIG (1982) and others. Let us call the various solutions to the necessarily arising need to group organisms in taxonomy, as arrangements. Let us specify that when studying the morphology of recent species, even if with the most modern methods of data capture and analysis, the result can only be phenological (phenogram, phenetics, etc.). We not only accept the omnipotent concept of evolution but respect it so much that we avoid its usage as a slogan. If we bear in mind that the end products of taxonomical arrangements, irrespective whether carried out by classification or by ordination, are never independent of the choice of methods, the points of view and the subjective quality of the person making the arrangement, we should be cautioned to be modest and pragmatic. The end products of these arrangements are not assessed in light of some kind of predetermined consensus, or rigid a priori aspects, but rather whether they fulfil the given practical goals. Hence taxonomy is a "pre-systematics" branch of science. If someone publishes his taxonomic results as some kind of replacement for syste-

matics, he might easily become ridiculous: this is not contradicted if a subsequent systematic analysis of a taxonomical arrangement shows that it is in good agreement with the phylogenetic classification of the group in question. However, in order to have the various arrangements and their authors to peacefully coexist, the rules of naming animals must be updated (see below).

Arguments and counter-arguments

We wish to emphasize that it is not the arguments in general that we are against, but we definitely object to futile arguments which do not even carry the possibility for agreement in them.

The argument on systematic problems among taxonomists can be traced to three different sources:

1. One kind of source can be the advice of a wise man as regards the objectives and taxonomical methods of a systematist. Such advice usually does not take into consideration the exploited state of our field of science and there is little hope that an outsider can extend realistic advices without being an active participant.

2. Excellent opportunities arise for provoking prolonged debates if a taxonomist propagates semi-digested systematic principles and embarks upon a crusade to carry his point to the end.

3. A few young graduates with brilliant ideas consider taxonomy as a hunting ground where aged, ignorant, to the new imperceptive taxonomist rabbits may be hunted. We do not mean to say that no advice is welcome from someone who has spent less time at the microscope than the addressee of the advice, but we find that advices are not readily taken from someone who has never looked through a microscope or who has just started doing so. If, in addition the propositions of our young colleagues are aggressive, arrogant and cynical, even those taxonomists will be confronted who otherwise would have been willing to take heed of the advice and consider their arguments.

We find an increasing number of polemic papers in the recent taxonomic literature. This may be a consequence of the fermenting debates currently pregnant in systematics. Those are not only the debates among the leaders of the outstanding evolutionary systematics schools (from which we expect the renewal of systematics), but also the debates over the "systems" devised by playful categorizing. Taxonomists working with other groups had for decades found cause for ridicule in the fact that any ornithologist worthy of his penny created one or two "systems". The increasing number of acarid and dipteran systems are also cause for concern (after HENDEL, HENNIG, STEYSKAL, RÖHNDORF, OLDROYD, ANDERSSON, GRIFFITHS, ACZÉL and others have erected a "system", some of them even more than one). The increasing number of taxa used is simply shocking: a single dipteran species may be included in the following higher taxa: order, suborder, superphalanx, prephalanx, phalanx, subphalanx, superfamily, prefamily, family-group, family, subfamily, tribe, subtribe, genus, subgenus. We have no intention of questioning the validity of monophyletic groups (e.g. Oppiidae, Sphaeroceridae, Aves, etc.) (see SIMPSON, 1953, 1959, etc.; RENSCH, 1960; SCHAEFER, 1976). We are concerned, however, about the fact that a considerable portion of the time and energy of systematists

is devoted to debates over "systems" and categories. The phenomenon that a systematist running out of argument will fall on his foe with the heat and hate of a religious warrior, may very well be observed in taxonomical debates. We think that such a consequence of the close cooperation between the two fields of science is not necessary.*

As a consequence of the above-mentioned we consider that debates on several, perhaps many concurrent arrangements of an animal group are not cause for a necessary debate. However, since the primary objective of taxonomy is to acquire better knowledge of the species, this aspect should be borne in mind when devising arrangements.

For us a species' name is that wonderful key which opens a whole mine of knowledge for mankind, in possession of which we can collate information so far gathered by science with the name. The biological distinctness of the majority of the species is so evident that even with the primitive tools of museum taxonomy (microscope, literature and some brain) 99–99.9% of the species can be identified. Since only a fraction of the distinctness of the various biological species is manifested by phenological characters which can be analysed by us, we should not claim any responsibility for the "remaining" unidentified part. Biochemical, genetic etc. methods may very well reveal many differential characters. The so-called "uncertain" cases are but dichotomies, stages in the continuous process of speciation. A species name is a code which becomes functional by setting up a reference set of morphological, bionomical (perhaps biochemical, etc.) characters, in possession of which the species coded by that specific name may be distinguished from its nearest relative. Ever since LINNAEUS it has been customary to use hierarchical categories to locate a given species: each taxon is defined by that set of characters held in common which at the same time distinguishes the taxon from the others. Hence the names of the taxa are character categories, and the higher in the hierarchical scale the taxon the fewer the characters held in common. We consider this archaic rather primitive method as the most suitable for devising useful arrangements, because this way any taxon name may automatically be included in identification keys.

We wish to re-emphasize that the above does not mean that we do not consider other arrangements useful and outstanding intellectual achievements. That we consider this method as the one to be used is due not to our modesty but to the following:

1. There are very few zoologists studying the fauna of the world and there are many "white patches" and insufficiently known groups of animals;
2. The fauna of the world is diminishing at a rapid pace, especially in the Tropics where the native vegetation is being devastated.
3. Medicine, agriculture and environmental protection increasingly demand that quick identification be carried out on species posing problems for those

* We will dispense with the contrary mistake, i.e. several taxonomists, saying that they are zoosystematists, peddle with systematic problems which they are not familiar with. Often enough these attempts result in mediocre papers on "systematics", in lengthy discussions operating with the vocabulary of evolutionary systematics but in reality saying nothing more than more or less typological statements. These mental miscarriages often question the taxonomic abilities of their author.

fields. In other words, the taxonomist of these days is not engaged in descriptions of hitherto unknown species simply because his abilities do not render him suitable for anything else, nor without the wise advices of systematists we would fall back to the slump of 18th century science. On the contrary we should realize that there is nothing more urgent than to carry out the work that has been left undone by the scientists of the 18th to the 20th centuries! We are very well aware of the anachronistic nature of our daily work. Our methods have been modernized which only means that progress is made at a faster pace. The responsibility is, however, ours and cannot be shared with anyone else. There is no doubt that the efficacy of the control of flies pestering free-grazing cattle, and hence milk production, is higher when measures are planned in light of fly taxonomical (bionomical, autecological) data. At the same time it is evident that milk production is not affected by the usage or omission of the superphalanx category. "The test of the pudding is eating" — that of taxonomy is whether its results are used by anyone, and in what way.

The essence of taxonomic work is to revise the taxonomy of various groups. There are three possible ways to do this.

1. The senior author of this paper (BALOGH, 1983) and WOAS (1981) collected all descriptions and illustrations of his study group previously published and selected a set of characters found in all or almost all descriptions, and on the basis of these defined the categories. In this method the concept of the "weakest link" is used, but the chain will be complete as all or almost all links will be joined. There is one undebatable merit of this procedure: extensive completeness.

2. The other way is to prepare original descriptions and figures of all hitherto described taxa and to erect new categories based on the characters obtained in this way. In this case, and in only this case is the taxonomist in a position to reject previously defined concepts of arrangements and taxa. Besides RAVEN, 1983; etc., one of the best examples for this kind of procedure is the revision most of the taxa of the dipteran subfamily Limosininae of Sphaeroceridae by ROHÁČEK (1982–83). In this case the concept of the "strong link" is used but the chain will be complete as all or almost all possible are included. It is only by using this procedure that we may reject an arrangement obtained by 1.

3. SENICZAK (1975) and WOAS (1986) used a third kind of procedure. Based on their own studies they prepared new modern descriptions of some species and classified most of the descriptions in the literature as useless. In this case the concept of "few strong links" is used: the taxonomist forces himself to reject everything done by his predecessors. The accruing chaos is even worse than that caused by using procedure 1.

Acceptance of this principle contradicts the driving forces behind the development of taxonomy. In our opinion the strategy of this branch of science necessitates that guerilla warfare methods are not to be used. The trouble with these publications lies not in the lengthy introductions in which the authors describe in what way they are guerillas. There is a place for even the 73rd theory of the extinction of Mesozoic reptiles and it is possible to find a forum for publishing papers of these guerilla antics as well. The trouble is that the authors, irrespective whether they are aware of it or not, subject their way of thinking to a single hypothetical point of view and fail to scrutinize their argument from other points of view.

There may be pragmatic circumstances besides the above which may have influence on the principles of arrangements. Perhaps the most important one is the current regulation of nomenclature which strictly stipulates that binomial nomenclature be used. This fact forces the taxonomist to use the generic category. If this is so, for us the genus is simply the lowest-named taxonomic category. A taxonomist engaged in the study of a group is not at liberty to use the genus or the subgenus as the lowest category, or perhaps the species group, as the history and the traditions of the taxonomy of the group are compelling. The problem is not simply that the "systems" devised by worthy predecessors have become "objects" published in papers and handbooks. For instance in the family Tachinidae there are traditionally many genera of a few species and in the Tipulidae there are a few genera each with numerous species. The genus *Drosophila* is of gigantic size simply because elevating the various subgenera to generic level would result in *Drosophila melanogaster* being no longer a *Drosophila* species, and no one is willing to propose such a splitting.

We substantiate our reasoning for using suprageneric taxa in light of the above with argument borrowed from taxonomy: we have no way of completely and accurately measuring the genetic differences between the species. The taxa of the various levels of the hierarchical arrangement are not all of the same age, they are not equivalent since the tempo of evolution is not the same in various groups and the life-span of the stem-species cannot be used as criteria. There are acarid species known from the Devonian (ca. 400 m.yrs B.P.) included in recent familial taxa (SHEARS, 1984) whereas the stem-species of birds are a few 10 million years old. (The problems of this issue are best and in most details discussed by W. HENNIG; we are not alone in not accepting his propositions).

A prerequisite for a true classification of a group is to know the phylogenetic lineages, ontogeny, etc. of the group in question. However, for instance the phylogeny of the oppiid mites is completely unknown and the ontogeny of only a fraction of the known species has been studied. For many large groups of dipterans only single incomplete pieces of information are available. Since neither of us has found pleasure in the brain-training game of erecting categories, we cannot boast with any "phylogenetic systems".

It is not from birth, and not by chance that we have become suspicious of the so-called "phylogenetic" methods of analysis. The palaeontological data are nothing but blur still-pictures of the phylogeny of living matter, so we believe palaeontological studies should not be considered phylogenetic inquiries at the same time, only evidence is provided for the evolutionary process. There is no reason to consider analyses of incompletely known morphological characters as phylogenetic work. The dichotomies of the past are "deliveries" where neither the mother nor the child can be distinguished: the evolution of living matter continues in both equivalent branches of the dichotomy. This is the point which questions the validity of purely cladistic analysis: the lineage conserving the ancestral single selected character may change in respect of other characters at a faster rate than the other lineage, in which a character different from the ancestral one has evolved. New "plesiomorphic" characters may take the place of apomorphic characters: equivalent configurations of setae, leg modifications and wing venations among dipterans are certainly determined by the same gene complex in various Acalypratae families. It is more likely that these genes have become even less manifested rather than independently evolving in various

groups. If this used to be the case in the past, we may fool ourselves by analysing the "Schwestergruppe". The isolated palaeontological finds provide no clues: we do not know whether the fossil in question was the spark-off of a lineage or it represents an "apomorphic" lineage leading to extinction, and whether there are any hidden characters in that species. The argument concerning "monophyletic or paraphyletic" evolution is futile in lack of complete or more or less so palaeontological series.

Let us briefly mention the two major fields of taxonomical "debates".

There are several proposals to modify the currently used rules of nomenclature. PAPP (1983), simply considering practical, purely taxonomical arguments, proposed that the rules pertaining to descriptions of species and those regulating the validity of species names be made more stringent in order to increase the chance of stability in face of priority. GRIFFITHS (1976, etc.) approached the problem from an entirely different point of view and proposed among others that the rules pertaining to the categories and their nomenclature be relaxed (e.g. the regulations specifying the suffixes of various categories should be abolished). GRIFFITHS proved that a classification (in the above sense, i.e. part of systematics) encompassing extinct and extant species poses unresolvable problems for traditional Linnaean nomenclature. It is not for the sake of provoking discussion that we quote from him: "The Linnaean order of categories is a branch of Aristotelian essentialism". "There is no logical necessity to classify monophyletic groups (taxa), i.e. to place them in families or categories". Since the "ancestral species" (stem-species) are not part of the hierarchy only the terminal (i.e. recent) species are, "the traditional form of Linnaean hierarchy must be modified in order to reflect the phylogenetic relationship among the species . . .". By introducing the concept that taxa are non-categorized hierarchies (GRIFFITHS, 1976 : 168 : "the unclassified hierarchy of taxa"), the problems of both systematic classification and taxonomic arrangement can be resolved. The most important outcome of this solution would be that many future arguments would never take place, e.g. the many worthy and smart colleagues engaged in analysing recent species by ordination methods would not be forced to painfully squeeze their results into classification categories. Until such time as these changes take place, we recommend to our colleagues who produce taxonomical arrangements lacking phylogenetic evidence (using PAPP's [1986] disrespectful expression, nothing-better-to-do systematists), that they turn to supraspecific categories only when absolutely necessary.

A typical field of futile and paper-wasting debates is to argue over the writings of long deceased ancestors.* Undoubtedly it is very time-consuming and laborious to loan, study and redescribe all type specimens previously not known or poorly known by us, we personally know how difficult it is, yet this is the only possible solution. If a type series becomes lost or destroyed, neotypes must be designated and described, or using a new specific name a decent description must be produced. It is not our duty to analyse the personal abilities of our predecessors. Their activities have come to an end upon their death and their papers, books and type specimens are all objectively of the same value to us.

* In order not to offend anyone or provoke further debates among the authors widely known in our fields of speciality, let us just mention O. DUDA (1935, 1938, etc.) who spared neither paper nor his readers' patience in speculating on what he thought his predecessors had thought in their publications.

We are confident that taxonomists should aim at acquiring better knowledge of all aspects of the various animal species (populations). Attempts should be made to provide better facilities for carrying out besides morphological studies genetic, physiological, biochemical and so-called autecological analyses. Therefore we think at the present stage of zootaxonomy it would most appropriate to discuss how to increase the standard of species descriptions, how to expand the forms and methods of the reference basis. In certain fields, e.g. in dipterology there is still redundancy in the communication of information: there are no guidelines for brief, to-the-point and at least partly coded descriptions. Acarologists have from the beginning almost obligatorily figured their new species, thus condensing a vast amount of information into small space, and elaborated a code system for the location and names of the various setae. A rationalization on information provision will become necessary for many animal groups because of the above mentioned problems and difficulties in publishing.

The other problem is also methodological. DE SOLLA PRICE (1967) long ago noted as an outsider that in a few "specially archival field, e.g. "zoo- and plant taxonomy", the proportion of citations of recent publications may be as low as 15%. Unlike other branches of sciences, in zoology there is no such thing as a frontline of research being about 40 publications in depth and all the second-line papers precipitating in reviews or even textbooks. It is hardly questionable that a frontline of results achieved with regular (not guerilla) tactics should also be established in taxonomy. To meet this end reviews, catalogues, monographs and official species lists of complete groups of animals need to be published. There is a need afford the same rank to revisions and monographs (proposed by HOWDEN, EVANS and WILSON (1968) and others) such as the one enjoyed by the "Systema Naturae", which means that there is also a need to change the rules of nomenclature. Today a taxonomist working on a larger group is flooded by a mass of information of thousands of publications.

The latter two issues are such that truly require a consensus of taxonomists in general. We would welcome taxonomists focusing future discussions on such problems.

In lieu of summary we wish to extend the following advice on how to avoid unprofitable debate on systematics among zootaxonomists. The wording of these advice has been affected by our fear that we may appear arrogant and conceited.

1. Respect biosystematics! Consider it what it is: the branch of biological sciences working on and synthesizing the greatest amount of factual knowledge. Do not belittle yourself but your attitude be controlled by a fear that successfully working in this field is the privilege of outstanding geniuses such as E. MAYR or G. G. SIMPSON.

2. Admit that there is no place in zoosystematics for rushing ahead: it is part of the essence of this science that significant progress may only be made if all or at least the majority of the species of the group in question are known to an "average" extent.

3. If you aspire for a Nobel price, it is time for you to quickly look for another profession. What this branch of science needs is diligent artisans and not brilliant brain-storming geniuses. The meticulous care for details and the many years spent looking through your microscope will earn you the respect for your taxonomical work.

4. It is impossible and quite unnecessary to convince Brontosaurus that their time is up: wait till they become extinct and their views will perish with them. Till then it should make you happy that the real data of the animal species collected by you should withstand the ravages of time.

5. If there is a contemporary taxonomist whose views you do not agree with, first send him or her your reprints, then through correspondence or personal discussion try to convince him or her of your opinion. If your partner should prove as unbending as Soós and PAPP (1984) were in their ideas on how to edit the palaeartic dipteran catalogues, compose your answer in a strictly objective scientific publication.

6. There might have been some slow-minded, one-eyed and even alcoholic persons among the founding doyens of zootaxonomy, but they are the partners for debates only for idiots and occultists. You do not necessarily have to respect your predecessors, but always consider their works with patience, in light of the standards of the era long past.

7. Save your wit for entertaining your guests at your cocktail party! Clever remarks, mocking (arrogance, aggressiveness) will not get you closer to your goal, as the most important criterion of a given scientific achievement is its present and future usefulness.

8. If you cannot respect your contemporary partner's opinion, be considerate and generous! Take pains not to show that what he is saying is nonsense.

9. Try as much as possible to avoid debates! Punish your colleagues producing „new systems” by ignoring their papers. Instead of debates focus your attention on describing your new species with meticulous care, employing methods in accordance with the technical standards of our age and try to meet the demands of the practical users of your work as much as possible.

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