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13th Young Systematists' Forum

THURSDAY, 1st December 2011, 9 am
Venue: Flett Lecture Theatre,
Natural History Museum, London, UK

The annual **Young Systematists' Forum** represents an exciting setting for Masters, PhD and young postdoctoral researchers to present their data, often for the first time, to a scientific audience interested in taxonomy, systematics and phylogenetics. This well-established event provides an important opportunity for budding systematists to discuss their research in front of their peers within a supportive environment. Supervisors and other established systematists are also encouraged to attend.

Prizes will be awarded for the most promising oral and poster presentation as judged by a small panel on the day.

Registration is FREE. Send applications by e-mail to (YSF.SystematicsAssociation@gmail.com), supplying your name, contact address and stating whether or not you wish to give an oral or poster presentation. Space will be allocated subject to availability and for a balanced programme of animal, plant, algal, microbial, molecular and other research. Non-participating attendees are also very welcome - please register as above.

Abstracts must be submitted by e-mail in English no later than 18th November 2011. The body text should not exceed 150 words in length. If the presentation is co-authored, the actual speaker (*oral*) or presenter (*poster*) must be clearly indicated in BOLD text.

All registered attendants will receive by e-mail further information about the meeting, including abstracts, one week in advance. This information will also be displayed on the Systematics Association website (www.systass.org).

Cover illustration: *Platanthera azorica*, a rare and controversial orchid endemic to the Azorean island Pico, and subject of a research project by Richard Bateman supported by a recent SRF grant. (top photo, © Richard Bateman).

Poetry and the Darwinian Condition

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A man said to the universe:
"Sir, I exist!"
"However," replied the universe,
"The fact has not created in me
A sense of obligation."

The American poet Stephen Crane's terse epigram nails with perfect precision the impact of Darwin's discoveries on humanity's sense of our own significance. Our existence does not give us grounds for pompous self-importance, it is just a 'fact' of nature. As the Victorian poet James Thomson put it in the most defiantly atheistic poem of his day, *The City of Dreadful Night*, 'We bow down to the universal laws, / Which never had for man a special clause' (section XIV, ll. 61-2). Darwin revealed a natural world formed by the brutal, aimless forces of random variation and natural selection. It was a revelation that was hard to square with the notion of a benevolent creator. Darwin himself commented in a letter to his friend the American biologist Asa Gray, 'I cannot persuade myself that a beneficent and omnipotent God would have designedly created the Ichneumonidae with the express intention of their feeding within the living bodies of Caterpillars' (cited Darwin, 2003, 492). If the Darwinian world were the creation of a divine designer, it seemed in Robert Frost's words a 'design of darkness to appall' ('Design', l. 13). And far from being an exception to these laws of nature, we ourselves were a product of them, a very recent twig on Darwin's famous tree of life, with no clear line separating us from apes and other animals.

Darwin's ideas took time to gain ground. Evolution had been in the air for some time when he published *On the Origin of Species* in 1859, and within a few years of that book coming out almost all the scientific community had come to accept it. Many scientists and lay readers were convinced of the argument for natural selection too, but others sought other motors for evolution and it was famously not until the synthesis of Darwin's theory with Mendel's genetics between the wars that the case for natural selection was recognised as conclusive. There was a strong if ultimately unsuccessful rearguard action fought too on behalf of human exceptionalism, especially in the wake of Darwin's *The Descent of Man*. Historically speaking, then, there was no sudden Darwinian revolution. But his ideas themselves are no less revolutionary for that. Darwin's understanding of nature and of humanity's place in it, very largely corroborated and fleshed out in extraordinary detail by the last one-hundred-and-fifty years of biology, has utterly transformed our understanding of ourselves and of the natural world. They mark the most radical re-evaluation of the human condition in our history. After Darwin, the human condition is the condition of living in and as a product of a Darwinian universe; it is the

Darwinian condition.

The biological sciences—evolutionary biology, genetics, ecology—define our place within the natural world. But to understand fully what it means to live in this Darwinian condition we need the imaginative resources of literature as well. Novels like George Eliot's *Middlemarch* or Ian McEwan's *Saturday* can explore in depth our life as social and psychological organisms in a secular world. Prehistoric fiction like William Golding's *The Inheritors* can open imaginative windows onto our evolutionary past; science fiction like H. G. Wells's *The Time Machine* or Olav Stapledon's *Last and First Men* can play out the possibilities of our evolutionary future. But it is poetry more than any other literary form that can help us to grasp for ourselves what it is to be a human being living consciously in a Darwinian universe. Where novels transport us into fictional worlds, poems transport us into new states of mind. Since the news of Darwin's theories began to break in the 1860s, poets from Alfred Tennyson to Ted Hughes have explored their implications for human beings and for nature as a whole. For some, Darwin's ideas spell something close to an existential disaster, undermining and even overturning their deepest beliefs and values. For others, the Darwinian world is not so hostile, more beautiful, even hopeful in its own way. Through reading the poems of these different poets we can retrace the paths their explorations have taken, building up our own ever richer and more complex mental maps of our Darwinian condition.

In my book *Darwin's Bards* (see review in issue 32 of *The Systematist*) I discuss more than thirty British and American poets who have responded to Darwinism over the past hundred-and-fifty years (Holmes, 2009). I look at what they show us about the kind of God that might preside over a Darwinian universe, about how we can face up to our own mortality now that immortal souls no longer seem compatible with what we know of our evolutionary origins, about our insignificance within the universe and our significance on Earth, about our relationships to other animals and about those animals themselves, and about our deepest personal and sexual relationships with one another. Here I am going to concentrate on just three poets and a handful of poems, which each encapsulate particular ways of thinking about the Darwinian condition. Thomas Hardy, George Meredith and May Kendall were among the earliest poets to comprehend fully the implications of Darwin's ideas. Each looked on Darwin's world through a different worldview. The author of several famous tragic novels including *Tess of the D'Urbervilles* and *Jude the Obscure*, Hardy saw a bleak landscape in Darwinian nature. His infamous pessimism is not as straightforward as it might seem, but he certainly earned the critic I. A. Richards's compliment that he was 'the poet who has most steadily refused to be comforted in an age in which the temptation to seek comfort has been greatest' (Richards, 1970, 68-9). If Hardy was an ambivalent pessimist, Meredith was an equally subtle optimist. Of all the Darwinian poets, Meredith conveys best the sense of being part of a vital living nature, both in life and in death. Where Hardy was effectively an atheist and Meredith close to a pagan, Kendall was a committed Christian. She reconciled her faith to evolution in part by rejecting the pre-Darwinian idea that human beings had a special place in the providential plan, or that it is possible to know such a plan through science at all (see Holmes, 2010). In her satirical poem, 'The Lay of the Trilobite', she skewers this kind of providential evolutionism with a comedy that is still funny and deft over a century on.

Hardy declared himself to have been 'among the earliest acclaimers of *The Origin of Species*' (Hardy, 1962, 153), yet Darwinism posed him what he called in the title of one poem 'The Problem'.

The Problem

Shall we conceal the Case, or tell it—
    We who believe the evidence?
Here and there the watch-towers knell it
    With a sullen significance,
Heard of the few who hearken intently and carry an eagerly upstrained sense.

Hearts that are happiest hold not by it;
    Better we let, then, the old view reign:
Since there is peace in that, why decry it?
    Since there is comfort, why disdain?
Note not the pigment so long as the painting determines humanity's joy and pain.

In this poem, published in 1901, Hardy gets to the crux of an issue that is debated every time a new book explaining evolution to the wider public is reviewed. To what extent should scientists who understand evolution make public their wider conclusions about the 'significance' of the 'evidence'? If they believe that the only coherent interpretation of Darwin's world is that it is a world without God, should they say so, publicly and boldly? Or should they politely decline to intrude on other people's beliefs, even if privately they think they are deluded? In Hardy's poem it is not church-towers but watch-towers which ring out the discoveries learnt by the more far-sighted scientists and other observers, including novelists and poets like Hardy himself. Their ringing is not a peal but a 'knell', however, showing that Hardy well knows that to be disillusioned of 'the old view' would be a grave bereavement for many people, as well as a distressing reminder of their own mortality. In the end Hardy decides against making the 'Case' for this disillusionment. Yet by the time he draws that conclusion it is too late. In allowing himself to speak candidly to a reader who he imagines is in agreement with him, he has already made it clear that in his view this case is conclusive. He refuses to 'disdain' the comforts of religious faith, but his poem makes that faith harder to sustain all the same. Whether or not we agree with Hardy at the outset, by the end of this poem we are aware that the colour has faded from the old world-picture even if its lines and forms appear to some people still to be in place. If 'The Problem' says one thing and does another, it is partly because Hardy is ambivalent himself about the nature revealed by Darwin. His poetry repeatedly mourns the loss of his more naïve, pre-Darwinian view of nature and humanity, yet at the same time he knew that ultimately he had been 'By truth made free', as he says in his last published poem, 'He Resolves to Say No More' (l. 18). 'The Problem' offers two competing solutions to the problem it raises—to keep silent or to speak. Hardy himself is unsure which is the right answer in principle, but in practice, in this poem and across his other poems and novels, he is persistently driven to speak out.

Hardy finds other thoughts and feelings besides mere disillusionment in Darwin. This is his poem on the subject of 'Heredity', from his book *Moments of Vision*, published in 1917.

Heredity

I am the family face;
Flesh perishes, I live on,
Projecting trait and trace
Through time to time anon,
And leaping from place to place
Over oblivion.

The years-heired feature that can
In curve and voice and eye
Despise the human span
Of durance—that is I;
The eternal thing in man,
That heeds no call to die.

If 'The Problem' anticipates current debates over science and religion, the 'family face' in 'Heredity' bears a striking resemblance to the selfish gene. What survives is not the individual but the 'trait'. Hardy is not concerned with the mechanism of heredity here, so much as the fact of it. Neither natural nor sexual selection come into the poem. But in giving the observable hereditary trait a voice Hardy personifies it in the same way that Richard Dawkins personifies the gene, which in Dawkins's population genetics translates to whatever components of a genome determine or increase the likelihood of a given trait. The characters of these two personifications are similar too. Hardy's poem invites us to imagine him walking along a corridor or down a staircase, perhaps in an old baronial hall, lined with portraits going back through the generations. As he moves from one portrait to another, the strong impression forms in his mind that these several people going back through time bear a strong family resemblance to one another. Soon it is that resemblance, not the individuals themselves, that seems to stare from each portrait. In its very persistence, it defies death, but equally it shows contempt for individual life. Like the selfish gene, all that concerns it is its own survival; like the selfish gene, it comes across as a sinister deterministic force undercutting our attempts to assert our own independence from our heredity. In both cases, the malignity of heredity is a product of the personification—neither the gene nor the face has any consciousness or even real agency—yet at the same time the personification gives us a new perspective on ourselves less as discrete individuals and more as part of a biological continuum which reaches back through time and over which each of us individually has very little control.

Hardy was acutely aware that this biological continuum, Darwin's tree of life, had other profound implications too. In a letter he wrote in 1910, he noted that 'Few people seem to perceive fully as yet that the most far-reaching consequence of the establishment of the common origin of all species is ethical' (Hardy, 1962, 349). He captures this ethical imperative through a highly original reworking of Darwin's own image in another poem (The Wind Blew Words) from the same collection as 'Heredity'.

The Wind Blew Words

The wind blew words along the skies,
And these it blew to me
Through the wide dusk: 'Lift up your eyes,
Behold this troubled tree,
Complaining as it sways and plies:
It is a limb of thee.

Yea, too, the creatures sheltering round—
Dumb figures, wild and tame,
Yea, too, thy fellows who abound—
Either of speech the same
Or far and strange—black, dwarfed, and browned,
They are stuff of thy own frame.'

I moved on in a surging awe
Of inarticulateness
At the pathetic Me I saw
In all his huge distress,
Making self-slaughter of the law
To kill, break, or suppress.

For many Victorian and twentieth-century ideologues, Darwinism seemed to authorise an ethic of vigorous, even violent, competition. If the natural order was one of struggle, who were men to countermand it? Better to enter into the spirit of it and battle to assert our own claims to the right to survive. Better, in Hardy's words, 'To kill, break, or suppress'. This kind of erroneous Social Darwinism involves the false step of taking what is as a guide to what ought to be. But it also latches on to one half of Darwin's vision—natural selection—while disregarding the other half—the tree of life. In 'The Wind Blew Words' Hardy uses the image of a wind-battered tree to introduce the principle that Darwin's tree of life implies the kinship of all living things. As a human being, Hardy is a twig on the tree of life, as is the tree itself. They are both part of the same whole. By identifying with that whole Hardy is able to invert Darwin's image so that the tree—the literal tree, and by extension every other branch of the tree of life—becomes part of his own body. Once Hardy has identified himself with the tree of life, other animals and other people, whatever their nationality or race, all become part of one immense self that he does not hesitate to call 'Me'. Hardy's poem was published at the height of both the First World War and the age of empire. In it he exposes war and imperialism, both of which claimed to be licensed by Darwinism, as acts of 'self-slaughter' on precisely Darwinian grounds. For all that his poem records a condition of inarticulate realisation, it is itself a masterful example of how poetry can articulate a subtle idea vividly, economically and powerfully. Reading this poem, we too can feel the 'huge distress' of the rest of the living world as our own.

Over fifty years before Hardy, George Meredith asked Nature to 'Teach me to feel myself the tree, / And not the withered leaf' in his magnificent but now little read 'Ode to the Spirit of Earth in Autumn' (ll. 154-5). This poem was published in 1862, only three years after Darwin's seminal book. But where Hardy wants to move us to feel the 'distress' of life to combat the drive towards war, oppression and exploitation, Meredith wants us to revel in an ongoing vitality that far outlasts our own short lives. At the same time, while Meredith celebrates 'the joy of motion, the rapture of being' (l. 180), he also accepts the prospect of not being with equanimity. As he writes:

Behold, in yon stripped Autumn, shivering grey,
Earth knows no desolation.
She smells regeneration
In the moist breath of decay. (ll. 186-9)

Like Hardy's family face and his immense, suffering Me, Meredith's Earth is a personification which enables us to see ourselves and our place in nature in a new way. It gives us a model both for how to live vigorously, with an intense awareness of life itself and its experiences, and how to die, accepting that each individual life is part of an ongoing process of living that does not stop when that one life ends.

Meredith faces death again in a calmer mood in the last lyric of a sequence he published in 1870 called 'In the Woods':

A wind sways the pines,
And below
Not a breath of wild air:
All still as the mosses that glow
On the flooring and over the lines
Of the roots here and there.
The pine-tree drops its dead:
They are quiet as under the sea.
Overhead, overhead,
Rushes life in a race,
As the clouds the clouds chase:
And we go,
And we drop like the fruits of the tree,
Even we,
Even so.

The rhythms and imagery of this poem set up a contrast between movement and stillness—the wind above the treetops and the quiet deep in the wood, life racing competitively and the falling dead. The end of the poem has a dying fall, as Meredith folds us into this natural contrast, so that we too drop like the spent pine-cones. The calmness of the poem marks a stoic acceptance of mortality, and Meredith invites us to join him in acquiescing to the inevitable natural processes of our own death. But even here he reminds us that in dying we are not wholly lost to the cycle of life. The dead pine cones were 'the fruits of the tree' from which the seeds have been dispersed by the rushing wind. We too drop, but we too are fruits of the tree of life, bearing seeds of new life, literally in our bodies and those of our children, and figuratively in the legacy left by the lives we have led.

Through their poems, Hardy and Meredith make us look with fresh eyes on the by-now familiar world of Darwinian nature, to reconsider what it means to be a part of a living tree of life, and to face up to the problems that this poses in terms of what to believe and what to say, how to live our lives and how to face our deaths, even how to define where our own selves reside. Their poems are thoughtful and serious and repay serious attention. But poetry can work through comedy too. May Kendall was in her mid twenties when her poem 'The Lay of the Trilobite' appeared in *Punch* in 1885:

A mountain's giddy height I sought,
Because I could not find
Sufficient vague and mighty thought
To fill my mighty mind;

And, as I wandered ill at ease,
 There chanced upon my sight,
A native of Silurian seas,—
 An ancient Trilobite!

So calm, so peacefully he lay,
 I watched him e'en with tears.
I thought of Monads far away,
 In the forgotten years.
How wonderful it seemed and right,
 The providential plan,
That he should be a Trilobite,
 And I should be a Man!

And then, quite natural and free,
 Out of his rocky bed,
That Trilobite he spoke to me,
 And this is what he said:
"I don't know how the thing was done,
 Although I cannot doubt it;
But HUXLEY,—he if any one
 Can tell you all about it:—

How all your faiths are ghosts and dreams,
 How, in the silent sea,
Your ancestors were Monotremes—
 Whatever these may be,—
How you evolved your shining lights
 Of wisdom and perfection,
From Jelly-fish and Trilobites,
 By Natural Selection.

You've KANT to make your brains go round,
 And CARPENTER to clear them,
And Mathematics to confound,
 And *Mr. Punch* to cheer them.
The native of an alien land
 You call a man and brother,
And greet with pistol in one hand,
 And hymn-book in the other!

You've Politics to make you fight,
 And utter exclamations,
You've cannon, and you've dynamite
 To civilise the nations.
The side that makes the loudest din
 Is surest to be right,
And oh, a pretty fix you're in!"
 Remarked the Trilobite.

"But gentle, stupid, free from woe,
 I dwelt among my nation,
I didn't care, I didn't know,
 That I was a crustacean;
I didn't grumble, didn't steal,
 I *never* took to rhyme:
Salt water was my frugal meal,
 And carbonate of lime."

Reluctantly I turned away,
 No other word he said;
An ancient Trilobite he lay
 Within his rocky bed.
I did not answer him, for that
 Would have annoyed my pride,
I merely bowed, and touched my hat,
 But in my heart I cried—

"I wish our brains were not so good,
 I wish our skulls were thicker,
I wish that Evolution could
 Have stopped a little quicker.
For oh, it was a happy plight,
 Of liberty and ease,
To be a simple Trilobite
 In the Silurian seas!"

For a topical comic poem, 'The Lay of the Trilobite' stands up remarkably well one-hundred-and-twenty-five years after it was first printed. It helps that T. H. Huxley is still remembered as Darwin's Bulldog, even if the physiologist William Carpenter—another friend and ally of Darwin's—is largely forgotten. But even without knowing about these individual scientists it is clear that what is going on in this poem is a encounter between a man who complacently presumes that human beings—particularly he himself—are the pinnacle of an evolutionary process that is the unfolding of a 'providential plan', and a Darwinian view of evolution (albeit somewhat garbled by the Trilobite) which puts him firmly back in his place. By assuming the Trilobite's vantage point, Kendall is able to mock the self-satisfaction and self-righteousness of her fellow Victorians, which are delightfully reversed at the end of the poem as the narrator wishes that he had not evolved so far after all. One reason why this poem, like Stephen Crane's epigram with which I began, is still funny is that the misconception of evolution as a process that moves towards the creation of human beings, with all the self-importance this implies, is still current today.

As a young man, Charles Darwin read poetry keenly. He enjoyed Shakespeare and Milton, Wordsworth and Coleridge, Byron and Shelley. He even took *Paradise Lost* round the world with him on the Beagle. But by the time he came to write his 'Recollections' for his family in the last few years of his life he had to confess that he had lost all taste for poetry. He could no longer 'endure to read a line' of it. Even Shakespeare was 'so intolerably dull that it nauseated [him]' (Darwin, 2002, 84). By the time Hardy, Meredith and Kendall were writing, Darwin's weak digestion had got the better of him and he had lost the stomach for poetry. He wrote that this was a ‘great regret’ to him (2002, 20). It might have been a still greater one had he known how perceptively and movingly these poets and others would interpret the world that he had shown them.

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The 2011 Biennial: some thoughts and thank yous

It’s always a challenge to capture the attention of the reader in the first sentence of two so I’d like to start by saying if you were not there, then you should have been. When I was writing for the newsletter last autumn, much of the activity of the Systematics Association Council was focused around the planning of the 8th Systematics Association biennial. Suddenly July was here this year and it was off to Queen’s University Belfast for the conference. The opening plenary given by Debashish Bhattacharya set the tone of the next few days. His presentation spanning how genomes of key algal groups are helping us to understand plastid and eukaryotic evolution and single cell genomics was awe-inspiring and inspirational, and the potential to be able to study endosymbiosis and horizontal gene transfer in individual cells futuristic in appeal. This also represented the start of the symposium on *Algal systematics: where next?* and was followed by a thought provoking talk given by John Raven who continued the theme of horizontal gene transfer, the implications for Rubisco genes, and the obvious functional outcome of oxygenic photosynthesis by eukaryotes to less certain outcomes.

The following day was phenomenal. The symposium: *Next generation systematics: studying evolution and diversity in an era of ubiquitous genomics* covered such a wealth of topics and demonstrated just how far genomics has progressed in such a short space of time and how this is influencing our understanding of the natural world. And if you have ever wondered who it is who writes Multiple Sequence Alignments for molecular sequence data, the ‘how to’ talk was given in entertaining fashion by Des Higgins, of Clustal fame which is now a couple of decades old and who described the new Omega version designed to tackle the ever increasing number of sequences that require alignment. His talk took me back to my early days of molecular sequencing where more or less everything was done by hand apart from making alignments using ClustalW! If you feel you have missed out, *Next generation systematics* is to be turned into a Systematics Association Special Volume to be published by Cambridge University Press (CUP).

There were two other symposia: *Arthropod systematics: are morphology, paleontology and molecules coming together?* and *Advances in using museum specimens and ancient DNA in systematics*. The arthropod symposium was animated with lively debates as contributors illuminated the perennially vexing problem of deep arthropod phylogeny from many different angles. However, at times perhaps just a hint of exasperation could be detected as well as even large phylogenomic datasets yielded conflicting results. Hence the participants considered the option of using the biennials as the venue for regular meetings on the topic of deep arthropod phylogenetics. The aspect of the symposium on using museum specimens and ancient DNA that kept coming home was just how valuable museum specimens are and that you never know when it might be possible to answer questions that have been around for a long time. For example, Oliver Smith’s fascinating talk on genetic archaeobotany, gave us an insight into what DNA from ancient cotton, flax and barley can reveal about domestication and the origins of agriculture, human population movements and climate change. Contributed talks covered *Theory and methods for palaeontological data, Animal and plant systematics and evolution*, and *Theory and methods of phylogenetics*.

There were so many excellent talks that it is impossible to mention them all but a few stood out for me. The first of these was presented by Tom Richards, who in the algal symposium (as a phycologist myself, no bias here) gave a hugely enthusiastic talk on the rappemonads, a new algal group discovered using environmental DNA methods. What else is out there to be discovered, particularly among the plastid-bearing microbes and how will this influence our understanding of the tree of life? I’d like to mention also Malte Ebach’s talk on a new method for discovering and identifying biotic breaks. Here, the principal method, under the mouthful Geobiotic Polyphasic Consilience or GPC for short, is to let the data define biotic breaks, rather than using useless geopolitical borders or other man-made or time-restricted constructs (where was New South Wales in the Ordovician?). I hope this catches on, and Malte when you have it worked out for the oceans I'd love to hear from you. The final presentation I will mention was the keynote address by Ralf Sommer, a world leading researcher on the evo-devo of nematodes. It was all gripping stuff and it was clear that the audience was captivated from start to finish. At last a holistic approach to the integration of evo-devo with population genetics and evolutionary ecology and a clear message that we must never forget the traditional biological routes of our subjects in this increasingly genomic and bioinformatic world.

As usual for the biennial, there was the opportunity for students to compete for the best oral presentation and poster. The overall standard of the student talks was extremely high and all the speakers are to be congratulated for their efforts. The prize for the best oral presentation went to Rachel Walker (Department of Plant Sciences, University of Cambridge) for her study of evolution and development of petal spots using the South African daisy *Gorteria diffusa*, which displays both elaborated epidermal cells and variation in overall petal spot morphology[1]. The best student poster was definitely not one for the arachnophobic. A stunning image of the head and eyes of a spider was a definite draw to George Briscoe's study of the development of 'universal' protocols to facilitate the rapid creation of mitochondrial whole-genome datasets in the True spiders[2]. We are extremely grateful to CUP who provided the prizes.

At this point I would like to mention the sponsorship we received for the biennial. We would also like to express our gratitude to CUP for their generous sponsorship of the meeting. They recently took over the publishing of the highly respected Systematics Association Special Volume series and the first two books they have published have been extremely well received and look great: Hodkinson, Jones, Waldren & Parnell (2011) *Climate Change, Ecology and Systematics*, and Fontaneto (2011) *Biogeography of micrscopic organisms. Is everything small everywhere?* We were also extremely fortunate and grateful to receive sponsorship from the following organizations: British Phycological Society, the Genetics Society and The Linnean Society. We also thank the Natural History Museum for all their support for the Systematics Association. We are also indebted to Queen's University Belfast who provided conference assistance and venues. We are also extremely grateful to Belfast City Council for enabling us to hold the conference banquet in the beautiful Belfast City Hall. I would also like to make a special thanks to Christine Maggs and her team at Queen's, who made beautiful floral arrangements to match the colours of the stunning conference poster.

So with another biennial over, the next major conference will be the 2nd Biosyst EU meeting which will be in Vienna in February 2013 and I wish the organizers every success.

Juliet Brodie (with assistance from Ronald Jenner)
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[1]Walker RH, Brockington SF., Rudall PJ, Glover BJ. Comparative transcriptomics and genomics: determining the regulators of petal spot development and the evolution of this specialised floral trait within the species complex *Gorteria diffusa*.

[2]Briscoe AG, Goodacre S, Masta S, Taylor MI1, Hurst G, Arnedo M, Creer S. Towards large-scale second-generation sequencing of full mitochondrial genomes in the Araneae.

New journal

European Journal of Taxonomy

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Big changes for names of algae, fungi, plants, and plant fossils

Botanists throw away the printing press and cancel Latin classes

Maria S. Vorontsova
Royal Botanic Gardens, Kew, email: m.vorontsova@kew.org

Botany, fungi, algae, and nomenclature are sometimes seen as the dull and conservative corner of the biological sciences. But botanists were anything but boring this summer in Melbourne and some revolutionary decisions were made by the ca. 200 people attending the Nomenclature Section of the eighteenth International Botanical Congress, 18-22 July 2011.

Rules governing names are modernised

Getting your plant names right is a complicated business. In order for *Rosa canina* L. to be a *correct* Latin binomial fit for use in your garden, the name must be *effectively* published, *validly* published, *legitimate*, and the earliest name to be published for this species (terms in the zoological Code are a bit different, but the idea is the same). All literature published since 1753 can contain names that must be considered and finding the right name is often a lengthy detective undertaking in the archives. The International Code of Botanical Nomenclature (ICBN, http://ibot.sav.sk/icbn/main.htm) is a set of rules and recommendations recognised since the "Vienna Rules" of 1905 and amended every six years by the Nomenclature Section, a five day meeting organised by the International Association for Plant Taxonomy (IAPT, www.botanik.univie.ac.at/iapt/index_layer.php) and held a week before the much larger International Botanical Congress (www.ibc2011.com). Institutional representatives carry institutional votes and each person present carries a personal vote.

Proposals to amend the code are compiled by the Rapporteurs after publication in the journal Taxon (www.ingentaconnect.com/content/iapt/tax), sent out to all IAPT members in a mail vote, then discussed and voted on in the Nomenclature Section following approval by the mail vote; propositions from the floor can also be discussed.

Latin is no longer required to describe new species

Plant habitats are disappearing every day but it is estimated that around 20% of plant species have not even been described yet. Undiscovered biodiversity is mostly in understudied places like the tropics and understudied groups such as large genera, many already available in our herbaria but not yet assessed by taxonomists. Describing new species needs careful evaluation of taxon boundaries. Many people do not realise that in order to describe a new plant species in 2011 one must also write a short description in Latin. As a graduate student I spent hours surrounded by ancient dictionaries trying to get the ablative plural adjectival endings correct, in agreement with the nouns and also in agreement with the botanical Latin tradition, not always the same as classical Latin. It was challenging for me, but what about all the people who do not have access to botanical libraries? And people who do not speak a Romance language? Latin was the international scholar's language made compulsory to make descriptions understandable to people who did not speak the publication language. Proposals to drop the requirement for Latin have been considered in almost every Nomenclature Section meeting to date, and to many people's surprise, this time the result was "yes". From 1 January 2012 English will be accepted as an alternative to Latin. It has been pointed out that the Church of England permitted an English language Bible almost 500 years ago in 1539. But do not throw away your copy of William Stearn's *Botanical Latin* just yet. This change only applies to describing new taxa. The name *Rosa canina* is still in Latin and follows Latin language rules: the gender of the adjectival epithet has to agree with the gender of the generic name. And much of the accumulated literature will remain in Latin.

New plant names can be published online

It is not always easy to connect a plant to its name. When a new species is described the name enters the pool of available names. The name is recorded by an indexing service: the International Plant Name Index (IPNI, www.ipni.org) for vascular plants, Tropicos (http://www.tropicos.org/Home.aspx) for bryophytes, AlgaeBase (www.algaebase.org) and Index Nominum Algarum (http://ucjeps.berkeley.edu/INA.html) for algae, and Index Fungorum (www.indexfungorum.org) for fungi. This description will likely be consulted by each future taxonomist revising the group. Many people are worried about possible loss of species descriptions and until now the Code has required at least two copies of the publication to be deposited in libraries. Following many years of discussion on digital formats, secure storage media, and data accessibility, a vote was taken and paper copies are no longer required. From 1 January 2012 a new plant name may be published online as a PDF (or successor format), as long as the online publication has an International Standard Serial Number (ISSN) or an International Standard Book Number (ISBN). Preliminary versions or later corrections are not accepted.

Register your mushrooms

New names of fungi must now be registered in a recognised name repository such as MycoBank (www.mycobank.org) and a unique identifier will be issued by the repository and included in the publication. This ensures that the international community is aware of all newly published names and information on the new taxa is made available. Such a system of name registration has also been proposed for animal names (via ZooBank, www.zoobank.org), and may eventually be introduced for plants, although there are worries that this limits the freedom of independent researchers. Following an additional amendment widely used names of fungi will also be fixed and protected from change.

One name to cover all life stages of a fungus

Sexual and asexual forms of a fungus can look very different from each other and it is not possible to establish that they are the same organism without using molecular technology. These different forms have been described under different names, meaning that one species of fungus could have one name for its sexual state, and one name for its asexual state. Now that the molecular technology is more broadly available this awkward situation is no longer permitted, and one species of fungus can only have one correct name, the name that was published first.

More flexibility and fewer kinds of names for plant fossils

The situation with names of fossils is even more complex than with fungi because individual parts of an organism are often preserved separately in the fossil record. One organ of one species can appear different depending on its life stage and history of preservation. Researchers build hypotheses on which fossils belong to one original organism but unlike the situation with fungi the hypotheses remain uncertain due to lack of information, and there is insufficient confidence to synonymise all the separate names (fossil plant names and the discussion surrounding them are explained by Cleal & Thomas in *Taxon* 59: 261-268). Individual parts of theoretical assemblages were called “morphotaxa” and these have now been disallowed to give greater flexibility. It is hoped that these changes will make it easier for the palaeobotanists to correctly follow the naming rules.

When is *Acacia* not an acacia?

Research has demonstrated that the trees and shrubs traditionally known by the generic name *Acacia* are in fact two separate evolutionary lineages. Australian Wattles are not the closest relatives of the iconic African (and Central and South American) acacia trees, so the two groups need to have different Latin generic names. Changing Latin names is confusing, expensive, and generally undesirable but the naming system exists to reflect evolutionary history as well as providing useful unique identifiers. Only one group can retain the Latin generic name *Acacia*. But which group? There is a larger numbeer of Australian acacia species, but African acacias have a great ecological significance in the African savanna. According to the rules, each genus has one *type species*, in this case the African *Acacia nilotica*, and the generic name stays with the type species. The type species was changed to the Australian *Acacia penninervis* at the 2005 International Botanical Congress held in Vienna in order to decrease the total number of name changes. The debate continued long after the Vienna Congress and several compromise proposals were made to the Nomenclature Section in Melbourne, including a proposal to allow the name Acacia to be used for both groups, and a proposal to change the names of both groups. The Nomenclature Section voted to uphold the decisions taken in Vienna in 2005 and the Latin generic name *Acacia* will be applied only to the Australian plants (www.ingentaconnect.com/content/iapt/tax, www.scienceinpublic.com.au/botany2011/the-acacia-debate). Of course this only concerns the formal Latin names and the use of the vernacular name acacia is not controlled by the Code.

ICBN transforms into ICNAFP: the International Code of Nomenclature for algae, fungi, and plants

This may have been the most revolutionary Nomenclature Section ever held, a fact also reflected in a change of title. An unexpected proposal from the floor suggested amending the title of the code, and it is hoped that this change will clarify which groups are governed by the Code, and help keep the rules for algae, fungi, and plants united under the same Code – recent understanding of higher level eukaryote relationships has shown that fungi and most algae are not closely related to plants. The word “organism” instead of “plant” will be used throughout to reflect the inclusion of algae, fungi, and plant fossils. The fact that fungal and algal names are regulated by the botanical code is a historic anomaly dating back to the days when fungi and algae were considered to be plants, but moving across to another set of rules is not really possible because the naming system needs to remain stable. It has been suggested that decisions on fungal names could be made at the International Mycological Congress (IMC, www.ima-mycology.org), but this may also prove difficult due to a lack of an established representation and voting system like the one at the Botanical Congresses. A Special Committee concerned with governance has been set up to explore these and other issues and report to the next Congress.

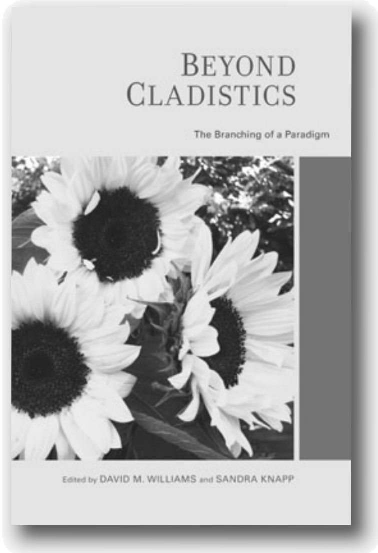
Other decisions will also have an appreciable effect on the daily work of a taxonomic botanist. A new Appendix will be added to the Code to list decisions on which names are considered to be sufficiently different to be unambiguously distinguishable (if two generic names or two species in the same genus differ only by minor spelling variation, one of them must be changed). The Editorial Committee may decide to publish the Appendices (including lists of approved and rejected names) separately from the Code itself, putting an end to the traditional thick volume printed after every Congress which has been getting thicker and thicker.

What about non-plants?

Separate communities of specialists have traditionally made decisions about different groups of organisms (explained by Knapp et al. 2004. *Phil. Trans. R. Soc. B* 359: 611–622). The International Commission on Zoological Nomenclature (ICZN, or the zoological code http://iczn.org) looks after Latin binomial names of animals with some historic exceptions such as protists (binomial means having two names, a generic name and a species epithet). The International Code of Nomenclature of Bacteria (the bacteriological Code, www.ncbi.nlm.nih.gov/books/NBK8817) looks after Latin binominal names of bacteria. Other sets of rules govern names that are not binomial (The International Code of Virus Classification and Nomenclature; International Code of Nomenclature for Cultivated Plants; International Standards for Naming Pathovars of Phytopathogenic Bacteria). Cross disciplinary work by the International Committee on Bionomenclature (ICB, www.bionomenclature.net/index.html) has produced the Draft Biocode 2011 (www.bgbm.org/biodivinf/docs/biocode2011/biocode2.html), bringing together the different sets of rules for binomial names and drawing up guidelines to apply across the Codes. Some people support the development of an overarching Biocode, but others do not like the idea of another set of rules on top of the special Codes already in use. Nomenclature is a historic discipline rich in local idiosyncrasies. Democratic independence of the separate communities is important to maintain. Would the Biocode just add complexity to what is already a confusing specialist discipline? Or should we all work together to make one united system? Join the discussion!

Book Reviews

Beyond cladistics. The branching of a paradigm



Williams DM, Knapp S. 2010. University of California Press. Hardback ISBN 978-0-520-26772-5, £44.95.

Let's cut to the quick. I was disappointed with this book. The title, *Beyond Cladistics*, promises much, suggesting that the contents will push the front line beyond the trenches. The Preface in fact tells us, "The book represents an attempt to document the nature and anticipate the future of cladistics" – "… to explore the possibilities that lie beyond cladistics …" (p. xi). This worthy goal took inspiration from the life and career of the late Chris Humphries of the Natural History Museum, London. Indeed, the first chapter, preceded by a catalogue of his writings, reviews the life and scientific contributions of Chris, but all too often the chapters in the book look backward to what was done, not forward to any new frontier. Only in a few scattered instances is there some vision in these chapters of the future. Furthermore, I found it difficult to get into these articles; they just begin abruptly with no abstract and little introduction as to what to expect. I think this modus operandi might have worked better in the original oral presentations at the Festschrift gathering, but something more is needed to prepare the readership for what is to follow in the printed form.

Chapter 2 on "Ontogeny and systematics revisited" begins with a look back at the 1985 ICSEB III symposium convened by Humphries, who also edited the resulting 1986 book, *Ontogeny and Systematics*. The authors tell us that volume played an influential role in the emerging field of 'evo-devo,' and they then proceed to reprise one chapter from that book that dealt with the matter of pollen form and spore development. However, the exposition that follows is only a bare summary of the original paper and subsequent work, and is loaded with jargon. As one example, we are told on p. 41, "These switches are interpreted under current models as being triggered at key concentrations within the colloidal environment of the primexine glycocalyx. We can only speculate, however, about whether gene-directed processes may be involved." What the blazes does that mean, and what is the point? The jargon is bad enough, but the statement fades away in a vague speculation. Some future this is!

Chapter 3, "Rooted in Cladistics," takes a look back at Humphries and Vane-Wrights intended early work on co-evolution of butterflies and their host plants. The effort was turned down for funding as being "too difficult." Is not this the story of science? — Creative people with vision have to beg money from people with neither vision, nor creativity. The author goes on to discuss the ideas of Humphries and associates for a "calculus of biodiversity"; it briefly examines the work of the Biodiversity and Conservation Laboratory at the NHM and the matter of phylogenetic diversity and the role it played in developing World-Map. The stimulating part of this chapter, however, comes near the end. Having recapped the history, the author, Dick Vane-Wright, establishes some distance from the Dawkinsian neo-Darwinist ways of thinking. He lashes out at the genes-rule-the-world paradigm (p. 57), a "deterministic and mechanistic model that is no longer even biologically realistic", and he warns us that in the future, our efforts to deal with the problems that are damaging the planet cannot avoid moving away from reductionism to a more holistic approach for solutions. Here we have a vision of the future, but why do I have the feeling that this prophet is a voice crying in the wilderness.

Chapter 4 is entitled "Do we need to describe, name, and classify all species?" It is a short and concise call to arms. It is one of the most well-organized, cogent, logical apologias for taxonomy ever – a poetic litany of reason. It deserves a wider audience than what it will get here in this book. I wonder, however, how do we break out of the seemingly unending series of pleas to save taxonomy and actually start to implement a real set of actions to hire more taxonomists. Without jobs, there is no future.

Chapter 5 examines "Floras and phylogenies" and is another good example of preaching to the choir. In a booster mode, the authors claim (p. 77) that taxonomy is on the verge of "…unprecedented change and perhaps renovation." I believe the authors rightly decry the recent penchant for making lists of species names we see in various global databases. These on-line lists, for which great sums of money have been spent to assemble, can provide listings of species, and sometimes they can even give the author of those names; but almost never can one easily get the details of the publication for those names. If you want to read the original description of a species, you'd be hard put to find any references easily in these lists. However, the authors argue that morphological characters are better than molecular ones, but has not taxonomy as a science 'evolved' beyond that kind of restrictive thinking? As an example of the value of the importance of morphology the authors cite Darwin's monographs on barnacles. True, the research that led to those volumes was critical in Darwin's conceptualization of the importance of variation in evolution, but the authors try to tell us that the barnacle monographs are still in use today. Please! After more than 150 years, cirripede classification has moved well beyond Darwin (see Newman, 1993). We will get nowhere with "change and renovation" if we keep harkening back to the past. The old ways are not the best ways.

Chapter 6 on "Island hot spots" I found rather boring; it really does not say much at all. Only at the end (p. 98) does the author break out and make a connection to Jared Diamond's 2005 book *Collapse*, where he expresses a "feeling of despair for many of the world's islands". We are left, however, hanging as to what to do. The author could have pushed the discussion if he had extended the islands of despair to consider 'this island – Earth.' He cites Vane-Wright et al's (1991) expression of the "agony of choice" as to where to focus our limited resources towards conservation; but the author skirts the real issue – there are limited resources because we have unlimited numbers of humans. The real question for the future is: how de we reduce population? If we do not, Nature will do it for us.

Chapter 7 centers on the flora of the Macaronesian Islands of the east Atlantic. Again, we have a focus on past work, but I got the feeling that many things have been left out of this exposition. True, there is big table that extends through four pages, but what does all that information mean? We don't know where many of these species listed are to be found; specific island names are left off the map. We are also told of lots of endemism, but the habitats in which these species listed are obscured in codes. We see a cladogram, but we get little idea of the time factor in the flora's evolution. We are told that the strongest connections of the flora are with the Mediterranean, but there are exceptions evident – are these hypotheses concerning the South African connections all too evident in the big table?

Chapter 8 looks backward on the history of collecting the flora in Macaronesia; it is a charming little story. One does wonder whether Darwin would have gone in a more botanical direction rather than barnacles if the *H.M.S. Beagle* had been allowed to land. However, one instinctively wants to ask what is on those islands now? It would appear that the relative ease of getting to the islands in the 18th and 19th centuries is at the crux of the problem today; has the even greater ease of mass tourism to these islands essentially doomed the flora?

Chapter 9 goes philosophical on us and plunges into the depths of "Monophyly and the two hierarchies." The chapter rattles on – 14 pages of text and notes. I must confess having a low titer for this kind of verbiage; the whole contribution is riddled with gobbledygook. Does anyone really care about removing (p. 153) "… the asymmetry between tokogenetic systems … and phylogenetic systems …"?

Chapter 10 examines "The steady resurrection of phenetics." There is a kernel of something useful here; the authors suggest (p. 171) that phenetics and cladistics can be viewed as "… ways to think about things … ," in other words, as multiple alternative hypotheses. *That* is a useful proposition. The authors, however, then go on to replay a story of numerical taxonomy that allows them to dispose, on p. 179, of phenetics because of some supposed "faulty history and philosophy." This kind of winner-take-all attitude is so tiresome. Can't we use whatever methods we may have at hand into the future to increase our understanding of nature? No, we have to believe (p. 187) "… that cladistic relationships are real, similarities abstract." Blame it all on de Candolle.

Chapter 11 shifts from plants to brachiopods. OK, 5000 genera of brachiopods cannot be ignored, and the textual dissection in this chapter of various stratigraphic diversity curves is entertaining. There has been no shortage of brachiopod workers, most of them paleontologists, and they have been busy little beavers erecting all sorts of taxa. The take-home message I got here was that if you ask what is a brachiopod [genus], one has to fall back on that old dictum – it's whatever any competent brachiopodologist says it is. So, what does all that say about what is to come, that we need to dissect past prejudices and inclinations of brachiopod workers before we can analyze them as a group in the future? Is this caveat not true for any group of organisms?

Chapter 12 on "The eukaryotic tree of life" is a nice historical overview. The author shows that each advance in technology has resulted in a reshuffling of the cards concerning that overwhelming dominant mass of the diversity of life, the unicellular organisms. This has resulted in finer and finer distinctions between clades. It would appear then that if the past is any indication of the future, we will see finer distinctions being made.

Chapter 13 reveals the story of the investigations of teleost relationships in light of Tethyan paleobiogeography. It is a potentially enlightening exercise, except that for most readers the names of taxa mentioned herein will mean very little. We are told (p. 264) that this kind of study can and should be done for other groups. Indeed, a similar exercise, albeit on a continental scale, is outlined in Chapter 14 for *Eucalyptus*. Again, this is a potentially interesting study, but I suspect that the names will mean little to most readers; one eucalypt looks much like another.

Finally, we come to Chapter 15 on "Wallacea deconstructed." This should have been an exemplary and stimulating chapter. It succeeds in part as a case study of empirical biogeography. I think the hypothesis tested – that Wallacea is a distinct biogeographic region – serves as a straw man. The historic overview is interesting, but of course the hypothesis is rejected. My problem with the chapter was of a more fundamental nature. We are told that Wallacea is the triangular area between Wallace's Line on the west and Weber's Line on the east. So, what are the definitions of Wallace's and Weber's lines? We are only informed that the Sunda Shelf is west of Wallace's Line and the Sahul Shelf is east of Weber's Line, but these were not how the lines were originally defined. Weber's Line marks the approximate point of balance between the Asian and Australian biotas, i.e., roughly 50:50. Wallace's Line marks the westernmost extent of Australian elements, i.e., to the west of the line we encounter the Asian biota. The authors of the chapter do not mention a third line, viz., Lydekker's Line, which marks the easternmost extent of the Asian elements to the east of which we encounter essentially the Australian species. Strictly speaking, Wallacea should encompass the entire transition zone, i.e., between Wallace's Line on the west and Lydekker's Line on the east. The empiricism of the authors still holds, but the region of their concern is rather sloppily defined.

In summary, I cannot recommend this book, even though it has an impressive list of worthy contributors. The Chris Humphries I knew was an interesting and forward-looking person. I think he deserved better.

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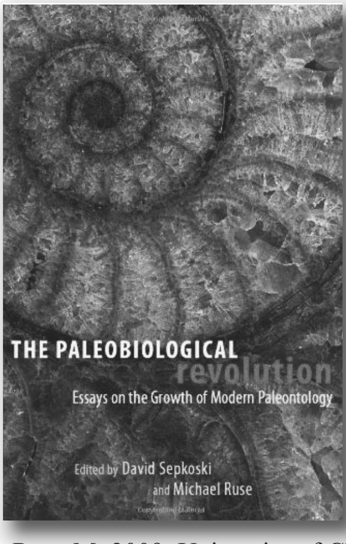
The paleobiological revolution. Essays on the growth of modern paleontology



Sepkoski D, Ruse M. 2009. University of Chicago Press. Hardcover ISBN 978-0226748610, £ 45.

“Palaeontology has long had a troubled relationship with evolutionary biology” exclaims the opening line of The Paleobiological Revolution. This book makes the laudable effort to chart the formation of palaeobiology and how it emerged as a modern field of analytical science. Through a series of essays, ranging from historical perspectives, personal commentaries, interviews and opinion pieces the combined authorship certainly covers a lot of ground. Understandably enough there is a definite focus on large-scale macro-evolutionary and computational palaeontology / palaeobiology in the book given the background of the field and the authors.

The book itself is divided into three main sections. The first of which deals with major advances in palaeobiology: David Sepkoski sets the scene detailing the emergence of the field in terms of the Modern Synthesis and some of the major divisions in thought between the palaeontological sub-disciplines and the gradual adoption of neo-Darwinian thought in palaeobiology. Mike Benton gives an account of arguably one of the current hot-topics, that of calibrating and constraining the molecular clock from a palaeontological perspective and the fidelity of the biological signal in the rock record, and in light of this bias what can we recover from fossil evidence. Richard Fortey follows up with a solid argument for the need for taxonomic expertise and classification in his chapter, which also delves into Ordovician biogeography. The classic palaeontological detective story regarding the discovery of the nature of the mysterious conodont animal follows from Derek Briggs and Richard Aldridge. Following onwards, William Schopf takes us through the history of Precambrian palaeobiology. Jack Horner gives an entertaining piece on the current who's who in dinosaur palaeobiology, including a colourful tale of the discovery of purported soft tissue in dinosaurs and the controversy surrounding the debate over the nature of these discoveries. He suggests that the sceptics didn't even come close to Maynard-Smith's 'high-table', an oft used metaphor throughout the book, referring to the paper (Maynard-Smith, 1984) in which he welcomed palaeobiology back to the 'high-table' of evolutionary biology—something that clearly has impressed strongly on the community. The first dedicated punctuated equilibrium chapter (Patricia Princehouse) follows, Francisco Ayala then gives a detailed look at the theory and implementation of molecular clock models and the recovery of biological signal from a fractious rock record.

The second section switches gear to focus more specifically on historical and conceptual issues, investigating questions such as whether model building and number crunching can hope to be accepted as equivalents of empirical experimental studies (Derek Turner) focusing especially on trends in body size evolution and biomechanics. The chapter on taxic methods I think in many regards is an exemplar of the debate that has raged between phylogenetic systematists and the classic numerical palaeobiology school of thought. Despite the assertions of multiple authors and studies to the merits of taxic techniques, ultimately they have failed to penetrate into evolutionary biology more widely. Although elegantly simple in implementation it seems unlikely that taxic methods will further impact a sceptical community of neontologists and systematists. David Fastovsky's chapter regarding societal influence in dinosaurs in popular culture features an analysis of everyone’s favourite 'non-avian dinosaur' Tyrannosaurs rex and how perceptions thereof have changed over time. Some interesting commentary also features in this chapter on the objectivity of science. Sue Turner and David Oldroyd give an excellent account of the discovery of the Ediacaran faunas and Reg Spriggs’ personal struggle for acceptance by the mainstream in palaeontology of the time.

Manfred Laublicher and Karl Nilkas discuss the German tradition in evolutionary biology and the emergence of the field of evolutionary developmental biology (evo-devo). David Sepkoski follows with another 'Punc-Eq' chapter. John Huss gives a detailed look at MBL models and clade shape, including a thorough look at phenetics and paleobiology. Joe Cain takes a look at the work of Steve Gould in the context of his relationship to George Gaylord Simpson, and of the all to common occurrence in science, that of the new generation proceeding to knife the previous, rather reprehensibly.

The famous Sepkoski et al. (1981) consensus paper forms the basis for Arnold Millers' chapter, which gives us some insight into the interactions between some of the big names in the field and the debate that has raged over the true biological signal contained in the rock record.

The final section is largely taken up with a series of personal accounts and opinion pieces by some of the major players in contemporary palaeobiology including Jim Valentine, Richard Bambach, Anthony Hallam, Art Boucot, David Raup, and David Jablonski. This definitely provides an interesting perspective on the development of palaeobiology and is interspersed with many amusing anecdotes from colleagues and students (as told by Rebecca German) alike.

The overall conclusion chapter by Michael Ruse ends on a somewhat downbeat note suggesting we should no longer seek acceptance by biologists; in effect palaeobiologists should be content to plug away in their own corner as it were. This is unfortunate given the often powerful results of combining palaeontological data with extant analogues in systematics (Donoghue et al., 1989), contra the suggestion of Patterson (1981) that fossils may have little or no effect on phylogeny. The view of palaeontology as 'stamp collecting' when current systematic work points to the crucial role of fossils in correctly reading the true nature of evolution is untenable. The dearth of discussion on systematics and the cladistic revolution is a major omission in my opinion. For example, the inclusion of fossils alongside extant taxa in cladistic analyses, to name but one; in doing so breaking the long branch attraction artefacts that have plagued efforts to reconstruct the tree of life (Wiens, 2005). Without a robust phylogeny in place upon which to base biological classifications i.e. the raw data sets which palaeobiologists analyse and draw their conclusions from, model-based analyses of diversity and evolution will have questionable empirical justification, especially if the underlying taxonomic framework is riddled with paraphyletic groups and taxa of dubious validity.

There are a couple of other areas I felt that were left under-mentioned in the book. Evo-devo is one of these. Although the importance of fossils in a comprehensive synthesis of evo-devo is alluded to in the terminal section of the morphological tradition chapter, the reader is left wanting more in many respects. Very little is mentioned of the search for homology between living and fossil taxa, especially when the relationships become unclear between the stem and crown groups of many phyla, which is odd when we consider how much weight Gould placed on the significance of early Cambrian problematica, exemplified in his interpretation of the Cambrian explosion as a function of punctuated equilibrium in 'Wonderful Life', views later contested by the palaeontological community (Briggs and Fortey, 2005).

The process of calibrating and constraining molecular clocks based on fossil dating is indeed included in the volume, which is very much welcome. Clock methods themselves are discussed at length from first principles in Francisco Ayala's chapter. I would, however, have liked to see a more comprehensive review of their current state and implementation.

Reiterating the earlier criticism, a more balanced treatment of the advances in systematics through the cladistic revolution and how they have shaped our view of evolutionary theory would have greatly improved the overall impact and relevance of the book. Another rather stark omission is the absence of any mention of the Red-Queen hypothesis (Van Valen, 1973), arguably one of the most successful concepts to emerge from the palaeobiological sphere that has integrated itself into modern evolutionary theory.

I would put this forward as my overall criticism of the book as a whole. It pulls in many directions at once, switching track too often to provide a unifying theme. As soon as you get to the meat of a theme you're shifted track onto a new one, which becomes somewhat frustrating. Though in fairness, a comprehensive treatment of all aspects of the field is nigh on impossible. This could never be described as a visually rich book but illustrations are clear and relevant where presented. There is also quite a degree of repetition throughout the book especially with regard to punctuated equilibrium.

So, it is worth the read? By all means. I think it mirrors the sometimes fractious nature of the field, while touching on some of the great philosophical debates within the community. It's not by any means what I would call a captivating page-turner and I found it somewhat difficult to read in large chunks. In spite of the flaws mentioned (admittedly, many of which stem from my personal bias towards Hennigian philosophy), however, I will concede that is does indeed do a pretty good job of what it sets out to achieve, an attempt to document the rise of palaeobiology.

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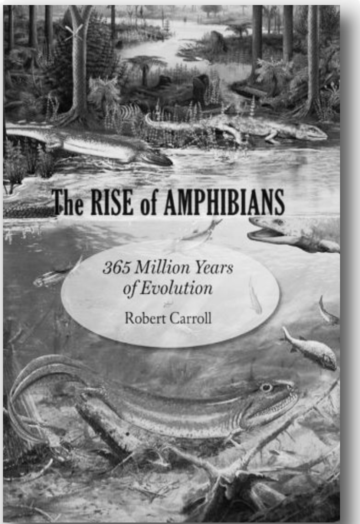
The rise of amphibians: 365 million years of evolution



Carroll RL. 2009. Johns Hopkins University Press. ISBN 978-0801891403, £34.

In layman's terms, amphibious (amphi, both and bios, life) refers to animals (or vehicles!) that function at the interface between water and land. To modern systematists, Amphibia is a monophyletic group encompassing Lissamphibia (frogs, salamanders and caecilians) and those extinct tetrapods more closely related to lissamphibians than to amniotes (reptiles, birds and mammals). However, the pre-cladistic concept of amphibian was broader and gradal, encompassing all tetrapods (i.e. animals with limbs not fins) that were not diagnostically amniote. Fossil amphibians in this sense included the ancestors not only of lissamphibians but also of amniotes. Modern phylogenetic systematics, of course, eschews such paraphyletic 'bins' but Robert Carroll is not a cladist and prefers 'traditional' methodology and terminology (e.g. 'mammal-like reptile', 'labyrinthodont'). His approach is made clear in one of the first figures in the book (Fig. 1.2, p.3) which depicts an evolutionary tree of the type ('balloon groups') found in vertebrate text-books more than half a century ago.

'The rise of amphibians' begins (Chapter 1), appropriately enough, with a summary of Earth history and the origins of life, followed (Chapter 2) by the diversification of multicellular animals and the origin of vertebrates. Chapter 3 reviews 'amphibian' (i.e. tetrapod) origins and is followed by a meaty catalogue (chapter 4) of Carboniferous tetrapods (some of which are amphibian). It is followed (Chapter 5) by an orientational pause that briefly considers and then rejects computational phylogenetic systematics in favour of more 'traditional' approaches. That barrier overcome, chapters 6-8 continue the review of Carboniferous, Permian and Mesozoic 'amphibian' diversity, leading finally to the vexed question of lissamphibian evolution.

Lissamphibian origins remains one of the major problems of amphibian palaeobiology, largely because the fossil record seems to be poor at preserving small tetrapods from freshwater/mesic environments. Three hypotheses compete for attention. One is that all lissamphibians are derived from the same small group of dissorophoid temnospondyl amphibians (e.g. Ruta et al. 2003). A second posits a common origin from a completely different archaic clade, the lysorophids (e.g. Vallin and Laurin 2004). The third, long favoured by Carroll, advocates a polyphyletic origin from two or more distinct ancestral groups (e.g. Anderson et al. 2008) and he uses Chapters 9-12 to restate this argument, moving from general considerations (Chapter 9), to frogs (chapter 10), salamanders (chapter 11) and caecilians (Chapter 12). For each group, a short review of the early fossil evidence is followed by a listing of common characters (derived, primitive and homoplastic). This serves as the basis from which to scrutinise a check-list of possible candidates for ancestry. Unsurprisingly he concludes, as previously, that frogs stem from amphibamid dissorophoids, salamanders from a slightly different dissorophoid group (branchiosaurs), and caecilians from lepospondyl microsaurs. The fossil data is reasonably up-to-date although description of the important temnospondyl *Gerobatrachus* (Anderson et al. 2008) presumably came out too late for inclusion. The final chapters deal with the diversification of crown-group lissamphibians (Ch. 13) and their long-term future (Ch. 14).

This is a difficult book to categorise. It is essentially a compendium and synthesis of the author's personal ideas on early tetrapod history formulated over a long career in palaeontology and in the tradition of Carroll's distinguished mentor A.S. Romer. Carroll is to be applauded for a nod at evolutionary developmental biology, but the book is flawed by its approach to systematic and offers no new synthesis. Its target audience is not clear. It has the size and format of a coffee table book and does contain a central splash of brightly coloured plates, albeit of rather stolid reconstructions. The book's introductory sections imply it is aimed at a non-specialist reader but the writing style and the minutiae of some of the character descriptions militate against that. This is certainly not a book you would buy for a palaeontologically interested friend or teenager, and undergraduates would find a general textbook of vertebrate palaeontology more useful. Anyone wanting to read about the early stages of tetrapod evolution would be better served by Jenny Clack's award winning 'Gaining Ground' and graduate students or specialists would find volume 4 of the *Amphibian Biology* series (Heatwole & Carroll 2000) a better reference source.

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A retrospective

Systematics and biogeography: cladistics and vicariance

Nelson G, Platnick N. 1981. Hardback ISBN 0-231-14574-3. Free electronic version: http://www.ucpress.edu/series.php?ser=spsy

Systematics & Biogeography: Cladistics and Vicariance by Gareth Nelson and Norman I. Platnick (1981), because of its blue dust jacket, is often referred to simply as the 'Blue Book'. It was published in 1981, during the close of the Cladistic Revolution. *Systematic Zoology* (now *Systematic Biology*), then the primary journal for systematics, published two reviews, back to back. In the first, Dan Brooks suggested that Nelson and Platnick had "come as close to 'proving' evolution has occurred" (see Brooks, 1982: 208; see also Brady, 1982 and Sneath, 1982). *Systematics & Biograph*y is often understood as the first complete summary of what came to be called 'pattern' cladistics, which, years later, some wished "would finally disappear" (Donoghue 2001: 756; see also Felsenstein, 2001). Now that view can be judged afresh as *Systematics & Biogeography* is once again available, now as a free downloadable electronic file (PDF, 29.6 MB), as part of the *Species and Systematics* book series on the University of California Press website:
http://www.ucpress.edu/series.php?ser=spsy

What was the significance of this book, and what is its significance today? Every now and then a scientific discipline undergoes revolution, an episode that changes the way a subject is perceived, the way it is understood and undertaken – a new vision emerges that prevents a return to the subject matter as it was before, a paradigm change, if you like, some genuine progress. In the last century, a revolution in phylogenetics and systematics began with the work of entomologist Willi Hennig (1950, 1966) via its interpretation by Lars Brundin (1966), a chironomid specialist. Some years later, the need for revolution was succinctly put by palaeontologist Colin Patterson:

"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)

That 'commonplace belief' was eventually rejected in favour of determining relationships from evidence provided by extant (and extinct) organisms (characters, homologies), a shift from the preoccupation of discovering direct ancestry from the fossil record to assessments of common ancestry. Hennig called his approach *Phylogenetic Systematics*, the title of his 1966 English book (Hennig 1966). *Phylogenetic Systematics* eventually became known as cladistics, hence the 'Cladistic Revolution'. The cladistic revolution overturned the central position of the last 100 years of palaeontology, changing Ernst Haeckel's *Systematische Phylogenie* into Hennig's *Phylogenetic Systematics*.

Revolutions either progress or stagnate. By the 1980s, alongside *Systematics & Biogeography*, two further books were published that dealt with cladistics. Each approached its topic from a different perspective (Eldredge and Cracraft, 1981; Wiley, 1981). All the authors (except Cracraft) were then at the same institute, the American Museum of Natural History in New York, USA. The three books differed in such a fundamental way that it would end up highlighting a significant rift within the systematic and biogeographical communities. In order to understand why these three books were published it is necessary to understand a little of the history of classification.

For biology to be comparative, classification is necessary. Biological ideas concerning laws of transmutation (things that change), processes such as photosynthesis and ontogeny, are all part of what might be understood as General Biology. General Biology is not necessarily comparative but it dominates the biological sciences as well as the public perception of biology. The comparative side – classification – is given little attention and usually considered to be either subjective or descriptive, and cannot compete with chemistry or the physical sciences in the quest for empiricism and experimentation.

Comparative biology – systematics and biogeography – is *not* experimental; it is *delicately* empirical (Ebach, 2005). Comparative biology is concerned with the discovery of patterns in nature, that is, the discovery of reoccurring relationships among taxa. Patterns inform about common, shared histories either between taxa (systematics) or biotic areas (biogeography). It is the evidence that demonstrates evolution having taken place. All mammals, for instance, are more closely related to each other than they are to any other organism. The taxonomy of mammals, then, has been right from the start. It forms a natural classification – an evolutionary or monophyletic group. Taxa discovered to be artificial, like reptiles, for example, are non-evolutionary or non-monophyletic. The taxonomy of reptiles has consistently been shown to be wrong and in need of revision. The question of how to discover natural (evolutionary) groups has been the core debate, probably first clearly articulated by Candolle in 1813: what constitutes affinity (homology) between organisms?

The question of homology relates to the question of relationship, a notoriously vague term with many different meanings. Genealogical relationship, for example, occurs when two unrelated organisms form another individual organism (e.g. parents and their off-spring). In this sense, genealogical relationships can be observed and documented. No single individual will know their Pleistocene ancestors, simply because any documented births and marriages are rather limited. The same is true for all organisms: homology therefore requires more than genealogy: it requires a classification.

Candolle's contribution to this debate was to distinguish artificial from natural classifications. All organisms are similar in one way or another. Relationships in natural classifications are affinities - that is, manifestations of the same thing. Artificial classifications rely solely on similarities. Considering any two things as similar in some way, relationships as similarities provide no evidence or information about natural groups and evolution. Artificial classifications, however, can be used to identify objects without the need to know affinities. This means that any person should be able to distinguish between different types of flowers without having to understand their affinities. A modern interpretation of Candolle's twin schema would be:

Similarity = artificial classifications = keys = identification

Affinity = natural classifications = relationship/homology = evolution

The simplicity of using similarities to draw connections between characteristics is possibly due to the lack of an efficient empirical method available at the time of Candolle. Artificial classification work sufficiently well to identify taxa. The error of 19th century naturalists was to use it to draw historical connections between different taxa. Similarity, as a tool for drawing speculations, combined with academic authority, resulted in confusion between natural and artificial classifications. Candolle's schema was synthesised:

Similarity = [natural] classification = genealogy/homology = identification = relationship

Ernst Haeckel, and other 19th century naturalists, compounded this confused equation by adding fossils as ancestors into these highly speculative connections (lineages, or, even, ghost lineages), between taxa. If genealogy need be extrapolated to all taxa, then fossils must clearly be the ancestors of extant taxa, just as the French are the descendants of the ancient Gauls and the Egyptian pharaohs' descendants of Osiris.

The naturalists of the 19th century, keen to use a new explanatory mechanism (natural selection) to assist in the discovery of ancestors, reversed Candolle's schema. Systematists have ignored this reversal to the detriment of classification, the discovery of natural groups and evolution. Explanatory mechanisms have since dominated systematics and biogeography to the extent that classification is, once again, being dismissed as irrelevant (Felsenstein, 2004). The chief blunder of systematists and biogeographers has been to confuse highly speculative hypotheses of genealogical relationship (modelling) with empiricism. Since

Haeckel, a number of attempts at highlighting and outlining the error and thus to promote natural classifications were unsuccessful (Naef, 1919, Kalin, 1945, Zangerl, 1948) – until Hennig's work (1950) appeared in English (Hennig, 1966) and Brundin (1966) applied Hennig's ideas to the discovery of transantarctic relationships for biting midges (1966).

One might ultimately understand cladistics (and Hennig's phylogenetic systematics) emerging from Stockholm, Sweden, via Brundin's midge study spreading from that source into palaeontology, zoology and botany (Williams & Knapp, 2010). Commenting on the reception of Hennig's work Brundin later noted,

"Hennig's epoch-making ideas went largely unnoticed for several years, but for my part there was no doubt that I had found the tool necessary for a successful analysis of the history behind the Antarctic vicariance pattern" (Brundin in Wanntorp, 1993:362: 1989:473).

For entomology the path was more directly via Hennig's systematic work on Diptera and his reflections on the possibilities of biogeography; Brundin recognized Hennig's monograph (1960) *Die Dipteren-Fauna von Neuseeland als systematisches und tiergeographisches Problem* as "a very important paper" (Brundin, 1963:426; see also Illies, 1965: 509; Schuh & Wygodzinsky, 1977:105; Page, 1989; Craw, 1992:90); the paper was to be translated into English by Petr Wygodzinsky (Hennig 1966b), the person who introduced Platnick to phylogenetic systematics (Farris and Platnick 1989).

Cladistics has been seen by some as a reaction to phylogeny reconstruction, or at least the framework in which Haeckel propelled it. In reality, *Systematics & Biogeography* was a detailed critique of Haeckel's legacy as well as an attempt to revive natural classification, as outlined in Candolle's 1813 *Théorie élémentaire de la Botanique*.

The Blue Book did help systematists and biogeographers gain a sharper focus on classification. Biogeography especially has benefited from a move away from simply charting out hypothetical pathways to classifying regions based on biotic composition (see Morrone, 2001; Ebach & Parenti, 2009). Classification of areas had brought about the area endemism debate, one that has revitalised biogeography and will in the future help phylogeographers develop a comparative method (see Arbogast and Kenagy, 2001). Systematics has benefited greatly in defining its aims. Rather than propose genealogies in highly questionable groups, systematists along with taxonomists have revised several groups and raised objections to artificial taxa such as protists, algae, reptiles and fishes.

The negative effects, however, have been brought on by the continued confusion between artificial and natural and classifications (Cavalier-Smith, 2010; Stuessy 2009), genealogy with classification (Brummitt, 2003), methodology with phylogeny (Felsenstein, 2004), genealogy with nomenclature (Phylocode) and, arbitrary similarity with classification (DNA Barcoding). These negative effects would easily be dismissed if classification is treated separate from genealogy, and hypothetical lineages and poorly defined taxonomic groups are revised. Moreover, the constant battle with creationism or "Intelligent Design" has placed evolutionary biologists on the offensive in that classification is immediately inferred to be evolutionary, whether it be natural or artificial.

30 years on, *Systematics & Biogeography* is still highly relevant –perhaps even more so. Those that wish to understand classification and its importance in discovering evolution will find it an invaluable resource. Those that wish to confuse classification as an inference of ancestors, lineages or genealogy, may learn something: namely that artificial classifications (i.e., paraphyly) have no place in evolutionary studies.

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Inspirations

An Interview with John Wiens

John J. Wiens is Associate Professor and herpetologist in the Department of Ecology and Evolution at Sony Brook University in Stony Brook, New York.

How would you summarize yourself in the form of a title of a scientific paper?
Schizophrenic herpetologist juggles empirical phylogenetics, systematic theory, evolutionary biology, and ecology with variable and questionable success.

Summarize the when and where of your academic career
I was an undergraduate at the University of Kansas from 1986 to 1991. It was a great experience because the graduate students and curators in the herpetology department at the natural history museum really took me in and got me started doing research, including three collecting trips to South America. After that I was a Ph.D. student at the University of Texas at Austin, with David Hillis and David Cannatella, from 1991 to 1995. During my third year at Texas, I got offered a job as a herpetology curator at the Carnegie Museum of Natural History in Pittsburgh. I was there from 1995 to 2002. In 2003, I joined the faculty in the Department of Ecology

and Evolution at Stony Brook University, and I have been here ever since.

When did you decide to follow the career path you are on now?
I got the herpetology bug pretty early, and I think that I decided that I was going to spend my life working with reptiles and amphibians when I was about 6 or 7. But it was not until my first semester at the University of Kansas, when I spent time in the natural history museum, that I decided I wanted to focus on systematics and evolutionary biology.

What are the main goals of your research, and what are your future ambitions?
My research interests are fairly diverse, but I am interested in making progress in three main areas: (1) reconstructing the phylogeny of reptiles and amphibians, (2) understanding and improving our methods for reconstructing phylogenies and delimiting species, (3) using phylogenies to understand evolutionary and ecological patterns and processes, like speciation, patterns of species richness, community assembly, morphological evolution, biogeography, and life-history evolution. I am also becoming increasingly interested in conservation-related applications of phylogenetic and evolutionary research.

What organisms have you worked on, and which are your favorite organisms and why?
I have worked mostly on reptiles and amphibians, but I have lately started to expand a bit into other groups of animals. But my favorites are frogs, lizards, salamanders, and snakes. Exactly why they are my favorites is hard to answer. I have just been crazy about them since I was a little kid.

How many hours per week do you work?
I would guesstimate that it is usually between about 70 and 80.

What percentage of time do you spend on each of your different responsibilities?
During the school year, I spend about half my time doing research and about half doing "other." "Other" includes teaching, committee work, meeting with students, reviewing papers, and handling papers as an editor or associate editor. A lot of my research time is spent analyzing data, writing papers, and helping my students and collaborators with analyses and writing.

How many undergraduate, PhD students, postdocs, and technicians are in your lab?
This varies quite a bit from year to year. At present I have three wonderful Ph.D. students (Caitlin Fisher-Reid, Xia Hua, and Dan Moen) and five new graduate students starting this fall. And I usually have about 3 or 4 undergraduates working in the lab at any given time. I have also had a lot of visiting students and researchers from other countries in the lab lately.

What gives you the most satisfaction and frustration in your job?
I think that finishing a paper that has turned out to be really exciting (at least to me) is one of the most satisfying parts. Conversely, dealing with comments from reviewers who found that same paper to be decidedly unexciting is one of the most frustrating parts!

Could you say something about the importance of international collaborations for your research?
Like most people, I think that international collaborations are hugely important for my research.

What kind of field work do you do and where has it taken you so far?
Most of my field work has involved collecting specimens and tissues for systematics research. I have done most of my field work in the Neotropics, including Mexico, Peru, Ecuador, and Argentina. I have been lucky enough to get some field experience in other areas too, like Europe and Asia.

Did any memorable incidents happened during field collecting?
One incident that sticks out in my mind was when I was shot at in southern Mexico. I was looking for treefrogs one night with Adrian Nieto Montes de Oca and Tod Reeder in the southeastern part of the state of Oaxaca, near the border with Chiapas. We were looking for frogs along a river at night, and I guess we must have walked too close to a farmer's field on our way down to the water. I heard gunshots, but I wasn't sure if they were meant for us. So, I crept over to where Adrian was and whispered excitedly "Hey Adrian! I think someone is shooting at us!" And he dryly replied "Yes, I think you are right. So maybe you should turn off your head lamp." Of course, he had a good point, and I think what he also meant was: "Or, if you are going to leave your headlamp on while someone is shooting at us, would you mind not standing right next to me?"

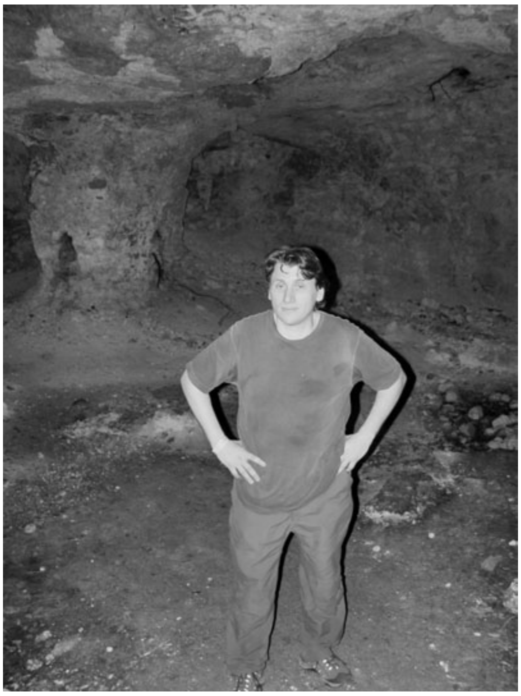
Is there any paper or book that has been very influential for your thinking?
I work on somewhat disparate topics, and so different papers and books have influenced my thinking in each area. For example, in systematic theory, I think that various papers by Joe Felsenstein, David Swofford, David Hillis, and John Huelsenbeck were really influential. In evolutionary ecology, papers by Robert Ricklefs, Dolph Schluter, Jonathan Losos, and Mark

McPeek were really important and inspirational.

Who was the most important mentor in your career?
I have had several great mentors. Bill Duellman, Darrel Frost, and Linda Trueb were the ones who got me started in systematics and professional herpetology, and gave me some great opportunities despite my being just an inexperienced undergrad. My Ph.D. advisors (David Hillis and David Cannatella) were also fantastic and supportive.

What is the best advice you have ever received?
I am going to cheat and mention two pieces of advice, both from my grad school days at the University of Texas at Austin. I remember that David Hillis did not give that much general advice to his students. But one day he spoke to the lab and said that the most important thing in picking your research topic is to do research that you really, really like to do. And then he added: but don't kid yourself, unless you have a lot of papers in really good journals, you are not going to get a job. I think those two considerations are really important for grad students to keep in mind.

The other thing that I will mention was a great course at UT-Austin that I took called "research design." This was given by Jim Bull and Craig Pease. Mostly, this course just consisted of Jim and Craig asking us questions about our planned Ph.D. research. But they were great questions like: what are the possible outcomes of your project? Will it still be interesting, regardless of which hypothesis the data support? Where do you plan to publish it? Is that an important journal in your field? How will the research impact the field? Who do you think will cite and use your study, and why?


John in the field in Mexico.

How many scientific publications do you have at the moment?
I have about 110 research papers published or "in press" at the moment.

Could you nominate any of your discoveries or papers as the most important?
In terms of simply finding interesting things, I would nominate two papers that were both published in 2003. One was a simulation paper published in *Systematic Biology* that showed that you could accurately place species in a phylogeny even if 95% of their data were missing (and explained why). The other was an empirical paper on species richness patterns in turtles that was published in the *American Naturalist* (with my first Ph.D. student, Patrick Stephens, as the lead author). This paper showed that species richness in different regions (and at local sites within regions) can be explained simply by how long the group of organisms has been present and speciating in each region. Many of my subsequent papers have been aimed at testing whether those two papers were (more-or-less) generally correct! Both findings are potentially relevant to any group of organisms, but there are certainly many exceptions to these results.

You have a lot of papers with co-authors. Is there a particular contribution you generally bring to these papers?
I think it varies from paper to paper, but maybe the most important thing is to try and bring one or two good ideas to each paper.

What skills do you think a successful researcher in your discipline must possess?
I think that there are many different ways to be successful in systematics, evolution, and ecology. Some people are great in the lab, some in the field, and some have great computational and analytical skills. Some people are great at just one or two of these, some are great at all of them, and some (like me) are more like "none of the above." But I think that it is important to learn how to ask important and novel research questions, to be able to rigorously analyze your data once you have collected them, and to have the writing skills to publish your work in the top journals in the field. I also think that being motivated and hard-working may be more important than any particular skill.

Do you have any tips for students aspiring to a career like yours?
I think it is an open question as to whether students should aspire to a career like mine. Specifically, I think that it can be advantageous to specialize on a particular topic or area, rather than spreading out to several. Regardless, I think that if you are going to have a research career, it is important to ask new and interesting questions, and not simply do the same thing

that everyone else is doing but in a different group of organisms. Asking new questions requires some creativity (and some chutzpah), and also knowing the literature well enough to know what has (and has not) been done before.

What do you think are currently the greatest impediments to achieving a successful career in your field, and how could you go about dealing with these?
Right now I think that the dearth of academic jobs is the greatest impediment. Once people get a Ph.D., it is hard for them to launch a career if they cannot get a permanent job. That is a hard one to deal with, for obvious reasons. But having a lot of good papers in top-tier journals will help get a job once a position opens up.

Calendar

1 December 2011
Young Systematists' Forum
Flett Lecture Theatre, Natural History Museum, London
For details see page 2 of this issue.

30 November 2011
AGM and President's Lecture
The Linnean Society, Burlington House, London
Speaker: Professor Jenny Clack, University of Cambridge
See our website for further details

19 April 2012
5th Annual Biodiversity Lecture
The Linnean Society, Burlington House, London
Details to be announced.

20 June 2012
75th Anniversary Systematics Association
Flett Lecture Theatre, Natural History Museum, London
Confirmed speakers: Sir David Attenborough, Richard Fortey
Further details to be announced.

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

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 Jon Bennett (membership@systass.org), St Pauls School, Lonsdale Road, London SW13 9JT, U.K.

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