The Systematist

Newsletter of the Systematics Association
Number 28 2007

www.systass.org ISSN 1744-5701

Inspirations
Kate Jones on bats and biodiversity

Quo Vadis
Whatever happened to cladistics?

Book reviews
The giraffe's long neck, neotropical savannas, and vertebrate phylogeny

Presidential Report
Status and objectives of the Systematics Association

What is taxonomy's unexamined impediment?

Editorial

New beginnings

Dear Readers,

This first issue of *The Systematist* in 2007 features various new beginnings. First, we would like to congratulate Professor Richard Bateman on his appointment as the new President of the Systematics Association. Long time active member of the Association, Richard also plays important roles in several other scientific organizations, including the Linnean Society and the Biosciences Federation. If Richard's involvement up to the present is any guide at all, he will undoubtedly lead the Association with charisma and strength. To inaugurate his presidential term, he reports in this issue on the current status and objectives of the Systamatics Association. Naturally, we heartily thank departing president Barry Leadbeater for giving the Association the benefit of his leadership over the past several years.

Second, the Association is strengthened by a cadre of enthusiastic new council members, whose expertise represents all domains of life, extinct and extant, and the full panoply of methods and approaches of modern systematics. For biographical synopses and selected mugshots of this colourful bunch, please visit our website at www.systass.org.

Third, this issue of *The Systematist* is the first to have benefited from the dedication of a new co-editor, Dr. Marcello Ruta. Marcello is based at the Department of Earth Sciences at the University of Bristol. Marcello works as a palaeontologist, but he reserves a special place in his heart for the microscopic kinorhynchs. Marcello's research interests span the gamut of deuterostomian diversity, including the evolution and phylogeny of primitive tetrapods, amphibians and amniotes, and the evolution of Palaeozoic echinoderms and the chordate-like stylophorans, and his focus ranges from local studies of character evolution to the broad phylogenetic aspects of the Permo-Triassic mass extinction. However, this new beginning is matched by a departure.

The Association extends its gratitude to departing co-editor Malte Ebach, whose innovating energy has served *The Systematist* well. Malte's efforts have been instrumental in the progressive anagenesis of this newsletter from humble beginnings, through loosely stapled intermediates, to its current glossy acme with ISSN number and citations in both the primary literature and printed media. Many thanks Malte for your initiative and your enthusiasm.

As usual, this issue is chockful of the customary interesting contributions. However, before reading on, please visit the backpage of this issue to find out how and when to ***register*** for the fast approaching ***6th Biennial Meeting of the Systematics Association*** in Edinburgh in August. With a full programme, including three themed symposia and very useful 'how to' talks on contemporary techniques in systematics, this meeting will offer much of value to both the fledglings and sages of systematics. Of course, there will also be beer.
Enjoy this issue of *The Systematist*.

Cover illustrations: The daisy *Gorteria diffusa* and its habitat in Namaqua National Park, South Africa (Copyright 2007 Meredith Murphy Thomas)

The Systematics Association is committed to furthering all aspects of Systematic biology. It organises a vigorous programme of international conferences on key themes in Systematics, including a series of major biennial conferences launched in 1997. The association also supports a variety of training courses in systematics and awards grants in support of systematics research.

Membership is open to amateurs and professionals with interests in any branch of biology, including microbiology and palaeontology. Members are generally entitled to attend the conferences at a reduced registration rate, to apply for grants from the Association and to receive the Associations newsletter, *The Systematist* and mailings of information.

For information on membership, contact the Membership Secretary, Dr G. Reid (membership@systass.org), Department of Botany, Natural History Museum, Cromwell Road, London, SW7 5BD, U.K.

The Systematist Newsletter of the Systematics Association.

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Taxonomy's unexamined impediment

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For many years taxonomy has been a sick patient with many well-wishers but no competent doctors. A sign of progress, if it can be called that, is that the hand-wringing over taxonomy's dim outlook is no longer confined to taxonomists themselves. Worry has spread to ecologists, conservation biologists, and even to the flagship journals Science and Nature. The decade of the 1990s was supposed to be a turning point when taxonomy would finally receive the support it deserved to lead the effort to preserve Life on Earth. This was when the Biodiversity Crisis became the cause celebre and taxonomy was universally proclaimed the key to understanding and overcoming the crisis. Taxonomy benefited from some impressive initiatives during that time. The Costa Rica National Biodiversity Inventory was founded and received substantial international support from both government and private funders. Other initiatives, such as the National Science Foundation's PEET (Partnerships for Enhancing Expertise in Taxonomy), Tree of Life, and Biotic Surveys and Inventories programs, the World Bank Global Environmental Fund and some of its spin-offs like the GBIF (Global Biodiversity Information Facility) project, were all products of this decade. Nor should we forget the numerous meetings, concept papers (such as Systematics Agenda 2000 [1994]), and associated workshops that in aggregate consumed substantial amounts of time and resources to devise strategies for better improving the status of taxonomy and documenting biodiversity. Finally, we had the Convention on Biological Diversity (the Rio Convention; www.biodiv.org), which is about as close as you can get to an international mandate for a vastly expanded taxonomic initiative.

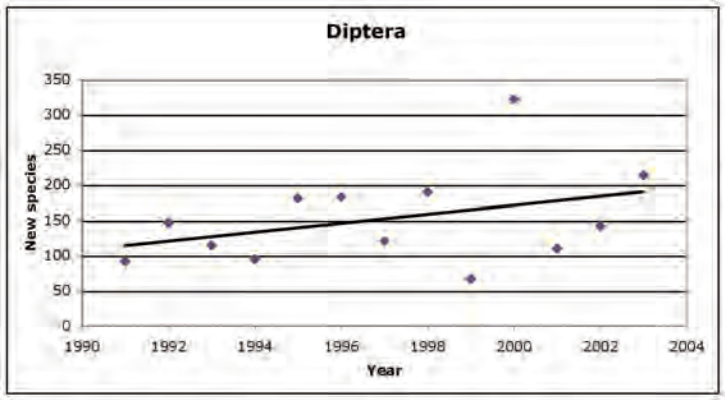

Figure 2. Number of new species of Neotropical Coleoptera described per year as recorded in the Zoological Record, 1990-2004. (Trendline applied by Microsoft® Excel.)

Despite all this support, in the past year we seem to be in the midst of another round of soul-searching over that perennial question of 'Whither Taxonomy?'. Although some claim that a new rosy future beckons (Wheeler 2004, 2005), other indicators are decidedly less optimistic (Blackmore 1996, Brown 2005, Stokstad 2005). Brian Brown's (2005) article in *American Entomologist* provides some sobering data on how the taxasphere has been meeting, or not meeting, the biodiversity challenge. Brown examined the status of the taxonomy of Neotropical Diptera over 7 years between 1997 and 2003 and found that, while there has been an unprecedented increase in numbers of specimens available for study, there has not been a concurrent upswing in the number of new species described. He attributed this to lack of support for Diptera taxonomists. However, his data also suggest that a decade-long period of good publicity and substantial

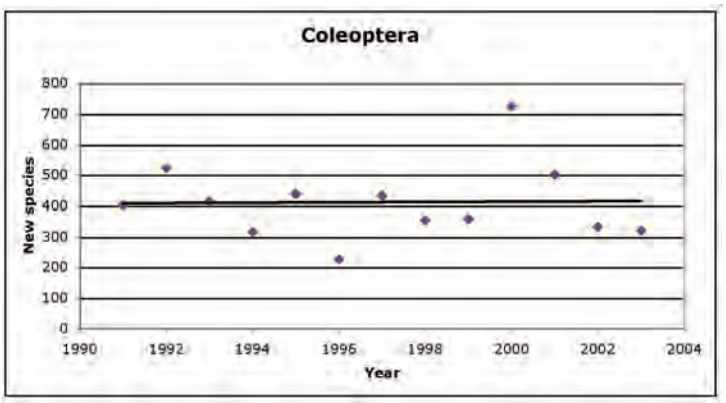

Figure 1. Number of new species of Neotropical Diptera described per year as recorded in the Zoological Record, 1990-2004. (Trendline applied by Microsoft® Excel.)

increases in funding have not translated into a commensurate leap forward in either our taxonomic knowledge of the world's biota or in the long-term health of the profession of taxonomy. Using different search criteria, Seberg (2004) found that publication rates of taxonomic works have been flat.

As a quick test of the generality of Brown's (2005) data, I counted new species descriptions in the Zoological Record for Neotropical Diptera from 1991 to 1997, extending Brown's data back to the beginning of the 'Biodiversity Decade'. Also, I counted new species descriptions for Coleoptera from 1991 to 2003. In theory, Coleoptera doesn't suffer from a relative lack of interested taxonomists, as Brown feels is the case with Diptera. While restricting the analysis to the Neotropical region is arbitrary, it is worth noting that this region has the two largest permanent tropical biology laboratories (La Selva in Costa Rica and the Smithsonian Tropical Research Institute in Panamá), and has been (as Brown notes) the locus of several large biodiversity grants. My thesis, then, is that there should be some observable increase in the description of new species if the last 15 years of taxonomy-related initiatives, projects, workshops, and technology has had any effect 'on the ground'.

Figures 1 and 2 show the results of these tabulations. Although Brown (2005) laments the slow pace of species descriptions in Diptera, this group does show a slight upward trend in species described (Fig. 1). In Coleoptera, however, despite its relative popularity and a much larger number of workers, the rate of new species discovery has been essentially flat (Fig. 2). It's actually worse: a single paper (Gordon 1999) accounted for over 200 new species for the year 1999. If his work is removed from the data, Coleoptera shows a negative species discovery trend during the 'Decade of Biodiversity'. This, by the way, despite two large and productive PEET grants for Neotropical beetles (Scarabaeidae [http://www.museum.unl.edu/research/entomology/products.htm#Publications], and Staphylinidae [http://www.nhm.ku.edu/ashe/aleo/]

The data indicate that 15 years of initiatives, programs, speeches, white papers, and some substantial financial support have resulted in no significant change in the rate of species discovery in two of the four mega-diverse insect groups (Coleoptera, Diptera, Hymenoptera, and Lepidoptera). If the taxonomic impediment were due solely to underfunding, one would expect at least a slight improvement toward the end of a decade's worth of increased support and generally favorable attention.

The Planetary Biodiversity Initiative (Wheeler 2004, 2005) is now the latest great hope for the 'taxonomic impediment'. But even if the PBI produces the advertised flood of new species descriptions (1,000 species per year for 5 years), it will still be only a small blip in the overall annual flow of taxonomic publications. Recent recommendations on how to 'fix' taxonomy and attract more public support have tended to focus on how to react to technological changes (Schram 2004; Wheeler 2004, 2005). We are told to embrace the Internet, use

molecular techniques (except anything that starts with CO1), get bigger, get more international. . . .

A frequently cited component of the taxonomic impediment is the shortage of taxonomists (Hubbell 2001; Wheeler and Cracraft 1997). While death, retirement, and lack of recruitment are all important factors, issues that are rarely mentioned are underemployment and the 'invisible poor' of taxonomy. By 'invisible poor' I mean the taxonomists who are often the only experts in a taxon, but who exist outside the inner circle of large institutions and large institutional grants. In a questionnaire Löbl and Leschen (2005) uncovered a rather startling fact: many and perhaps even a majority of taxonomists do their work at home and without financial support (in their largely European sample, 89% worked without funding and 91% did their taxonomic work at home). This apparently includes many who on paper have taxonomic duties as part of their employment responsibilities. Löbl and Leschen's (2005) questionnaire was directed at a completely different matter-attitudes of coleopterists to DNA barcoding and Phylocode-and the data on support emerged from their background questions to the respondents. The respondents to this poll were mostly European beetle workers, but this group makes up a very large and active subset of the world's taxonomists. The authors of the study acknowledge that the percentages they found for Europe may be different in the United States. Schram (2004:1) states that taxonomy was until recently a 'cottage industry'; Löbl and Leschen's (2005) data indicate that is still very much the case. Far from being a quaint but unimportant backwater of systematics, it may be that the 'cottage industry' is still what is actually driving systematics today, producing most of the alpha descriptions that are the essential ingredients for the flashier fields of phylogenetics, conservation, and biodiversity studies.

This has important implications for efforts to 'revolutionize taxonomy'. Taxonomists have been chided (Schram 2004:7) for 'defending to the death their island redoubts'. But when your island redoubt is all you have and all you're ever likely to get, of course you're going to defend it. To indulge in the metaphor of revolutions, those that include and benefit only a well-connected urban 'bourgeoisie' (in this case taxonomists in major institutions) and ignore the 'working class' (the unfunded and volunteer taxonomists) have little chance of any long-lasting success. An example of where our revolution may be going wrong is the recent wave of proposals for web-based taxonomy and databases such as GBIF (www.gbif.org), ZooBank (Polaszek et al. 2005), MorphBank (www.morphbank.net), or uBio (www.ubio.org). These initiatives (all of them about applying new technology to existing data rather than generating any more new data) mopped up a not inconsiderable fraction of the available money during the Biodiversity Decade. Despite their promises of eventually making a taxonomist's life easier, at the moment they only represent added IT chores for the many taxonomists working on a shoestring trying to publish their research. Promoters of these initiatives regularly speak of 'getting the taxonomic community to buy into' their projects, an expression which, translated, often means that the promoters themselves have no funds to spare and the taxonomist is expected to act as an unpaid data entry secretary.

A frequently cited component of the taxonomic impediment is the shortage of taxonomists. Issues that are rarely mentioned are underemployment and the 'invisible poor' of taxonomy

One of the truly innovative ideas that did come out of the Biodiversity Decade was the creation of parataxonomist programs to help with inventories of the huge tropical floras and faunas. Parataxonomist programs were set up in Costa Rica, New Guinea, and several other tropical countries. So far, the largest and most permanent parataxonomist program is the Costa Rica National Biodiversity Inventory (INBio). An INBio parataxonomist was 'a full-time collector and processor of specimens and natural history information for the inventory site in which he or she is resident and specialized' (Janzen 2004:182). In the late 1990's Costa Rica had more than 50 parataxonomists working on insects, mollusks, nematodes, plants, and fungi. However, although touted in numerous articles as one of the waves of the taxonomic future, INBio now stands at a crossroads with a clouded financial outlook (Stokstad 2005). By the time the current financial crunch that was the subject of Stokstad's article occurred, the parataxonomist corps at INBio had already been reduced to a shadow of its former size. The immediate cause of the problem was the end of two large grants and a changed world where mega-funding for biodiversity no longer exists. There was also a gradual decline in the high levels of participation and the sometimes incandescent enthusiasm on the part of the foreign collaborators. This triggered a downward spiral where money got tighter, not as many collaborators could spend as much time at INBio, and the administration, noting this apparent drop-off of interest, followed the unfortunate footsteps of many museums and diverted more of their resources toward the 'infotainment' model that Wheeler (2004) lamented.

In INBio today we may be seeing the future of many of the other innovations being advanced as the way to 'save' taxonomy: databases, uBio, ZooBank, World Wide Web-based

taxonomy. Sooner or later, these new cyber-gadgets will no longer be new but will still require substantial commitments in money and personnel to continue to keep them up-to-date and useful. Will administrators and taxonomists themselves keep up their initial enthusiasm for feeding databases and keeping Web pages current? Or will this become the next alpha taxonomy?

While it is easy to focus on the overall level of support for taxonomy (and find it wanting), it is decidedly less comfortable to examine the distribution of taxonomic support. The current funding situation for taxonomy, at least in the United States, is heavily skewed toward large museums and universities, and is focused on 'big' projects that involve an arsenal of high-tech, computer-intensive techniques. If this was the group doing most of the actual taxonomy, the current policy would be quite sensible. However, the evidence we have available suggests that the most productive segment of the taxasphere-taxonomists in smaller institutions, state agencies, retirees, and 'unsalaried' taxonomists-is effectively locked out of what financial support that still exists. The National Science Foundation PEET program, widely cited as the answer to the disappearance of taxonomists, has produced a respectable number of graduates and some very nice taxonomic products. But how many PEET graduates are able to do the full-time the taxonomic work they were trained to do? Landrum (2001) proposed changes in the funding priorities of PEET and other systematics programs that would have spread available resources more equitably around the functioning taxasphere. As of 2001, 40 PEET grants ranging from US$550,000 to $750,000 had been awarded. While Landrum applauds the good intentions of PEET and especially its brief to study the understudied among taxa, he notes that 40 labs are not sufficient to inventory the world's biota and the PEET money would do much more good if it was distributed at levels between $30,000 to $60,000, thereby supporting about 15 times as many taxonomists (Landrum 2001). Landrum's ideas, as far as I can tell, have fallen on deaf ears. Training the next generation of taxonomists does little good unless there is a next generation of jobs in taxonomy. Many, perhaps even a majority of taxonomists are living in a world where the benefits of NSF and GBIF rarely penetrate. Taxonomy is forced into the background of taxonomists' lives, workers quietly drift out of the profession, students trained in taxonomy claw for pitifully few positions, and many leave their master's and doctoral theses (unpublished) as their first and last contribution to taxonomy. This situation could quickly be reversed if all the institutions now paying only lip service to the importance of taxonomy and biodiversity (e.g., major research institutions that will only hire a taxonomist who agrees to spend all his or her intellectual energy on molecular studies, conservation organizations that demand hard data on biodiversity but expect to get it free off the Web or donated by already overextended taxonomic professionals, journals that demand high rigor in all aspects of methodology except taxonomy where any guess at a family-level identification is apparently sufficient for publication) would make even modest efforts to employ or otherwise financially support taxonomists.

The current prescriptions for curing taxonomy of its ills are not likely to have the desired effect because they focus on the wrong symptoms. The main thing holding back taxonomy is not so much lack of support as the exclusion of a large part of the taxasphere from what support there is. The electronic media and databases and molecular technologies that are prescribed by university and government movers and shakers are largely irrelevant to an unfunded taxonomist working at home. Nor are these 'cutting edge' toys that relevant to the users (and ultimate funders) of taxonomic research. Most users of taxonomy would like to be able to identify plants and animals (especially the vast numbers of still unstudied tropical plants and animals). Most taxonomists would be happy to work on these very biotas and provide the public with the identification tools they want and need. With few exceptions, most of the controllers of funding for taxonomy have relegated the needs of the public and the desires of a large portion of the taxasphere to a sort of second-class science. Should we be surprised that taxonomy is ailing?

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Meeting Report

Plants, people and evolution

In August this year the Linnaean Society hosted a meeting to celebrate the career of Dr Barbara Pickersgill. The meeting,, co-organised by Dr Mark Nesbitt (Royal Botanic Gardens, Kew), Dr Julie Hawkins and Ms Sarah-Jane Senior (University of Reading) was sponsored by the Systematics Association and the Annals of Botany Company. Barbara's career, encompassing a past presidency of the Society for Economic Botany, a Linnean Gold Medal, and more than 30 years at the University of Reading was celebrated by a full-house of 100 delegates. A remarkable number of Barbara's past students attended, both as speakers and delegates.

Delegates were welcomed to the meeting by Prof David Cutler, as President of the Linnean Society, and by Prof Vernon Heywood, Prof Emeritus University of Reading and long-time colleague of Barbara. The scientific programme was well received. From rice to wheat and cotton to cacti, speakers highlighted the complementary approaches which unlock the past of globally and locally important plant species. Evidence from archeology, ethnobotany, genetics, phylogenetics and reproductive biology was presented, the broad range reflecting Barbara's areas of expertise and influence over the years. Prof Spencer Barrett FRS, an ex-student of Barbara's (Barbara was co-author of his first published research paper) made a presentation to Barbara of Jean Andrews' original drawing of the 5 cultivated *Capsicum* species. The drawing forms the frontispiece of her Andrews' book *The Peppers Cookbook: 200 Recipes From The Pepper Lady's Kitchen*. A copy of the recipe book was also presented, both book and drawing wrapped in chilli pepper wrapping paper. The chilli pepper theme continued in the Library where delegates could admire some early publications on *Capsicum*, courtesy of The Linnean Society's archives, over drinks. The programme with abstracts is at www.kew.org/scihort/ecbot/ecbot-pickersgill.html.

SRF Reports

Meredith Murphy Thomas
University of Cambridge

I recently completed a very successful field trip to South Africa, thanks to the generous funding provided by the Systematics Research Fund. My trip brought me to the Namaqualand area of Northwest South Africa for five weeks during its world famous spring wildflower bloom. On my trip, I collected material of the endemic daisy, *Gorteria diffusa*, and was able to observe all of the various morphotypes of this species across its range (roughly 300 miles by 1000 miles). *G. diffusa* exhibits a striking flower spot at the base of some of its ray florets, which has been shown to attract its pollinator,

the bee-fly *Megapalpus nitidus*. The size, shape, colour, and cellular composition of the flower spot is highly variable between the different populations of *G. diffusa*, and the various forms elicit different behaviours in the pollinator. Some unique forms have even recruited alternative pollinators. The different populations of *G. diffusa* maintain very clear boundaries between each other, with only the occasional narrow hybrid zone. With my colleague, Allan Ellis of the University

of KwaZulu-Natal, we were able to conduct insightful pollinator choice experiments, collect samples for an AFLP fingerprinting analysis of the various populations, and compile a valuable image database of the various morphotypes. Look out for upcoming original papers on the fascinating dynamics of this plant/pollinator relationship!

Murphy Thomas is a PhD student based at the University of Cambridge with Dr. Beverley Glover in the Department of Plant Sciences. Her co-supervisors are Dr. Vincent Savolainen and Dr. Paula Rudall at the Royal Botanic Gardens, Kew.

Wolfgang Wüster
University of Wales, Bangor

The aim of this grant was to contribute to the resolution of the systematics of New Guinea's death adders by the collection of further samples for mtDNA analysis, with specific focus on the northern part of the island, which had not been sampled in previous studies

The applicant visited Papua New Guinea from 5 September 2005 to 4 October 2005. Fieldwork was carried out at several sites in Madang Province along the North Coast of Papua New Guinea. Samples of death adder (genus *Acanthophis* were collected from Bogia and Karkar Island. Additionally, through contacts made during the visit, the applicant was able to gather additional specimens from Southern Highlands, Central, Gulf and Western Provinces, leading to a much more comprehensive coverage of the country.

The mtDNA haplotype tree revealed by sequencing of the cytochrome b and ND4 mitochondrial genes is shown in Figure 1. The tree obtained demonstrates clearly that all Acanthophis sequences from New Guinea except *Acanthophis rugosus* from the Merauke area of West Papua, Indonesia, group together. Western Province constitutes the terra typica

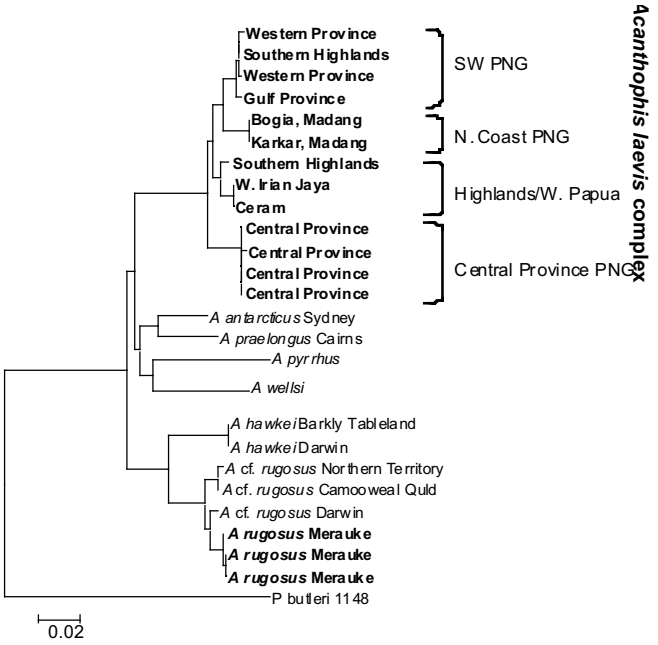

Figure 1. Neighbour-joining tree of death adder (*Acanthophis*) haplotypes. Taxa in **bold** represent New Guinea populations. Note that all New Guinea populations except *A. rugosus* are monophyletic. Bracketed taxa are part of the *Acanthophis laevis* complex.

of *Acanthophis laevis*, the oldest name available for this cluster of populations, which we therefore refer to as the Acanthophis laevis complex. Multivariate analysis of morphological data (not shown) confirms this finding. Although species limits within this complex require further investigation, our work has provided clear evidence that, with the exception of *Acanthophis rugosus*, all death adders in New Guinea do in fact belong to a single, reasonably cohesive clade, despite considerably variation in external morphology. Previous workers had suggested that different populations of this complex had closer affinities with Australian forms than with each other - our data clearly refute this hypothesis. The SRF grant has thus allowed us to construct a clear phylogenetic framework for New Guinea death adders, and thus the underpinnings for detailed research into species limits and the historical biogeography of the *A. laevis* complex.

The data gathered here provide an

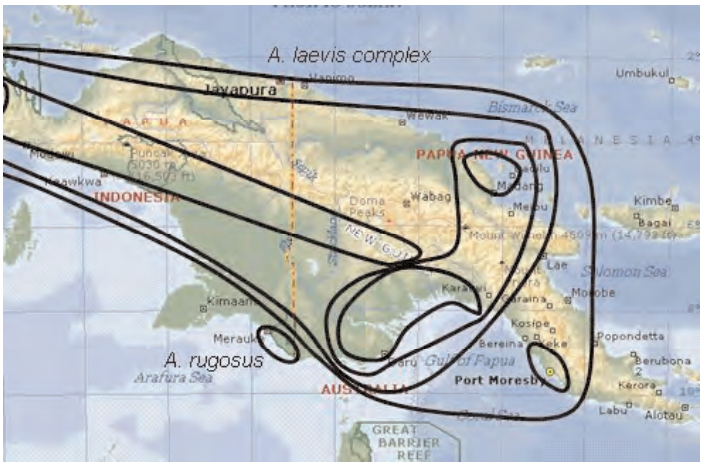

Figure 2. Phylogeographic pattern in New Guinean death adders Note that all populations except *A. rugosus* form a monophyletic group, and the sister group relationship between north coast and southwestern PNG haplotypes.

essential phylogenetic framework for the New Guinea death adders. However, given the physiographic complexity of the island, a more detailed picture of patterns of genetic variation is desirable prior to publication. For this reason, the applicant is still in the process of acquiring and analysing additional samples of death adders from other parts of Papua New Guinea to generate a more comprehensive analysis. Additional samples from West Papua (Indonesia) are being analysed, and collaborative research links forged during the applicant's visit to PNG are already yielding additional material from areas not currently covered. We have therefore held back from publishing the existing data until a more comprehensive analysis can provide the data for a more paper in a higher impact science journal.The phylogeographical pattern revealed in our tree is of considerable biogeographical interest, in that, surprisingly, the Madang (northern coast) populations cluster closely with populations from the southwestern part of PNG, on the other side of the mountains, whereas populations from Central Province form a very distinct cluster that forms the sister group of all other populations of the *A. laevis* complex. The phylogeographic pattern is shown in Figure 2. Interestingly, patterns of morphological variation are incongruent with the phylogeographical pattern, populations from the northern coast being more similar to those from Central Province than to those from southwestern PNG.

Wolfgang Wüster is a lecturer in the School of Biological Sciences, University of Wales, Bangor, U.K.

Zoltán Korsós
Hungarian Natural History Museum, Budapest

The purpose of my visit was to work on the millipede (Diplopoda) collection of the Canterbury Museum, in collaboration with Peter M. Johns, Research Fellow, who, in the past forty years, accumulated a considerable zoological material covering almost the entire territory of New Zealand. During previous discussions (we also met in 1995 when I carried out a two-month study trip to NZ), we selected an endemic and little known millipede genus, the *Eumastigonus* Chamberlain, 1920 (Spirostreptida: Iulomorphidae), for taxonomical revision; there were eight species described up to now, and about 30 more are expected as new for science from NZ.

Table 1. „Species groups" of the studied material belonging to the genus *Eumastigonus.*

„SPECIES"	TUBES ASSOCIATED
E. insulanus	40
close to insulanus	5
E. distinctior	3
close to distinctior	8
E. kaorinus	10
E. hemmingseni	7
E. kaikourae sp. n.	30
E. hallelujah sp. n.	20
E. otekauri sp. n.	18
E. „unilineatus" sp. n.	18
E. „Vinegar Hill" sp. n.	4
E. „Wanganui" sp. n.	5
E. „totara" sp. n.	12
E. „Goose Bay" sp. n.	9
several sp. n. (dubious?)	15
new genus (not Eumastigonus!)	2
North Island females	12
unidentified females	13
Total	231

During my four-week stay in the Canterbury Museum, I studied about 400 vials (16 jars, with cca. 25 tubes each) amounting to about 2000 specimens (5 individuals in every tube, in average). They were all properly labelled as regards collector, locality and date; additional details on habitat and distribution were provided by Peter M. Johns. The collecting localities represented almost the whole New Zealand. In the limited time given, the entire material could not be described and identified; however, the individuals studied were grouped into 14 „species" (see Table 1). A general introduction to the taxonomy of the genus *Eumastigonus*, and descriptions of two selected („compact") new species for science (*E. „kaikourae"* and *E. „hallelujah"*) are being prepared as a joint manuscript for publication in a high standard international scientific journal (Zootaxa).

In addition to the laboratory (col-

Table 2. Collecting localities, in collaboration with Peter Johns.

Region	**Township** (locality)	**Geographical coordinates**	**Altitude** (m a. s. l.)	**Habitat**	**Date** (2006)
Canterbury	Christchurch, West Melton	S43°31' – E172°22'	187	in garden	13-19 May
Canterbury	Rakaia Gorge flood terrace	S43°31'– E171°39'	230	*Sycamore–Cordyline* mixed forest	20 May
Canterbury	Rakaia Gorge walkway	S43°31'– E171°39'	280	degraded broadleaf shrub	20 May
Banks Peninsula	Hinewai Reserve	S43°48' – E173°01'	460	*Nothofagus fusca* remnant	21 May
N Canterbury	Hope River, Engineers Camp	S42°33' – E172°21'	525	*Nothofagus fusca* forest	27 May
N Canterbury	Hanmer Springs, Jollies Pass	S42°29' – E172°52'	825	*Nothofagus cliffortioides* forest	27 May
N Canterbury	Porters Pass	S43°17' – E171°44'	946	mountain tussockland, under stones	28 May
N Canterbury	Craigieburn Forest Park	S43°09' – E171°43'	821	*Nothofagus cliffortioides* forest	28 May
Kaikoura	Hapuku Scenic Reserve	S42°18' – E173°43'	55	coastal broadleaf forest	3 June
Kaikoura	Mt Fyffe Conservation Area	S42°21' – E173°34'	192	coastal broadleaf forest	3 June

lection) work, with the kind help of Peter M. Johns, I also carried out field collectings during the weekends. Altogether we visited eight places (see Table 2), including two (Kaikoura and Craigieburn) which proved to be type localities of the two proposed new species (*E. „kaikourae"* and *E. „hallelujah"*, respectively).

On June 1st I presented a talk for the Museum staff with the title „Challenge in the 21st century: the Hungarian Natural History Museum, Budapest", in which I gave a short summary of the history of the Budapest Museum, a general overview about its collection and research organization, as well as an outlook into the future.

I feel that my visit was very successful; in the short time I spent in the Canterbury Museum, additionally supported by the Systematic Association (The Linnean Society, London), we achieved a great progress in the systematic collection of millipede specimens belonging to the family Iulomorphidae, New Zealand. In collaboration with Peter Johns, and based on extant material, we clarified the taxonomical position of the genus *Eumastigonus*, prepared of the description of two new species, and sorted out the material in species groups for future study. To continue the project, we are looking to funding possibilities for another visit in 2007.

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Details of the SA research grants, conference bursaries and funding for the organisation of meetings can be found at: www.systass.org

Report from the President

Current status and objectives of the Systematics Association

Richard Bateman

Biosciences Federation and Royal Botanic Gardens Kew

It has become sufficiently de rigeur for incoming presidents of the Association to review its current status that I would feel uncomfortable breaking that continuity. In fact, this extended article is based on a document that I first drafted as President-elect in September 2006, when presenting the Association's credentials as a potential founder member of a new Europe-wide systematics organization, BioSyst Europe (issues move sufficiently rapidly in politics that BioSyst Europe has already provisionally agreed to heavy involvement in the 2009 biennial conference).

This document also informed, and has in turn been informed by, a series of quarterly working groups established in January to 'horizon scan' in the specific areas of leadership, events, finances and publications. In preparation for these working groups, at its January meeting Council reviewed progress made in the wake of the Association's Williams report and of the ensuing subcommittees (late 2003-early 2004) and the joint Association/Linnean strategic working group (early 2005). It was heartening to realize that most of the recommendations of previous committees had ultimately been enacted, and it proved reasonably straightforward to immediately enact others that were designed primarily to improve the modus operandi of Council.

In reviewing our current position it is important to distinguish between areas of the Association's activity that operate satisfactorily (e.g. the Biennial conferences) from those where improvement may be desirable. Given that we currently have, and are likely to continue to have, limited resources in terms of both finances and manpower, it would prove difficult to promote radical new areas of activity; indeed, initiating a new activity may well require abandonment of an existing activity, and there are no obvious candidates for the axe in the following account. Moreover, the goals listed below are strongly interconnected in a web-like fashion, which makes them very difficult to discuss (or modify) in isolation. It is, perhaps, primarily a question of prioritizing where we choose to exert tension on the existing web.

Mission

To promote all aspects of systematic (comparative) biology - including microbiology and palaeontology, professional and amateur, theory and practice - through conferences, workshops, publications, grants and the media.

Activities and output

Most of the areas outlined below are currently under active review.

(1) Biennial international conference

Established in 1997, international biennial conferences have thus far been held, in chronological order, in Oxford, Glasgow, London, Dublin and Cardiff. Plans for the 2007 biennial in Edinburgh (28-31

August) are well-advanced. The 2009 biennial will be the first to be held in 'mainland' Europe, specifically in Leiden (and will conclude the tenure of the current President). The steering group for the Leiden meeting was recently established under the chairmanship of Prof. Erik Smets (Director, National Herbaria of the Netherlands); the meeting will take place in a state-of-the-art conference centre that is currently under construction, and is provisionally scheduled for 11-14 August. We estimate that it will attract twice the usual attendance, as it will be held jointly with the German, Austrian, Swiss and most likely the French systematics organisations.

Organised and advertised well in advance (the posters have become collector's items), Biennial meetings typically attract ca 200 attendees from a global catchment, about half originating in Britain. All attendees are encouraged to present talks, typically resulting in a programme of two (occasionally three) parallel sessions (those planned for Edinburgh concern floras/faunas, speciation mechanisms, and the diversity of research projects currently sponsored by the UK's research councils). On each day one of the sessions is likely to be a themed symposium organised by invitation, since one such symposium is typically run on each of the three days of the meeting (see below for discussion of the breadth of acceptable topics). Student attendance at the meetings is subsidised by competitive bursaries (currently 10-12). One perceived problem regarding the Biennial meetings is that they have become increasingly dominated by botanists, though fortunately some of our sister organisations in Europe are reputedly dominated by zoologists!

(2) Published Systematics Association symposia

Organisers of one- to three-day symposia approach the Association for assistance with both funding the symposium and publishing the resulting volume. If accepted by both Council and publishers, the organisers are immediately awarded funds (normally £2000). We typically fund three to four such meetings per annum, though we are concerned that these are increasingly concentrated during the Biennial conferences. Symposia are often held jointly with other societies, most commonly the Linnean Society. Council would like to adopt a more proactive stance to symposia, commissioning meetings in priority areas (e.g. the role of taxonomy in underpinning studies of climate change).

(3) Other symposia

As part of its wider brief to promote systematics, the Association offers no-strings sponsorship to organisers, either within or outside the Association, who plan to hold systematics-related meetings but do not wish to publish proceedings with the Association (or do not wish to publish them at all). We typically fund three or four such applications a year to a maximum of £1000 (typically £500), though it is questionable whether this level of expenditure can continue; it may be preferable to divert some resources to published symposia (see #2).

(4) Annual lectures

Two prestigious annual lectures are given by invitation of council. The recently established Huxley lecture takes place in the summer and celebrates a selected aspect of evolutionary biology. The long-standing winter lecture immediately follows the AGM and is more eclectic; every third year this presentation is given by the outgoing President. It has recently been suggested that a third annual lecture should be instituted, concerned with policy-related aspects of systematics and biodiversity studies.

(5) Young Systematists Forum

Held in one of several London venues the day after the Association's AGM since 1999, the Young Systematists Forum is a one-day meeting designed to encourage several (usually 12-16) postgraduate students to present their research in an informal setting to an informed audience. Typically held at the NHM, the audience increased substantially in 2006 due to a welcome influx of Continental postgraduates.

(6) Workshops and educational outreach

The Association currently has little activity of this type. However, during the 1990s, it collaborated with the Natural History Museum to run a successful series of week-long workshops on cladistic methods. There clearly remain opportunities to develop new systematics-related training courses and/or textbooks, which offer the prospect of increasing both our impact and our revenue.

(7) Symposium volumes (cf. #2)

Since its inception the Association has produced, or is producing, 77 edited volumes derived from symposia; that is, two or three per annum. Typical sales are 1000 (until recently published as hardback plus softback) for conceptual volumes and 500 (hardback only) for taxon- or biome-based volumes; the two categories of volume have been produced in roughly equal numbers, though understandably, conceptual volumes are now beginning to dominate. Our current publisher is Taylor & Francis/CRC. Contemporary discussion topics include pressure from publishers to increase prices too rapidly and/or to abandon overly specialist volumes, while Council aims to maintain good production quality and to increase numbers sold via a sensible pricing strategy.

(8) Other volumes

The Association has also produced 11 stand-alone volumes on specialist technical topics. The only such vol-

ume published in recent years, the well-known cladistics manual, has sold several thousand copies over a long lifespan, suggesting that a significant market still exists for what are, in effect, specialist textbooks (cf. #6).

(9) Newsletter

Established in 1993, two or three issues per annum of the Newsletter have been circulated during each of the last ten years. It has gradually evolved to its current format of 24 glossy pages in black-and-white. It incorporates Association business, reports of conferences (including those organized by the Association), reports of field trips (including those financed by the Association), and book reviews (including those published by the Association). Perhaps most significant in recent years have been well-informed opinion pieces concerning issues such as molecular systematics, DNA bar-coding, web-based taxonomy, human origins and vicariance biogeography; these articles encourage citation (both formal and informal) of the Newsletter.

(10) Electronic-only taxonomic journal

Our sister organization, the Linnean Society, has long published three successful journals (with a fourth likely to be added), but it has effectively ceased to publish stand-alone volumes. In contrast, the Association focuses on publishing stand-alone volumes but has never produced a journal. However, concern jointly expressed by the Association and the Linnean Society over the increasing difficulty of publishing basic descriptive taxonomy has led us to discuss at length the possibility of inaugurating a new electronic-only journal for this purpose. Should the decision be made to implement this scheme, the initial focus is likely to be botanical (cf. Zootaxa) and the initiative to be led by the Linnean Society, as the Association presently lacks the necessary resources.

(11) Website [www.systass.org]

The website has become the Association's primary method of disseminating information and documentation; for example, the Newsletter and conference funding application forms can be downloaded, applications for the Systematics Research Fund and CoSyst grants schemes can be uploaded, and the schedule of meetings is regularly updated. Recently upgraded by Webmaster Rupert Wilson, the Association's website is now linked to those of several other systematics-related organizations. Further upgrades are currently under discussion.

(12) Systematics Research Fund

The Systematics Association Small Grants scheme - more recently amalgamated with most of the Linnean Society's previous grant schemes to form the Systematics Research Fund (SRF) - has, through a rotating panel of six assessors, made 12 rounds of annual awards since its widely welcomed reappearance in the autumn of 1995. In early years, annual awards totalling ca £6,000 were typically made to 8-10 of the 30-35 applicants, who were dominantly from the European Union. By spring 2007, awards totalling £29,000 (derived from four sources) were made to 32 of the 94 applicants, who represented a truly international constituency. Thus, allowing for inflation, the scheme has triumphantly trebled in both size and geographical reach, though the percentage of successful applicants has steadfastly remained one quarter to one third. In 2007, applications switched to a fully electronic format, in the wake of a similar development for the new CoSyst initiative (cf. #13).

(13) CoSyst: collaborative research fund for systematic biology

The Association and Linnean Society have over the last year negotiated an agreement with the main biological research councils in the UK, BBSRC and NERC, to receive over three years a total of £225,000 to disburse to research groups that link systematists with non-systematists. The primary goals are to embed systematics more firmly in biology by generating new collaborations through grants of up to £20,000. The resulting seed-data will increase the probability of funding for subsequent proposals that include systematics when they are subsequently submitted to the research councils for full research grants. This scheme is available only to UK-based systematists via the Association's website, and is assessed by a panel of six senior systematists presently chaired by Julie Hawkins. The first round attracted 48 applications of excellent mean quality, but even with a generous supplement from the Linnean Society, funds allowed funding of only seven (15%) proposals.

(14) Awards

Prizes are awarded annually at the Young Systematists Forum for the best oral presentation and best poster, and every two years at the Biennial meeting for the best student lecture and best student poster. The panel of assessors is usually convened by the Chair of the Grants & Awards Committee.

(15) Policy-related activities

This is an area that may well have been under-exploited by both the Systematics Association and the Linnean Society. The two organisations have occasionally combined to represent systematic biology in some key fora, such as two reviews of the UK systematics base conducted by House of Lords Select Committees in 1991 and 2001.

Over the last three years, the Association and Linnean Society have together provided the only organismal biologist to sit on the Executive Council of the Biosciences Federation, which now

represents, either directly or indirectly, almost 70 scientific societies in the UK and has become the primary independent voice on UK policy in the biological sciences. The Federation is currently becoming more involved in European policy-making. As its present part-time Head of Policy, the SA President has been exploring new opportunities to promote systematic biology in the political arena. The Association will shortly be canvassing its membership for people willing to act as policy advisers at short notice.

In addition, in September 2006, he attended a meeting in Vienna that was convened primarily to establish a new pan-European umbrella organization for systematics, provisionally titled BioSyst Europe, that aims to represent a broader constituency than the existing CETAF.

Finances

The Association has always operated on very limited funds. Its total assets peaked in the early 1990s when they exceeded £100k; they currently stand at ca £70k. However, turnover is relatively high, reflecting the frequency and range of activities supported by the Association (#1-15 above). The main sources of income are membership fees (£10 pa), royalties from published books, and a modest surplus from the Biennial meetings. The main outlays are Biennial meetings, other scientific conferences, and co-financing the Systematics Research Fund.

Although they are declining only slowly, finances have become a cause for concern in the Association (for example, turnover for a single Biennial meeting usually exceeds £40,000, leaving our meagre reserves temporarily depleted and presenting cash-flow problems). Although this situation has been temporarily relieved by a one-off £20,000 grant kindly donated by the Mingulay-Prewell Charitable Trust and negotiated by present SA Treasurer Colin Hughes, our existing resources in no way match the breadth of activities currently pursued by the Association.

Membership and Structure

The Association has a notional membership of ca 500 but a paid-up membership of only ca 350. The figures are fairly stable; modest peaks coincide with the biennial meetings, presumably reflecting the discounted registration fees available to members.

Council meets for approximately three hours, four times a year, at the Linnean Society in Piccadilly, plus the one-hour Annual General Meeting. The current organisational structure was largely established in 1996 and revised in 2003. It consists of the President (competitively elected), Secretary, Treasurer, Membership Secretary, Meetings Committee Chair, Publications Committee Chair, Grants & Awards Committee Chair, Biennial Committee Chair (when appropriate), President-elect (when appropriate), and ca 18 other members (appointed via co-option where necessary). Each councillor serves a three-year term on council, which operates democratically. Committees of typically six councillors meet as and when required. No councillor is paid a salary, and councillor's expenses are minimised. As of February 2007, their biographies appear on the SA website and Newsletter.

Relationship with the Linnean Society

The Systematics Association separated from the Linnean Society of London in 1937. It was founded by a coterie of advocates of the 'New Synthesis' led by Julian Huxley, who had become frustrated by what they regarded as the hidebound traditionalism of the Linnean Society. They tended to be younger and more active, and were more likely to be professionals, than the average Linnean Society member, as well as being far less well-resourced. Arguably, these generalised statements remain true today.

Setting aside its revolutionary origin, the Association has in recent years worked increasingly closely with the Linnean Society. Most of the Association's meetings, other than the Biennial and Young Systematists Forum, are held in the Linnean Society's prestigious rooms in Burlington House, Piccadilly in return for subsidized fees. The Association's archive is held in the Linnean library. Each organisation effectively maintains an observer on the other's council. The organisations collaborate in funding and managing the Systematics Research Fund and, latterly, CoSyst. A joint SA/LS temporary Systematics Working group was established at end-2004 and first reported in March 2005. There are no current plans for similar initiatives, though there are obvious opportunities in areas such as the proposed annual Policy Lecture and the development of a database of UK expertise in systematics policy.

Book Reviews

The giraffe's long neck: from evolutionary fable to whole organism

Holdrege, C. (2005). Nature Institute Perspectives 4. The Nature Institute, Ghent, New York. ISBN: 0974490636 (paperback) US$14.00.

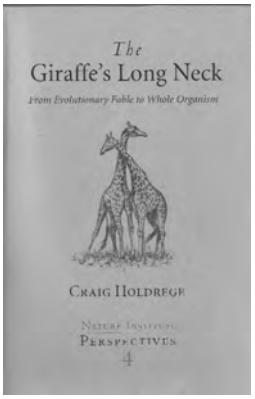
The Giraffe's Long Neck is a unique contribution that views the organism through the experience and perspective of the naturalist. Such books are rare. The majority of texts dealing with single organisms read like suspenseful survival stories, discussing

where an organism comes from and whether, given the vast upheavals in climate, they will make it to the end of the book. Holdrege, on the other hand, offers the reader insights to understanding the form of the giraffe and, by extension, invites us to consider all organisms in context.

The book consists of four chapters that cover existing theories of giraffe form, notably its long neck, physiology, development, ecology and evolution. The text is interspersed with elegant black and white line drawings and the text is written in an easy conversational style. The first chapter "Evolutionary stories falling short" is a critique of present evolutionary theories in relation to the giraffe's neck. Like most, I recall being taught sometime in school that the giraffe's long neck is an example of an adaptation, an explanation that championed neo-Darwinian theory over "inferior" Lamarckianism. In our texts there was a picture of an upright giraffe apparently feeding, with the story that by having a long neck the giraffe could reach the rare green foliage and therefore survive. Since no one in class experiences the giraffe directly, we accept the story and go no further. This is the starting point for Holdrege's argument. In considering the giraffe feeding it appears to have a long neck. With legs splayed cumbersomely to reach down while drinking, giraffes appear to have awkwardly short necks (Fig 1). What do we as taxonomists gain from this insight? Holdrege has exposed advantageous adaptation as an evolutionary fable, showing that in focussing on a single part of the giraffe we have lost sight of the whole organism.

Holdrege is no anti-evolutionist. His pursuit to discover the whole organism and its relationships to other ungulates is more cladistic than Mayrian. Chapter 2 shows Holdrege's ability to clearly communicate the anatomy and taxonomy of the giraffe to a general audience as well as to enlighten the evolutionary biologist, especially in matters concerning the history of comparative biology.

Chapter 2 is an intense study of the giraffe's anatomy and how it relates to the whole. The relationship between its long neck and long legs is a configuration that gives the



impression of a "long" neck during feed up high and a "short" neck whilst drinking. This directly challenges the "adaptational" point of view, which suggests that the giraffe has a long neck - an extension within a generally normal ungulate body. As Holredge points out, the giraffe seen as a whole organism also has long legs, in fact the whole animal is vertically lengthened, which is correlated to a horizontal shortening.

Chapter 3 concentrates on the giraffe and its landscape. The chapter is interwoven with black and white photographs and sketches depicting the giraffe's movement and behaviour such as "necking". The last part of the chapter is dedicated to "feeding ecology" and the giraffe's relationship to the Acacia dominated habitat.

The final chapter is perhaps the most thought provoking in the book. Holdrege challenges the current trend within evolutionary biology, which is to provide convenient explanatory mechanisms, which ends up "replacing the true complexity of the matter with a simplified scheme" (p. 83). I couldn't agree more. Holdrege refers here to the practice of reducing complexities of relationship into unidirectional scenarios along the lines of those "who begot whom" stories of kings of old. The challenge is to recognise that the current emphasis on framing and favouring certain theories can actually hinder our understanding of an organism. Significant increases in our knowledge of the groups we study is to be found in close examination of the many complex relationships between organisms, discoverable through our experiences and in the patterns that we find. Erasing that 'tree of arrows' diagram that my biology teacher drew on the blackboard, tracing fish to amphibian, amphibian to reptile and so on until it pointed at us, begins with a change of outlook. If only we interrogate and seek out the patterns, we can know that fish, amphibians and reptiles are not actually natural groups and that there are far more "complex" relationships at stake.

"We can no longer look at animal evolution as the outcome of the interaction of casual mechanisms. We are always led back to beings that evolve in relation to other beings and to the many inorganic forces of nature. Beings interact and co-evolve; yet each evolves in its particular fashion. No being is reducible to something else. This is central riddle of life on earth, of development, and in the end, of evolution" (p. 92).

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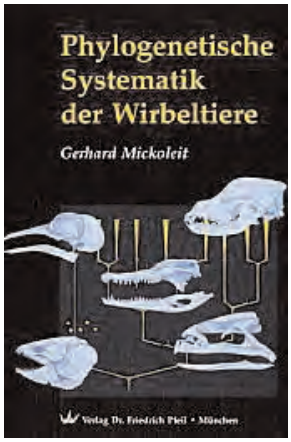
Phylogenetische Systematik der Wirbeltiere

Mickoleit, G. (2004). Verlag Dr. Friedrich Pfeil, Munich.ISBN: 3899370449 (hardback). €98.00.

Comparative biologists who cannot read German may find, with this remarkable book, an excellent reason to learn it! In his short preface,

Gerhard Mickoleit explains how his book was initiated. Back in the 1970's Willi Hennig (1904-1975), the founder of phylogenetic systematics, had a project of writing a series of compendia entitled Taschenbücher der Zoologie (Pocketbooks of Zoology), and entirely structured according to his phylogenetic principles; that is, the



search for sister-group relationships based on shared derived characters. The first two volumes (in fact volumes 2 and 3) on invertebrates were actually published and re-edited several times, the latest editions being supervised by Hennig's son Wolfgang and Gerhard Mickoleit (Hennig 1984, 1986). Before his death, Hennig had already prepared a manuscript for a third volume on chordates, which his son managed to get published in the journal Zoologische Systematik und Evolutionsforschung (Hennig 1983).

However, and although it is a remarkble piece of work, this manuscript lacked illustrations and badly needed updatings. Mickoleit was then asked by the publisher Gustav Fischer (Jena) to prepare a more complete and illustrated version of the unfinished chordate volume, but focusing exclusively on vertebrates, and designed on the basis of Hennig's conception of his "Pocketbooks". Meanwhile, Gustav Fischer (Jena) merged with Spektrum and the "Pocketbooks" project was abandoned. For many years, and despite the enormous work Mickoleit had taken on by doing numerous illustrations himself (many are original, and based on dissections, or redrawn to be made more didactic) and by improving his text as new data came out, the manuscript remained in limbo.

Thanks to the publisher Dr. Friedrich Pfeil, well known by palaeontologists and comparative biologists in general for the high quality of the books he produces, this manuscript is now finally published. Mickoleit cared for retaining the spirit of the two volumes published by Hennig, which were designed as compendia for students. It is essentially a list of taxa presented in a phylogenetic order, with a description of their autapomorphies. Additional characters ("weitere Merkmale"), which may be either debated or ambiguous, are mentioned for most taxa, as well as a brief dicussion of conflicting theories of relationships. Remarkably, the table of contents at the beginning of the book is presented in the form of a very large phylogenetic tree, which helps the reader in immediately localizing a particular taxon in both vertebrate phylogeny and the book. Mickoleit wants the book to focus almost exclusively on morphology, with only occasional allusions to physiology, ecology or behaviour, when the latter may be regarded as corresponding to an important phylogenetic signal. Fossil taxa are only mentioned in passing, when they illustrate important stem groups, as in the case of stem gnathostomes, stem tetrapods, or stem mammals, and help in resolving an ambiguous homology relationship. Each section dealing with a particular major clade includes a tree, where debated relationships are indicated by a polytomy, but discussed in the text. The difference with the two invertebrate volumes written by Hennig lies in the more abundant and outstanding illustrations, but also (unfortunately so!) the weight of the book (1.9 kg!), which makes it no longer a "pocketbook".

This being said, we are grateful to Mickoleit for providing us with such a superb volume, which is far more than a "compendium". It is a splendid heritage of the old German tradition in comparative anatomy, repatterned according to the Hennigian spirit. Like many vertebrate morphologists I always enjoy opening at random a volume of the Bolk et al.'s (1931-1939) Handbuch der vergleichenden Anatomie der Wirbeltiere, and spend hours sorting the various states for one particular kind of character out of such an enormous mass of mixed data (even though I'm aware that some data are outdated), and reconstructing trees of stomachs, guts, brains, or spleens. It is a very austere and exclusive pleasure indeed, but a good training for the memory. Now, after the Hennigian revolution in systematics, we have Mickoleit's book, with all characters sorted according to Hennig's principles. Perhaps, no more fun with Bolk et al., but what a gain of time!

By comparison to other textbooks on vertebrate anatomy and/or phylogeny that have been published during the past fifteen years or so Mickoleit's volume is unique...in the extremely clear phylogenetic presentation of character distributions

By comparison to other textbooks on vertebrate anatomy and/or phylogeny that have been published during the past fifteen years, or so, Mickoleit's volume is unique in avoiding mixing widely different kinds of data, and in the extremely clear phylogenetic presentation of character distributions. In his preface, Mickoleit says that he did not want do enter the controversies raised by molecular sequence-based

phylogenies during the past ten or fifteen years. His decision is perfectly justified: he is an outstanding anatomist and, alas, one of the last ones, but may not have the same competence for assessing the respective qualities or failures of the numerous molecular sequence-based trees of the vertebrates (or major vertebrate taxa) published every year. Mickoleit thus considers that it is best is to let molecular phylogeneticists see how the patterns of their own trees cope with his morphology-based tree. Therefore, when dealing with some nodes that are the subject of controversies between morphologists and molecular phylogeneticicts, he carefully explains the complexity of certain characters, regarded as synapomorphies, in order to justify the unlikeliness of their being homoplastic. Mickoleit belongs to those comparative anatomists who rely more on the quality of the characters; that is, complex characters as more reliable indicators of phylogeny than tree statistics derived from very large and often unchecked data matrices.

Apart from Arnason et al's (2004) mitochondrial DNA-based tree, which is strongly at odds with the current sonsensus about gnathostome interretationships, most previous molecular sequence-based vertebrate phylogenies generally agree relatively well with the morphology-based ones, at any rate at the level of the relationships of the major clades. In fact, only four major unresolved nodes of vertebrate phylogeny remain the subject of controversy between molecular phylogeneticists and morphologists, and even between morphologists themselves: the hagfish-lamprey-gnathostome node, the ginglymod-halecomorph-teleost node, the coelacanth-lungfish-tetrapod node, and turtle relationships among amniotes. At a lower level, there are, of course, many other problematic nodes, such as the osteoglossomorph-elopomorph-clupeocephalan node among teleosts, or the frequently re-patterned molecular phylogeny of placental mammals. More sequences of more genes from a larger number of species and analyzed with different methods may, in the future, help in resolving these nodes by providing a supposedly very strong phylogenetic signal (or statistical support, such as in the Bayesian method), as it seems to be currently the case for the hagfish-lamprey-gnathostome node. However, will this help in understanding what actually happened with the characters, notably when accepting a sequence-based phylogeny entails a large number of reversals or homoplasies that morphologists regard as unlikely?

The only slight probem I met with in this otherwise outstanding book is perhaps the use of certain names for higher taxa, which differ from those that are widely used since the 19th century

Development remains the third source of information which could help in justifying such an appeal to transformation processes. Palaeontology once allegedly also had the power of "telling the true story" by means of series of "intermediate forms" ordered in time, but has it ever helped in resolving major controversies either beween morphologists, or between morphologists and molecular phylogeneticists? As to vertebrate phylogeny, there are indeed a few instances where it did. In the 1980's, there was a widespread Leitmotiv that fossils never, or rarely, overturn patterns of relationships based on extant taxa (Patterson 1981), and this infuriated most palaeontologists. At that time, the morphology-based phylogeny of the extant vertebrates, was regarded as relatively stable, and more or less with the same general structure as presented in Mickoleit's book. Controversies often came from the non-phylogenetic consideration of fossil taxa, with the curious aim at demonstrating the existence of widespread homoplasy (e. g., Jarvik 1981). Nowadays, this alleged stability of vertebrate phylogeny is somewhat troubled by the proliferation and sometimes wide range of diversity of sequence-based phylogenies, which are of course based on extant taxa. In such cases, which particular tree could fossils possibly overturn?

The only slight problem I met with in this otherwise outstanding book is perhaps the use of certain names for higher taxa, which differ from those that are widely used since the 19th century, and may generate confusion in the mind of students or biologists who are not familiar with nomenclature. An example is the use of the name Osteognathostomata Hennig, 1983, instead of the widely known and used Osteichthyes Huxley, 1880, because the taxon originally did not include tetrapods, and the suffix "ichthyes" now misleadingly refers to fishes only. Although it is helpful when they do so, names of taxa may not necessarily refer to a defining character. There are admittedly no rules (or not yet) for the use of taxon names above the family level, but Mickoleit should perhaps have mentioned that Osteognathostomata has the same contents as Osteichthyes in the current sense, or that Rhipidistia now replaces the similarly misleading name Choanata, which referred to the homoplastic internal nostrils (choanae) of lungfishes and tetrapods.

Although Phylogenetische Systematik der Wirbeltiere is written in superb German that can be easily understood by somebody who has a good scholarly basis in this clear and precise language, an English translation would certainly make it become a success worldwide, as the book has no equivalent in English.

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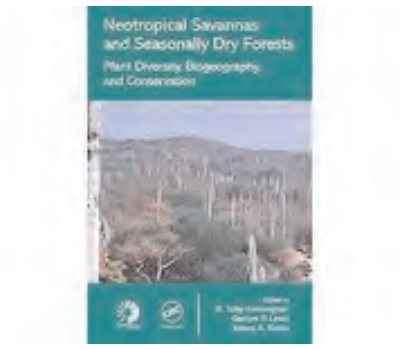
Neotropical savannas and seasonally dry forests: plant diversity, biogeography and conservation

Pennington, R.T., Lewis, G.P. and Ratter, J.A. (Eds.) (2006). CRC Press, Taylor & Francis Group. ISBN: 9780849329876 (hardback) £68.99.



This book is the product of a plant diversity symposium that formed part of a more broadly focused conference, Tropical savannas and seasonally dry forests, ecology, environment and development, held in Edinburgh in 2003. The book brings the much neglected seasonally dry tropical forests (SDTFs) and savannas of the New World centre stage.

The editors have been able to bring together an excellent set of contributions from a diverse set of authors... While the book is not comprehensive in geographical coverage, it does provide a remarkably authoritative account of these dry ecosystems.

Dry tropical forests and savannas have often been viewed as the poor relations of the more diverse and headline-grabbing tropical rain forests. Shifting the focus to dry forest and savannas is well-justified. Neotropical SDTFs and savannas, such as the Cerrados of Brazil, harbour high levels of species diversity, and especially endemic species diversity, in their own right. They are also of even greater conservation concern than wet forests being much more disturbed, fragmented and reduced. In fact, intact or even reasonably undisturbed SDTFs are now so reduced in area, that SDTF is reckoned to be by far the most threatened tropical forest type. The Brazilian Cerrados are also acutely threatened, not least by the tidal wave of soya bean production sweeping across Brazil. There is also growing evidence to suggest that the tempo and patterns of species diversification may have been very different in wet and dry Neotropical forests (Pennington et al., 2006). A good understanding SDTF and savanna diversity and diversification is likely to be crucial to understanding the historical assembly of Neotropical plant diversity in general.

Few people are better placed to address the imbalance between study of rain forests and seasonally dry ecosystems than the editors of this book. Toby Pennington has spear-headed various collaborative SDTF initiatives from monographic work on SDTF plant groups to large scale phylogenetics of SDTF genera that have helped to define the extent, distribution and phytogeography of SDTF as a coherent biome with a wide but highly disjunct distribution across the Neotropics (Pennington et al., 2000); Jimmy Ratter is a world expert on the flora of the Brazilian Cerrados; and Gwilym Lewis has worked throughout the SDTFs of the Neotropics for several decades on one of the most diverse plant families, the legumes. Between them, the editors have been able to bring together an excellent set of contributions from a diverse set of authors, more than half of them from Latin America, to compile what is without doubt the definitive work on Neotropical savannas and seasonally dry forests.

While the book is not comprehensive in geographical coverage, it does provide a remarkably authoritative account of these dry ecosystems. Most chapters adopt a specific country or regional focus to provide an account of essentially a single one of the many disjunct SDTF or savanna areas. Indeed, the book comprises in large part a grand tour

from the Paraguay-Paraná basin in the south through the dry inter-Andean valleys and the Cerrados and Caatingas of Brazil, to the Pacific coast of Mexico and the Caribbean in the north. The understandable constraints imposed by politically defined boundaries in many of these studies and chapters are frustrating in their limited synthesis of broader patterns. For example, there are separate chapters on the inter-Andean dry valleys of Bolivia, the SDTFs of Peru and the SDTFs of Ecuador, but little attempt to integrate these into a wider synthesis of Andean SDTFs as a whole. This is obviously a pity, especially given the close connections between some individual SDTFs e.g. of Pacific NW Peru and S Ecuador. This fragmentation is almost as extreme as the disjunctions between the SDTF fragments themselves and reflects the reality of much recent research effort in the Neotropics and the challenges to be surmounted to reach a more integrated synthesis. On the plus side, this book goes some way to meeting those challenges. This is the first time such a comprehensive set of regional and country studies has been brought together in the one volume and the conference in Edinburgh and this published volume have succeeded in making welcome steps towards greater panAmerican discussion and integration.

The preponderance of local and regional accounts in this book is offset by the excellent introductory overview chapter and a set of more synthetic chapters focusing on specific methods and approaches. The introductory chapter by Pennington et al provides an authoritative, thought provoking and up to date overview of our understanding of these major Neotropical ecosystems - it is a must for all students of Neotropical plant diversity and biogeography. In addition, there is an interesting set of chapters illustrating methods and approaches - in paleoecology, population genetics and phylogenetics - that are being used to investigate diversity and to formulate new hypotheses about the historical assembly of Neotropical SDTF and savanna biomes. Each of these uses new data from SDTF studies to illustrate the utility of these approaches to understanding diversification, providing excellent pointers to what can be done using these techniques and kinds of data, particularly in combination with each other. As such these provide the springboard for an exciting agenda for future work.

This is number 69 in the Systematics Association Special Volume Series, and in many ways epitomizes one of the great values of this series in providing an outlet for authoritative accounts in specialist areas and topics. In general the book is well presented and edited. The quality of some of the black and white photographs is disappointing, probably because of the rather poor quality paper used by the publisher for this volume. However, this is offset by a very welcome 8-page colour inset that reproduces higher quality versions of key figures in colour. Sadly, the largest constituency of potential readers of this volume across Latin America itself will in particular find the price of the volume off-putting. Fortunately, the editors have already found a small pot of money that will help to make sure this volume finds a home in at least a few of the many libraries in herbaria, universities and government research agencies across Latin America where it will be most useful.

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Colin Hughes
University of Oxford

2007 Sir Julian Huxley Lecture

Prof. Olivier Rieppel
The Field Museum, Chicago

Species: kinds of individuals or individuals of a kind?

Wednesday 11 July 2007, 6pm
The Linnean Society, Burlington House, Piccadilly, London

The thesis that species, and monophyletic taxa, are individuals (particulars) was argued by Willi Hennig, and was later introduced into the English speaking community of systematists by Michael Ghiselin and David Hull. Ghiselin and Hull presented the view of species as individuals as a disjunctively opposed alternative to the more traditional view of species as classes, sets, or natural kinds. However, an argument can be made that each species is an individual (i.e., a spatio-temporally localized complex whole or integrated system) that instantiates a natural kind. The consequence is that species names, which are sortal terms, function as proper names in some context (e.g., an evolutionary theory employing the phylogenetic species concept), but as general names associated with natural kind terms in other contexts (e.g., conservation biology).

Quo Vadis

Whatever happened to cladistics?

David M. Williams

Botany Department, The Natural History Museum, Cromwell Road, SW7 5BD, U.K.

"Someday, someone will write on the 'Epicycles of cladistics'. Chapter 1 will be on reversals" (Nelson, pers. comm.)

Today phylogenetic studies are dominated by mathematical (and quasi-mathematical) techniques, the recent literature containing a plethora of equations and statistics providing the promise (or lure) of arithmetic certainty when techniques are applied in the correct way at the correct time. There is no shortage of books (Salemi & Vandamme 2003, Semple & Steel 2003, Felsenstein 2003, Gascuel 2005, Sakhar 2006). In a recent review of Gasceul's book, Felsenstein noted:

"The chapters are mostly written as reviews, and many have new content. Aimed at mathematicians, computer scientists, and statisticians, the book should serve them well as an introduction to phylogenies. It may also help mathematically inclined biology graduate students understand the mathematics of phylogenies, though for them, of course, my own book would be better" (Felsenstein 2006, p. 872).

Felsenstein's own book is a not insignificant addition to this growing numerical literature (Felsenstein 2003). He includes a brief 'digression' on the history of its subject, the maturing of numerical taxonomy (Felsenstein 2003, p. 123 et seq.). Oddly, palaeontology is not mentioned in his history, although he later devotes a chapter to "Phylogenies and Paleontology" (Felsenstein 2003, p. 547 et seq.). Here Felsenstein provides some introductory comments on the use of fossils in phylogeny and their role in discovering ancestors: "…If the fossil record of a group has been searched thoroughly enough, then we should not only be allowed to interpret fossils as ancestors, we should be encouraged to do so" (Felsenstein 2003, p. 547). Felsenstein refers to a handbook, Man's Place in Evolution (1980), published by the British Museum (Natural History) (now the Natural History Museum) to accompany an exhibition of the same name (Felsenstein 2003, p. 547). That exhibition opened in May 1980 (the book was published a little later) and was something of a catalyst to a controversy already underway in the pages of Nature and The Biologist (for a review see Schafersman 1985). That controversy was fuelled, if not ignited, by the reform of palaeontology that took place in the 1960s and 1970s (Nelson & Platnick, 1984, p. 156; Nelson, 1989, p. 71, Nelson 2004, Williams & Ebach 2004).

Perhaps it is timely to revisit the concerns of palaeontologists in the 1960s and 1970s and to note its relationship to the growing body of numerical literature. One beginning was with Gareth Nelson who in 1969 presented a lecture to the scientific staff of the American Museum of Natural History. That presentation has only recently been published (Williams & Ebach 2004). David Hull wrote that "the paleontologists present were furious, storming out etc." (Hull 1988), but since Hull (and before the printing of the essay in Williams & Ebach 2004), only Henry Gee (2000) commented on Nelson's presentation, citing one sentence, relevant to Felsenstein's concerns above:

"…looking for ancestors in the fossil record seems to be like looking for honest men: in theory they must exist, but finding them in practise, alas, is another matter" (Nelson in Williams & Ebach 2004, p. 706, see Gee 2000, p. 147).

Nelson's target was palaeontology, or more precisely the palaeontological method, and its impact on comparative biology. As Colin Patterson noted some 20 years later,

"By about 1960 palaeontology had achieved such a hold on phylogeny reconstruction that there was a commonplace belief that if a group had no fossil record its phylogeny was totally unknown and unknowable" (Patterson 1987:8).

The old time palaeontologists were convinced that they really could read evolution from the rocks such that only if there was a 'good' stratigraphic succession of fossils ("searched thoroughly enough") could phylogenetic trends of characters be known. The upshot was that palaeontology claimed special knowledge with respect to phylogeny - that they had a special method. William Diller Matthew, for example, wrote in 1926:

"No one carries a more solid conviction of the truth of evolution than the field paleontologist…he has seen it himself ineffaceably inscribed in the records of the past." (Matthew 1926:454).

While not all palaeontologists expressed themselves with such reli-

gious fervour, many did. George Gaylord Simpson, Matthew's successor, wrote in 1961 "…fossils provide the soundest basis for evolutionary classification…classifications have come to depend more on fossils than on recent animals" (Simpson 1961), to which the entomologist Lars Brundin remarked that "…when the classification of mammals has 'come to depend more on fossils than on recent animals' that is in reality not a demonstration of the overwhelming importance of the fossils…but a sign of fateful misconception of phylogenetic relationships…" (Brundin 1966). Brundin had commented on a tree published by Alfred Sherwood Romer, a phylogenetic tree with ancestors placed at the nodes (Romer, 1962, p. 35). "It visualizes phenomena connected with phylogeny", wrote Brundin, "not phylogeny itself."

To be sure of the target, Nelson added:

"Now when I have spoken on these matters in the past, it has sounded to some as if I had taken upon myself the task of denigrating paleontology and individual paleontologists. That is not so. I am concerned with what I find is a real problem" (Nelson in Williams & Ebach 2004, p. 711).

Are there real problems today? Patterson's earlier comments on palaeontology, with a little adjusting, might be made more appropriate for today:

"By about 1990 molecular data had achieved such a hold on phylogeny reconstruction that there was a commonplace belief that if a group had no molecular data its phylogeny was totally unknown and unknowable."

While there may be subscribers to that viewpoint, it might appear overly harsh, as many morphologists - even palaeontologists - still function quite happily today. One might justly criticise molecular data but that would miss the point. The past criticisms of palaeontology concerned method, the palaeontological method, not the data, the fossils. And that too is the case here. So the statement needs further modification:

"By about 1990 numerical systematics had achieved such a hold on phylogeny reconstruction that there was a commonplace belief that if a group had no numerical analysis its phylogeny was totally unknown and unknowable."

The effort should be to find real problems, or at least recognise that there are problems, never mind possible solutions. Here is one.

The data come from a paper published a few years ago by Goloboff and Pol (2002). They are made up. Their example was not intended to demonstrate anything about data matrices of characters. Goloboff and Pol (2002) were exploring trees and their combination, rather than characters. But the matrix can be interpreted as a series of derived characters for the purpose of this example. Goloboff and Pol presented two trees:

A(B(C(D(EF))) + C(D(E(A(BF)))

The trees can be reduced to their separate nodes, four for each tree:

A(B(C(D(EF)))	C(D(E(A(BF)))
1. A(BCDEF)	5. C(ABDEF)
2. AB(CDEF)	6. CD(ABEF)
3. ABC(DEF)	7. CDE(ABF)
4. ABCD(EF)	8. ACDE(BF)

The nodes can be represented as a series of binary variables in a data matrix:

	1	2	3	4	5	6	7	8
'Root'	0	0	0	0	0	0	0	0
A	0	0	0	0	1	1	1	0
B	1	0	0	0	1	1	1	1
C	1	1	0	0	0	0	0	0
D	1	1	1	0	1	0	0	0
E	1	1	1	1	1	1	0	0
F	1	1	1	1	1	1	1	1

Parsimony analysis of that matrix returns four trees:

C(D((AB)(EF)))
C((AB)(D(EF)))
(AB)(C(D(EF)))
C(D(E(F(AB))))

Each tree differs with the exception of an AB node, suggesting that the appropriate summary for these data is (AB)CDEF. As an aside, UPGMA analysis of the same matrix finds C(AB)(F(DE)), and a 'neighbor-joining' (NJ) analysis finds C(AB)(D(EF)), both solutions differ but retain the AB node. So as the AB node keeps re-occurring, it might be assumed a true result, or at least part of a true summary.

Where does it come from? It is not present in the data. Inspection of the table of nodes above reveals no AB group. Inspection of the matrix above reveals no AB group, unless the AB portion of character 2 (the zero's) is interpreted as such - but the zero implies either the absence of a character (state) or that it is plesiomorphic. Of course, the AB group might be 'explained' by referring to processes that, in truth, we know nothing about - it is a reversal. To discover reversals is to implement a method of optimization, the sorting and shuffling of characters on trees. Or is optimization another fateful misconception?

Regardless of explanation, there is no escaping the fact that this AB node is manufactured, the program (whatever program is used) created it - there is no AB group. The word 'create' might seem rather harsh. However, one justification for this result is summed up by the notion that the result is "…consistent in making explanations that are consistent with the optimality criterion of parsimony". They seem consistent with UPGMA and NJ as well.

In his 1969 presentation Nelson noted some of the Brundin's critics. Philip J. Darlington was one. He referred to Brundin's account of the history of mammals: "If, in the face of the fossil record, they say that this has been the history of mammals, they invite ridicule" (Darlington, 1970, p. 11). Divorced

from cladistics, but not from palaeontology, was Romer's comment, a critique of Erik Stensiö's work: “This geological sequence seemed to most workers an evolutionary sequence…Stensio…comes to a conclusion …startlingly different….Acceptance of… (his)… thesis makes it necessary to assume that the geological record is upside down. This is difficult to believe” (Romer, 1968, p. 31-2).

Invite ridicule. Difficult to believe. Consistent with parsimony. These comments seem all of a piece.

It does indeed seem as if the data matrix is the last bastion (or refuge) of phenetic taxonomy, a grid of data unhindered by any hypotheses

Consider a final examination of the matrix above. What exactly are the entries, the 0's and 1's? Observations? Hypotheses? Hypotheses of what? They are evidently not shared derived characters. What these entries seem to be are simple similarities - that is, the phenetic ideal writ large, data with no theory - until one is applied via the program.

It does indeed seem as if the data matrix is the last bastion (or refuge) of phenetic taxonomy (Williams & Ebach 2006), a grid of data unhindered by any hypotheses. The anomalies of the results - the AB node - are those associated with the workings of a program, not with the resolution of some hidden evolutionary result, things yet known to us. The programs (whatever program that might be) act like the palaeontological record of the past: a way to understand things that, in some general sense, have no meaning, “…a sign of fateful misconception of phylogenetic relationships…”.

Whatever Happened To Cladistics? Seen through mathematical goggles, it turned into Phenetics - and that really isn't a good thing. Seen as a critique of palaeontology, it should turn its attention to mathematics and phylogeny. Who knows, maybe there's another revolution waiting to happen.

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Inspirations

To be a systematist today is to be lucky. Rather than being restricted to a cubic meter of working space at a desk or a lab bench where your time is spent revisiting daily routines with scarcely any variation in outlook or techniques, modern systematists are fortunate enough to parallel the diversity of their study objects with a broad array of data gathering and analysis tools that are employed in a diversity of working habitats, from the library to the field. Kate Jones neatly illustrates these various meanings of diversity in her work on bats and biodiversity.

Taking the papers on which Kate is listed as an author, and which are currently in press, one gets a measure of the different kinds of diversity in a systematist's life. In these papers Kate and a collection of 16 co-authors address such diverse issues as conservation of island bats, phylogenetic tools for studying bat evolution, sexual dimorphism in mammals, mammalian life histories, and global trends in zoonotic disease emergence.

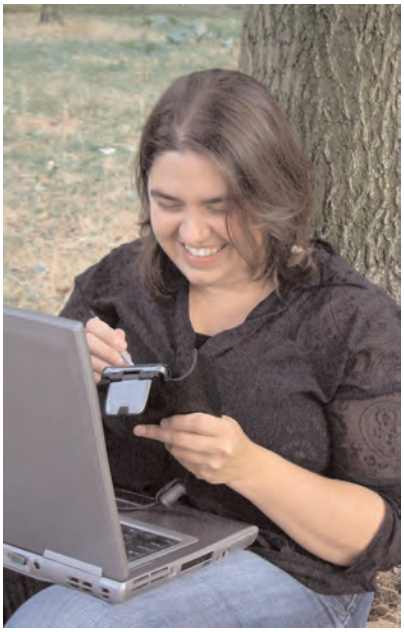
Another paper on which Kate has recently worked received worldwide media attention last month. Published in *Nature* this study reported that the extinction of dinosaurs had far less effect on the rise of mammals than previously thought. Not surprisingly, this is Kate's favourite paper, at least for now. This issue's *Inspirations* provides a glimpse into Kate's fascinating work.

Introduction

How would you summarize yourself in the form of a title of a scientific paper?

Nothing makes sense in biology except in the light of evolution': using evolutionary frameworks to understand past, present and future biodiversity.

Summarize the when and where of your academic career beginning with your undergraduate days



Kate Jones fixing equipment on a bat monitoring field trip in Romania

I did a B.Sc in Zoology at Leeds University in 1993 followed by a Ph.D in bat evolution at the University of Roehampton, London in 1998. After spending the next few years on various research fellowships in the US, I have finally settled back down in London at the Institute of Zoology, Zoological Society of London.

When did you decide to follow the career path you are on now?

I think that I decided that I wanted to get a Ph.D when I was about 12, after watching Indiana Jones and Raiders of the Lost Ark. It was just something about the way the baddies said 'Docktor Jones' while he was dangling off a cliff. My research interests crystallized at Leeds after doing a couple of undergraduate projects on bats and reading about the wonders of the evolutionary process in Stephen J. Gould's 'Wonderful Life'.

What are the main goals of your research?

I am interested in understanding how evolutionary processes produce past, present and future global biodiversity patterns using a multi-disciplinary approach. For example, I synthesize fossil and phylogenetic information to investigate past biodiversity trends; the distributions and traits of modern organisms to understand extinction processes and current distributions; and climate and human demographic models to forecast future biodiversity loss.

What are your favourite organisms and why?

Bats of course! Well, they are just really cool - they fly, echolocate, there are loads of them, live everywhere, no one knows anything about them and have terribly bad reputation. They include the smallest of all mammals, the bumblebee bat, which weighs in at just 2g; the most ugly mammal, the wrinkle faced bat, which has a face like a car crash and a flap of skin which it pulls over its head at night; and the sucker-footed bat which has suction disks on its arms to help it to stick to the underside of leaves in Madagascar.

Work and responsibilities

How many hours per week do you work?

Hmm, definitely too many, but I do

have a lot of fun.

What percentage of time do you spend on each of your different responsibilities?

I travel quite a lot, either to meetings with collaborators or field projects in different parts of the world. I somehow manage to fit in data analysis, writing papers, reading, supervising students and writing grants in between that.

How many undergraduate, PhD students, postdocs, and technicians are in your lab?

Currently, I have three PhD students (Natalie working on phylogenetic community structure in mammals, Savrina on factors determining population declines and extinction, and Alanna on the evolution of echolocation in bats). I also have a number of technicians that help me with web database programming of our global datasets on mammal biological traits, geographic distributions and abundances, diseases and phylogenetic information.

What gives you the most satisfaction and frustration in your job?

Most satisfaction that I get is from taking a broad multi-disciplinary approach to my research which leads me into many completely different research areas including ant, dinosaur and mammal diversification, predicting human emerging diseases, the trade off between brain size and mating systems and tracking bats across Transylvania. The biggest frustration is that there are always many more things that I am interested in than I have time to do!

Do you have any international collaborations?

Again, probably too many!

Fieldwork

What kind of field work do you do and where has it taken you so far?

I must confess that I don't do that much field work as I am a statistical modeler. Most of my field work has involved collecting bat distributional and abundance information which has taken me to the remotest bits of Borneo, the UK, the Caribbean, Africa, Eastern Europe and the very wildest bits of Central Park in New York.

What kinds of organisms have you collected?

Bats and bat echolocation calls.

Did any memorable incidents happened during field collecting?

We were collecting bat echolocation calls using a detector stuck on the top of a car and driving around slowly in the middle of the night looking very suspicious at what we didn't quite realize was the Romanian/Hungarian border. It took us quite a lot of time to explain what we were doing to the frightening number of armed Hungarian border guards that appeared out of nowhere.

Influences

Is there any paper or book that has been very influential for your thinking?

I think that the book that really opened my eyes to just how amazing the earth's biodiversity is and how fragile and how contingent its evolution is on random earth events was Stephen J. Gould's 'Wonderful Life'. This inspired me to want to investigate the evolutionary processes driving these biodiversity patterns and study evolution.

How was the most important mentor in your career?

I have been extremely fortunate to have worked with the very best evolutionary biologists in the world, each of which has had a huge impact on my life - both for my career and personal development.

What is the best advice you have ever received?

Take the good things that people say about you, extremely personally and the bad things people say about you, not personally at all!

Output

How many scientific publications do you have at the moment?

Hmm, not sure - maybe 60?

Your work depends on databases you assembled. What kind of data do these databases contain?

I have a number of different databases that myself and collaborators have put together over the years. For example, life histories of all mammals, their geographic distributions and evolutionary histories. I also am working on new web technologies to host data interactively online and we are trialing out with a new project called 'iBats' which stores and analyzes bat echolocation data along driven transects collected all over the world by volunteers.

Could you nominate any of your discoveries or papers as the most important one, or the one (or several) that you personally like best?

My latest paper in *Nature* 'The delayed rise of present-day mammals' is by far my favorite! We put together all the existing evolutionary relationships of all current mammal species into one 'supertree' and traced back the important evolutionary origination and diversification events in mammalian history. We found that (contrary to popular

belief) dinosaur extinction at the KT boundary did not give rise to the diversification of present-day mammals but the important events happened 10-15 MYA after the extinction event. We got a lot of press coverage and it was a huge amount of fun!

You have a lot of papers with co-authors. Is there a particular contribution you generally bring to these papers?

I guess I like working with teams of people because you can ask much broader, more far-reaching questions. My involvement in projects really does depend on the project, leading some of them, contributing data and ideas, phylogenetic analysis, making cocktails etc.

Advice

What skills do you think a successful researcher in your discipline must possess?

Strong analytical and programming skills, a critical mind, ability to work across disciplines and to communicate your science to others, and a sense of humor when things go horribly wrong.

Do you have any tips for students aspiring to a career like yours?

Whatever you decide to focus on, I think you need to be passionately interested in it. The job is a hard one, long hours, poor pay, low job security, constant rejection from granting agencies and journals. If you don't think you are doing something worth while that you love, then it is definitely not worth it. On the other hand, I get paid to ponder about the processes generating the amazing biodiversity on this planet and travel the world to see it! Not a bad trade off.

Calendar

18-20 June 2007
A Linnean Tercentenary Celebration: The Evolution of the Animal Phyla
The Royal Society, London
For further information see: http://www.royalsoc.ac.uk/event.asp?id=4163&month=6

11 Jully 2007
Sir Julian Huxley Lecture by Olivier Rieppel
Linnean Society, London
Title: **Species: Kinds of individuals or individuals of a kind?**
For further information see page 18 of this newletter and www.systass.org

28-31 August 2007
6th Systematics Association Biennial Conference
Royal Botanic Garden, Edinburgh
For further information see: http://www.systass.org/biennial2007/

5 December 2007
9th Young Systematists' Forum
Flett Lecture Theatre, The Natural History Museum, London
Registration is free. Please send applications by email to Dr. Julier Brodie (j.brodie@nhm.ac.uk), stating your name, contact address, and whether or not you wish to present a talk or a poster. For further information see: http://www.systass.org/ysf/

12 December 2007
Annual General Meeting and Lecture by Chris Stringer
Linnean Society, London
More information will follow and can be obtained from www.systass.org.

6th Biennial Meeting of the Systematics Association

Royal Botanic Garden, Edinburgh 28-31 August 2007

Deadline early registration and abstracts 30 June

Major Themed Symposia

1. Floras and Faunas Serving Biodiversity Research

2. Speciation

3. UK Research Councils Systematics Symposium

Special feature

'How to' Talks on important emerging technologies and techniques by leaders in the field

See www.systass.org/biennial2007/