

WHEN IS A NOMEN REALLY DUBIUM?

TOWARD REAL STABILITY IN CHIRONOMID TAXONOMY

THROUGH BETTER SYMBIOSIS WITH THE CLASSIC COLLECTIONS

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„One essential cause of the generally deplored synonymy confusion in all branches of entomology lies in the improper or often only very superficial use of the older sources in which earlier discoveries have been laid down.

This malady is not remedied if for any species quotations are given from ... any ... grandmasters of our discipline without first having verified exactly, whether these quotes are really correctly placed there. On the contrary, quotations that are merely copied from other works only cause new and great errors.

Of course, it cannot be asked of every entomologist to undertake the tedious and time-consuming comparisons with older sources; but then one should better refrain from citing authors one did not compare, or one should follow those entomologists who have made it their special task to critically review and evaluate the older sources. ... The complete appreciation and consideration of older sources, however, is an unavoidable necessity just because the results of thorough research from any time are not lost to posterity, and therefore ought to be preserved conscientiously so that one can continue to build on them. Yet another reason is the respect for the work of deserving earlier researchers who certainly did not record their results expecting that subsequent generations would completely ignore them any more than we would want our successors to ignore our own works.“

Before you check from whom and what time I translated the above introduction (you will find the answer at the end of this article), consider how very much its essential points still apply to chironomid taxonomy today. Whether you do or don't agree so far, I sincerely hope we may all benefit from an earnest look at the following 'high'lights.

Ever since I started out in chironomid taxonomy it has repeatedly amazed and bothered me how constantly one runs into problems of nomenclature or similar historic obstacles, which have to be overcome - often slowly and tediously - before one can achieve even small-scale goals like the reliable identification of a single, common species (e.g. SPIES 1998, SPIES 2000).

The number of 'classic' type specimens still preserved as standards for the scientific names at the roots of all our fields of study is surprisingly and fortunately large. However, the status of revision of these collections is a rather illogical, random patchwork, the combined effect of the too low number of specialists and their personal preferences and logistic constraints. As a result, some chironomid names are now being widely used although no type material or other objective basis exists for them (e.g. *Chironomus plumosus* LINNÉ), for others the current interpretation has never been compared to the preserved type material or even clearly disagrees with it (several Meigen species; personal observation). A third group contains names officially out of use as being 'dubious' although well-preserved type material has been available all along.

Apart from the more theoretical problem that such a system hardly meets the requirements of the scientific method (e.g. that results must be objectively verifiable by others, and systems consistently organized), the situation, in my opinion, has long been impeding real progress within our field and in relation to others. I personally know of several capable colleagues who ended up turning away from chironomid taxonomy in part because they felt the nomenclatorial confusion would never be overcome. We are all aware of the scores of ecological studies still omitting the Chironomidae even though their information content would be at least as high as in the groups enumerated instead. And who would bet a penny on the durability of a tree whose roots and stem are largely hollow, consisting of species concepts not stabilized by comprehensive, type-based revisions?

Scientific taxonomy in general and of the Chironomidae in particular began in Europe, and for most of the nearly 250 years since Linné this region has enjoyed the highest density of active workers. Therefore, let us take the latest catalog of Palaearctic taxa (ASHE & CRANSTON 1990) as a model to estimate how chironomid species are distributed among the categories 'recognized' by current practice versus 'dubious' or otherwise out of use. For this test I chose the subfamily Tanypodinae, because it is large but easier to count than the Chironominae and Orthocladiinae, and because I have looked into it the most in recent months with regard to the topics relevant here. My count from the catalog is that **out of 370**

species-level Palaearctic Tanypodinae 135 (a shocking 36.5 %) are considered nomina dubia or worse by ASHE & CRANSTON (1990). Even if this subfamily were not fully representative for some reason, the Chironomidae as a whole can hardly be expected to be in very much better shape.

The situation would be bad enough if the ‘dubious’ category was made up mostly of names for which it has been proven that reliable information for their interpretation no longer exists. For practical taxonomy all such permanently dubious names could be ignored and laid to rest only in catalogs. Unfortunately, just a little scratching at the surface of the available information brings to light the ugly reality that **for a large number of supposedly dubious species irrefutable type material in sufficient condition is still preserved in musea across Europe.**

In CHIRONOMUS 13 I briefly reported on how insufficiently the current use of names by J. W. Meigen is based on his collection and original illustrations, both kept in Paris (MNHN) since 1840. Much more significantly, this case is apparently paralleled by those of the two most prolific chironomid describers at least of Europe:

J. J. Kieffer and M. Goetghebuer. Again using the Tanypodinae in the Palaearctic catalog as an example, I have produced a ranking of authors by number of original species descriptions published. The result shows one undisputed champion probably for all times to come, with **46 % (170 out of 370) of all Palaearctic Tanypodinae names, and even 51 % of the ‘dubious’ ones, going back to Kieffer.** Goetghebuer is a distant second with around 10 % shares in either category and the total. The three leaders in the overall ranking (Kieffer, Goetghebuer, Meigen) together account for almost 2/3 of all Tanypodinae names registered in the Palaearctic catalog (ASHE & CRANSTON 1990).

The point here is that the Goetghebuer (IRSNB, Brussels) and Meigen collections, although they do not contain material on all their authors’ species, still have much more to offer than has been put to use so far. The same can be said for several other important workers, e.g. Zetterstedt (SÄWEDAL 1974a, b) or even Fabricius (ZIMSEN 1964). To those we should finally also add Kieffer, with some restrictions, for the following reasons (see Figure):

Figure: Excerpt from an original specimen determination list exchanged by letter between A. Thienemann (more flowing handwriting, e.g. column headings) and J. J. Kieffer (more angular handwriting, e.g. most entries in right column).

Nummer	Ortsangabe	Kategorie	Name	Dr. de Siefel 1912
1				
2	7. III 12 am Wiefelder Meer Rind		<i>Procladius barbatitarsis</i> n. sp.	
3	4. III 12 auf dem Jeminder Meer Rind		<i>Procladius nigricollis</i> n. sp. <i>Procladius schmidti</i> n. sp.	
4	7. III 12 Damm Spt. Seemünd.		<i>Trichotanytus septentrionalis</i> n. sp. <i>Dactylocladius (Luscha) sp.</i>	
5	2. III 12 am Moossee Meer		<i>Camptocladius curvatus</i> n. sp. 1 ♂ <i>Cricotopus variflorus</i> n. sp. 1 ♂	
6	4. III 12 auf dem Ickhamben Meer		<i>Chironomus (vittimontellii) (Luscha) sp.</i> <i>Chironomus (vittimontellii) (Luscha) sp.</i> <i>Tanytarsus (Luscha) sp.</i>	
7	5. III 12 Holzmaar, Rind		<i>Cricotopus limnophilus</i> n. sp.	
8	7. III 12 Damm, Quelle		<i>Camptocladius pentasema</i> n. sp.	
9	Alfbach in Moos	Wasser	<i>Gomphonema heteronema</i> n. sp. albocera n. var. <i>Trichotanytus foliatus</i> n. sp.	
10	Sillenfeld	Wasser = Meliscom-lyngbyen	<i>Brachocladius bryophilus</i> n. sp.	
11	Fauler See	am Holzmaar 5. III 12	<i>Metriocnemus longicornis</i> n. sp. <i>Metriocnemus brevispinis</i> n. sp. 1 ♂ <i>Camptocladius fenestratus</i> n. sp. 3 ♂	
12	N. 6		<i>Trichotanytus foliatus</i> n. sp. (nach A. Thienemann)	

Years ago, the late F. Reiss showed me a file folder with dozens of original, handwritten items from the correspondence between A. Thienemann and Kieffer, and also some similar letters between F. Lenz and Kieffer, covering the period from 1908 to Kieffer’s death in 1925. E. J. Fittkau and Reiss had rescued these from being destroyed at Plön after Thienemann’s death. During their long and intensive collaboration, Thienemann had sent Kieffer determination sheets (see Figure) along

with specimens in alcohol to be determined, listing for each sample the date and source („Herkunft“, middle column in Figure) or at least a descriptive designation („Bezeichnung“, left column; e.g. in line 9: „Alfbach in Moos“). Kieffer usually returned the sheets after entering the results of his determinations from each vial, often including number, sex and condition of specimens seen, and frequently more or less extensive comments. Occasionally, first

identifications were altered, either by Kieffer before return of the letter, or afterwards by Thienemann (see line 13, bottom of right column).

Reiss, lamentably, never found the time or a helper to process the numerous sheets, letters and postcards in detail. But in the spirit of our discussions about them I have recently excerpted all taxonomically relevant information into an electronic file containing some 2800 individual species- or genus-level records.

The Thienemann and Lenz collections at the Zoologische Staatssammlung Munich (ZSM) contain many original slides, alcohol vials and field notebooks with handwritten records that can be matched to entries on the correspondence documents. By comparing these data to those in their authors' publications on the corresponding taxa, a number of specimens have already been identified as undoubtedly constituting type material, and many more are hoped to still follow. For additional information on the background, and on practical problems to respect in such attempts, see SPIES (1998). The latter paper also makes it likely that additional sources of type specimens (in that case the Zavrel collection in Brno, Czech Republic) could be easily located if we started looking.

This brings up the point that major improvements will only be achieved in close symbiosis with the musea harboring the relevant collections. Since many of us are not ourselves employed at such institutions, we will need their help with the continued preservation and availability of specimens and data on them, for example the commendable presentation of type specimen lists on the Chironomid Home Page. In return, through our constant attention to the classic and recent treasures gathered at these musea, we can fulfil our duty to support them in justifying their existence against political and economic pressures.

If we combine approaches like the above with the long overdue examinations of those 'classic' type specimens already known to exist but not yet revised, we can realistically expect to substantially increase the number of solidly type-based and therefore really and meaningfully stable names. Obviously, through the achievements of the past decades we have already reached a certain degree of stability with a set of names in current use. However, given the extent of the problem outlined above and how much of it is virtual, i.e.

could be solved, we must be prepared for quite a number of changes. But there is no good reason why the status quo of today - an arbitrary point in time - should be largely frozen solid just to have stability for stability's (or rather rigidity's) sake. It does not make sense, for example, to use the Meigen name for a species that has been recently revised, but permanently exclude the Meigen name for the species next to the first one in the same museum box simply for the historic accident that nobody has yet managed to revise the group the second species belongs in. Instead, let us now take the missing steps toward a better stability consistently based on names verifiable through type material.

In summary, here are some **proposals for how to proceed in the future**, which I would seriously urge you to consider, discuss in public (e.g. over the Chironomidae-L e-mail listserver), and yes - follow in practice as applicable to your own work:

1. Please do not just keep adding more and more new species ornaments to the top of the family tree while the roots and the stem are slowly rotting away (pinned midges don't last forever, much has already been lost). Imagine how long an overburdened but hollow tree can stand and bear fruit. Instead, with at least some of your descriptive work join a hopefully concerted effort to stabilize the basis of our system by locating, revising and identifying unattended type specimens at any institution within your reach.

2. If you are not exclusively working in taxonomy, logistical reasons may keep you from ever seeing type material for most species you write about. Instead you are probably relying upon the identifications, keys etc. in the works of others. If so, have you been paying attention to what steps the authors of these references took to ensure that the names they are propagating are rooted in the proper type material? In the future, when identifying and reporting on species please try to use sources directly or indirectly based on the study of types. Do not uncritically copy names from works without such links to verifiable scientific facts. **Always cite the references with which you identified your species.** This way, if the name of such a species is later changed, it will be much easier afterwards to still trace back which species you had.

3. Send me your thoughts, and I'll try to coordinate and report to the community what developments there may be.

„To whomever the above results do not appear satisfactory I would like to say that I myself regard them as of only minor importance. Their purpose, however, is to stimulate other dipterists to express their views and opinions ... , so that ... a definite result can finally be achieved, and thus our archives may be purified of the thousand dubious things which are disturbingly and paralyzingly obstructing any steady and thorough progress. In my opinion, the revision of earlier authors has to be undertaken in all seriousness. We must finally definitely know, which results by one or another may and should be accepted and preserved, and

which discarded as forever indecipherable. The dragging along of unclear terms and unjustified names is damaging our science more than anything.

If I may rely among my colleagues upon the very peculiarity of mankind - I mean the one that they most readily speak if they can speak against something - then I hope to see all my intentions soon fulfilled."

J. R. SCHINER (1856 !!), translations and omissions from the Austrian original by M. Spies.

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