ades of systematics workers. Gary Steck met Dr. Plaumann last year, and this stimulated Dr. Norrbom to write him, relative to Diptera business. There is now a special museum built in Nova Teutonia (Brazil) to house the Plaumann Collection, and in the close of a recent letter, Dr. Plaumann states "I am near the 89 years of age; the last year 04 September [1990] the President of Germany Mr. von Weizsacker, conferet a order to assist in for me the highest distinction the "Grand-Cros of Meritum", because of my scientific work in entomology."

**Roy Snelling** (Dept. of Entomology, Natural History Museum of Los Angeles Co., 900 Exposition Blvd., Los Angeles CA 90007) reports: "Contrary to some rumors, I did not remain in a demised state when my heart ceased to function on 2 June 1990. Although CPR failed to revive me, a persistent doctor with a set of electric paddles got me jump-started on the magical third try. Maybe she should have left well enough alone - anybody dumb enough to come back to this world has likely suffered oxygen-deprived brain damage. My so-called "near death experience" was a crashing bore. There was a brief period of extreme vertigo, then nothing. Nada. Zilch. No tunnel, no lights, no singing. I think all those jokers who claim such things are having us on. One moment I was awake with one doctor on either side, in lotsa pain, a long "blink", and when I open my eyes I'm surrounded by doctors and gadgets up the u-no-wat. Amazing.

"Well, a bunch of discomfort, a little zipper-job on my thorax and I'm better than before, so they tell me. We'll see how long this triple by-pass works. Six weeks after surgery, I was back on the beach playing volley ball. Enough of that."

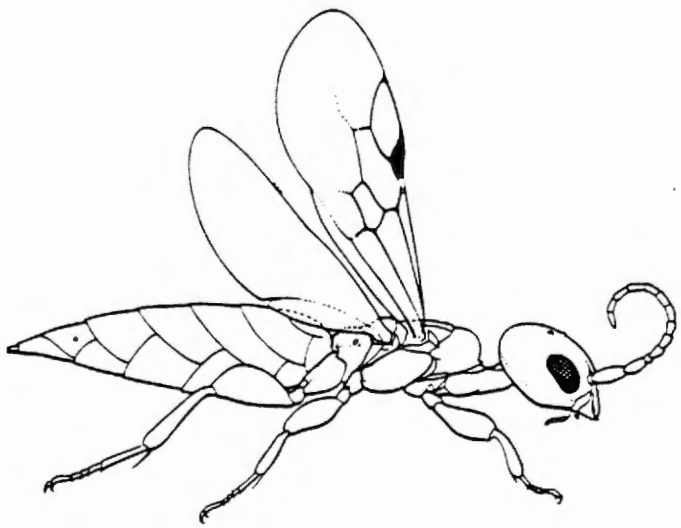


## SPHECOS ERRATA

In Alex Rasnitsyn's note "Paleoentomology in Moscow" (**Sphecos** 21:7), the president of Kuperwood Enterprises should read Eric **Kuperman**.

Also, the page numbers for Carpenter & Rasnitsyn 1990, Mesozoic Vespidae, Psyche 97, should read 1-20 (**Sphecos** 21:21).

In **Sphecos** 20:39, the title of a paper by Jean Leclercq (1989) should read: En vue d'ue monographie du genre *Lindenius* Lepeletier & Brullé (Hymenoptera Sphecidae Crabroninae). Bull. Soc. Roy. Sci. Liège 58:419-444.



*Ycalpoca evansi* Nagy (Scolebythidae), a parasite of Cerambydicae in southern Africa and Australia.

## FORUM

### Typified Names of Higher Taxa Once Again

*by*

**Alex Rasnitsyn**
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I agree with Peter F. Yeo (**Sphecos** 20:5) that the typified names of higher taxa are troublesome to introduce in routine usage and that they are "fairly difficult for students of taxonomy but it must be a good deal worse for non-taxonomists and laymen, who will probably fail to grasp the system". I am afraid, however, that centuries ago it was even more difficult to teach students and laymen that the earth is not flat and resting on whales' backs, but is instead spherical and hanging in empty space without any support. And they had to learn that the immediate advantages of a simple concept ultimately turn into disadvantages when compared to the more correct idea, even if it is harder to grasp.

I am very certain that it is necessary to typify names of all taxonomic categories because the problem is not just a nomenclatural one. Contrary to the claim that "The Code refrains from infringing upon taxonomic judgement, which must not be subject to regulation or restraint" (ICZN: xiii), principles are compatible only with particular taxonomic concepts, and the type principle is among them. As a result, if we accept the principles we should abandon some particular versions of taxonomy. Similarly, when following one of the respective taxonomic concepts, we have to abandon the type principle. I have examined the subject in wider context in a manuscript available from the Editor.

The principle of type means that "The name-bearing type provides the objective standard of reference by which the application of the name it bears is determined, no matter how the boundries of the taxon may change" (ICZN #61a). In practice we apply taxonomic names to specimens identified through the use of the taxon diagnosis (a set of characters delimiting the taxon) rather than by direct comparason with the type. There is no contradiction, however, as a diagnosis must always be constructed and, if necessary, modified to cover the characters of the type, and in case of uncer-

tainty the comparison to the type is *ultima ratio* is resolving problems in applying the name.

The type is merely a site to which the name is pinned like a tag. The establishment and use of a type is a rather specific procedure which should have a distinct area of correct application. I argue that the typified taxon name is most compatible with the taxon-continuum as defined by Ponomarenko & Rasnitsyn (1971): a continuus chain (or net) of subtaxa where neighboring subtaxa are more similar to each other than to any member of another continuum. A continuum is like a cloud which can change its volume (its array of component subtaxa) and characters (the place and configuration of the taxon in the space of characters); it can be born, split and dissolve (become extince). The long history of taxonomy has shown that the typified name is the best solution to the problem of how to give name to such a cloud in the most stable and unambiguous way.

A continuum is a constituent taxon of the phenetic and, when modified into a monophyletic continuum, of the traditional ("evolutionary") system. Monophyly is taken here in its primary, wider sense, covering both monophyly (holophyly) and paraphyly of cladistic usage. It is used to phylogenetically test the phenetically constructed continuum, the procedure being described in detail in the mentioned manuscript.

Non-typified names have other meanings and areas of application. The simplest case is just a name of an idea, such as morphological terms, and it shares all the respective advantages and disadvantages. Such a name must not be misguiding: it should be accurately descriptive or otherwise meaningful in one way or another, a rule hardly observed in the existent names of many higher taxa. What is more important, it does not seem possible to establish any hard rules regulating the creation and application of such names: you can remember the case of abdomen/gaster/metasome, and this is quite a typical situation. The current usage of names of higher categories is similar to the above case, and many of use are aware of nomenclatural confusion existing above the superfamily level. There is an ever worsening effect of using non-typified names which I will go into later.

Non-typified names are also compatible with the concept of the taxon-individual, proposed and defended by Ghiselin (1974-1987). The individual can be identified by direct indication of its name (this is John, that is Mary, or: this is *Homo sapiens*, and that is *Pan satyrus*). The individual can be born and can die, but cannot be divided into parts each able to pretend to inherit the name. Also, an individual can have parts and not have members (specimens, examples). Initially, it was the species which has been claimed to be individual, because the gene flow maintains its integrity. This explanation cannot be unquestionably applied to the species, much less to higher categories. To cover all taxa, another approach has been developed. It has been proposed that the holophyletic taxon (=monophyletic in the cladistical usage, i.e. a taxon which includes an ancestor together with all its descendants) is an integral entity due to lines of descent connecting its constituent subtaxa. Ia am afraid that the proposal is not correct, however, because any section of the line of descent is equivalent as a joining factor, and no one of them could be preferred to another to be broken while delimiting taxa.

Indeed, follow me for a moment in that the ancestral species of the sawfly family Xyelidae has been a direct descendent of a species belonging or closely related to *Permonka* (the only Mesozoic genus of the otherwise Paleozoic order Palaeomanteida). Further, let you believe that another xyelid species has given rise to the first species of *Undatoma* or another but closely related stem genus of Xyelotomidae, the oldest and probably ancestral family of the tenthredinoid sawflies. I claim that not a single section of the descent line connecting *Permonka* and *Undatoma* is cladistically more weak than another: the *Permonka* species ancestral to Xyelidae has been equally related to both the first xyelid species and its own ancestral species within *Permonka*. Similarly, the first *Undatoma* or another ancestral xyelotomid species) has been equally closely related to its xyelid ancestor and to its xyelotomid descendant. As a result, until we abandon the approach we have no legitimate reason to break any particular line of descent to delimit a taxon, unless we break all of them. Therefore only the living world as a whole can be considered the taxon-individual (providing that life is monophyletic).

The next area where the not typified taxon name can be legitimately applied is the case of the system with taxa defined by their characters (providing the latter do define and not merely diagnose taxon). However, "the character does not constitute genus, but the genus the character" (Linne, 1751, #169), and this is quite true concerning the higher taxa, whose diagnoses are especially difficult to compile in a way excluding exceptions.

It is also possible to introduce cladistic taxa into the system without referring to the type. Their place at the cladogram could be pointed our instead. You could claim, for instance, that Apidae *s.l.* is a taxon forming a sister group of Larridae *sensu* Lomholt and together with the latter being a sister taxon of Sphecidae *sensu* Lomholdt. Though logic of the approach directly implies this way of constructing the cladistic system, cladists do not reject the type principle, despite it being profoundly connected to the similarity based notion of continuum. This is probably because the cladogram is generally evaluated as not reliable enough to serve as the only base in constructing a system and applying taxonomic names.

You can agree or disagree to the the above considerations. You will probably agree, however, that in the above respects, as well as in any other, higher taxa do not differ from the lower ones in posessing features which make them more fit to apply non-typified names. In other words, the higher taxa have no reason to be treated nomenclaturally in any other way than the lower ones, except the necessity to learn some volume of novelities. However tempting might seem the latter reason for the moment, the price of saving the learning time and efforts will be high in the long run. While saving a little in this way, we risk losing our feeling of higher taxa as full and equal members of the system, subjects deserving the same consideration that lower taxa enjoy. Running this way long enough, we are at risk of disregarding higher level taxonomy and phylogeny, or at least of uncoupling them from lower taxa. In my opinion the price will be too high to persist in refusing to extend the Code provisions to all taxa.

Comments by Steyskal (**Sphecos** 20:5) are partly easier to accept. I do follow Rohdendorf's proposal concerning the endings of the names of higher

taxa, though I don't consider myself their ardent defender, much less their defender against Dr. Steyskal. I can only remind that the ordinal ending **-lda** has been proposed following paleontological practice, for most order names in the groups abundant in the fossil record have this ending; the same seems true concerning the subordinal ending **-lna**. I consider these endings as plural for **-ldum** and **-lna**, respectively, but I have no special reason for this. Generally speaking, I am ready to consider and accept any better substantiated system of endings, **when it will be proposed and accepted by other students.**

Refences

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**Nomenclature for the XXI Century?**

*by*

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The current International Code of Zoological Nomenclature is not the peak of perfection and does deserve further improvement. However, the way and scale of changes reported by Savage (1991), as a supposed proposal by the International Comission of Zoological Nomenclature for the coming meeting of the International Union of Biological Sciences, fill me with misgivings.

The slogan of the proposal is "to free systematic biology from the tyranny of the past" (**Sphecos** 21:17). This sounds too close to what bolsheviks have been practising in my country since 1917 and which resulted in the return of the country back to the Middle Ages. The slogan seems, however, to be only a manifestation of the more deep and popular drive to free science of elements of the art in order to reach more objectivity. These elements, including the importance of rich personal experience and deep knowledge beyond the particular area of study as the basis of intuition, are destined to be replaced by sophisticated rules and scientific programs which would permit reaching the reproducible (= objective) result irrespectively of differences in personal background. I doubt whether the goal is attainable in full. More important, however, is that our slow success in replacing obscure personal creative abilities by explicit rules of research makes such replacement premature as a proximate goal. An attempt to force the process would create unwarranted faith in too imperfect available rules and programs, and would lead to a disregard of the traditional approach. The result will be temporary growth of knowledge acquired accompanied by deterioration of the scientific level, with the balance being highly negative in the long run.

The above considerations are personal and subjective. To balance them, I would further concern myself with more specific problems of the nomenclature.

The proposed regular edition of the Official List of Names in Current Use (LNCU) as an updated Systema Naturae for the aims of nomenclature (that is, the one providing the basic array of available names of taxa and invalidating all previously proposed names) does not free taxonomists from the task of chosing between available names. Some LNCU names will be found to be synonymous while among the post-LNCU ones (and between them and LNCU ones) there will be both synonyms and homonyms. The principle of priority will lose much of its domain, for all LNCU-names would have the same date, while the inter-LNCU time interval is supposed to be short enough to leave limited space for the application of the principle. This puts the question of replacing rule, the answer on which being not plain. The principle of priority is congenial to the human mentality because it has its roots in the general respect of elders, and yet I remember when scientists neglected the princple. Still longer can be the way toward the common respect to another principle. If the proposal is accepted, I think it will be difficult to avoid a long chaotic transition period.

No less disturbing is the proposal to limit issuance of taxonomically available names by a selected set of journals, book publishers and languages (further referred to as the Code of publishers and Code languages). This proposal has nothing to do with the misgivings described in the second paragraph, however. Moreover, being successful, this step can improve the state of taxonomy very much. The problem is that success is not easy to achieve, for the proper preparation will take much time, money and effort, while the rush acceptance of the proposal as a law of nomenclature would be disastrous. I mean at least with conditions in the USSR, and possibly in some other countries with good traditions in taxonomy, there is little chance to follow the proposed limitation, unless a reasonably long transitional period will be established accompanied by a special program of assistance.

The above apprehension is born by appreciation of the following circumstances. Taxonomists are rather numerous in the USSR, and they provide a considerable amount of taxonomic research. At least part of this work is considered important in the country and abroad. Most of the taxonomists have a rather small knowledge of foreign languages, very few of them have ever published anything abroad, and practically nobody is in the position to meet page charges of western journals. That is why, when confronted with the new rules, it would not be possible for them to obey. Neither will they stop their activity, but being ignored by their Code-adherent colleagues, they would have to work for their immediate community. The result could be a schism in taxonomy and the origin of something awful, similar to the Soviet Creative Darwinism of the 1950s.

To escape this unfortunate perspective, I would suggest several ways to help those experiencing difficulties in accommodating themselves to the new rules of nomenclature. The easiest seems to be to help the better publishers of a nation to reach the level of the Code standards. For this they need to be organized for publication of taxonomic descriptions in the Code languages, including help in the translation and editing of contributions, and probably also in obtaining machines, materials and technology. Also it should be suggested to the existing (foreign)



DECEMBER 1991

# SPHECOS 22

A FORUM FOR ACULEATE WASP RESEARCHERS

## THOUGHTS FROM THE MUD D'AUB

This issue is follows the last one later than I would like, but not that much material has come in, and both Terry and I have been pretty busy with other things. I hope that the lack of much input from the readership is just a quirk of 1991, and not an indication that interest in **Sphecos** is waining!

1991 has been a remarkable year in world history, and the changes in Europe are ongoing as we go to press. We have attempted to update our mailing addresses to reflect the changes in the various former Soviet Republics.

The **Journal of Hymenoptera Research** is a reality and issue one will be packed with interesting papers. Those of you who are not members of the society should join. You will miss out on a fine journal if you don't. Full details on how to join will be found in the announcement on pages 19-21 of **Sphecos**.



## RESEARCH NEWS/HELP NEEDED

**Sérvio Túlio Pires Amarante** (Museu De Zoologia Da Universidade De São Paulo, Cauxa Postal 7172, 01064 - São Paulo, Brazil) writes: "I have recently finished my M.S. work and I am beginning to work on my Ph.D thesis project. I decided to revise *Penepodium* (and *Dynatus*), so I can make a study that may solve many problems, involving the taxonomy, biogeography and phylogenetics of all (?) species in a period of no more than four years. I am intending to work here for some months, to become acquainted with the species characters and the generic problems, and then I would like to go to the Smithsonian Institution for three or four months.

"I have just identified a new species of *Pisoxylon* and I am intending to describe it. This species is strikingly different from *P. xanthosoma* and shows some characters that may lead to a redifinition of the genus."

Sérvio would appreciate any information or loan of material in the genera *Penepodium* or *Dynatus*.

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**Øistein Berg** (Bastadveien 73, N-1370 Asker, Norway) writes: "I am only an amateur who has collected Aculeates for only three years. I have not yet specialized in any particular field, but I do have a weakness for the Eumenidae (and the bee-family Megachilidae). I am mostly interested in the faunistics of the Norwegian species. However, I also collect when travelling abroad. Last winter I collected 230 aculeates (including 34 Sphecidae) in South-East Asia (chiefly Malaysia). My main problem is to have identified what I collect, both here and abroad. Would anyone be willing to identify specimens for me?

**Eduardas Budrys** (Entomology Laboratory, Inst. Ecology, Akademijos 2, Vilnius 232600, Lithuania) says: "Last year (1990) studies on systematics (as well as many other areas of fundamental science) were not financed enough because of the hard economic situation in Lithuania. As a result, I had to interrupt my taxonomic work on palearctic Pemphredoninae and to do work very different from my own (e.g. ecological analysis of agricultural territories being improved). Fortunately, this year the situation is going to be normal; the governmental financial support of fundamental sciences is resumed. Consequently, I am continuing my revision of palearctic *Diodontus*.

**Jim Carpenter** (Museum Of Comparative Zoology, The Agassiz Museum, Harvard University, Cambridge, Massachusetts 02138) writes: "When I move to the American Museum (of Natural History, New York, in early 1992), I'll have the services of Ward Wheeler's molecular lab available. They report success in amplifying DNA sequences from samples of the Epiponini collected into ethanol during my last foray into Amazonia. Success thus seeming assured, we'll write a collaborative grant to fund a technician when I get to New York. I have specimens of all but three polistine genera in ethanol, and the technician will work up a DNA data set to add to those being assembled by Wenzel, Kojima and I. Which brings me to the request. We'll be looking for further col-