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.**

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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.

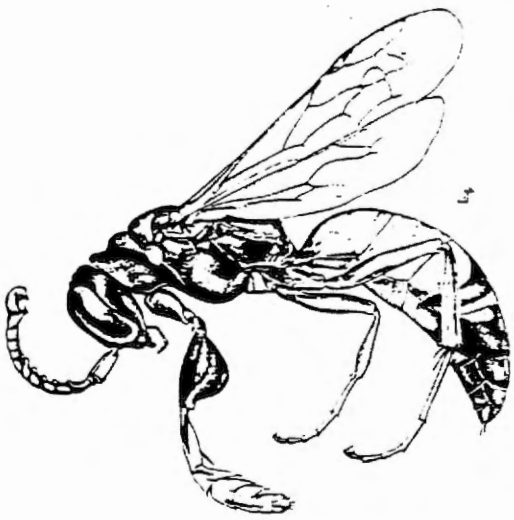
The above apprehension is born by appreciation of the following circumstances. Taxonomists are rather numerous in the USSR, and they provide a considerable amount of taxonomic research. At least part of this work is considered important in the country and abroad. Most of the taxonomists have a rather 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)

Code publishers to provide exemptions from the page cost as well as help in translations (or at least in linguistic control of the draft translation submitted) for the taxonomists concerned. Of most help for them would be the availability of funds for covering the publication expenses. And last but not least would be continuing attention to the affected students in order to inspire them to contribute to the Code publishers, e.g., invitations to contribute.

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Didineis nodosa Fox (Sphecidae: Nyssoninae)

Book Reviews, Classifications and Dogma*

by
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In **Sphecos** 20:29, Pagliano and Scaramozzino are said to be guilty of not accepting Brothers (1975) and Gauld's & Bolton's (1988) proposal of only 3 superfamilies of Aculeata. And Menke says that "Catalogs, and similar works... are not the proper vehicles for making changes in classification since such actions need to be supported by evidence...".

Well, these authors in their very useful work made no change in classification! They simply continued using the usual and very convenient classification most European if not also other hymenopterists prefer, that of the Royal entomological Society **Handbooks** (following O.W. Richards) which is also that of the **Catalog of Hymenoptera North of Mexico** (views of K.V. Krombein still hold in the 1979 edition, with Krombein's comments in vol. 2: 1573).

I believe Pagliano and Scaramozzino were right because Brothers, Lomholdt and others have merely presented working hypotheses which, in spite of their merits, should not imply immediate changes in the usual classification and nomenclature. There are many notes in **Sphecos** (including no. 20) which indicate that Brothers views are still and will be challenged. And the book of Gauld & Bolton has yet to be accepted by all of us as the dogmatic Bible for Aculeata. Moreover, I find nowhere irrefutable evidence that Börner (1919) and Lanham (1960,1988) were entirely wrong. We still know no piece of a fossil which could be presented as one of those primitive wasps on the way to becoming a bee, but we are sure that sphecid wasps and bees have gone their own way perfectly separated since more than 100 million years. Thus I do not feel at all motivated to place our good old Sphecidae in "Apoidea" as if they were bees or bees to be.

Classification and nomenclature, pragmatic as they must be, have not the purpose nor even the possibility to reflect correctly hypothetical phylogenies, not even the most elaborate cladistic ones. I do not mind if that attitude seems to be "hors-jeu"; I am quite at ease to note that my views agree on all points with those of one of the most perspicacious zoologist of this century, Ernst Mayr, who wrote to end his still pertinent "Cladistic analysis of cladistic classification" (1974; reprinted 1976): "In conclusion, it is evident that, no matter how useful cladistic analysis is, it cannot be automatically translated into a classification".

* title suggested by Menke

SCIENTIFIC NOTES

An Interesting Interaction Between *Steniolia obliqua* and a Large Bombyliidae

by
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While collecting in eastern Nevad this summer, my wife Carla and I bot observed an interesting interaction be tween *Steniolia obliqua* and a larg species of Bombyliidae, probably a *Anthrax* on a dirt road in Great Basi National Park. A small stream of wate ran across the road and hundreds o sphecids and eumenids darted over th wet earth in great numbers. Again an again we observed the *Steniolia* an the bee flies facing off at about 1.5 fee they flew about a foot or so off th ground. In every instance that I ob served, it appeared that each motion o *Steniolia* was closely mimicked by th bombyliid (or vice versa?) with both in sects continuously facing each othe They would fly this way for a few sec onds, circling back and forth, until th *Steniolia* would make a lightning fas suike at the bombyliid. Immediately th bombyliid would dart to one side o away. All of the encounters that I ob served ended up with the bombyliid apparently escaping, but I did find well paralyzed bombyliid on the groun where all this was occurring. I hav asked Bohart if he has seen similar ac tivity and he tells me that he has not. am wondering whether anyone els has seen anything like this or know any references that discuss such b havior. Evans and Gillaspy (1964) hav a discussion of *Steniolia* hunting b havior but do not describe somethi exactly like the above. The behavior the bee flies is curious because th appear to recognize *Steniolia* as threat, but do not fly off immediately.

By the way, Carla and I had a wo derful trip through Nevada, Utah, Ari na and Mexico with excellent acule collecting. Since we are also both int ested in moths (I originally intended study noctuids and she likes arcti and sphingids) we have fun collect night and day.

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 sucess 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-