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# Hamuli

Newsletter of the International Society of Hymenopterists

## In this issue ...

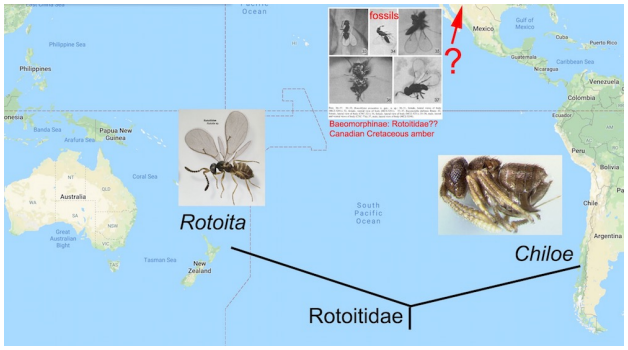


*Three academic generations! Three visits to the same spot on the top of Puyehue by* Chiloe *hunters!*

## Collecting "Moss Wasps" (Rotoitidae) at the southern ends of the World

By: *John Heraty, Krissy Dominguez and Austin Baker, University of California, Riverside; Jim Woolley, Texas A&M University; Lars Krogmann, Natural History Museum Stuttgart; Ralph S. Peters, Zoological Museum Koenig Bonn*

Rotoitidae is a key group to understanding the phylogeny of Chalcidoidea. The family includes two monotypic genera positioned after Mymaridae and, based on morphology, it is thought to be the sister to the rest of Chalcidoidea (Gibson & Huber 2000). However, monophyly of Rotoitidae has never been confirmed by molecular data. Besides their presumed phylogenetic key position, they have a unique distribution. *Rotoita basalis* Bouček & Noyes occurs only in New Zealand and *Chiloe micropteron* Gibson & Huber is restricted to Chile. Gumovsky et al. (2018) described several fossil species of *Baeomorpha* (Rotoitidae) from northern latitude late-Cretaceous amber deposits, wherein they designated a '*Baeomorpha* Realm'. Interestingly, another undescribed rotoitid species has been found in southern latitude Burmese amber, which is nearly 99 million years old, suggesting a more widespread and even older distribution! No one has yet sequenced *Rotoita*, and it was critical to our NSF-funded project on chalcidoid phylogenetics. So, John Heraty and Austin Baker, Jim Woolley, Lars Krogmann, and Ralph Peters converged on New Zealand in December 2017 to capture fresh specimens.




*Distribution of* Rotoita *and* Chiloe *(Rotoitidae) and a plate from the description of* Baeomorpha *by Yoshimoto (1974) pointing to the northern recovery of fossil Rotoitidae*

*New Zealand team. L to R: Ralph, Austin, Lars, Jim and John, crossing the Cook Strait*

*Pans and a view of Lake Rotoiti, New Zealand*

*Rotoita* was first described as a genus and a new family in 1987. Only 16 specimens were known. It occurs almost everywhere that is forested in New Zealand and summer is a good time to collect it. *Rotoita* was collected in a Malaise trap over a stream, sweeping beech-podocarp forests, from moss, and in sphagnum/native grasses habitat. The type locality is St. Arnaud on the South Island, so this is where our first collecting took place. Getting people to converge from all over the world to one location at the same time proved a bit difficult, and of course we were not smart enough to correctly read itineraries that involve a country that is over the international date line, but eventually we got to our wonderful B&B in St. Arnaud. It was a beautiful cabin that backed onto a southern beech (*Nothofagus*) forest and overlooked a large sphagnum bog with native grasses, which could have been the presumed type locality of *Rotoita basalis*. Our dining room (lab) had a beautiful overlook across Lake Rotoiti. Short story: about 15 Malaise traps, roughly 300–400 yellow pan traps per day, and 5 people sweeping like maniacs. One brochure in the cabin led us to one particular forest trail (Braeburn Walk). Using our hints from the labels of *Nothofagus* and moss, we focused part of our collecting on a unique spot that was selected by Austin. In a patch of forest less than 10 meters square, we eventually collected four specimens! Ten days into the trip Jim discovered the first specimen of an undescribed *Rotoita* species in a Malaise trap sample—it is difficult to imagine the excitement that ran through our veins that day, and it was hard to wipe the smiles off of our faces. Three others were collected in pan traps from the same site. Lars was king of the trip though—on the north island he collected a *Rotoita* male (previously unknown), and what appears to be a true *Rotoita basalis* by sweeping with the Noyes-style screen sweep net.

There were lots of other exciting discoveries in New Zealand, including a tree full of orussids with enough of the endemic *Guiglia schauinslandi* to put into RNAlater for transcriptome work, and another collection of the xiphidriid *Moaxiphia* collected into ethanol for normal sequencing. It was a strange place to collect considering what is missing—few leps, virtually no butterflies (a few cabbage whites), very few ants, and even very few chalcidoids. Parasitic wasps were almost entirely represented by a staggering diversity of diapriids! On the South Island, there was also an obnoxious blackfly, *Austrosimulium ungulatum*, that has been called ‘namu’ in Māori, or perhaps more appropriately ‘New Zealand’s

blood-sucking summer nightmare'. Despite the lack of many insects, and the blackfly nightmare, the country is spectacular!

Rotoita *collecting locality at Braeburn Walk*

Rotoita *from Malaise trap*

*Six nets*

A second team formed by Krissy Dominguez, John, Lars and Jim went to Chile in February 2018 for the second expedition to work on *Chiloe*. We have DNA for *Chiloe*, and knew exactly where to collect it, but the idea was that we might be able to resolve the biology of the group, which is unknown other than that they were apparently associated with litter. The trip to Chile was one of returns for John. Lubomír Masner first collected at Terao on the island of Chiloé, with Lucho Peña in 1988. Heraty, John Pinto, Jeremiah George (UCR grad student) and Lubo collected in 2005, and then 30 years after the first collection we returned again to the same site. As always, some adventures. Our rental car was not ready for pickup when we arrived and none were available otherwise. An overnight in Puerto Montt (providing an opportunity for the first of many wonderful seafood dinners), a long wait for our Alamo rental dealer and we finally had our 4-wheel drive Pathfinder. On to the farm of Pablo "Checho" Escobar (who worked for 30 years for Lucho) and collecting began. The facilities had been developed by Lucho to provide a research station in a 'protected' area of lowland Valdivian forest. There were some cows to keep things interesting, especially when they drank all the water from the pan traps and took out a Malaise trap, but otherwise an idyllic place on the coast overlooking the salmon farms. After the first day of panning with gold (YPTs), Krissy found the first *Chiloe*. Also, *Archaeoteleia* (Lubo's woo-hoo taxon) filled the pans. A particular highlight of this part of the trip was the presence of several families who were vacationing at the station with something like 15 kids, who were immediately recruited to help us find *Chiloe* (see pic of one pan-trapping team).

We did a different form of pan-trapping this time by monitoring groups of 10 pans to isolate exactly where *Chiloe* was coming from. It was soon very apparent that *Chiloe* was only found associated with moss. We also retrieved at least one specimen from a Berlese of moss. What was interesting was the presence of moss in the habitats in New Zealand where we collected *Rotoita*. We are convinced that whatever the hosts of these species are, they are in moss. We also found many peloridiids (moss bugs) in the Berlese and YPT samples from Chile. They are the sole members of Coleorrhyncha and considered to form a relictual insect group with a similar southern-end distribution to *Chiloe*, and could be a potential host. We compared *Chiloe* adults to the size of a peloridiid ovariole, and they make a good match. However, thousands of moss samples later, no deposited peloridiid eggs were ever found, so we have no absolute proof. We found other potential host groups, such as the rarely collected Myerslopiidae (Membracoidea), that also have southern-end distributions and are associated with moss. What was cool was to find that *Chiloe* survived the soap-solution in the YPT, and even immersion in ethanol while sorting (similar to some aquatic diapriids?). It gave us a chance to observe live adults, and observe their ability to jump! See the video: `https://youtu.be/zQ3wAki3KXM` (near the 0:57 mark). We really didn't have great expectations that we would resolve

the biology, but we never thought that we would observe live *Chiloe*.

After our Chiloé Island adventures, we dropped Lars off at the airport and then headed up to Nahuelbuta National Park to collect in the coastal forest of massive *Nothofagus* and *Araucaria*, and then on to Puyehue to collect at the lowland site where *Chiloe* had been collected – as well as the high elevation water tank (1120m) where Lubo had collected a single *Chiloe* by skimming the water. We had no luck with the skimming, but the common theme of all sites remained the same—lots of moss and what appears to be *Sphagnum*.

*Chile expedition. Clockwise: The pan trap army (Jim, Amanda, Benjamin, Francesca, Santiago); moss heaven trap site;* Chiloe micropteron*,* Araucaria *(Nahuelbuta); skimming the water tank at Antillanca; pans along the moss road; Peloridiidae.*

Overall, both trips were successful and great fun, and now we sit back and wait for the sequence of *Rotoita* to finalize our placement of this enigmatic taxon. ○

**Acknowledgements** We would like to thank Darren Ward and John Early in New Zealand for their help with permits, Cor Vink for help with ethanol in NZ, Alfredo Ugarte and Christian González for help with the Chile permits and collecting, 'Checho' Escobar and his family for helping us to collect, and of course the National Science Foundation grant NSF-DEB 1257733.

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## A $3 Homemade Winkler Sampler

By: *Michael L. Ferro, Clemson University, Department of Plant and Environmental Sciences, Clemson, SC, USA; spongymesophyll@gmail.com*

Beware the explorer that takes a peek below a centimeter scale, for they may find such a vast and diverse bestiary that the larger world will hold little fascination. The minute caverns and fissures of leaf litter, leaf mould, humus, duff, etc. have been called the "poor man's tropical rainforest" (Giller 1996), and rightly so, for the forest floor harbors an enormous diversity of small life, especially Hymenoptera (and not just those pesky ants). Exploration of the leaf litter generally begins with an inventory using a Berlese funnel or a Winkler collector/eclector/extractor/sampler. Owens and Carlton (2015) compared Berlese funnels with Winkler samplers for beetle catch and found that both worked equally well (but to maximize results, run the sample a long time, 5–10 days!). Whereas Berlese funnels require electricity, bulbs, cords, *etc*., Winkler samplers only need a place to hang and are generally more field friendly. Silva and Feitosa (2017) used Winkler samplers to collect parasitic Hymenoptera with soil or litter hosts at 14 localities and collected 600 specimens from 15 families.

Currently "Winkler collectors" and "Mini-Winkler collectors" are available for purchase in the United States from `http://www.santetraps.com` and cost US $180 and $90, respectively. In Europe a "Xeroeclector" is available from `http://www.entosphinx.cz/en/` for €125.60. Both are high quality, durable equipment, however, a half dozen or more may be beyond the financial resources of a teacher, researcher, or hobbyist.

A Winkler sampler is basically an inner bag with "large" holes that holds a sample of leaf litter surrounded by an outer bag with "small holes" to keep specimens in, but allow air flow. A collection vial is attached to the bottom of the outer bag to collect specimens that wander or fall in. Small Winkler samplers (Fig. 1) can be made for about US $3 each in the following manner. First, the supplies you'll need:

1. 22 in. × 25.5 in. giant organza bag (about $1.25 from `http://yourorganzabag.com/`, but can be purchased elsewhere)

2. 12 in. × 15 in. mesh laundry bag (*e.g.*, the "Essentials" bag from Dollar Tree Store; $0.36 or 3/$1)

3. 120 ml Screw Cap Tube 42 × 114 (I used #60.597.001 from Sarstedt `http://www.sarstedt.com`; $0.75)

4. Hot glue `https://en.wikipedia.org/wiki/Hot-melt_adhesive`

5. Short piece of rope

**Construction** Turn both bags inside out so the tight seams are on the inside. Cut the center out of the top of the screw cap tube, and hot glue the cap, facing out, to the bottom of the organza bag (Fig. 1A, C). Place a piece of paper between the two sides of the bag so the hot glue doesn't stick the two sides together. Cut a hole in the bag against the inside of the glued cap. Tie a rope to the laundry bag (60 cm from bag to top of loop) so it can be suspended in the organza bag (Fig. 1A).


***Figure 1***. *A Winkler sampler lets you collect a world of possibilities!* ***(A)*** *all the parts apart;* ***(B)*** *sampler engaged;* ***(C)*** *close-up of collection vial, outer organza bag, and inner sample bag;* ***(D)*** *sampler stuffed into the collection vial.*

**Operation** Place the laundry bag over a container while you fill it with leaf litter. Jiggle and bang it to make the loosest particles fall out and then pour them back in the top. No need to zip up the laundry bag, remember you want things to escape. Hang the laundry bag by the rope on the hook where the Winkler will be left to run. Slip the organza bag over the laundry bag from underneath and close the organza bag by pulling the drawstrings. Hook the drawstrings over the same hook to which the rope is attached (they should be the same length now). ***PLACE A LABEL IN THE COLLECTING VIAL*** (did you notice I emphasized that, because some people forget, or, worse, write on the outside—this must never be done), add preservative, and attach the collecting vial. Keep the sampler out of the wind and don't bump it, otherwise you'll get a dirty sample. Let hang at least 24 hours, but 3–5 or even 10 days is better. Then collect your wasps.

(Feel free to send those annoying beetles to the author!)

Using the current design, nearly the entire unit can be stuffed into the collecting jar for ease of travel and storage. Numerous improvements/alterations can be incorporated. For example, samples may be easier to load and unload if the holder has a solid frame.

**Uses** Obvious uses include personal collecting, research, school groups, summer camps, college classes, *etc*. Older students could make their own. However the low cost and small size mean that Winkler samplers could be "abandoned" to run in the field and recollected later. For example, if a camping/collecting excursion involves a multiple day hike and a return along the same path, Winkler samplers could be discreetly placed in protected areas just off the trail at the place where the leaf litter was collected. A few days later samples and samplers would be recollected. Something similar could be done on a long drive. The low cost also means that samplers could be left behind at field stations, schools, *etc*. for use by others. ○

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## Call for western North American *Dialictus* (Halictidae)


By: *Joel Gardner, PhD student, Gibbs lab, University of Manitoba; gardner1@myumanitoba.ca*


I am beginning a revision of the *Dialictus* of western North America and need your specimens! I am interested in anything west of about 100 degrees longitude. If you have any troublesome western *Dialictus* you want finally to get names on, contact me at gardner1@myumanitoba.ca, and we can talk about how many and which specimens to send. Wish me luck on this monumental task! ○

*I once had a strange* Dialictus
*I thought Gibbs was wrong, or he tricked us.*
*But the tergal pubescence*
*was apically dense;*
*it was just a Confusing* Halictus*!*

Lasioglossum vierecki *Crawford, 1904. Photo by Joel Gardner*

## North American *Pristiphora* and a call for specimens


By: *Spencer K. Monckton, York University, Toronto, Ontario, Canada; s.monckton@gmail.com*


Sawflies are a fascinating, but relatively little-known group. While it's easy to find a member of the public willing to 'save the bees', it seems the only people who've even heard of sawflies, apart from entomologists, are gardeners who would happily eradicate the one or two introduced species with which they're familiar. You can imagine, then, that studying sawflies in a bee lab feels a bit like bringing a durian to a garden party. Lucky for me, most of the Packer Lab are fans of durian and sawflies alike.

In fact, I'm quite happy to be the odd one out; to be surrounded by bee specimens while studying their distant cousins is to have a constant reminder of the marvel of Hymenopteran diversity. It was following a systematic revision of a group of small Chilean bees (MSc), after all, that I decided to turn my focus to sawflies in Canada's expansive North (PhD). Cool-and-damp is to sawflies what hot-and-dry is to bees, and it turns out there are quite a few species of the former right here in North America's massive backyard.

As many hymenopterists know, sawflies are unusually diverse in Northern regions. This is apparently most pronounced in the Palaearctic, but is true as well of the Nearctic (1). In fact, the diverse subfamily Nematinae (Tenthredinidae) is thought to have had a Laurasian origin on what later became North America (2). While efforts to revise the Nematinae in the Palearctic have most recently resulted in a large and thorough revision of North-Western Palaearctic *Pristiphora* (3), such efforts have been comparatively absent for the Nearctic fauna.

As such, the primary objective of my PhD is to revise the 50-odd North American members of this genus. My PhD builds on unfinished work by the late Horne R. Wong, and I am grateful to have permission from the Canadian Forestry Service to expand upon his work.

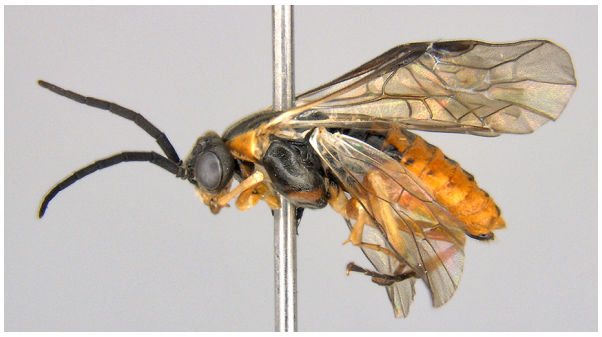

***Figure 1***. Pristiphora bivittata *(Norton, 1861) lateral view, female.*

Besides their taxonomy, I am also interested in sawfly biogeography and host-plant use. Sawflies are thought to have diversified through repeated range expansions and contractions as a result of cyclical climatic changes throughout their evolutionary history (4), and have seemingly undergone numerous dispersals and back-dispersals between the Palearctic and the Nearctic. My focal genus *Pristiphora*, in particular, may have dispersed between Eurasia and North America at least nineteen separate times (5). My own preliminary results suggest multiple dispersals even within a single species.

Sawflies can be highly host-specific (4), such that host plant associations are important for both ecological and taxonomic reasons. Host plants can be inferred from specimen labels of reared sawflies or from careful field notes (*i.e.*, identity of swept plants from which larvae are collected), but where such information is unavailable, molecular methods show promise. I've already had some success extracting plant DNA from ground-up tissue of larvae collected on known hosts, and I hope to publish on this soon. Rearing can be difficult and time-consuming, and this approach offers a relatively low-cost, low-effort alternative.

These areas of inquiry raise all sorts of interesting questions, and such questions require specimens to answer. Though Wong assembled an excellent collection of *Pristiphora*—and an invaluable resource to my work—the most recent specimens were collected in 1989. I have been updating this material with specimens from the Biodiversity Institute of Ontario, the Lyman Museum in Montreal, and the Royal Ontario Museum in Toronto, and I'll be seeking loans from several other large collections, which together probably contain the majority of North America's sawfly specimens (Canadian National Collection of Insects – CNC; Smithsonian – NMNH; Illinois Natural History Survey – INHS; Academy of National Science in Philadelphia – ANSP). But, sawflies are not often collected in large numbers, so it will be essential to bring together loans of specimens from many smaller collections to ensure good coverage.

***Figure 2***. *Collecting in Churchill, Manitoba (photo credit: A. Solecki)*

Hence, a call for specimens: I would be very happy to hear from anybody willing to loan specimens of *Pristiphora* (including *Melastola*, *Neopareophora*, *Nepionema*, *Pristicampus*, and *Pristola*) collected in North America, particularly from less-frequently sampled localities in the North. Specimens from Siberia are also welcome. I'm happy to sort and identify sample lots of "Symphyta" stored in ethanol (*e.g.*, sorted from field traps), and I can pay shipping costs if necessary. ○

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## A short autobiography and why Ichneumonidae taxonomists don't love DNA?

By: *Donald L.J. Quicke, formerly Professor of Systematics, Imperial College London, now retired living in Bangkok, Thailand; d.quicke@email.com*

Amongst other things, I am a phylogeneticist, specialising in the larger parasitoid wasps, *i.e.* Ichneumonoidea. I started working long ago on braconine braconids because just after graduating from Oxford I spent some months in wonderful Tsavo National Park (East), in Kenya, near the then small township of Voi. In those halcyon days, obtaining collecting and export permits was relatively easy and hardly political—though on my second visit in 1977 I did have to listen to a short rant from a wildlife officer before he issued me a permit (with a wry smile). How the wretched Convention on Biological Diversity has harmed the conducting of biological and biodiversity research is a complete tragedy—and all science ministers around the world should be working for its removal, or at least, introducing softening amendments.

Back in 1976, during my first tropical trip, I lived in a small thatched house built by the WWF for Prof. Malcolm Coe (Oxford University), one of my most appreciated mentors. The house was near the foot of 'Malima wa Simba' (lion hill), on which dwelled a huge pride of about 40 lions who every night would go hunting buffalo which were also abundant. The flush toilet was outside! My first night there I was terrified because the window glass was about 2 mm thick, and a lion or two (I didn't look) came to sniff around grunting as they do, just outside of my window. Before I got a house boy, he was 60+ years old when he arrived a month later, my 'washing up' was done by putting my dishes outside for the night—first the porcupines and other mammals would lick my plates clean, then the ants would come and do all the 'rinsing'—a quick dusting and the plates were ready to use again.

Amazingly, I was given permission to walk around the park, on my own, as long as I didn't wander into the thick, buffalo-ridden, *Suaeda* bush along the Voi River Valley. I'm quite surprised that I am still alive. I still absolutely refuse to do compulsory 'no one must go alone into the forest' type fieldwork (something I got into trouble for on several occasions at my previous employer, Imperial College London). For goodness sakes, if one walks around a tree in an African forest and encounters a forest elephant, you will die—If two of you do the same, you will probably both die; just try not to be bitten by a deadly snake, and even if you do, and you are miles from camp and many miles from any hospital, you'll almost certainly die anyway, and in so doing just panic your companion, who might well die as a result.

Of course, after a while during my stay at Tsavo, I did venture into the thick Suaeda bush in search of parasitoid wasps, and I survived to tell the tale despite numerous scares. Sadly, in that bush I found very few interesting wasps, but I did once hear a very large mammal nearby—An elephant? A rhino? A cape buffalo (probably the most dangerous)? I retreated as quietly as I could and waited for 10 or 15 minutes 50 m or so from the woodland edge, whereupon a young boy emerged followed by a goat and a cow. On yet another occasion near there, I found myself in the middle of a herd of elephants—initially I had thought they were the famous orphans reared by Daphne Sheldrake—but there were only two of those at the time and there were definitely six surrounding me! Don't look them in the eye and hope they don't have any youngsters with them. Once, having run out of water (or maybe forgotten to take any with me) I had to drink from a small water hole dug by elephants in the otherwise dry Voi River valley—dead insects, a bit of mammal shit, but I was ok—didn't even get the runs.

My first trip had initially been meant to target the Chrysididae to test an hypothesis that the most distinctive colour patterns would be the rarest. However, I found chrysidids to be surprisingly uncommon and encountered few of my initial targets. But, instead I collected large numbers of obviously diverse (even with a hand lens), bright red, black and yellow, or nearly all black, large braconid wasps. I could by then identify most braconid subfamilies as there weren't quite so many as there are now, and most of those that I collected belonged to the subfamily Braconinae, with a sprinkling of agathidines, doryctines and a few cardiochilines. These braconines fascinated me because nearly all British braconid wasps are small, and the diurnal ones are typically mostly black or at least dull. Well, on my return after three months in the bush and a bit of a virus, I'd lost 4 kg and frightened my parents nearly to death. I mounted my remaining specimens (some had been destroyed by ants and more by local olive baboons breaking in to my house through the thatched roof (shades of Alfred Russell Wallace)), took myself to the Natural History Museum in London, and introduced myself to the curator of Hymenoptera there who at that time was Tom Huddleston—very taciturn, but to me also wonderfully helpful. On my first meeting with him in late autumn 1976, I showed him my mounted Tsavo specimens. He scratched his head and pulled out one specimen, a large black and red wasp, strongly dorsoventrally compressed. He said "I think that this is a *Platybracon*." The name gives it away somewhat. So we went to the collection, and sure-enough it was. Well, overawed I then asked Tom about all the others, and he shook his head and said that the group (Braconinae) had never been revised. [Fahringer had done a bit but useless by current standards even in 1976]. What did I do? I said "Well, in that case, I'll revise them." Tom smirked. However, I persevered. I started borrowing types from many very helpful curators of the major European collections, and I soon started noticing character systems that had been completely ignored (or maybe mentioned in passing as if they were largely unimportant). And thus my first few papers on the tropical Braconinae genera started to appear.

At this time I was doing my PhD on snail neurophysiology under the supervision of Dr. Robin Brace at Nottingham University, I encountered some problems. Robin was enormously jealous (or angry) about my interest in wasps whilst I was meant to be working on snails. He hid my wasp post, including hiding parcels of type specimens sent to me behind his office desk. He even had me banned (or rather tried to) requesting inter-library-loan photocopies about wasps ... it was a hard time and I actually got an ulcer because of it. Luckily, the head of department, Prof. Peter N. R. Usherwood (of glutamate as an insect neurotransmitter fame), was very supportive and 'had words' with Robin. Time passes and Robin and I finally made it up. He lost his lectureship finally for lack of productivity.

Time moves on, and in 1993 I received a NERC advanced fellowship at Imperial College London (then part of University of London), though based out in countrified Ascot, about 30 miles west of London, at their Silwood Park ecological and nuclear reactor campus. My fellowship was part of the NERC Initiative in Taxonomy which had been instigated as a temporary result of a House of Lords select committee on the future of taxonomy in the UK. My boss was Sir Charles Godfray (though he was not knighted back then). As part of that programme, I started doing molecular phylogenetics on ichneumonoids with the able assistance initially of Dr. Mike Tristem and then of Dr. Robert Belshaw, not to mention various wonderful students and technicians. Although getting grants for systematics in the UK was always hard, I was successful with all my applications—at least eventually. I am quite sure that the same or similar applications for research on insect systematics would never get beyond the first hurdle now, and even then applications had to be justified in terms of wanting to understand ecology. Oh! And by the way, in order to do this ecological research I need also to do some braconid phylogenetics!

During the early molecular days (after the very, very earliest ones in which we used radio-phosphorus, and darkrooms) we started to generate interesting results. My group initially investigated the braconid subfamilies Opiinae+Alysiinae (Gimeno *et al*. 1997) which are still a big unknown, Aphidiinae (Belshaw & Quicke 1997), overall ichneumonoid phylogeny (Belshaw *et al*. 1998) and phylogeny of the Braconinae which were the group that I had started on (Belshaw *et al*. 2001). After some playing around (experimenting) my group and others more or less homed in on a combination of slow-evolving 28S rDNA and fast-evolving cytochrome oxidase 1 to investigate phylogenies of many groups, depending on level of funding and time sometimes supplemented with elongation factor 1-alpha or some other more intermediately evolving gene (*e.g*. Mardulyn & Whitfield 1999; Whitfield *et al*. 2002; Sharkey *et al*. 2006; Quicke *et al*. 2008; Zaldivar-Riverón *et al*. 2008, 2009; Kittel *et al*. 2016). In fact, now there are very few substantially-sized braconid subfamilies that have not received detailed molecular analysis. Further, the molecular phylogeny of the whole of the Braconidae has been revisited by Sharanowski *et al*. (2011) using several gene fragments including 28S.

It was around that time that I heard that the late Dr Ian Gauld of the NHM had a student, Jacques Dubois, based in the Muséum National d'Histoire Naturelle, Paris, working on the molecular phylogenetics of ichneumonid wasps based around the Pimplinae. Jacques created quite a large dataset of mitochondrial 16S rDNA and cytochrome oxidase 1 gene sequences, both genes for 59 pimpliformes species which was quite a large dataset for the time (Dubois 2005). Nothing has ever been published of these molecular results to the best of my knowledge, and the acquired data are, to date, unavailable, or even perhaps lost. Whilst this is very sad it probably actually reflects the fact that both these two gene fragments are really fast-evolving, and probably alone cannot yield reasonable phylogenetic estimates, at least not at the level Ian and Jacques were interested in. Additional data from more slowly evolving genes would be needed. The failure of these sequences to yield phylogeny estimates that were broadly concordant with perceived relationships is hardly surprising, but I believe, had very serious consequences that are still ongoing today.

Kees van Achterberg and I started collaborating on a large morphological phylogeny of the Braconidae in the late 1980s resulting in our infamous publication (Quicke & van Achterberg 1990). Wharton *et al*. (1992) didn't like it one bit, partly because of the results with our trees suggesting a single major origin of endoparasitism and thus the non-cyclostomes being recovered as a derived group within the cyclostomes by association with the endoparasitic cyclostomes such as Rogadinae, Opiinae and Alysiinae. And of course Wharton *et al*. were right; the trees that morphology suggested were very wrong (but see van Achterberg & Quicke 1993 for a reassessment of a reassessment). Whilst some of the sociological aspects that followed were far from pleasant, Kees's and my erroneous phylogeny was in fact the start of something big. The data are data, and if they suggest something blatantly wrong then there must be other interesting things going on. The resulting exploration and subsequent understanding that many character sets connected to life history are terrible phylogenetic indicators (Quicke & Belshaw 1999, Belshaw & Quicke 2002) led to a great deal of scientific output.

Many ichneumonoid taxonomists were and are based in institutions such as museums that in the past have not had ready access to grants or molecular facilities and expertise. They have therefore concentrated on morphology and morphological phylogenetics. This was of course the only approach possible before molecular methods were developed. Actually, it seems to me, that various ichneumonid workers probably did assemble morphological matrices, just as my group and co-workers had done for the Braconidae, and had also analysed them—but they never published any of their results. If they did such analy-

ses, why wouldn't they have submitted them for publication? After all, such phylogenetic hypotheses could lead to many future, hypothesis-testing studies.

In stark contrast with the Braconidae, most large subfamilies of Ichneumonidae have received no molecular phylogenetic attention, and certainly little has been published. Only a few studies have tried to assess overall relationships within the Ichneumonidae using molecular data (a very few exemplar taxa have been included in some larger studies of the Hymenoptera but not enough to allow reconstruction of subfamily relationships). Most molecular phylogenetic studies on the Ichneumonidae to date have concerned only subgroups, *e.g.*, Laurenne *et al*. (2006) and Santos (2017) both on the Cryptinae and to a lesser extent on the Ichneumoninae, Quicke *et al*. (2005) on some aberrant genera of 'Ophioniformes', Klopfstein *et al*. (2011) on the Diplazontinae and Rousse *et al*. (2016) on the Ophioninae.

Whilst there is now a general consensus based on molecular data that the Braconidae divide early into the cyclostomes (+ Aphidiinae + Mesostoinae + other cyclostomes) and the non-cyclostomes, there is no such consensus regarding the Ichneumonidae. Co-authors and I have suggested that the Xoridinae are likely the sister group to the rest of the Ichneumonidae (based on both morphology and DNA data) but nothing has been forth-coming from specialists on Ichneumonidae apart from old, ill-thought-out concepts about tryphonines *et al*. showing the least derived morphology. Even in a recent molecular investigation of the huge and morphologically difficult subfamily Cryptinae *s.l.*, one important, though hardly hard to obtain, group—the Alomyini—was excluded (Santos 2017). The relationships of the Hybrizontinae (which have even been treated as Braconidae in the past) are still hung in the balance and there have been no recent attempts to place them more securely using DNA data, though in this case their highly derived sequences probably means that some different approach from normal Sanger-sequencing of a few 'usual suspect' gene fragments might be required.

I suspect that ingrown preference for particular phylogenetic outcomes (the received 'wisdom') has enormously hampered both morphological and molecular phylogenetic investigation of the Ichneumonidae. I might go as far as to say that many ichneumonid workers have an hatred of molecular information because results are not in accordance with the 'wisdom'. If my view of the situation is correct, then a Shakespearean 'sea-change' in the attitude of ichneumonid phylogeneticists to molecular data is required, otherwise that family will for ever lag behind the Braconidae. Ichneumonidolists need to embrace DNA data at all levels, though given recent shifts in funding priorities, obtaining grant support for even doing straightforward Sanger sequencing of a few genes for a range of ichneumonids could be rather difficult. I'm glad that I got in when life was easier. ○

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## Who's that hymenopterist?

This is the first entry of a short series of photographs that tell stories about our history. Please let the editor know (adeans@gmail.com) if you have photos you'd like to contribute!

Can you name these famous hymenopterists? Here are two hints: The year was 1964, and the photo was taken on Lamont Street, in Washington, D.C. See bottom of page 12 for the answer!

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**Answer:** Who are those hymenopterists on page 11? Lubo Masner and C.F.W. Muesebeck. Thanks to Lubo for sharing this photo!