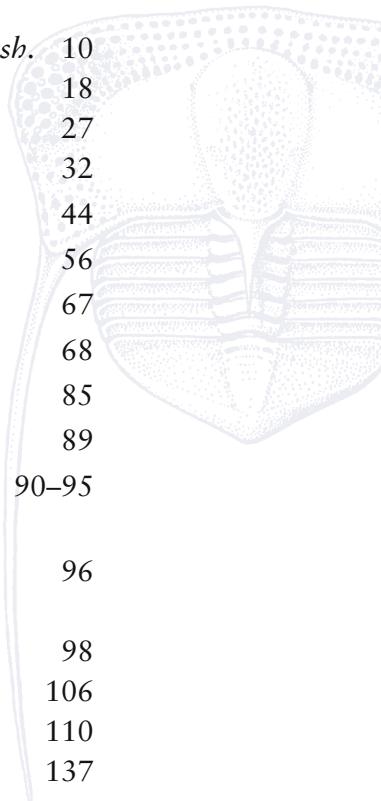


The Palaeontology Newsletter

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Reminder: The deadline for copy for Issue no 58 is 11th February 2005

On the Web: <<http://www.palass.org/>>

Association Business

48TH ANNUAL GENERAL MEETING AND ANNUAL ADDRESS

48th Annual General Meeting and Annual Address

Saturday, 18th December 4.45 to 5.10 pm

MACC Congress Centre, USTL campus at Villeneuve d'Ascq.

AGENDA

1. Apologies for absence
2. Annual Report for 2003
3. Accounts and Balance Sheet for 2003
4. Changes to subscription rates
5. Presentation of the Lapworth Medal
6. Presentation of the Hodson Fund
7. Presentation of the Mary Anning Awards
8. Election of Council and vote of thanks to retiring members
9. Annual address

H. A. Armstrong

Secretary

AGM MINUTES 2003

Minutes of the Annual General Meeting held on Wednesday, 7th May 2003 at the Natural History Museum, London.

Apologies for absence: Prof. D.E.G. Briggs, Prof. S.K. Donovan, Dr S. Evans, Dr S. Gabbott, Prof. J. Hancock, Prof. D.A.T. Harper, Dr E. Harper, Dr J. Hilton, Dr A.L.A. Johnson, Dr I.J. Sansom.

1. Annual Report for 2002. Agreed, proposed by Dr Donoghue and seconded by Dr Purnell.

2. Accounts and Balance Sheet for 2002. Agreed, proposed by Dr Polly and seconded by Dr Manning.

3. Election of Council and vote of thanks to retiring members

- i. Dr Smith extended a vote of thanks to the retiring members of Council Dr J. Clack, Dr A.L.A. Johnson, Dr I.J. Sansom, Dr S. Gabbott, Dr P. Orr and Dr Milsom. Dr Purnell extended a vote of thanks to Dr Smith.
- ii. It was noted the following members of Council would be moving to new posts: Dr M.A. Purnell (Vice president), Prof. E.N.K. Clarkson (Ed.) and Dr Polly (Ed.). Dr Hilton will take over responsibility as Web Officer.
- iii. New members of Council include Dr. L. Anderson (Ed., co-opted), Dr Per Ahlberg (Ed.).

4. Sylvester-Bradley Awards

- i. Dr Smith announced eight grants had been awarded to Broughton, Gladwell, Harrington, Hernandez-Castillo, Lane, Joomun, Tetlie and Zuykov

5. Proposed change to Constitution article 3

- i. It was agreed to include the category of Honorary Life Membership, proposed by Prof. E.N.K. Clarkson, seconded by Dr Cusack.

Howard A. Armstrong

Secretary

Sylvester-Bradley Award

Awards are made to assist palaeontological research (travel, visits to museums, fieldwork *etc.*), with each award having a maximum value of £1,000. Preference is given to applications for a single purpose (rather than top-ups of other grant applications) and no definite age limit is applied, although some preference may be given to younger applicants or those at the start of their careers. The award is open to both amateur and professional palaeontologists, but preference will be given to members of the Association. The awards are announced at the AGM.

Council will also consider awards in excess of £1,000, particularly for pilot projects which are likely to facilitate a future application to a national research funding body.

Electronic submission of applications, through the website, is preferred and will comprise a CV, an account of research aims and objectives (5,000 characters maximum), and a breakdown of the proposed expenditure. Each application should be accompanied by the names of a personal and scientific referee. Successful candidates must produce a report for *Palaeontology Newsletter* and are asked to consider the Association's meetings and publications as media for conveying the research results. **Deadline: Friday 26th November 2004.**

Prestigious new website award from PalAss

“The Golden Trilobite Awards”

Over the past few years there has been an exponential increase in the number of quality websites promoting palaeontology and its allied sciences. These are widely used by professionals and amateurs alike and are a valuable resource for our science. Council has decided to recognise the work that goes into developing these sites by awarding an annual **Website Award**. Awards will be announced at the AGM and published on our website and in the *Newsletter*. There will be two awards, decided by Council members for the best institutional and amateur sites.



To get the ball rolling for this year, two websites have been recognised as reaching the highest standards:

Institutional Award: The echinoid directory

<<http://www.nhm.ac.uk/palaeontology/echinoids/>>

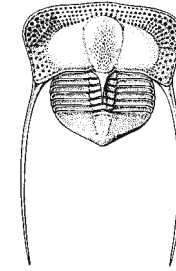
The echinoid directory is ostensibly a taxonomic resource for the scientific community in which the genera and higher taxa of echinoids can be simply and rapidly identified. There are currently over 350 pages of detailed information about echinoid taxa, including colour images of the highest quality. The site also provides an introduction to the anatomy, biology, ecology, ontogeny and phylogeny of echinoids, as well as a key to aid identification. The site has been designed and created by Andrew Smith of the Department of Palaeontology, Natural History Museum, London.

Amateur Award: Fossils of the Gault Clay and Folkestone Beds of Kent, UK

<<http://www.gaultammonite.co.uk/home.html>>

This site is dedicated to the fossils of the Lower Cretaceous Albian (Gault Clay and Folkestone Beds) in the county of Kent, south-east England, providing a pictorial identification to the fauna. The colour photographic images are of the highest quality. The site is based on the work and collections (much of which has subsequently been donated to the Natural History Museum, London) of the late Jim Craig, and is maintained by Fred Clouter.

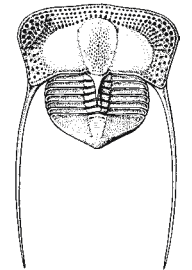
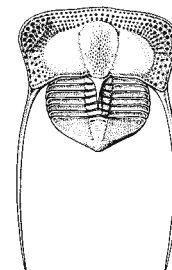
Please send your nominations for the 2005 Awards to <secretary@palass.org>.



Palaeontology: CALL FOR SHORT PAPERS!

From January 2005 *Palaeontology* will be published in A4 size with a new layout. In line with this development space will be reserved for rapid publication of short papers on topical issues, exceptional new discoveries, and major developments that have important implications for evolution, palaeoclimate, depositional environments and other matters of general interest to palaeontologists. Papers, which should not exceed 6 printed pages, should be submitted in the normal way, but they will be refereed rapidly and fast tracked, on acceptance, for publication in the next available issue.

Submission of longer review papers is also encouraged, and these too will be given priority for rapid publication. While *Palaeontology* maintains its reputation for scientific quality and presentation, these developments will ensure that the Impact Factor of the journal reflects its status as a leading publication in the field (rising to 1.19 in 2003).



ASSOCIATION MEETINGS



48th Annual Meeting of The Palaeontological Association

Lille, France 17 – 20 December 2004

The 48th Annual Meeting of the Palaeontological Association will be held in the Congress Centre (MACC) of the Université des Sciences et Technologies de Lille (USTL), on its campus at Villeneuve d'Ascq near Lille. The opening session with a seminar on Palaeobiogeography will be held on Friday afternoon, 17th December, in the lecture hall of the Beaux Arts Museum in the centre of Lille. In the evening the Icebreaker Party will be held in the Natural History Museum of Lille, in the heart of the Capitale Européenne de la Culture 2004 (<<http://www.lille2004.fr/>>).

The technical sessions will consist of two full days of talks in the main lecture hall of the Congress Centre at Villeneuve d'Ascq on Saturday 18th and Sunday 19th December, with poster sessions in an adjoining lecture hall. Technical sessions are open to all aspects of palaeontology. All talks will be scheduled for 15 minutes including five minutes for discussion; there are no parallel sessions so it is possible that some proposed oral contributions will have to be rescheduled as posters. On Monday 20th December two geological excursions will be organised; one to examine the Mesozoic sediments on the coastal sections in the Boullonnais area, the other to the Palaeozoic (Cambrian-Devonian) rocks exposed in the Brabant Massif, Belgium.

Seminar : Palaeobiogeography

Friday, 17th December 2004 (part of the Annual Meeting)

For the second time, an afternoon seminar is added to the Annual Meeting. This afternoon of thematic talks and discussion will focus on palaeobiogeography and take place on Friday, 17th December 2004. The seminar will highlight the importance of fossils to understanding past and present palaeogeographical and biogeographical patterns. The seminar will include lectures by the following speakers:

- Fabrizio Cecca (Paléobiodiversité et Paléoenvironnements, Université Pierre et Marie Curie, Paris)
- Richard Fortey (Department of Palaeontology, The Natural History Museum, London)
- Pascal Neige (Biogéosciences, Université de Bourgogne, Dijon)
- Brian Rosen (Department of Zoology, The Natural History Museum, London)
- Chris Scotese (Department of Geology, University of Texas at Arlington)

Attendance at the seminar is free to conference participants, but only if booked in advance (limited number of seats available in the Musée des Beaux Arts Amphithéâtre). Please do not turn up on the day without informing the organisers.

Venue and Travel

Details about the City of Lille can be found on the website at <<http://www.mairie-lille.fr/>>. Lille is less than two hours from London by Eurostar (<<http://www.eurostar.com/>>), and tickets at reduced prices are available if you book some weeks in advance. Lille can also be reached easily from Paris and its airport Charles de Gaulle (about one hour) and from Brussels (about 40 minutes) by TGV high speed trains. The Congress Centre can easily be reached by Metro.

Accommodation

Accommodation will mostly be organised in hotels around the Congress Centre at Villeneuve d'Ascq. This is approximately 15 minutes' walk from the conference venue. Participants are free to book alternative accommodation at a wide range of prices. Please note that the strict deadline for reservation of accommodation through the organisers is **10th September**. After this date, accommodation must be arranged by the participants themselves.

Registration and booking

Booking for accommodation, field excursions and also abstract submission for the conference must be completed by **Midnight on Friday 10th September 2004**. After this date participants will have to organise their own accommodation in Lille in order to attend the meeting. After 10th September registration for the meeting is still possible but will incur additional costs. Final registration for the meeting will be Friday 3rd December.

Registration details and online registration

<<http://www.palass.org/forms/XAnnualRegistration.html>>

For the second time, registration, abstract submission and payment (by credit card) are by online forms at <<http://www.palass.org/>>.

Outline Programme:

Friday 17th December :

Afternoon seminar, Palaeobiogeography

Lecture hall of the Beaux Arts Museum, Lille

Icebreaker party

Natural History Museum of Lille

Saturday 18th December :

Scientific sessions, followed by :

Annual Address: *Palaeontologia de profundis* by S. Bengtson (NRM, Stockholm)

Congress Centre of the Université des Sciences et Technologies de Lille

Members reception & Annual Dinner

University Restaurant 'Charles Barrois'

Sunday 19th December :

Scientific sessions:

Congress Centre of the Université des Sciences et Technologies de Lille

Monday 20th December :

Post conference excursions

Travel grants to help student members (doctoral and earlier) to attend the Lille meeting in order to present a talk or poster

The Palaeontological Association runs a programme of travel grants to assist student members presenting talks or posters at the Annual Meeting. For the Lille meeting, grants of up to £100 (or the Euro equivalents) will be available to student presenters who are travelling from outside continental Europe. The amount payable is dependent on the number of applicants. Payment of these awards is given as a disbursement at the meeting, not as an advance payment. Students interested in applying for a Palass travel grant should contact the Executive Officer, Dr Tim Palmer, by e-mail at <palass@palass.org> once the organisers have confirmed that their presentation is accepted and before 10th December 2004.

Annual Address

This year's Annual Address of the Palaeontological Association will be given by Prof. Stefan Bengtson and will take place during the Association's Annual Meeting, on Saturday 18th December 2004, at the Congress Centre of the Université des Sciences et Technologies de Lille.

'Palaeontologia de profundis'

Stefan Bengtson

Swedish Museum of Natural History, Stockholm <Stefan.bengtson@nrm.se>

Palaeontology is about life in deep time. The deeper we go in palaeontological time, however, the more we encounter also other aspects of depth: the depth of our understanding, the depth of our convictions, and indeed the very foundations of our science and of science in general. Through recent work in the depths of palaeontological time, I have experienced various fascinating perspectives of *palaeontologia de profundis*, ranging from the joy of working with people, rocks and fossils Down Under to the despair at the depths of our pigheadedness when deep convictions seem violated. Scientific work needs creativity, scepticism, and even conviction, in a process that can be thought of as Darwinian, but the way we let these interact with each other decides how good our science will be. In addition to scientific examples, I will give a seasonable illustration from music: Arthur Honegger's *Une cantate de Noël* (A Christmas Cantata) opens with the ancient cry of despair *De profundis clamavi* (Out of the depths I cry), but the stunningly complex web of carols woven by Honegger later in the same cantata lifts the sense of despair by showing the beauty of multiple ideas in clashing harmony. Palaeontology arises out of the depths, and with the help of clashing opinions it flies. In the end, as every flier knows, convection is better than conviction to keep you in the air.



Lyell Meeting 2005: Applied Phylogeny

London 9 February 2005

The 2005 Geological Society of London Lyell Meeting, sponsored by the Joint Committee for Palaeontology, is to be organised by The Micropalaeontology Society (Joint Convenors Haydon Bailey and John Gregory). This prestigious one-day meeting will be held at Burlington House, London on 9th February 2005. It is intended that the meeting will comprise three sessions, arranged stratigraphically (Palaeozoic, Mesozoic and Tertiary), each session with an invited keynote speaker, with the opportunity to discuss a complete range of macrofossil and microfossil subject areas within the proposed theme.

Contributors are asked to consider a single phylogenetic lineage and to pursue its development and application, both stratigraphically, and to any other area of applied usage. It is intended to publish the proceedings of the meeting at the earliest possible opportunity as a Special Publication of the Geological Society (authors' notes will be distributed prior to the meeting).

Proposed titles and abstracts should be sent to Haydon Bailey either via e-mail, or to the address below, as soon as possible so that a complete programme can be drawn up. Further details of this meeting will be made available once an initial programme has been established. Details will also be posted on the TMS website at <<http://www.tmsoc.org/>>.

Haydon Bailey (Network Stratigraphic Consulting Ltd, Unit 60, The Enterprise Centre, Cranborne Road, Potters Bar, Hertfordshire <haydonbailey@btconnect.com>) and John Gregory (Kronos Consulting, 33 Royston Road, St Albans, Hertfordshire AL1 5NF <john@jgregory.demon.co.uk>)



Rewriting the history of life: exceptionally well-preserved fossils and our understanding of evolution

BA Festival of Science, Trinity College Dublin, Ireland 5 – 9 September 2005

Fossils are familiar objects to many people. The petrified remains of shells, bones and other rot-resistant hard-parts of organisms are the standard fare of museum displays and rock collections. But this view of fossils is misleading: looking only at hard parts gives a very distorted view of the history of life. This session will focus on recent discoveries of amazing fossils that preserve what normally rots away (dinosaurs with feathers, bizarre 500 million year old worms and other ancient oddities) and how they are reshaping our view of the evolution of life on Earth. Check out the BA website or contact the meeting organiser for further details: Dr Patrick Orr <Patrick.Orr@ucd.ie>, tel 00353 1 7162323, Department of Geology, University College Dublin.



From our Correspondents

The tainting of *Proc. Biol. Soc. Wash.*

Maybe I was simply obtuse most of my life. Perhaps the blinkers only just now fell from my eyes. I must admit that it never had my explicit interest in the first place, too little time and too many other things to do. Maybe I had never really looked around me before I started noticing it. Sure, I had heard that there were differences between here and there, 'them' and 'us,' but you hear so much that turns out to be false. Perhaps what you need is to experience both sides. Now I have done that. Now I see. It's real.

Two weeks ago I moved to the United States after having lived in the UK for the past two years, and in the Netherlands in the preceding 30 years. In trying to find some balance on the tightrope between career and private life, I decided to move to Davis, California, on a long-term tourist-visa, to join my girlfriend, who had recently accepted a postdoctoral position at UC Davis. The current lack of a salaried job has left me ample time to contemplate my experience of the differences between North-Western Europe and the USA. Indeed, the pizzas here are bigger, and so are most cars. Beer is more expensive here than in Amsterdam, but not Cambridge, and the magpies have yellow beaks here, not black. The weather is also certainly better than I'm used to. While friends and family back home are slowly slipping into the inevitable dimness of fall and winter, we are enjoying 30 degrees Celsius in October, and sun most days, unless the smoky clouds from nearby forest fires cast their hazy shadows. However, the difference that I was alluding to in the first paragraph is the striking contrast in the prominence of religion as an overt issue in everyday life.

America is bustling with excitement about the upcoming presidential elections. Eighteen days from now this country will have to choose between president Deficit and senator "I have a plan." At the moment it is entirely unclear who will win, but one factor that will most certainly weigh into the equation is the religious conviction of both candidates. For example, last week a radio programme was dedicated entirely to discussing the important role of religious views in American politics, and in the third presidential debate a few days ago the moderator asked both presidential candidates about the roles that religion played in their personal lives. Bush, who less than a year ago in his inimitable wisdom stated that creationism is just another 'theory' for the origin of biodiversity, confided that he prayed a lot, as well he should. On the university campus many street corners are littered with billboards for a multiplicity of religious fraternities, discussion groups, and social clubs. You need never walk alone here. And this week I applied to become a volunteer docent in the Sacramento Zoo. The fourth question in the application form was: "In training we teach evolution. Would this present a problem for you?" I don't think they are referring to the difficulty of the topic here. Yesterday, my mentor in the zoo asked me if I knew that evolution is a "sensitive subject here." "I've heard rumours," I told him. He continued: "You don't have to dodge the issue when confronted with it, but you know, when it comes up, just let the public know that it's your own personal view of the world, and there are evidently other opinions." "Right, don't worry," I told him. I'm pretty sure that I will not get into a row when I'm out there in the zoo with my little exhibit wagon filled with pelts, skulls, feathers, photos, and other educational props. Not unless they really ask for it.

While the current state of the world is responsible for my own rapid promotion, or demotion, depending on your perspective, from agnostic to fully fledged atheist, I have no problem at all with these diverse manifestations of faith. To each their own. However, in science, the introduction of faith has always caused, and will continue to cause friction. The commotion that recently ensued after the peer-reviewed journal *Proceedings of the Biological Society of Washington* published a paper that supported intelligent design (ID), is a poignant example.

Stephen Meyer's article in the June issue of *Proc. Biol. Soc. Wash.* (PBSW), stood out conspicuously from the other papers. Amidst articles reporting the description of new species of crustaceans and sponges, Meyer's "The origin of biological information and the higher taxonomic categories" was sure to attract attention. And that, indeed, it did. As soon as the loyal supporters of PBSW unwrapped their copies of the new issue, things went pear-shaped. Indignant subscribers bombarded the editorial office of PBSW demanding an explanation (I want to thank Fred Schram here for supplying some juicy information for this piece). Surely this must be a joke, albeit a bad one. It was suggested that it would be wise to retract the paper. It would also be opportune for the journal to issue a formal apology to appease the outraged readers. Some subscribers threatened to resign their membership. Others actually did. The scientific newspapers *Science* and *Nature* got a scent of the action, and they issued editorials on the incident. Websites quickly filled with debates and scathing commentaries.

A sample of the adjectives and nouns that were fired off in printed and electronic commentaries betrays the strongly charged emotions of those involved in this incident. The opposing camps fired at will: 'outrageous,' 'truly frightening,' 'rubbish,' 'serious damage,' 'wolf in sheep's clothing,' 'tactical victory,' 'anti-science,' 'thwart intellectual freedom,' 'inappropriate for publication,' 'politically incorrect!' Who was to blame? Stephen Meyer? The editor who handled Meyer's manuscript at PBSW, Richard von Sternberg? Didn't Sternberg have ties to the baraminology study group, a bulwark of, mostly, young earth scientists who are severely challenged to incorporate any earthly productions older than about 10,000 years into their worldview? Were the scientific referees to blame perhaps? A scapegoat was badly needed, and if one couldn't be found among the living, then perhaps among the dead. Who had appointed Sternberg as research associate at the National Museum of Natural History in the first place? Wasn't it clear at the time that the blinding glow of Sternberg's two PhDs was at least partially dulled by the sticky veneer of ID, and its connotations of non-materialistic mysticism? Who knows. The person who nominated Sternberg for his appointment is dead. Drastic measures were contemplated. Should the journal perhaps be killed after taking such a fall? Its soiled reputation might be beyond repair. Never mind that all the fuss is probably lifting the readership of this small quarterly journal, which chiefly caters to the specialist needs of taxonomic experts, to a level unprecedented in its history. When the extensive scientific fraud of Jan Hendrik Schön was discovered in 2002, a host of Schön's fantastically fabricated papers was retracted from *Science* and *Nature*, but as far as I remember there were no suggestions to kill these beloved tabloids. Euthanasia might be too harsh a medicine. However, a thorough investigation was imperative. The learned council of the Biological Society of Washington convened a meeting. They reached a unanimous verdict. The council's statement "deemed this paper inappropriate for the pages of the *Proceedings*." In all of the 124 years of the journal's existence something like this had never occurred. The council promised to review editorial policies.

Let's take a deep breath, sit back in our chair, and calmly take the measure of the situation. Let me say upfront that I'm not an expert on the topic. Ever since the Jehovah's witnesses, whom, much to my mum's chagrin, I invited inside my parental home for coffee, failed to convince me of their creation story, I have not read any ID literature or creationist pamphlets. The reading I did for this column is intended as a one-off exception to this rule.

Since Meyer did not append an abstract to his paper, let me present a capsule review. Meyer provides a selective critique of the ability of current neo-Darwinian evolutionary theory fully to explain the origin of evolutionary novelties in general, and the evolution of the major animal body plans in particular. Meyer argues that it is highly improbable that 'random' variation and natural selection can lead to the evolution and functional divergence of genes and proteins. Moreover, he argues that current findings from evo-devo provide no insight into the evolution of animal phyla because most changes to genes that are important in establishing body plans are deleterious, if not fatal. According to Meyer, further attempts to explain macroevolutionary change with the help of self-organization, punctuated equilibrium, structural constraints, and generative or morphogenetic rules, or any combination thereof, are equally impotent. Meyer is puzzled. The mystery remains. But despair not, for revelation is on its way. After seventeen pages of biology, Meyer suddenly presents his epiphany: "Could the notion of purposive design help provide a more adequate explanation for the origin of organismal form generally? Are there reasons to consider design as an explanation for the origin of the biological information necessary to produce the higher taxa and their corresponding morphological novelty?" The final four pages of the article argue that, indeed, there are such reasons, and that they are compelling.

Meyer's perspective is distinctly rare in the peer-reviewed scientific literature, which is just as well. But, really, why all the fuss in the media, on the Internet, and in the halls of academia? Should PBSW feel that its reputation is soiled beyond redemption? Should people scream "outrage!" and drop their subscriptions to the journal in a knee-jerk reflex? Could the incident really even be called a "tactical victory" of ID, as *Science* puts it? I think that the only harm done to science is that PBSW published a paper that is evidently slipshod science, due to a flagrant failure of the reviewing process. However unfortunate that may be, it is nothing new, and it happens to the best. As a result, the only trophy that proponents of ID can really boast about at home is that ID is promoted in a paper that should never have passed the reviewing process, as was belatedly realized by the council of PBSW. In fact, that Meyer promotes ID in his article is, I think, largely beside the point.

I will spare you a lengthy scientific response to Meyer's arguments, but I need to qualify my conclusions a bit. If you have the energy, which I haven't, to read a very long, very detailed rebuttal of Meyer's paper, check out <<http://www.pandasthumb.org/pt-archives/000430.html>>. If you want to read an equally long and detailed response, check out the webpages of the Discovery Institute in Seattle, where Meyer is director and senior fellow of the Center for Science and Culture.

In my most favourable judgment, Meyer's paper reads like a student report. He has evidently read a lot of papers, and he has the best intentions of providing a critical discussion of his chosen topic. And, considering what he has read, he does an OK job. I would let him pass,

probably with a B. However, he would not get an A or A+ because the literature that he has selected is severely biased. Many readily available papers that depart significantly from his conclusions are omitted without excuse, and the logic of his arguments is not always as tight as it should be. On the most general level, Meyer doesn't understand the bare-bones mechanics of natural selection acting on 'random' variation. He concludes that there are no "functional criteria", or "goals," that natural selection can use to guide evolutionary change. Meyer never even mentions the struggle for survival. Organisms exhibit heritable variation in their traits, and they compete for limited resources. Those with the most favourable traits in a given situation and environment, which, of course, they may help to create through their activities in niche construction, will on average have a better chance of surviving, and on average they will leave more surviving offspring that inherit their traits, in the next generation. There you have your "goal," Dr Meyer. Evolution is about trying to achieve maximum representation of your genes in the next generation while competing with others. No teleological connotations necessary, the language notwithstanding. But, of course, Dr Meyer really knew this all along. He explicitly accepts the power of natural selection in shaping the adaptations of organisms to their environments, and he dutifully takes the customary Galapagos finches out of their dusty box. Here Meyer seems so close to solving his own problem, but, unfortunately, he doesn't.

Meyer doesn't discuss adaptive landscapes, competition between organisms, or differential fitness of organisms. And in his discussions of the high improbability, if not impossibility of the evolution of organismic novelties, from genes and proteins to morphological structures, he never delves more than ankle deep into the corpus of accumulated knowledge. Because mutations of early acting genes often fatally disrupt embryonic development, he concludes that animal body plans must necessarily be locked in stone. Because experimental studies show that many amino acid changes can cause a protein to lose functionality, it must be virtually impossible to navigate protein morphospace to yield the diversity of proteins around us. And where did all the new genes and proteins that are necessary for the development of the first animals come from anyway? These, indeed, are tough questions to answer. But, they are not hopeless, as Meyer seems to imply. But in order to see that, Meyer should have studied a bit harder. The evolution of protein families would show him that structural and functional divergence of proteins is possible, and widespread. It would also show that many of the genes and proteins needed to make animals are already found outside the animals, and that many of the genes with essential roles in the development of complex bilaterians are increasingly identified in cnidarians, and even sponges. Never mind the incredible potential of altering phenotypes via regulatory evolution. Never mind redundancy, gene duplication, and functional divergence of proteins. Never mind the power of changing expression patterns of conserved developmental genes to modify morphology. Never mind that the invention of evolutionary novelties can be underpinned by genes known to be already present, and functional, in other contexts in the animal's development. And never mind the potential for change inherent in the presence of large numbers of putative, but apparently unused, binding sites for developmental regulatory genes, throughout animal genomes. At one point Meyer concludes the insufficiency of natural selection as an evolutionary force acting on genetic variation, because not all of the phenotype is directly determined by genes. Well Dr Meyer, we have known for a very long time that organisms are rife with emergent properties above the level of the gene, and there are epigenetic factors with a role in morphogenesis as well. No surprise for any biologist. If Meyer

had touched upon hierarchical selection theory here, he would see there is no real problem, or at least one much smaller than he sees. Haven't some biologists argued that instead of acting strictly on genes, natural selection may also act on morphologies? Never mind all these important findings and ideas. You will not find any of these niceties in Meyer's paper, and yet, he feels confident on the basis of his scanty literature review, that our current understanding of the working of evolutionary mechanisms is grossly insufficient to address in any meaningful way the problems of reconstructing animal deep history. I will not attempt to tackle Meyer's defence of the compelling logic of ID. Suffice it to say that I disagree with his statement that "the possibility of design follows logically from a consideration of the deficiencies of neo-Darwinism and other current theories."

I am the first to admit that we haven't solved the problem of the origin of animal body plans yet. We're not even close. But, there is undeniable progress. The recent advances in evo-devo and molecular phylogenetics are but two examples of how we are managing to get increasing grip on the slippery problems of deep history reconstruction. However, we have no illusions that we already understand everything. But in the spirit of *Science as a way of knowing*, as the late John Moore titled his enjoyable book, we have to continue searching, taking the little loose ends to pull ourselves forward. Meyer doesn't see, or doesn't want to see these loose ends. And that is where the crux of the problem is. Rather than continuing to trust on the ability of science to make progress, as it always has, Meyer is willing to throw up his hands in bewilderment, and exclaim miraculous intervention of an intelligent designer. That's not the spirit of science. Meyer's paper was neither deep nor comprehensive enough to merit being called an adequate review by any standard, certainly not in view of his profound conclusions. But I'm willing to give Meyer the benefit of doubt. I think that he genuinely tried to understand the state of the art of current knowledge. He concluded it couldn't explain the origin of animal body plans, and he proposed an alternative "explanation." If he had indeed delivered an adequate review that concluded that certain questions will forever be beyond the grasp of science, and that ID might provide a helping hand there, then I would have no qualms. After all, that's just metaphysics. I advise Meyer to apply his critical eye to more science, and see if his conclusion is really the only solution. He may yet see the light...

Meyer's criticisms are a bit more taxing than the standard flimsy roadblocks that creationists have tried to erect in the way of evolutionary theory. However, so far evolutionary theory has had no problem in its unwavering march through fundamentalist blockades. No exception here. I think that Meyer's paper shouldn't have been published because it was an inadequate review. The blame for this lies wholly with the refereeing process, for which the editor is ultimately responsible. And here is where it gets interesting.

PBSW's former editor, Richard M. von Sternberg, PhD., PhD., has distanced himself from the council's statement that Meyer's paper should not have been published (see <<http://www.rsternberg.net/>> for Sternberg's defence). He defends his decision to publish Meyer's paper because it "set forth a reasoned view," a decision furthermore supported by the referee reports of three alleged "experts" in evolution and molecular biology. After having read Meyer's paper, I'm baffled why the combined erudition of Sternberg's two PhDs, one in molecular evolution and one in systems theory and theoretical biology, and the judgment of the three external experts didn't stop the publication of Meyer's paper. It does indeed appear that the

referee reports were supportive of the paper's publication after revisions. The decision to publish leaves no doubt that Dr Dr Sternberg is an extraordinarily open-minded guy. I wish there were more people like him.

But, wait, there is something wrong. Sternberg is not exactly a mainstream thinker. Although not supporting ID (see <<http://www.apologetics.org/>> for information on ID), he calls himself a "process structuralist." Whereas proponents of ID, such as Meyer, are "agnostic regarding the source of design and ha[ve] no commitment to defending Genesis, the Bible or any other sacred text," Sternberg adopts an intellectual position from which he contemplates the world in a general ahistorical, systems-oriented, and non-evolutionary way (not anti-evolutionary, I hasten to add). Compared to the worldviews of creationists, the necessary commitments of Meyer's and Sternberg's worldviews are minimal. Whereas Meyer is agnostic with respect to the character of the intelligent designer, Sternberg seems to be agnostic even with respect to the passage of time. I would not hesitate to nominate Sternberg's outlook on life as a candidate for another juicy chapter in Michael Shermer's delicious *Why people believe weird things* (1998), if only time's arrow wouldn't prevent me from doing this!

But it becomes much, much weirder if we allow Sternberg to be our admission ticket into a world of extreme bravery and shocking delusion: the world of the baraminologists. Sternberg's outlook on the world may be construed as candidly open-minded, and nothing more. But this conclusion is stretched to its limits when we meet the fellows with whom Sternberg is hanging out. Sternberg is associated with the "Baraminology Study Group," and he is on the editorial board of their "Occasional Papers of the Baraminology Study Group." In many respects baraminology is phylogenetics from the dark side, the almost exact mirror image of systematic biology. It is the study of the taxonomy of "created kinds," or "baramins." The goal of baraminology (see <www.bryancore.org/bsg/aboutconcepts.html>) is precisely antithetical to the goal of phylogenetics in biology. Baraminologists search for the discontinuities separating independently created groups of organisms. Using terminology eerily reminiscent of cladistics, baraminologists seek to identify "the unbridgeable chasms between body plans" upon the basis of which they erect holobaramins, monobaramins, polybaramins, and apobaramins. Discontinuities are located between "forms for which there is no empirical evidence that the character-state transformations ever occurred. The mere assumption that the transformation had to occur because cladistic analysis places it at a hypothetical ancestral node does not constitute empirical evidence." Using a range of techniques, such as Analysis of Patterns (ANOPA), baraminologists try to map out the structure of creation. This generates some wonderful, and very brave stuff, especially because the investigations are couched in terms of science, such as "tests," and because the papers endeavour to bring biblical Scripture and the findings of science into congruence with each other. I call this "brave" because the baraminologists first dispose of virtually all hard-won insights from the historical sciences, ranging from archaeology to astronomy. They discard over 99% of geological time by compressing the Earth's history from more than four billion years to a mere couple of thousands of years, and then use scientific reasoning to reconstruct all that happened in this shortened period. As an indication of the amusing results, let me give you some examples from the fourth issue of "Occasional Papers of the Baraminology Study Group," which contains the proceedings of the "Discovering the Creator" conference.

Joseph Francis, co-colleague of Sternberg on the editorial board of the journal, presented a paper documenting the benevolence of God by showing that microbes must have been created as good organisms. Their nasty pathogenic and parasitic habits must have arisen after the Fall of man from the Garden of Eden, because, expectedly, before the Fall there could have been no death and disease. In another paper, of which Francis is co-author, it is similarly argued that viruses must have performed “beneficial functions” before the Fall. Another paper struggles with the question of the implications of death before the Fall, as suggested by evolutionary theory and the fossil record. Another argues that, perhaps, animals that display “natural evil”—*i.e.* predators, pathogens, and parasites that kill other organisms—have two sets of genes, one for “benign morphology and behavior,” and one for “malignant morphology and behavior.” The former set of genes would then have been expressed before the Fall, while the second set of genes would only have become active after the Fall, with the origin of death and evil. This paper also includes my absolute favourite citation: “Satan *et al.*” And there is more. One paper is concerned with squirrel phylogeny and biogeography. It argues that, of course, squirrel biogeography must have been strongly influenced by the great Flood, since the squirrels, like all other animals, would have had to have dispersed from Ararat after the Ark landed there. I would think that if that were indeed the case, by now we would already have incontrovertible molecular phylogenetic evidence that showed that the highest volcano in Turkey is unambiguously the unique cradle for all animals. And so the papers continue.

Obviously, scientists need not worry. This is just a parallel research programme. If you don’t believe in the literal word of the Bible, then you need not be concerned with baraminology. We should hardly expect baraminologist papers infiltrating peer-reviewed science journals. You wouldn’t think so, would you? A relatively “scientific” paper, such as the one by Meyer could be excused, but the Ark of Noah? Surely such writings would never slip past our watchful eyes, would they? Unfortunately, it has already happened.

Last year, one time PhD student and research assistant of Stephen Jay Gould, Kurt Wise, presented a talk at the Seattle annual meeting of the Geological Society of America on “The evolution of the creationist perspective on the fossil Equid series.” And Wise is no liberal when it comes to the Bible. In his book *Faith, form, and time: what the Bible teaches and science confirms about Creation and the age of the Universe* (2002) Wise argues that biblical and scientific evidence suggests that the universe was created by God in six 24-hour days, less than 10,000 years ago. Wise, quoted in an essay by Richard Dawkins in *Free Inquiry* magazine a few years ago, says that even “if all the evidence in the universe turns against creationism ... I would still be a creationist.” Now, that is faith, not science. If such opinions receive airtime at our scientific meetings, we are inviting trouble. However, sometimes the creationists are not so easy to spot. For example, they may follow Meyer’s strategy of hiding their message, without any clue from the title or abstract of their works. For example, one of the contributors to the fourth issue of “Occasional Papers of the Baraminology Study Group,” Paul Nelson, presented a poster at the 2004 annual meeting of the Society of Developmental Biology titled “Problems with characterizing the protostome-deuterostome ancestor.” In what is ostensibly a scientific paper, Nelson and co-author argue for the discontinuity of the ontogenies of the bilaterian phyla, and that their evolutionary divergence is not supported by any evidence, but only by the imposition of evolutionary theory.

This kind of investigation neatly answers the goals of baraminology, establishing how obviously unbridgeable the gaps between animal body plans are. However, I think that, even among scientists, there is an exaggerated sense about how different the body plans of animal phyla actually are. Without going into too much detail here, let me open a little can of worms for you. Here is a nice selection of wormy beasties, some acanthocephalans, gastrotrichs, nemerteans, platyhelminths, gnathostomulids, rotifers, nematodes, pogonophorans, and nematomorpha. Now here is a nice microscope for each one of you. Your task it to sort the worms. How many of you will say “Well, there are some smaller and some bigger ones, but Jeez, they all look alike,” and how many will say “Easy task! Evidently we are dealing here with precisely nine discontinuous types, phyla I presume, which are separated by unbridgeable gaps in organization”). Right, you get the picture. True, there are famous differences in the organization of different phyla, and if that is all that you want to see, well, that is what you will see. However, if you look in a bit more detail, there are striking similarities, homologies even, which are evident as well. Nobody claims that rotifers and gnathostomulids are members of the same phylum. But ultrastructurally, they show an amazing number of correspondences. Far from an unbridgeable gap separating them, I would argue that they share pretty similar body plans, with modifications, of course.

These incursions of faith into science are worrying. Wherever science has not yet cast its illuminating light, the supernatural or metaphysical can and will always be unwrapped by some of us, to provide an explanation. Ironically, as we push back the limits of our ignorance, and increasingly difficult problems remain to be solved, it will exactly be there where “we” will meet “them.” There is no way around it. However, what we can and must do is to battle ignorance to prevent uninvited intrusions of faith into what I think is legitimately the domain of science. As long as we live in a world where State Superintendents of middle and high schools still consider evolution to be merely “a buzz word that causes a lot of negative reaction,” as opined early this year by the inimitable Mrs Cox from the state of Georgia, then we have a lot of teaching to do.

And as far as accusations go, that we are intolerant of people who pursue very abstract research, with no proven connection to the material world, and seemingly immune to empirical testing, but where the sheer beauty of ideas becomes an important epistemic value, well, for them I have one answer: Edward Witten is professor at Princeton’s Institute for Advanced Study.

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Debate: Time and Rocks

Time and time-rock again: an essay on the (over)simplification of stratigraphy

Introduction

The recent paper by Zalasiewicz *et al.* (2004a) and the essay by Zalasiewicz (2004) were stimulating and enjoyable. There is much in common between the views of Zalasiewicz *et al.* (2004a) and Walsh (2001; 2003) on this subject, among them being our preferred restriction of chronostratigraphic terms to stratified rocks, and our agreement that geochronologic units necessarily define the content of corresponding chronostratigraphic units (see discussion of the “rock-time,” “yin and yang,” and “time-rock” models by Harland, 1978 and Walsh *et al.*, 2004). However, although I now also agree with their suggestion regarding the use of temporal modifiers, I am not convinced that chronostratigraphic units and terms should be abandoned, and my views on this subject are quite different from those who have already defended chronostratigraphic units in the Geology Online Forum and in volume 56 of this Newsletter.

Temporal Modifiers

The historical distinction between the concepts of, say, Lower Cretaceous rock and Early Cretaceous time arose when the rock units were originally defined in a local type area, where a physical superpositional relationship between “lower” and “upper” subdivisions could be seen. For example, the span of time subtended by the lower part of the type Cretaceous in the Anglo-Paris-Belgian basin (Greensand and associated strata) was called “Early Cretaceous” and the span of time subtended by the upper part of that succession (the Chalk) was called “Late Cretaceous” (Harland *et al.*, 1990:56). However, the GSSP concept has reduced or eliminated the definitional role of these original type sections (Zalasiewicz *et al.*, 2004a; Walsh *et al.*, 2004). As such, in most places on Earth, the phrase “Lower Cretaceous rocks” has no independent meaning except as a synonym of the phrase “rocks of Early Cretaceous age,” which is why Walsh (2001) noted that the term “chronostratigraphic position” was unnecessary. I therefore agree with Zalasiewicz *et al.* that it would be acceptable to abandon the modifiers “lower” and “upper” and use “early” and “late” for geochronologic *and* chronostratigraphic units. The fact that European geologists have employed “inferior” and “superior” in both contexts without issue demonstrates that such usage would not be problematical (J. Ogg, *in* Pratt, 2004). Time of origin is clearly a legitimate attribute of material objects, so phrases like “Early Devonian Series” and “Late Cretaceous rocks” should not be regarded as heretical. I am glad that the Vice-Chair of the International Subcommission for Stratigraphic Classification has come to the same conclusion, albeit from a somewhat different direction (Embry, 2004). The contrary arguments of Heckert and Lucas

(2004) are unconvincing because they consist of mere assertions. It is also noteworthy that Bassett *et al.* (2004) do not explicitly address the issue of temporal modifiers, which in my view is where Zalasiewicz *et al.* stand on the most solid logical ground.

Clarifying Some Confusion About Chronostratigraphic Units

Zalasiewicz *et al.* (2004a) have gone one large step beyond just proposing that we simplify our temporal modifiers. They have also suggested that all chronostratigraphic terms (e.g., system) be abandoned in favour of geochronologic terms (e.g., period). They noted that the distinction between geochronologic and chronostratigraphic units has not been clear “to the greater part of the professional (or student) geological community,” and I have made the same observation (Walsh, 2001). Unfortunately, the understanding of these simple concepts has been hampered by the failure to illustrate them in major stratigraphic codes and guides (Whittaker *et al.*, 1991; Rawson *et al.*, 2002), or by the use of geologically unrealistic diagrams when they *are* illustrated (e.g., Hedberg 1976, figs. 12, 14). The North American Commission on Stratigraphic Nomenclature (NACSN, 1983) provided clear diagrams of most of the other kinds of stratigraphic units it discusses, but the only diagram that shows geochronologic and chronostratigraphic units is the one explaining what a “diachronic unit” is supposed to be (NACSN, 1983, fig. 10). More importantly, and inexplicably, none of the relevant diagrams in Hedberg (1976) and NACSN (1983) show any unconformities or hiatuses, such that geochronologic and chronostratigraphic units appear to be indistinguishable from one another, and thus indeed “redundant.” However, once the relationship between nonmaterial geochronologic and material chronostratigraphic units has been rigorously defined and illustrated in *geologically realistic diagrams* (*i.e.*, showing that most of geologic time in most areas is not represented by strata), then the differences between these two categories are easily understood (Walsh, 2001, fig. 1; Walsh, 2004, fig. 2).

Nevertheless, a philosophical digression must be made here. Chronostratigraphic units consist of all of the actually-existing strata in a given geographic area that were formed during a specified geochronologic unit. Because chronostratigraphic units are composed of material rocks, they are generally considered as material units themselves. In an important sense, however, they are only human abstractions, because their exact content can only be conceptualized after their defining geochronologic units are specified (Walsh, 2001; 2004). Therefore, depending on the context, one can view a chronostratigraphic unit as either a material entity or as an abstraction, without involving a logical contradiction. This situation is not unique to chronostratigraphic units, but is inherent in the conceptualization of all human-defined classes, such as the Baby Boomer Generation (Walsh, 2003).

Problems with the Simplified Nomenclature

Now back to the main issue. In my view there would be two problems with the simplified nomenclature proposed by Zalasiewicz *et al.* (2004a), one immediate and one potential. These are 1) a lack of precision in some contexts, and 2) the probable migration of chronostratigraphic unit-terms to other categories, should their proposal be accepted.

Starting with the first problem, suppose we want to discuss the Maastrichtian Standard Global Stage as it occurs in Western Europe. This is going to be problematical without using the term

“stage,” because all stratigraphic names like “Maastrichtian” have a large amount of historical baggage associated with them. They have been used in the past in various lithostratigraphic, biostratigraphic, and unconformity-bounded senses, so the phrase “The Maastrichtian strata of western Europe” (one of the formulations suggested by Zalasiewicz *et al.*, 2004a) is just not precise enough for unambiguous communication. Given this phrase, are we talking about the strata that have traditionally been considered to be lithologically characteristic of the Maastrichtian, or biostratigraphically characteristic of the Maastrichtian, or characteristic of the Maastrichtian synthem and/or depositional sequence, regardless of whether these various strata are all now included in the Maastrichtian Stage as formally defined by GSSPs (*e.g.*, Odin and Lamurelle, 2001)? Zalasiewicz *et al.* correctly note that we could be more specific and say “The strata of western Europe that were formed during the Maastrichtian Age,” but why use all of these words when a much shorter equivalent (“The Maastrichtian Stage of Western Europe”) is already available? Zalasiewicz *et al.* (2004b) acknowledge that the latter version “provides ... an effective shorthand means of communication,” and this seems to me to be a sufficient reason for retaining chronostratigraphic terms.

Nevertheless, if Zalasiewicz *et al.* are mainly opposed to the *mandatory* use of chronostratigraphic terms, especially in the context of unstratified crystalline rocks (Zalasiewicz, 2004:81; Zalasiewicz *et al.*, 2004b), then I would agree. In my view, stratigraphic codes and guides exist only to give authors and editors guidelines on how a codified term should be used, *if* a given author decides to use it. For example, these documents (and editors who designate them as the arbiters of correct usage for their journal) cannot legitimately require an author to use the term “stage” or “assemblage zone” or “synthem” in his paper; they can only require that if an author *chooses* to use these terms, then he must use them in the sense in which they are codified (see Walsh, 2000, p. 768, for an example of unwarranted editorial heavy-handedness). Thus, geologists who would prefer to write “The strata of Western Europe formed during the Maastrichtian Age” rather than “The Maastrichtian Stage of Western Europe” should not be forced to pen the latter, although such chronostratiphobia would again be idiosyncratic in my view.

A second, potential problem with the simplified nomenclature proposed by Zalasiewicz *et al.* (2004a) involves the likely migration of the unit terms system, series, and stage to other stratigraphic categories if chronostratigraphic units should be abandoned. For example, the currently codified unit-terms for unconformity-bounded units (supersynthem, synthem, and subsynthem; Salvador, 1994) have not been widely used. It therefore seems likely that some would suggest that the newly liberated terms system, series, and stage be appropriated to replace them. Thus, the Karroo System might live again in its original unconformity-bounded sense. However, it seems obvious that by making system, series, and stage more ambiguous than ever, terminological confusion would increase.

Chronostratigraphic Units Are Not Redundant

In my view Zalasiewicz *et al.* (2004a) are incorrect to say that chronostratigraphic units are “redundant.” Because the stratigraphic record is riddled with unconformities, chronostratigraphic units (being composed of material rock) cannot be isochronous over any

appreciable geographic area. That is, the ages of the boundaries (*i.e.*, the local base and top) of a given chronostratigraphic unit in most areas will usually not correspond to the beginning and ending of its defining geochronologic unit (Wheeler, 1958; Harland, 1978; Walsh, 2001; 2003). This distinction is of great importance in my view, and although I disagree with most of M.-P. Aubry’s recent chronostratigraphic philosophy (Walsh, 2004), I do hold the same opinion expressed by Aubry (1995:268) on this point: “Because there is not a one to one relationship between geological time and stratigraphic record, I would argue against Harland and others (1990) and Harland (1992) that it remains essential to use a dual terminology to distinguish between temporal and [chrono]stratigraphic terms.”

Now I can already hear some howls of protest out there, so another digression is necessary. Some of you are shouting: “How can Walsh possibly say that the ages of the boundaries of a chronostratigraphic unit usually will not correspond to the boundaries of its defining geochronologic unit?! The boundaries of the Devonian System, by definition, must everywhere correspond in age to the boundaries of the Devonian Period!” If this were true, then Zalasiewicz *et al.* (2004a) would be justified in regarding chronostratigraphic units as redundant. But it is not true. By definition, the “boundaries” of any material entity A occur at the physical interface between “A” and “not A.” For example, the boundaries of the United States occur wherever the physical interface between “United States” and “non-United States” occurs. Thus, the southern boundaries of Louisiana, Mississippi and Alabama certainly do form part of the southern boundary of the United States, even though the boundary of the U.S. here is not located as far south as the southernmost points of Texas and Florida, and is also not physically contiguous with the northern boundary of Mexico. For the latter concept, we use the appropriate term—the United States–Mexico boundary.

Similarly, where middle Devonian strata unconformably overlie Cambrian strata, the lowermost Devonian stratum certainly does form the lower boundary or base of the Devonian System *in this section*; this stratum just isn’t the same age as the Silurian/Devonian *Period* boundary, and it is that *temporal boundary of fixed age* that the golden spike really defines. Laterally removed from the GSSP and indeed at most places around the world, the material base of the Devonian System will necessarily be younger than the beginning of the Devonian Period. Taking as a concrete example Hutton’s famous unconformity at Siccar Point, the upper bounding surface of this unconformity *most certainly is* the base of the Devonian System in this section, even though we are well aware that these beds are substantially younger than the beginning of the Devonian Period as defined by the golden spike at Klonk.

Importantly, the American Commission on Stratigraphic Nomenclature (ACSN, 1965:1700) clearly recognized the fact *that chronostratigraphic boundaries are of variable age* when it stated:

“... a surface of unconformity inevitably varies in its age or time-value from place to place and hence can not at all fulfil the theoretical requirements of a chronostratigraphic boundary. At the same time it may provide a useful *guide* to the placement of System boundaries **and it is, of course, frequently the local physical boundary of an incompletely represented System** [boldface mine].”

Here the ACSN states the obvious—that the **physical boundary** of a chronostratigraphic unit will frequently be a surface of unconformity, which is precisely the situation illustrated in figure 1 of Walsh (2001). For this very reason, however, and contra NACSN (1983), Hedberg (1976), and Salvador (1994), *synchronous boundaries cannot be a necessary attribute of chronostratigraphic units*, which again must be composed entirely of physical rock if a meaningful distinction between nonmaterial geochronologic units and material chronostratigraphic units is to be upheld.

Chronostratigraphic Units Help Prevent Confusion

The most important reason for retaining chronostratigraphic units stems from the realization that H.D. Hedberg's main purpose in promoting these units was not to separate them from geochronologic units. It was to separate them from several other categories of material stratigraphic units with which they had long been confused, such as lithostratigraphic, biostratigraphic, and unconformity-bounded units, all of which had been assigned the rank terms system, series, and stage by different workers at one time or another (Hedberg, 1976, chapter eight; Salvador, 1994, chapter ten). Recent claims that sequence-stratigraphic units are really chronostratigraphic units, the continuing confusion of biostratigraphic units with chronostratigraphic units, and the persistent misuse of the term “stage” for regional unconformity-bounded units illustrate the point (Walsh, 2004).

Given the current importance of sequence-stratigraphic units, let's clarify the relationship between them and chronostratigraphic units. Sequence-stratigraphic units can be recognized on many scales, but consider the global early Eocene (Ypresian) marine transgression and regression. The set of all strata on Earth that were formed in response to this eustatic event (let's call it the global Ypresian depositional sequence) might include all open-marine deposits, all marginal-marine deposits, and even all non-marine coastal deposits whose deposition was controlled by the fluctuating base level of the world ocean during this transgression and regression. But the *Ypresian Standard Global Stage* consists of *all existing strata on Earth that were formed during the Ypresian Age*. It therefore includes many additional rocks whose origin had nothing to do with the Ypresian transgression, such as the voluminous continental strata deposited in the intermontane basins of North America, and various other early Eocene non-marine and volcanic rocks found on other continents. So, a clear concept of the Standard Global Ypresian Stage (a chronostratigraphic unit) is important in this case simply to help overzealous sequence stratigraphers understand what a sequence-stratigraphic unit *is not*.

Chronostratigraphic units also need to be retained because almost 40 years after Hedberg (1965), prominent workers insist on confusing them with biostratigraphic units. For example, in the biostratigraphy chapter of his textbook *Bringing Fossils to Life*, Prothero (1998, p. 173) stated:

“As stratigraphers have become more careful in documenting ranges and describing type sections, inevitable disputes over boundaries have arisen. The type section or areas of two successive *biostratigraphic* units are usually in two separated areas, and there may be no overlap. Often, neither section preserves the boundary between the two units, so biostratigraphers must search for a third area where the transition is recorded. Ideally,

this section should be as continuous and fossiliferous as possible, with several taxonomic groups to compare [*italics mine*].”

Clearly, Prothero is here discussing *chronostratigraphic* units, even though he *calls* them “biostratigraphic units.” Confirmation is provided on the very next page. After mentioning the definitions of the Ordovician/Silurian, Silurian/Devonian, and Devonian/Carboniferous boundaries, Prothero (1998b, p. 174) then stated: “Many other *biostratigraphic* boundaries are currently under discussion ... [*italics mine*].” These statements would make Schenck and Muller (1941) and Schindewolf (1970) proud, and not surprisingly, Prothero's (1991) detailed treatment of the formal definition of the Silurian/Devonian boundary appears in his chapter on *biostratigraphy* and not in his chapter on *chronostratigraphy* (where it clearly belongs). The point here is that Prothero is an author of numerous top-selling stratigraphy textbooks, unwittingly teaching an entire generation of undergraduates the long-obsolete view that systems, series, and stages are biostratigraphic units! (Walsh, in revision 1, 2, 3).

Zalasiewicz *et al.* (2004a) did not address the application of chronostratigraphic concepts to contexts other than the standard global geologic time scale, so I will briefly do so here. In my view, it can clarify our thinking to say that a given stratum belongs to the Taxon X biochronozone, even if that stratum lacks fossils of Taxon X or indeed lacks fossils of any kind (separation of biostratigraphic from biochronostratigraphic units; Arkell, 1956:5–6; Hedberg, 1965). Similarly, given sufficient evidence we can say that a particular section belongs to the Matuyama Reversed-Polarity Chronozone, even if that section is currently normally polarized (separation of magnetostratigraphic from polarity-chronostratigraphic units). Such a statement helps us to understand that observed magnetozone boundaries are not necessarily the same age as any actual reversals of the Earth's magnetic field.

The Verdict

Although I agree with several of the suggestions of Zalasiewicz *et al.* (2004a), I cannot go along with their main proposal, because the simplification they have hoped for in one field of stratigraphy would prove to be an oversimplification when applied to other areas of our science. Indeed, if we didn't have chronostratigraphic units now, we would soon have to invent them, just to keep some of our less rigorously-minded colleagues from overestimating the temporal significance of their favourite sequence-stratigraphic, unconformity-bounded, biostratigraphic, lithostratigraphic, and magnetostratigraphic units (cf. Hedberg, 1965).

Nevertheless, Zalasiewicz (2004:81) has offered an alternative that should satisfy all parties because it involves freedom of choice: leaving “a dual option open in such a way as not to clutter up the lives of those scientists who did not need to, or wish to, use and be confused by the time-rock classification.” Fortunately, this option already exists, provided that journal editors understand their role in legislating the use of technical terms, as discussed above.

The Real Meaning of Simplification

In regard to the title of this essay, readers of some of my previous papers (Walsh, 1998; 2000; 2001) might think I believe that stratigraphy should be as complicated as possible, but that's not true. I believe as Callomon (2003:263) believes: “Stratigraphy is diverse but simple—provided its concepts are clearly defined and separated.” My contention is that a seemingly

complex classification will be simple to understand and apply if its hierarchical structure is logical and the definitions of its categories are internally consistent. Unfortunately, significant parts of, for example, the North American Stratigraphic Code (NACSN, 1983) are quite illogical (Harland, 1992; Walsh, 2001; 2003), so of course they are difficult to understand. Who knows how many eager young North American stratigraphy majors who, unable to comprehend the incomprehensible mystery of Article 66, have run screaming to the igneous petrology lab, never to open a stratigraphy textbook again? Fortunately, Lucy Edwards of the NACSN has recently acknowledged that “even the Stratigraphic Guide [*i.e.*, the North American Stratigraphic Code] is unclear on the distinction between the categories of time and time-rock” and that “the solution to the confusion lies in fixing the vagueness in the Stratigraphic Guide [Code]” (in Pratt, 2004). These statements are much appreciated given the rather different opinions previously expressed by Easton *et al.* (2003), and I can only hope that Walsh (2003) and the detailed recommendations I made to the NACSN at its annual meeting in 2003 have helped to bring them about.

So, in my view at least, things are looking up. The time is ripe for a genuine simplification of stratigraphy because many and perhaps a majority of active stratigraphers now realize that geochronologic units do indeed define the material content of their corresponding chronostratigraphic units (Harland, 1978; 1992; Walsh, 2001; 2003; 2004; Walsh *et al.* 2004; Zalasiewicz *et al.*, 2004a). This is really half the battle that Zalasiewicz and his colleagues are fighting, and when this fact is formalized in a logical hierarchical classification of various time and time-rock units in future editions of stratigraphic codes and guides, the rest will fall into place. I thank Jan Zalasiewicz for discussions and for suggesting that I submit this essay to *Palaeontology Newsletter*; I look forward to his response.

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Reply: Stratigraphy over-simplified? — or a question of perspective?

Steve Walsh does indeed offer a distinct perspective on the utility of time-rock stratigraphy, and draws up a nicely-argued case for its retention. He has an excellent grasp of the relevant literature, so perforce I must match the seven-league boots of considerable scholarship with the crudely-hewn clogs of blunt enquiry. As he says, the points of contention between us are few: the field is narrowed to stratified rocks, and “upper/lower” modifiers have been removed from the discussion. So what are the points of debate now? Let’s take them one by one.

- **A lack of precision in a unified nomenclature.** It seems to me still—thus far—that one can be as precise as one wants to be regarding the relation of time, rock and deduced process using either a dual or unified terminology. To take the Maastrichtian, the formulation ‘strata deposited during the Maastrichtian Age’ is of course a longer phrase than ‘the Maastrichtian Stage’ but it is just as unambiguous. In this sphere, brevity does not always equate with clarity. Furthermore, I’m not so sure that simply the use of ‘Maastrichtian strata’ would inevitably lead to dangerous confusion, as the meaning would generally be clear from the context. After all, geologists frequently use time and time-rock terms quite interchangeably in everyday communication (at least, before a sharp-eyed editor hauls them up) with little sign that the Edifice of Geological Thought is imperilled.

There is an acid test here. Can one systematically write papers on stratigraphic topics using *only* geological time terms? Are there any occasions where one cannot get by without time-rock terms? My limited experiments to date suggest that time terms alone will suffice—though the experiments do need to run longer.

Logical possibilities are one thing. There’s the important question of the terminology that people *are used to*. Using unified terminology, of course, one can’t say that ‘the base of the Quaternary’ any more than one can say ‘the base of January’; one should say rather ‘the beginning of the Quaternary’ (though one can, of course refer to the base of a Quaternary succession, just as one can refer to the base of a January snowfall). To say the ‘beginning of the Quaternary’ does not—even to me—roll so neatly off a Pavlovian tongue. Words have a history and force of their own, and evolve to mean different things to different groups of people. Thus, it would be senseless for French-speaking geologists to try to replace the universally-used ‘inférieur/supérieur’ with ‘précoce/tardif’; the former modifiers have come to have a temporal as well as a topological meaning and are universally understood.

Now, as my colleagues on the Stratigraphy Commission and I have argued, the time/time-rock distinction in the English-speaking world is decidedly not universally understood. Nevertheless, the nomenclature of both time and time-rock classification is in common, if frequently mangled, usage. The terms themselves won’t fade away quickly, whatever formal recommendations are made. How often, for instance, is the K/T boundary referred to as the K/P boundary? Still, the inextricable tangle of habit and meaning that form an ever-more-burdensome baggage around stratigraphic terms may be illuminated, if not resolved, by

trying to be clear about the phenomena they represent. Here I'm entirely at one with Steve Walsh. Which brings us up to the next point...

- **The incompleteness of successions.** Thus, the argument that locally a System has boundaries different from that of the equivalent Period, as in the case of the Devonian strata at Siccac Point. Well, there are probably very few places where a Period is represented by a continuously deposited succession. One can say that there are strata of Devonian age (or Devonian depositional age, if one wanted to exclude all ambiguity) at Siccac Point, without implying that the entirety of the Devonian is represented there by rock. Similarly, one can talk about a January snowfall, without implying that snow fell all through January without a break. And snowfalls generally have diachronous bases, as the snow clouds that produce them scud hither and thither. Yes, one can make a System/Period distinction in this way if one wishes, but I'm not sure that it is necessary to an effective description of what, in geological terms, happened when and where. The Devonian, as defined by GSSPs above and below, is simply an interval of time within which processes, such as the deposition of sediment, took place.
- **Consequent migration of the meaning of terms.** This is a novel slant, and deserves consideration. If, let's say, the formal use of words such as 'System' and 'Series' was abandoned, would those words, now left redundant but still echoing within the crania of tens of thousands of geologists, find—or be found—other phenomena to describe? Safer by far to keep them where they are ... or is it?

Words such as 'series' and 'stages' certainly have had different meanings in the past. They even have different meanings today, among different communities of geologists in different parts of the world. Changes of meaning do cause confusion, in the short to medium term. How great is the danger in this instance? I find that hard to judge. In the particular instance quoted, that of synthems, then that particular term does seem destined to fade away, lamented by few. However, the phenomenon it describes – that of unconformity-bounded units, seems mostly to be now subsumed under the general umbrella of sequence stratigraphy (which itself has an evolving terminology).

It's more likely that, whatever (if anything) is formally recommended about the time-rock terms, they will continue to be used—as noted above—by those geologists who find them useful. Such a continued use, even if informal in itself, will likely be some form of barrier to the further evolution of these terms. Of course, the community of geologists who will use these terms will be... I was about to say stratigraphers, but the word stratigraphy itself has of course evolved (between Hedberg and Salvador, I think) to encompass those who decipher earth history from unstratified as well as stratified rocks. We are all stratigraphers now, and about time too.

So we need a new term for stratigraphers dealing with strata, those for whom this argument is germane. Perhaps we should call this group of earth scientists 'strata-types', to avoid terminological confusion. Among the strata-types, there will be those who might prefer to adopt the unified terminology, perhaps because (as in my case) they might lack a little theoretical solidity. Perhaps this group of stratigraphers should be described as 'hollow-

types'. The persistence, or not, of the time-rock terms will likely, ultimately, depend on how many hollow-types continue to be preserved among the strata-types, in museums and other places of learning. Now, isn't that a clear and unambiguous enough summary of a Darwinian struggle between competing terminologies?

I think, paddling furiously towards the safety of a sensible shoreline, that it is hard to predict how our stratigraphical language will evolve, except that it undoubtedly will. The danger of sinking deeper into a terminological quagmire shouldn't, therefore, stop us trying to work out which direction we think we should be trying to head towards.

- **Blurring of rock, time and fossil units.** Steve Walsh has nicely demonstrated how the distinctions between biozones, sequence stratigraphic units and chronostratigraphic units can become blurred. The argument here is that Systems, Series and so on should be retained in order that the fossil- and sea-level-based units do not encroach too much upon the territory of geological time. I'm not sure of this: the holy trinity of rocks, fossils and time helps me, at least, maintain a grasp of the distinction. Is a holy quartet of rocks, fossils, time and time-rock quite as elegant? Here, I'll offer the observation that it's quite easy for time-proxy units to slip towards being thought of as time-units, especially if the proxies are pretty good. Perhaps we ask too much of them sometimes. Which brings us to...
- **Biochronozones...** which might just be an example of where the lily might be being gilded to achieve a bewitching sheen, like that of fool's gold. Now fossils offer excellent proxies for the time record, agreed. Biozones are a useful way of constructing effective quasi-temporal pigeon-holes out of a plexus of evolving lineages of any major fossil group. Biozones, though, are also imperfect, even in the Jurassic. They are always diachronous (even the base of an ignimbrite is diachronous, for a pyroclastic flow takes many billions of nanoseconds to traverse from a crater rim to the plains below; I exaggerate here, but perhaps not altogether ludicrously, as my colleagues Richard Brown and Mike Branney (2004) have resolved very finely-spaced depositional events in the accretion of a single ignimbrite).

Even on the standard rough-hewn geological timescale, biozones are often demonstrably diachronous. The (relative) range of my favourite graptolite—the short-ranged and beautifully distinctive *Monograptus crispus*—is, for instance, decidedly diachronous—on standard biostratigraphic grounds—between Wales and Bohemia. So fossils, while a splendid guide to time, need watching like a hawk at the high-resolution end of things.

So biozones I understand. But the role of a biochronozone... Now, as I understand it (and I stand to be corrected on this), this encompasses all the time subtended by the maximum temporal range of the defining taxon of that biozone, and represents an interval to be extended into all of the strata deposited during that time, whether fossiliferous or not. I more or less understand this idea in general, but am a little puzzled as to how it might work in practice.

Inasmuch as I understand evolution, at least that of the graptolites that I work with, they generally don't seem to show the kind of gradual evolution that Peter Sheldon (1987)

painstakingly demonstrated for the Builth trilobites. Most graptolite taxa tend to, well, just appear in any individual section, as more or less distinct entities, the spirit of punctuated equilibrium presumably hovering somewhere nearby. Evolution always, dammit, seems to have happened somewhere else. Presumably a taxon arises somewhere out there, generally as a small isolated population *pace* Ernst Mayr. Then, some time later, it breaks out of its geographic confines to spread around the world. It may spread quickly, like wildfire, or more slowly, or stepwise, and part of that spread will have been modulated by climatic or oceanographic conditions, and the way these changed with time. The taxon flourishes for a while, and then, outcompeted by its successors or wrongfooted by some environmental change, it dies out, but isolated relict populations may hang on for a while here and there before finally succumbing—though perhaps one or two of these may give rise to successor species which might in turn spread over a good part of the world.

As biostratigraphers, we mainly, I think, see the arrival of our key taxa into sections (sampling, at best, a few square metres of seafloor, if that, and recalling the strictures of Messrs Signor & Lipps), and we also see its local demise. These are our tie-lines, for practical purposes. We normally don't chance upon the birthplace of a new taxon: I can recall only one half-way decent candidate of this phenomenon in the graptolitic successions I've looked at. And we don't normally find the relict late populations; hence the flurry of excitement a few years ago over the Wrangel Island mammoths, which we now know were grazing that tiny portion of steppe while the Pyramids were being built. A more pronounced example of diachronous extinction was of course mooted by Sir Arthur Conan Doyle in his *Lost World*, an example of entirely ridiculous proportions, until, that is, one remembers the coelacanth.

Even if we had wonderful geographical control, and locate birthplaces and final resting places of taxa, we would simply encounter a different type of uncertainty: where, in a continuous and continuously recorded lineage, does one draw a boundary between two successive taxa? Peter Sheldon's Builth trilobites, in this situation and at this scale, don't lend themselves easily to taxonomic pigeon-holing.

So, while to seek to correlate biostratigraphic tie-lines into unfossiliferous strata is a perfectly valid exercise, the attempt to formalize this into time-units based upon ideally compartmentalized taxa takes me, at least, into unfamiliar and potentially treacherous terrain, given the amoeba-like, fuzzy-edged geometries encompassed by taxa through space and time. At least a golden spike represents a single point of certainty, even if all correlation that follows is uncertain.

So in conclusion... and after something of a detour—where should we go? Of all the reasons quoted for retention of the dual time-scale, the most telling seem to me to be the familiarity and convenience of the time-rock terms for part of the geological community working today. Tomorrow, though, a generation on, stratigraphy will have moved on (think how far it has moved in the last quarter-century), and the eternal verities may have shifted a little also. Even today, we can operate effectively without using the time-rock terms, though at times we have to use unfamiliar language, and need to consider exactly what we want to express. The last point may not be altogether a Bad Thing.

I'm conscious of the growing importance of stratigraphy to much broader-based studies of how the Earth functions, and especially of climate change. These are pressing matters, for time is short and the water around us is quite literally rising. Time is part of the lingua franca of science as a whole while time-rock is not. A simplified—*i.e.* unified—time-scale should not, of course, be mandatory; it might be recommended, perhaps. Nevertheless, if widely applied in practice, it might just help the sciences talk to each other more easily. They certainly need to.

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Palaeo-math 101

Regression 3: How to avoid Lying with Regression Analysis

Thus far in this series we've been focusing on how to perform a linear regression analysis; what error-minimization models are available, the circumstances under which use of different linear models may be appropriate, and the effects of data standardization. By this time you should understand that linear regression is actually a bit more complex a topic than is usually portrayed in textbooks, but you should also feel confident that this complexity can be turned to your advantage in planning and carrying out such analyses. Before we plunge even deeper into this subject, though, it would be a good idea to devote at least one column to the topic of interpreting the results of a regression analysis. Here we'll see, as we have before, things are not as straightforward as they might seem.

Let's start by considering the very first regression result we obtained for our example trilobite data from the first essay in this series (see Regression 1 and the Regression 1 worksheet). In that first analysis we used the standard least-squares error-minimization model to estimate the following linear relation between glabella length (x) and glabella width (y) that minimized deviation in the latter.

$$y = 0.64x + 2.51 \tag{3.1}$$

Is this a good model of variation within those data? Is it adequate for the purpose of data characterization? Or could these numbers be hiding some underlying pattern that might affect our interpretation of these data? By just looking at (or presenting) the raw slope and intercept coefficients it's difficult to know what to make of this result other than to accept that this is the equation of the straight line which minimizes deviation along the glabella-width axis. The problem, of course, is that the same line may be calculated for a large number of data geometries (Fig. 1).

Given the vastly different interpretations we would give to the glabella length–width relationship under these different circumstances (e.g., a regular and predictable relation in Fig. 1A, no relation whatsoever in Fig. 1D), and the failure of the estimated regression lines to distinguish between these alternative patterns *per se*, it seems clear that we need to look beyond the simple calculation of a linear regression line's equation if we are to use regression to gain a deeper understanding of our variables (which, after all, is the real point of any quantitative data analysis). Fortunately, our friends over in the statistics department have anticipated our problem and relatively easy-to-apply solutions are available.

Obviously, the four cases illustrated in Figure 1 differ from one another primarily in magnitude and distribution of the deviations between the linear model's predictions—represented by the regression line—and the observed data points. Although we previously referred to these deviations as errors, they are also often referred to as 'residuals' in the sense of being a proportion of the observed data left over after the (in this case) linear pattern has been accounted for or 'explained' by the regression model. Residuals are easiest to calculate

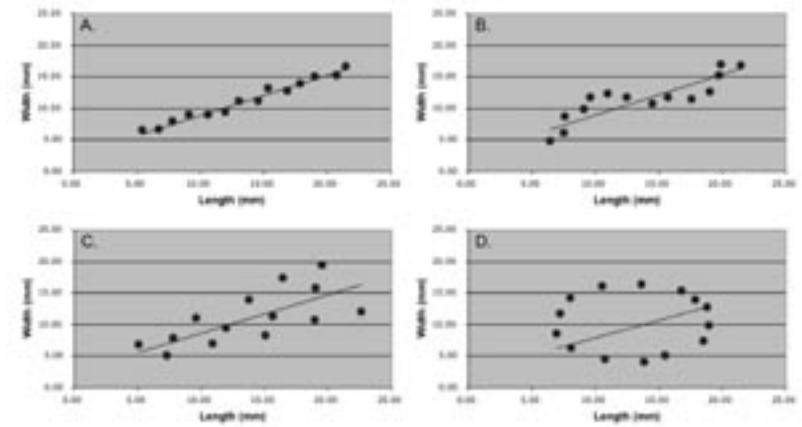


Figure 1. Four alternative data patterns that produce linear regressions of non-zero slope. A. Linear regression through linearly distributed data. B. Linear regression through data with a linear trend but non-linear distribution. C. Linear regression through data in which the error is proportional to variable magnitude. D. Linear regression through data exhibiting no linear trend, but a heterogeneous distribution of point density (heterogeneous point densities are characteristic of random data).

for least-squares regressions where they are defined as the difference between the observed y -value (y_i) and the y -value predicted by the regression equation (\hat{y}_i)¹.

$$y'_i = y_i - \hat{y}_i \tag{3.2}$$

$$y'_i = y_i - (mx_i + b)$$

where m and b represent the least-squares regression slope and y -intercept respectively. Figure 2 illustrates the residual plots for each of the alternative data geometries shown in Figure 1.

Although the data patterns in Figure 1 were devised to be obvious, it is, in most instances, a good idea to inspect residual plots like these to determine whether a linear regression analysis has been an appropriate model for the characterization of any particular dataset. If your residual plot looks like the pattern shown in Figure 2A, a linear regression analysis was appropriate. More specifically, in order to be regarded as appropriate the residual plot should (1) exhibit no linear trend in itself, (2) form a scatter about a flat, horizontal line through the residual bivariate mean whose deviations approximate a normal distribution, and (3) be arranged such that the standard deviation of variability in the residual values is the same everywhere along the regression line, regardless of the x -axis interval selected (a feature known by the rather scary term 'homoscedasticity'). Any other residual plot geometry is problematic.

¹ Throughout this essay I will be assuming a 'y on x' least squares regression. For an 'x on y' least squares regression reverse the x s and y s in the equations.

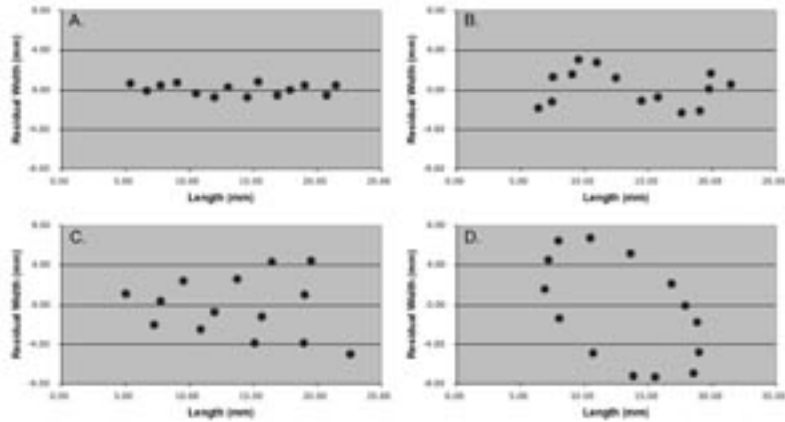


Figure 2. Plots of least-squares residuals about the linear regression lines for the alternative data geometries in Fig. 1.

In Figure 2B, even though the data pattern contains a consistent linear trend, the fact that the residuals exhibit such a well-defined, non-random structure suggests the linear model did not provide an appropriate characterization. In this case, the marked wave-like pattern indicates that the positions of successive points are dependent on the positions of preceding points (the signal of strong autocorrelation). The form of this particular autocorrelated pattern indicates use of a curvilinear regression model would be preferable.

Figure 2C represents a problem of a different kind; one that is even more common than autocorrelation. In this instance the standard deviation of the residuals from the linear model increases as a function of the x -variable's magnitude. This is the 'heteroscedastic' condition. Pronounced heteroscedasticity usually means that large variable values are not being estimated with the same precision as smaller values. Alternatively, this situation could signal the effects of a fundamental structural constraint, such as allometry. Regardless of its origin, the greater variability of the data at one or the other end of the distribution affects the precision with which a linear relation can be estimated. The usual solution to this problem is to apply a data transformation (e.g., logarithmic transform) which minimizes the effect of such scaling differences, and then repeating the regression analysis on the transformed data.

Finally, the somewhat artificial example of Figure 4D is there to remind us that linear regression analysis can find structure in data that deviate only slightly from being structureless. In this instance, the appearance of a non-zero regression slope is due solely to a slight asymmetry in the density of the data distribution. This residual plot also suggests marked non-linear (and autocorrelated) structure.

Graphically determining whether a linear regression model is appropriate to apply to a bivariate dataset is only the beginning. From here it is appropriate to ask how well does the linear model fit these data? This question is usually approached by partitioning the variance exhibited by the observed data into parts. Once again, the calculations are most easily approached for least-squares regression analysis, since the variance of interest is subsumed entirely by a single variable (the y -axis).

As we discussed last time, the variance is based on calculation of the sum of the squared deviations from the sample mean.

$$SS_T = \sum (y_i - \bar{y})^2 \tag{3.3}$$

If this quantity is taken to be the total sum of squares, another quantity can be defined as the sum of squares of the regression and calculated as the sum of the squared deviations of the y -values estimated by the regression equation (\hat{y}_i) from the sample mean.

$$SS_R = \sum (\hat{y}_i - \bar{y})^2 \tag{3.4}$$

In the simplest case, where all data points lie precisely on the regression line, $SS_T = SS_R$. In all practical cases, though, there will be some degree of deviation between the observed and estimated points. The difference between these values, then, is a measure of the dispersion of residual values about the regression line, a quantity termed the sum of squares of the error.

$$SS_E = SS_T - SS_R \tag{3.5}$$

While SS_E is a measure of the absolute error of regression, an arguably more useful comparative statistic can be determined from the ratio of SS_T and SS_R . This is the so-called r^2 -value, also termed the coefficient of dispersion.

$$r^2 = SS_R / SS_T \tag{3.6}$$

Whereas the SS_E can adopt any positive value and is sensitive to scaling differences between the variables of different regression analyses, r^2 is a measure of relative dispersion and constrained to vary between 0 and 1. Thus, this statistic measures the goodness of fit between the data and a linear model in a manner that is comparable across analyses involving different datasets. Because it is a ratio, r^2 also has no units (whereas the SS_T , SS_R and SS_E unit is the square of whatever unit in which variable y was measured). For this reason r^2 is often reported as a percentage. If $r^2 = 0$ no linear relation exists between x and y . If $r^2 = 1$ a scatterplot of x and y forms a perfectly straight line.

If you read statistical textbooks you will often come across statements to the effect that the square-root of r^2 is the 'correlation coefficient' (usually represented symbolically by r). This is not precisely true. We will discuss the correlation coefficient in detail in a future essay. For now, suffice it to say that r^2 is related to r mathematically, but differs in that r may adopt any value between -1 and 1 whereas r^2 is constrained to vary between 0 and 1 (see equations 3.3–3.6). Thus, $\sqrt{r^2}$, which is technically called the multiple correlation coefficient and which I will refer to as r_m , is also constrained to vary between 0 and 1, unlike the Pearson product-moment correlation coefficient (r).

If you have had a course in statistics or read the regression section of a statistics text you will know that the 'significance' of a least-squares linear regression is typically tested with an F -statistic. The F -distribution (which was named for the eminent British statistician Sir Ronald Fisher) is the distribution of values expected by drawing two samples of varying

sizes randomly from a normal distribution and calculating, for all possible combinations of such samples, the ratio of their variances. Because the precision with which the population variance can be estimated by samples varies with the size of the sample drawn, the form of the *F*-distribution changes for different combinations of sample sizes. Hence, the large number of tables devoted to the *F*-distribution in textbook appendices.

Basically the *F*-distribution tabulates how different the ratios of sample variances drawn from the same normal distribution can be expected to be. If the observed difference between variance ratios exceeds (say) 95 percent of the expected values, one is justified in rejecting the null hypothesis that the samples were drawn from the same distribution. An *F*-test is typically applied to regression problems by comparing the ratio of the variances attributed to the regression and residual (or error) sums of squares. By choosing this statistic to examine we are implicitly asking the question ‘Did performing a linear regression analysis have any effect on reducing the deviation of observations from the sample mean?’ If the regression analysis ‘explains’ a large component of variation observed in the sample, this should be reflected in a marked difference between SS_R and SS_E , such that the distribution of the values used in their calculation differs from the ‘single sample’ expectations of the *F*-test.

In order to perform this *F*-Test we first need to change the SS_R and SS_E values into variances. As we saw in the last essay, this is done by turning these component estimates of the total deviation into mean values. The mean squared deviation (= variance) of the total deviation is calculated by dividing SS_T by one less than total sample size ($n - 1$, see previous essay for an explanation of this convention). The mean squared deviation (= variance) of the values estimated by the regression is calculated by dividing SS_R by one less than the number of factors involved in the regression’s calculation. Since, for a linear regression, there are two such factors (the slope and the *y*-intercept), this value is 1. The mean squared deviation (= variance) of the residual values is calculated by dividing SS_E by the difference between these two denominators, $(n - 1) - 1$, or $n - 2$. Once these values are in hand, the *F*-statistic is calculated as follows.

$$F = (SS_R) / (SS_E / n - 2) \tag{3.7}$$

To illustrate these calculations on a real dataset, let us return to the trilobite glabellar data from the first essay of this series.

Table 1. Trilobite Data

	Genus	Length (mm)	Width (mm)
1	<i>Acaste</i>	5.10	3.46
2	<i>Balizoma</i>	4.60	6.53
3	<i>Calymene</i>	12.98	14.15
4	<i>Ceraurus</i>	7.90	5.32
5	<i>Cheirurus</i>	12.83	12.96
6	<i>Cybantyx</i>	16.41	13.08
7	<i>Cybeloides</i>	6.60	6.84
8	<i>Dalmanites</i>	10.00	9.12
9	<i>Delphion</i>	8.08	10.77
10	<i>Narroia</i>	15.67	9.25
11	<i>Ormathops</i>	4.53	4.11
12	<i>Phacopdina</i>	6.44	6.94
13	<i>Pricyclopyge</i>	21.53	14.64
14	<i>Ptychoparia</i>	12.82	9.36
15	<i>Rhenops</i>	22.27	17.56
16	<i>Sphaerexochus</i>	4.93	6.21
17	<i>Trimerus</i>	16.35	15.02
18	<i>Zachanthoides</i>	13.41	8.51

The equation of the least-squares regression through these data is $y = 0.64x + 2.51$. What we now want to know is whether this a good linear model of these data? Whether a linear model is appropriate for these data? And whether this linear model has had any significant effect on the error structure of these data?

We start graphically, by calculating the least-squares regression residuals and plotting these on a scatter diagram (Fig. 3).

Overall these residuals look pretty good. While there is an aspect of heteroscedasticity in the fact that the two most extreme values are separated from the remainder by what appears to be a noticeable gap both ends of the scatter exhibit a somewhat smaller deviation from the regression line than the middle group

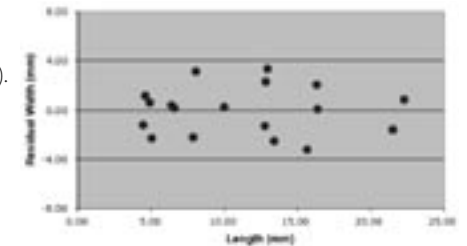


Figure 3. Scatterplot of least-squares regression residuals from the trilobite data (see equation 3.2).

of points, I would not judge these to be serious problems for this dataset. Such effects could easily be a by-product of the small size of this dataset. Based on this graphic evidence, I would conclude that a linear model was appropriate. But is this particular linear model a

good fit? To assess that we use equations 3.3 to 3.6 to complete the following table.

Regression Equation	$y = 0.64x + 2.51$
SS_T	286.92mm ²
SS_R	223.95mm ²
SS_E	62.97mm ²
r^2	0.78

Based on these statistics there does seem to be a large amount of the total dispersion accounted for by the regression (SS_R) (implying a corresponding marked reduction in the residual dispersion SS_E). The r^2 value estimates the proportional variance accounted for by the regression at 78 per cent and provides us with a means for comparing this result to the results of other regression analyses. But is this error reduction result good enough to be considered statistically significant? To ask this question another way, 'Can I assure myself that the difference between SS_R and SS_E reflects the linear trends and accounts for a meaningful proportion of the variance in the population from which the sample was drawn?' To answer this question we use the terms of equation 3.7 to complete the following table.

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Squares	F-statistic
Total	286.92mm ²	17	16.88mm ²	56.90
Regression	223.95mm ²	1	223.95mm ²	
Error	62.97mm ²	16	3.93mm ²	

Consulting a table of F -statistic critical values for a significance level (one-sided) of 0.5, degrees of freedom (numerator) of 1, and degrees of freedom (denominator) of 16 we see that any F -statistic ≥ 4.49 is considered adequate for rejecting the hypothesis that there is no difference between the variance attributable to the regression and the residual (or error) variance with a 95 per cent certainty. Since our calculated F -statistic is much greater than this critical value, the least-squares regression of these data has passed the statistical significance test.

Up to this point we've been focusing on least-squares regression because the equations used to quantify the quality of such regressions are fairly straightforward, easy to calculate, and backed-up by extensive theoretical investigation. In no small measure, this is because least-squares regression concerns itself with deviation in only a single variable. Reduced major axis (RMA) and major axis (MA) regressions are different beasts entirely because they both involve estimation of the joint variation of both x and y variables. This is much more complex and, for some reason, has not been explored extensively by theoreticians in the context of regression analysis. With a little imagination and a bit of plane geometry, though, one can imagine what an equivalent set of calculations would look like for the RMA and MA cases.

Reasoning from analogy, the univariate, least-squares SS_T and SS_R parameters, both of which are represented as squared deviations (distances) from the \bar{y} , need to be replaced by

equivalent squared deviations from observed and estimated data points referenced to the bivariate or joint mean (\bar{x}, \bar{y}) . In the case of the SS_T , this is a simple sum of squares of the straight-line (Euclidean) distances between each point in the dataset and the bivariate mean.

$$SS_T = \sum (x_i - \bar{x})^2 + (y_i - \bar{y})^2 \tag{3.9}$$

The SS_R value, then, represents an equivalent quantity after the estimated positions of the observed data points have been projected onto the linear regression line.

$$SS_R = \sum (\hat{x}_i - \bar{x})^2 + (\hat{y}_i - \bar{y})^2 \tag{3.10}$$

The trick in dealing with RMA and MA regressions lies in obtaining these estimated \hat{x}_i, \hat{y}_i values. Recall that, unlike least-squares regressions, both RMA and MA regressions estimate regression lines by minimizing the combined lengths of the line segments between the observed data points and the regression model arranged such that each segment is oriented perpendicular to the model (see Regression 1, Fig. 4; Regression 2, Fig. 2). Given these geometric relations, the estimated \hat{x}_i, \hat{y}_i values for a RMA or MA regression must be obtained by projecting the observed data points onto the regression line in a manner that respects this error-minimization geometry. One way of accomplishing this is by determining the intersection point between the regression line and the equations of a series of lines that pass through each data point and have slopes perpendicular to that of the regression line (Fig. 4).

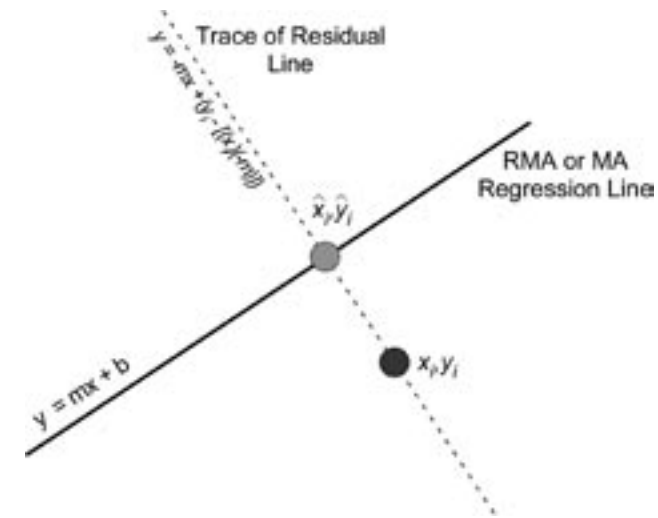


Figure 4. Geometric relations used to estimate the projections of observed points onto a reduced major axis or major axis regression line. The black dot represents an observed data point lying at some distance from the regression line. The position of the projected point (grey dot) can be estimated as the intersection point of the regression line and a line perpendicular to the regression line that passes through the observed data point.

Equations you can use to project data points (x_i, y_i) onto a RMA or MA regression line follow.

Slope of a line perpendicular to regression line $y = mx + b$

$$m_{residual} = (m) (-1) \tag{3.11}$$

Intercept of a line perpendicular to regression line $y = mx + b$ and passing through point (x_i, y_i)

$$b_{residual} = y_i - [(x_i) (m_{residual})] \tag{3.12}$$

Projected x_i -value (\hat{x}_i) onto regression line $y = mx + b$

$$\hat{x}_i = (-1) [(b - b_{residual}) / (m - m_{residual})] \tag{3.13}$$

Projected y_i -value (\hat{y}_i) onto regression line $y = mx + b$

$$\hat{y}_i = m\hat{x}_i + b \tag{3.14}$$

Once these values have been obtained, quantities analogous to SS_E and/or r^2 can be calculated (see equations 3.5 and 3.6). The Excel worksheet that accompanies this column summarizes these calculations for the trilobite glabellar data. As you will see, use of the RMA and MA approaches to estimating a linear model improves the overall fit of a linear model to these data markedly.

Let me close this column with a few observations, an encouragement, and an exception. The complications involved in obtaining \hat{x}_i, \hat{y}_i values for RMA and MA regressions (equations 3.11 to 3.14) go some way to explaining why few statistical textbooks discuss these regression models. Whereas \hat{y}_i can be obtained by a simple subtraction and $\hat{x}_i = x_i$ in the world of least squares, a rather complicated and non-intuitive geometric construction was necessary to accomplish the same estimations for linear models determined under the RMA and MA conventions. The methods I have outlined above, however, show that, contrary to many textbooks, it is not correct to say that RMA and MA regressions cannot be used to estimate the values of such variables. Least-squares regression still has the edge in terms of being able to estimate a value of y in the absence of any information other than the corresponding value of x . But, given an observed x, y value and the equation of a RMA or MA regression line, either \hat{x} or \hat{y} or both can be estimated; the calculations required are just a bit more involved. To the extent that the regression relation is the 'true' relation between two variables, and deviations from the regression model are 'true' error, the equations I develop above allow RMA and MA regressions to be used in a manner completely analogous to least-squared regressions.

Moreover, because \hat{x} and \hat{y} can be estimated for RMA and MA regressions, significance testing using the classic F -test approach is also possible for these models. The alternative F -test method I have proposed here is *ad hoc* only in the sense that other approaches to these calculations are possible. I have modelled my approach on a procedure that is analogous geometrically to the F -test used in least-squares regression analysis. If you are willing to

assume—or if you have evidence—that both variables are distributed in a bivariate normal manner, you may apply the F -test I've outlined with as much confidence as the least-squares F -test. If you suspect your data may not follow normal distributions you may still apply it as a rough-and-ready tool for indicating (not demonstrating) the statistical significance of a RMA or MA regression; just as you would a least-squares F -test. I created the RMA–MA versions of the F -test to fill an obvious gap in the pantheon of regression analysis techniques and I would be interested to know how it performs. The results obtained for the trilobite dataset appear reasonable; even a bit better than I had hoped.

As for the exception, I began this discussion by citing an example of an autocorrelated data pattern (Fig. 1B) that, I claimed, should not be analyzed by linear regression methods because to do so violated the assumption of homoscedasticity. I stand by that claim in the sense that, if you did attempt to characterize such patterns using linear models your models would ignore what is arguably the most interesting aspect of these patterns. Still, this is not to say that linear methods could not be used as part of an analytic approach to such data.

Figure 5A shows the well-known Sepkoski (2002) Phanerozoic extinction-intensity data. Note that this data set, like the example shown in Figure 5B, is composed of a complexly autocorrelated data sequence representing the contrast between so-called mass-extinction and background-extinction events, all superimposed on a declining trend: the background extinction gradient. When Jack Sepkoski and David Raup first published their family-level extinction data they recognized these patterns and wanted a way to separate them so as to estimate better the relative magnitudes of the extinction-intensity peaks. To do this they selected the standard approach to removing a linear trend from such data and performed a least-squares linear regression analysis of percent extinction magnitude on time. The resultant residual data (shown for the 2002 genus-level dataset in Fig. 5B) served as the basis for most of their (and others') subsequent analyses. The point is, these quantitative tools, which are extraordinarily useful in and of themselves, become even more so when deployed in sympathetic combination with other such tools.

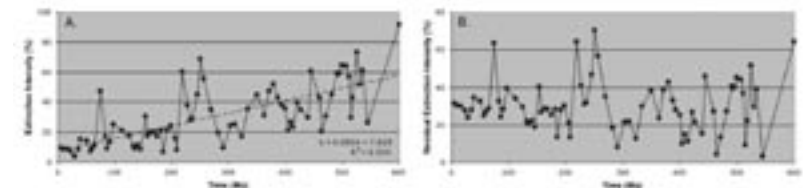


Figure 5. Use of least-squares linear regression analysis to estimate and remove the Phanerozoic background extinction gradient from extinction intensity data.
 A. Raw extinction intensity data (from Sepkoski 2002) with a plot of the regression trace and equation.
 B. Patterns of residual extinction intensities after removal of the declining linear trend. Trend normalization via regression analysis is a typical first-step in the analysis of many time series.

Letters

Several interesting responses-comments-questions have come in since last time. Perhaps the most technical was the following.

Do you know of a simple method for determining whether a value within a set of data differs significantly from the mean for that data set? Relatively small numbers (n = 7 to 14).

Anonymous

There are two ways to answer this question, both of which are relevant to the material I covered in the last newsletter. As with most statistical questions, the first step to getting a clear handle on the correct approach is to get a handle on what question you're really asking. This question can be read to be asking "What is the probability that observation x was drawn from a distribution (characterized by mean \bar{x} and standard deviation s)?" If this is the question you're interested in, let's take the following set of observations (also supplied by the questioner)...

n	\bar{x}	n	\bar{x}
1	2.42	7	5.09
2	2.45	8	5.19
3	4.52	9	5.26
4	5.00	10	5.48
5	5.06	11	6.60
6	5.08	12	7.19

...and let's say we want to know how likely we were to draw the observation 8.18 from this population. The question is not idle. The average interval between values is 0.43 whereas 8.81 is 1.62 units from the largest previously recorded value. That having been said, there is one interval (2.45–4.52) in which the difference is larger than 1.62. What to do?

The first thing to check is where 8.81 falls in terms of standard deviation units from the mean. We discussed this last time in the section on data standardization. Conversion to standard deviation units is accomplished by calculating the z-score of 8.81 which involves subtracting the sample mean (5.17) from this value and dividing the difference by the sample standard deviation (1.38). This z-score is 2.63, which means that 8.81 is 2.63 standard deviation units from the sample mean. If we are willing assume the sample was drawn from a normal population, we can look up the proportion of the normal distribution that lies beyond to 2.63 standard deviation units above the mean. This value is 0.0043. If we are willing to accept a 5 per cent chance of being wrong we can regard it as very unlikely that the value of 8.81 was part of the distribution from which we drew our sample, provided our sample (1) was drawn from a single population, (2) was drawn from a population of values that did conform to the normal distribution and (3) is composed of values that were drawn randomly and independently from that distribution. If we are unhappy with assumption 2 (and few palaeontological populations are normally distributed) we can relax it by using Chebyshev's Theorem to estimate a non-parametric version of the same test. This theorem holds that the

fraction of any dataset lying within at least k standard deviations from the mean is:

$$p = 1 - (1/k^2)$$

Applying this equation to the example data we can say that at least 0.86 (86%) of a population with a mean of 5.17 and a standard deviation of 1.38 would lie at or below 2.63 standard deviations above the mean. That of course, means that as much as 0.14 (14%) of the distribution could lie beyond 2.63 standard deviations above the mean. Thus, we are left with an ambiguous result. The value 8.81 would seem to lie outside the bounds of a normal distribution so constituted, but perhaps not outside the bounds of a non-normal distribution. The obvious next step (other than to collect a larger sample) would be to check to see if the 12-point sample conforms to the expectations of a normal distribution. Unfortunately, a discussion of procedures for normality testing will need to await a future column.

Norman MacLeod

Palaeontology Department, The Natural History Museum

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Don't forget the PalaeoMath 101 web page at

<<http://www.palass.org/pages/Palaeomath101.html>>

Meeting REPORTS



International Geobiology Course
University of Southern California – Agouron Institute
 Catalina Island 11 June – 23 July 2004

The rapidly evolving interdisciplinary field of geobiology studies the interaction of life and its environment on Earth and beyond. Here I report on a very stimulating geobiology course that I attended this Summer run by the University of Southern California, Wrigley Marine Science Institute and supported by the Agouron Institute. This intensive six-week graduate-level training course was an extremely challenging and rewarding opportunity that is open to young scientists (pre and post doctoral) interested in the field of geobiology.

The course began with a week-long field excursion in the Canadian Rockies led by **John Grotzinger** (MIT), **Andy Knoll** (Harvard), **Dawn Sumner** (UCD), **Frank Corsetti** (USC) and colleagues. The twenty student participants were drawn from around the world and with diverse backgrounds in geology and microbiology, but with the common aim of developing an interdisciplinary language and understanding of geobiology. The field sites studied included Devonian stromatoporphid reefs near Banff, anoxic shales of the Frasnian-Famennian boundary, Mississippian carbonates near Lake Louise and a tourist stop near the Burgess Shale. These localities provided a great introduction to the geobiological puzzles of the rock record and provoked excellent discussions on the outcrop of Phanerozoic ocean chemistry, biosphere



Students and instructors in the field near Waterton Lakes, the Belt Supergroup Alberta, Canada.

evolution and climate change. We then travelled west across the rooftop of the Rockies visiting the Radon Hot Springs and the “Paint Pots” of Kootenay National Park, where we discussed microbial-mediated iron precipitation, BIFs and extremophiles whilst paddling in the ochre-rich mud. We concluded the fieldtrip near Waterton Lakes in the Belt Supergroup, where we were treated to **Don Winston’s** (U. Montana-Missoula) great wealth of field experience, stories and banjo playing! Our field discussions focused on the stromatolites and ‘molar tooth’ fabrics of the MesoProterozoic carbonates, themes that were developed further in the course.

The second, five-week phase of the course was spent at the Wrigley Marine Science Centre (WMSC) on Catalina Island California.



Wrigley Marine Science Centre Catalina Island, with labs, accommodation and waterfront facilities all on site.

Here course directors **Kurt Hanselmann** (U. Zurich) and **Will Berelson** (USC) led an intensive schedule of lectures, labs, workshops and symposia, joined by visiting tutors and speakers every week. The topics covered were diverse, including: genetic approaches to geobiological problems (**D. Newman**); applications of microelectrodes in biogeochemistry (**W. Ziebis, C. Riemers**); techniques in molecular biology (**B. Stevenson, J. Spears, S. Dawson, P. Hugenholtz**); manganese cycling and biogeochemistry (**B. Tebo, D. Burdige**); anaerobic methane oxidation, rates and zones of diagenesis (**M. Alperin, W. Berelson**); microbial culturing techniques and biosignatures (**K. Neelson, K. Hanselmann**) anammox and giant sulphur bacteria (**G. Kuenen**). (A more complete list of the lectures is available at <<http://wrigley.usc.edu/geobiology/#dates>>). To accompany these lectures there were hands-on lab exercises with highlights including: the development of genetic screens for iron-oxide reductants; DNA extraction and the sequencing of microbial samples; and sediment profiling using microelectrodes. We also undertook a class research cruise to collect and analyse water and sediment samples from the nearby San Pedro

Basin. For many of us this was our first experience of a research cruise and we were joined by two alumni of the course, who had returned to investigate further a possible microbial origin for anomalous nitrate peaks in the sediment pore waters.



Students and instructors taking sediments cores from the San Pedro Basin, California.

Whilst we were on the island the course also hosted three research symposia with international speakers and attendees joining us on the island. At the first symposium, “Early Evolution of Microbial Eukaryotes,” we heard about protistan life cycles in the modern ocean (**Dave Caron** USC); ongoing studies of Proterozoic protozoa and vase-shaped microfossils (**Susannah Porter**, UCSB); insights into cytoskeleton development in basal eukaryotes (**Scott Dawson**, UCB); the origins of photosynthetic organelles in eukaryotes (**Charles Delwiche**, University of Maryland); and finally molecular trees and the misnomer of “prokaryotes” (**Norman Pace**, University of Colorado, Boulder). The second symposium discussed “Anoxic Marine Sediments Ancient and Modern,” with talks on: the metazoans and protists that inhabit steep redox boundaries (**Joan Bernhard**, WHOI); the latest on clathrates and microbially-mediated seafloor methane systems (**Jerry Dickins**, Rice University); new palaeo-oceanography proxies and Late Quaternary oxygenation of the West coast of North America (**Tom Pedersen**, U. Victoria); the sulphur cycle of marine sediments (**Bo Barker Joergensen**, MPI Bremen); and the role of anoxic sediments in the global sulphate and carbon cycles (**Dan Schrag**, Harvard). The third and final symposium was equally ambitious, covering “Redox Changes in Earth History,” with talks on genomic and biochemical approaches to the transition from an anaerobic to an aerobic world (**Robert Blankenship**, ASU); the oxygenation of the early terrestrial atmosphere from three different approaches, atmospheric modelling (**David Catling**, U. Washington), Palaeoproterozoic snowball events and manganese deposits (**Joe Kirschvink**, Caltech), and oceanic redox models (**Paul Falkowski**, Rutgers University).

This final symposium closed with the latest news from the Mars Exploration Rover (**David Des Marais**, NASA Ames) that provided a further, much discussed perspective on early habitable environments.

In the final three weeks on Catalina Island we also had the opportunity to design and conduct a research project to test and develop some of our new ideas and skills. I chose to work with co-student **Nina Knab** (MPI Bremen) on intertidal microbial mats at a local site on Catalina Island that complemented my DPhil research on stromatolites. We investigated the relationship between the composition of the microbial mat community, the mat morphology and the inter-tidal regime, discovering a new cyanobacterial group in the process. We tested the effects of metazoan and protistan grazing on the mats, and concluded that grazing is a constructive force essential to maintaining the “fitness” of the mat. We also investigated the capability of the mat to trap and bind different volumes and grain sizes of sediment, an important mechanism of stromatolite accretion. This was just one of the many research projects that was presented at the end of the course, to illustrate the diversity covered; others included: experiments with yeast and plaster to investigate a microbial origin for molar tooth fabrics (**E. Flemming**, UCD); halophilic microbes from the Salton Sea (**J. Bailey**, USC); experiments to produce a/biotic manganese oxides (**A. Toes**, TU Delft). It was perhaps the unique setting of the WMSC with the labs, accommodation and waterfront facilities all on site, that encouraged a friendly, supportive atmosphere and enabled us all to immerse ourselves fully in the geobiology course, with the odd canoeing and snorkelling trip (including late night phosphorescent dinoflagellates!).

In summary, this Geobiology course was an amazing scientific experience and I would strongly encourage other graduate students to apply. The opportunities to learn and practise new techniques, discuss ideas with other students and leaders in the field was unparalleled. I made many new friendships and initiated exciting new scientific collaborations. At present the UK and Europe perhaps lag behind the USA in nurturing the growing field of Geobiology. As a young scientist in this field I see great scope for comparable workshops, Summer schools and conferences here in Europe. For further information on the USC-Agouron Institute Geobiology course check out the website below and links to other geobiology resources.

University of Southern California-Agouron Institute Geobiology course:

<<http://wrigley.usc.edu/geobiology/>>

Geobiology Oxford:

<<http://www.earth.ox.ac.uk/research/geobiology/geobiology.htm>>

NSF Biogeosciences:

<<http://www.biogeosciences.org/index.htm>>

Virtual Journal of Geobiology (Elsevier):

<<http://earth.elsevier.com/geobiology/>>

Geobiology (Blackwell-Synergy):

<<http://www.blackwellpublishing.com/journal.asp?ref=1472-4677>>

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**Fossils, fakes and forgeries**

BA Festival of Science, Exeter University 6 September 2004

Fossils fire the imagination, but to what extent can fancy interfere with the communication of science? When does a life-like reconstruction of an extinct creature, even made with the best intentions, grade into fantasy? How do we know? What criteria should we employ? And how can we tell if we're being spoofed? Such were the weighty issues on the table at the Palaeontological Association's session at the BA Festival of Science in Exeter on 6th September.

First, there's outright fraud, and the most notorious fossil fraud was Piltdown Man. In 1912, fossils purporting to come from a new and ancient species of hominid were unearthed in gravels near Piltdown in Sussex. As everyone knows, the fossils were fake—even though it took four decades to expose the hoax. Even less tractable has been the mystery of the hoaxer's identity. Local solicitor Charles Dawson, who 'discovered' the finds, has always been the prime suspect, although blame has, over the years, been attached to one or more of a cast of characters to rival the most lurid crime novel—from palaeontologist and Jesuit Pierre Teilhard de Chardin to author Sir Arthur Conan Doyle. Brian Gardiner (King's College, London) has long maintained that Martin A. C. Hinton, Late Keeper of Zoology was the guilty man, which is why his recent paper on the case (Gardiner, 2003) is particularly welcome as a statement of record—a paper presented at this meeting.

How can frauds persist for so long, even when—in retrospect—they seem crude? Self-delusion is an important factor. The idea that human ancestors would have had large brains and ape-like jaws was so ingrained among turn-of-the-century anthropologists that Piltdown Man, with its modern-looking (indeed, modern!) skull and ape-like (indeed, from an ape!) jaw was precisely what people wanted to see.

Another instance of the persistence of fraud, fuelled by self-delusion, was the case of Beringer and his 'iconoliths,' discussed by Paul Taylor (Natural History Museum, London), who has had a chance to examine such material that remains, and has recently published a full account (Taylor, 2004). One Johann Beringer (1667–1740), a prominent physician from Würzburg, received around 2,000 faked fossils, purportedly from limestone exposures at Eivelstadt, and published some of them in his now-notorious *Lithographiae Wurcebergensis* (1726). By today's standards the fossils are transparently fraudulent—among the cornucopia of improbability are birds



preserved in flight; plants complete with flowers and roots; spiders on their webs, even celestial objects and Hebrew inscriptions of the ineffable name of God. The fraud was soon exposed as the work of jealous rivals. To be fair to Beringer, he resisted the temptation of formally naming the fossils (no *Eoanthropus dawsoni* for him), and even wrote extensively on the possibility that the fossils were faked, an idea he dismissed. That he got as far as publishing them says much about the imperfect knowledge of fossilization in Beringer's time—people did not have a clear idea of what fossils ought to have looked like—as well as the degree to which Beringer took the truth of the fossils on trust.

Self-delusion is clearly something for which scientists should be watchful: in my own presentation, I argued that it is an even greater problem than malicious fraud. It is a wonder, in fact, that fraud does not occur particularly often, given that the entire scientific enterprise rests on trust. The informal motto of the Federal Bureau of Investigation may well be 'In God we trust—everyone else we check out,' but scientists and publishers cannot afford the luxury of such scrupulous oversight. When a manuscript arrives at the office of a journal editor, the editor cannot afford to repeat the experiments described in any one paper, nor travel to a remote location to check matters for himself. He is forced by circumstances to assume that what is presented therein is an honest account. The same constraints apply to the referees asked to assess the technical merits of the manuscript.

At first sight, the mechanisms of science publication leave themselves wide open to outrageous abuses of trust. There are two reasons, I think, why fraud is less of a problem than it might be. The first is that the community of science is small and interdependent. The same person who submits a manuscript on Monday will be asked to review the manuscript of a colleague on Tuesday, edit a journal on Wednesday, and serve on an ethics panel on Thursday, so there is a strong incentive to Do As You Would Be Done By.

The second reason is that even in a general climate of mutual trust, the truth will out, even if it takes a long time to emerge. When research after 1912 on genuine fossil hominids suggested that they generally had small brains and modern-looking jaws, the opposite situation as found in Piltdown Man, the Piltdown fossils were cited more as anomalies than fakes. After all, given that scientists and editors are almost completely unable to police themselves except by example, one cannot afford to doubt the veracity of any scientific account—for once started, when would you stop? That way madness lies. One could argue that Beringer was a victim of a similar set of circumstances: he was unwilling to believe that he had been tricked, despite the copious evidence before his own eyes. The same might be said of the suspension of disbelief attendant on the examination of one of the Piltdown artefacts, a bone carved in the shape of a cricket bat. The first Englishman, the hoaxer seems to be saying, would not be properly accounted without such an object—and yet the scientific establishment still fell for it.

Beringer is thought to have been a pompous and self-opinionated person, ripe for a fall. The same might have been said of Arthur Smith Woodward, the curator at the Natural History Museum thought to be the principal intended victim of Piltdown. Beyond practical jokes, however, the Piltdown and Beringer cases raise questions about the degree to which we are all complicit in reconstructions and restorations of past life, all of which require a measure of imagination. The line between responsible reconstruction and outright fantasy was the focus of

opposing presentations from Paul Barratt (Natural History Museum, London) and Mike Benton (University of Bristol). The presentation of reconstructions depends very much on the context in which they are placed. In other words, we, the viewers, must be given information *additional* to the reconstruction before we can judge whether or not to believe it as 'truth'.

Criticisms of the TV series *Walking With Dinosaurs* concern not the reconstructions themselves—which all agree were breathtaking—but the fact that the script was constrained by its documentary format to present, with equal weight, contentions that varied from known fact to unsupported hypothesis. Benton, an adviser on the series, defended it stoutly by appealing to our general sense that any and all reconstructions of prehistoric animals must, by their very nature, be speculative, even in a documentary format or, for that matter, a scientific paper. In other words, the subtext of the programme asked us to be complicit in the suspension of disbelief, despite its format. As Barrett showed, contextual—or explicit—health warnings can be useful, even in the most unlikely cases. When reconstructions are presented in an explicitly fictional setting (Barrett used B-movies such as *One Million Years BC* to make the point), one can still learn useful things about science.

So, does Racquel Welch's fur-lined lingerie represent a greater contribution to human wisdom than Beringer's iconoliths? The question is less easy to answer than it might first appear. For, as we have seen, appearances are deceptive rather than objective, as any object requires for its comprehension a contract of complicity between our own perceptions and the world around us.

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IGCP 503 Early Palaeozoic Palaeogeography and Palaeoclimate
Erlangen, Germany 1 – 4 September 2004

<<http://www.pal.uni-erlangen.de/IGCP503/>>

Following the success of the IGCP Project 410: *The Great Ordovician Biodiversification Event: Implication for Global Correlation and Resources* (1997–2002), UNESCO-IUGS has endorsed its successor project, IGCP 503: *Early Palaeozoic Palaeogeography and Palaeoclimate*. The project will last five years, each year having a specific theme that will contribute to the overall project synthesis:

- Year 1: Ocean and climate modelling integrating C, O and Sr isotopes.
- Year 2: Evolutionary palaeoecology.
- Year 3: Changing palaeogeographical patterns.
- Year 4: Early Palaeozoic events and stratigraphy.
- Year 5: Early Palaeozoic sea-level change.

The historical Erlangen Schloss (castle), set in the spectacularly symmetrical Schlossgarten of the small university town of Erlangen, Franconia, Germany, was the venue for the first international symposium of the IGCP 503 project. For a first symposium, attendance was astonishing: in excess of 200 delegates from over 20 countries, 43 speakers and 38 poster presentations made for an enthusiastic start to this project.

Axel Munnecke (Erlangen, Germany), the symposium chair, opened the symposium and welcomed all to the first session. He must be given special mention for providing all delegates with a smooth running and efficient conference.

Florentin Paris (Universite de Rennes) summarised the achievements and lessons learnt from IGCP 410 (see review by H. Armstrong in this volume of *The Great Ordovician Biodiversification Event* for a detailed synopsis). The take-home message from IGCP 410 was that through close integration and collaboration, we now have a bountiful database of clade-group diversity patterns in the Early Palaeozoic from which Ordovician researchers can now apply total Earth systems models.

The Ordovician records several abrupt C and O isotope excursion events that are documented globally and can clearly be associated with specific environmental perturbations; for instance, **Dimitri Kaljo** (Tallinn University, Estonia) reported mid-late Ordovician carbon isotope excursions from Baltoscandia and recognised two periods of high variability in the mid-Darriwilian and the Caradoc-Hirnantian. The mechanisms influencing these changes and the subtle interplay with biotas are poorly understood, especially with regard to changing ecosystems: *e.g.* how does the global carbon sink budget and distribution change with the demise of planktonic organisms in the late Ordovician? However, before we can begin to consider such questions, we must be aware that our interpretations of stable isotopic data from biominerals may be seriously jeopardised without considering and accounting for the effects

of inter-generic isotope fractionation (in strophomenids and pentamerid brachiopods at least; **Torsten Bickert**, Universität Bremen). This was reiterated by **Michael Joachimski** (Erlangen, Germany) who separately documented dissimilar palaeotemperatures from Silurian conodonts and brachiopods, differing by as much as 8°C. With this point in mind, Torsten demonstrated that the Llandovery-Wenlock trilobite-conodont extinction occurs at the base of a major $\delta^{13}\text{C}$ perturbation indicating that the process (antiestuarine circulation and salinity change from high evaporation) was a driving force of local extinction and not the mechanism causing the isotopic shift itself. Resolving one aspect of biotic and abiotic system interactions, **Bradley Cramer** (Ohio State University, USA) enthusiastically and eloquently demonstrated how rising pCO_2 in the Wenlock of Tennessee was concurrent with an increase in organic carbon burial in deep marine waters and that the site of deep water formation changed from low to high latitudes during this time. This was a fine example of an approach to investigation that should now be applied to established Ordovician events.

It has long been established that the end-Ordovician (Hirnantian) glaciation is synchronous with definite $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ excursions. These shifts are taken to be associated with the two-phase mass extinction at this time, though in light of the lectures received in the first session, we should perhaps be looking towards more substantial physical evidence for subtle patterns in the development and decline of the Gondwanan polar ice cap. To constrain the true cause and effect of the pre-Hirnantian pCO_2 fall, **Achim Herrmann** (Washington University, USA) proposed, from atmospheric and ocean general circulation models, that the fall in atmospheric pCO_2 would only be a direct cause of glaciation if it fell below eight times that of present day atmospheric levels (PAL). This is significantly lower than calculations made previously of a pCO_2 decrease twelve to twenty times PAL. This figure may fall further still if estimates of mean global elevation are increased in models. In addition to this, the spatial distribution of marine organisms in the Caradoc conforms to climatic and oceanographic simulation models following this pattern.

Of a similar level of controversy is the exact process and mechanism by which the end-Ordovician glaciation came to a close. Current thought proposes two short scale (<1Ma) eccentricity cycles controlling the melting of a Laurentide-scale ice sheet. However, **Javier Álvaro** (Université de Lille, France) documented six minor regional Hirnantian cycles during glacial regression in the eastern Anti-Atlas of Morocco from newly recognised terrestrial geomorphological features. From the same school, **Julien Moreau** (Université de Strasbourg, France) used seismic sections to identify five small-scale sequence stratigraphic events representative of sporadic glacial melting cycles in western Gondwana from North and West African Hirnantian sediments. This is further supported by geophysical mass-balance models expressing the extent and waxing and waning of the North African Gondwanan ice sheet (**Daniel Le Heron**, Université de Strasbourg, France). Focusing on physical evidence in the marine realm, **Pat Brechley** (University of Liverpool, UK) described, from rather esoteric quarry outcrops in east Wales, limestones composed of erosional debris derived from Rawtheyan material in an otherwise clastic submarine channel. The limestone became karstified during marine regression and subsequently covered by shallow marine sand during marine transgression. These physical features therefore record subtle end-Ordovician glacio-eustatic sealevel fluctuations. Physical evidence such as this surely now paves the way for work investigating the geochemical and biological counterparts both locally and globally, but to do

so we must be aware of the global changes that such a cyclical pattern would produce. Taking a geochemical stance, **Howard Armstrong** (University of Durham, UK) applied a present-day model of anoxia to understand the formation of ubiquitous 'hot' black shale deposition on the periglacial continental shelf of northern Gondwana. In their study area, the Batra Formation, Jordan, sequence stratigraphy, microfacies analysis, biomarkers and spectral markers of TOC data were used to show anoxia developed as a consequence of obliquity-forced seasonal variations, ice sheet melting and eutrophication resulting in elevated bioproductivity and anoxia (Strakhov hypothesis).

The final session of the day was an open session and consisted of a number of presentations on various aspects of Ordovician, Silurian systems and even a mention of the Devonian. **Guillermo Albanesi** (Universidad Nacional de Córdoba, Argentina) shed some light on the metamorphic history of the enigmatic Argentine Precordillera representing the north-western Gondwanan margin by applying the conodont colouration index. **Li Yue** (Nanjing Institute, China) discussed the pattern of recovery of Silurian reef development in the Ord-Sil of the Yangtze Platform in south China. These form a sensitive proxy for equatorial climate reefs. Development was favoured during interglacial episodes but following the glaciation, did not then reappear until the Aeronian.

Day Two commenced with an invited lecture from **Robin Cocks** (NHM, UK) who gave a comprehensive review of known Ordovician palaeogeography and highlighted areas of current controversy. Problems concerning Ordovician palaeogeography were the key issue in this session, a problem that **Chris Scotese** (University of Texas) suggested may be rectified through greater incorporation of GIS systems. He emphasised the need for a standard approach to palaeogeographical reconstruction and the need for accessible datasets for GIS systems. This he again reiterated in the final closing lecture of the conference.

Of great concern was the exact position and rotation of the Baltica palaeocontinent throughout the Ordovician. The well-established exposition that Baltica rotated by a full 90° was challenged by the palaeogeographical models of Scotese and plate tectonic theory. To complement this controversy, recent geomagnetic work from the Urals places Baltica at a palaeolatitude of 20°S throughout the early to mid-Ordovician and at 30°S in the late-Ordovician rather than at 10°S by the late Ordovician (**Natalia Lubnina**, Moscow State University, Russia).

The fifth session saw an attempt by workers to begin to integrate phylogeny-based abundance data with palaeogeographical models, one of the aims of IGCP 410 and a major objective of this new project. However, **Thomas Servais** (Lille, France) highlighted potential flaws with such approaches if one does not choose the fossils carefully. Extant dinocysts and acritarchs exhibit distinct provinces in modern oceans; however, those in the Arenig are particularly bad for provincial studies for they appear to be more cosmopolitan. Servais and co-workers identify provinciality in assumed cosmopolitan fauna and suggest that for each previously disregarded organism one needs to be identified for specific types of water body; hot and cold for instance, that will hence be a good biogeographic marker.

The final day started with a series of presentations to inspire a movement for the integration of biotic-abiotic palaeomodelling and addressed several key issues pertaining to climate in

the Early Palaeozoic; notably, was there actually an end-Ordovician warming event prior to glaciation, and whether or not the significant decrease in $p\text{CO}_2$ that is observed in the Hirnantian was one of the actual causes of glaciation that thus drove extinction. **Richard Fortey** (NHM, UK) drew attention to the pole-ward migration and distribution of endemic trilobite and brachiopod faunas prior to the initial onset of the Hirnantian glaciation. The encroachment into high latitudes by equatorial taxa and an accompanying global gondwanan bryozoan limestone distribution between clastic intervals point to an episode of warmth prior to glaciation. This is contrary to the conclusion made the previous day by **Lesley Cherns** (Cardiff University, UK) of a cool episode at this time. By analogy with cool water Quaternary bryozoan mud mounds from Australia, she attributes bryozoan mud mounds from Libya, interpreted from seismic sections, in the Ashgill to represent mud mound development in a high latitude cool water slope or outer ramp environment. The constraints on the exact age of these mounds are uncertain and rely on unresolved conodont data.

Documenting clade-group diversity patterns, as in IGCP 410, is essential for understanding Palaeozoic Earth system processes, and forms a strong platform and database that can now be utilised in IGCP 503. Many of the works in the seventh and eighth sessions represented the final legacy of IGCP 410 and avoided a holistic discipline integration that is the essence of IGCP 503. This point aside, we now have, amongst others, complementary data regarding chitinozoan diversification (**Aicha Achab**, Centre Géoscientifique de Quebec), the disparity of Orthid brachiopod diversity and distribution (**Dave Harper**, University of Copenhagen, Denmark), Avalonian trilobite diversity (**Alan Owen**, University of Glasgow, UK), conodonts from the Urals (**Svetlana Dubinina**, Russian Academy of Sciences), Tremadocian stylophoran echinoderms from Korea (**Bertrand Lefebvre**, Université de Bourgogne, France) and endemic Silurian thelodonts from Central Asia and the Siberian Platform (**Zivile Zigaite**, Vilnius University, Estonia).

The final session of the symposium was a second open session, with a broad and varied range of topics covering many general aspects of Early Palaeozoic life, sediments and systems even including glaciation in the mid-Cambrian (**Andrei Dronov**, St Petersburg State University, Russia). **Enrique Díaz-Martínez** (Spanish Geological Survey) presented a variety of data from the Ordovician-Silurian boundary near La Paz (Bolivia) that could be integrated in the future in order to gain an understanding of western gondwana evolution at the end of the Ordovician. The closing address was a second presentation by **Chris Scotese** who demonstrated an alternative GIS palaeoclimate model, the Parametric Climate Model (PCM), to previous computer simulations such as global climate models (GCMs). In essence, Scotese demonstrated an impressive use of geological data to set conditions from which his PCM model will run, applied this to the Palaeozoic but then went on to extrapolate some twenty million years into the future. This may seem ludicrous, but when one identifies the confidence with which we can reliably use geological, geochemical and palaeontological evidence for interpretations, as Scotese did, extrapolation a short distance into the future is just as plausible as interpreting from the deep past.

The conference was supported by a quality social programme to relieve all from each day's intense aural barrage. Following the closure of the first day, all delegates had the opportunity to sample the delights of the rich history and architecture of Erlangen under the enthusiastic

guidance of the official Erlangen tour guide team. Of particular interest was the old university prison-come-gentlemen's club in the Schlossgarten fountain water tower where it was considered a rite of passage to be an inmate. Modern university justice systems just don't seem to compare with those of the eighteenth century.

The afternoon of the second day was occupied by an excursion to the infamous palaeontological Mecca of Solnhofen, offering all delegates the chance to visit one of the working quarries and try their hand at finding the tenth *Archaeopteryx* specimen. Unfortunately, not being accustomed to such young rocks and large fossils, the discovery of the day was two small-fry fish and plenty of the pelagic crinoid *Saccocomas*. To avoid disappointment, everybody did get the chance to see two of the real *Archaeopteryx* specimens. In the evening, after everyone had dusted themselves down and slipped into their finery, delegates moved on to the vaulted cellars of the Hotel Bayerischer Hof to sample a smorgasbord of delicious German culinary delights at the conference dinner. This was accompanied by the soothing voice of the hotel's very own in-house valseur and a fine accompanying accordion soloist.

To add the perfect ending to the final day, a trip to the Kitzmann brewery was organised where all received four bottles of complimentary beer. Thus the first International Symposium on Early Palaeozoic Palaeogeography and Palaeoclimate (IGCP 503) was a major success. In a project such as this it would seem quite easy to continue to produce lists and patterns of organism distribution and diversity but IGCP 503 goes further than this. It takes this initial data and combines it with similar lists and patterns from other disciplines. It is a strongly interdisciplinary project that focuses on the Palaeozoic world as a system and aims to explain the curves produced by IGCP 410. We now have the resources and data to begin to explain palaeontological systems, and why not start with the Ordovician?

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>> **Future Meetings of Other Bodies**



Society of Integrative and Comparative Biology
San Diego, California 4 – 8 Jan 2005

For details of this meeting see <<http://www.sicb.org/meetings/2005/index.php3>>.


The following two items are part of this meeting.



Symposium on Terminal Addition, Segmentation, and the Evolution of Metazoan Body Plan Regionalization

Recent insights into the developmental basis of body plan specification provide a new perspective upon major patterns of Metazoan diversification. Terminal addition, a process by which the body of bilaterian animals grows at a posterior growth zone that is most clearly displayed in segmented animals, is a common condition found among disparate metazoan groups. Terminal addition is both a morphologic and a developmental phenomenon. Consequently, it can be examined through the study of development of modern organisms where molecular tools are available for the comparison of developmental process among taxa. The symposium aims to balance discussion of developmental mechanisms against historical evidence chronicled in the phylogenies of both living and fossil groups. We hope that it, and the resultant volume, will play a significant role in emphasizing the strengths of an integrated approach to the evolution of posterior body patterning.

Meeting sponsored by: SICB Divisions of Evolutionary Developmental Biology, Systematic Biology, Vertebrate Morphology and Invertebrate Zoology, the Paleontological Society and the Palaeontological Associations. This two-day symposium, organized by Nigel Hughes and David Jacobs, will be held at the Annual Meeting of the Society for Integrative and Comparative Biology, 4 – 8 January 2005, at the Town & Country Hotel, San Diego, California. For full details of the programme, including field trips, see <<http://www.sicb.org/meetings/2005/symposia/terminal.php3>>




Evolution and Development of the Vertebrate Dentition

The programme includes the following speakers: Moya Smith (KCL, UK: 'Developmental models for the origin of vertebrate dentitions'), Philip Donoghue (Bristol, UK: 'Evolution and development of the skeleton in the earliest vertebrates'), Anne Huysseune (Ghent, Belgium; 'Patterning of development in tooth replacement in osteichthyan dentitions'), Robert Reisz (Toronto, Canada; 'Origin of dental occlusion in tetrapods, signals for terrestrial vertebrate evolution'), Tim Mitsiadis (KCL, UK; 'Recovery of teeth in birds'), Renata Peterkova (Czech; 'Phylogenetic memory of developing mammalian dentition'), Todd Strelman (New Hampshire,

USA; 'Genetics and development of the cichlid dentition'), Ken Weiss (Penn State, USA; 'Evolutionary genetics of dental development'), Paul Sharpe (KCL, UK; 'Development and evolution of dental pattern') and Jukka Jernvall (Helsinki, Finland; 'Mammalian dental diversity').


Sponsored by SICB Divisions of Evolutionary and Developmental Biology, Vertebrate Morphology, Cell Biology, and Systematic and Evolutionary Biology (primary).

Organizer: Moya Smith, e-mail <moya.smith@kcl.ac.uk>.



4th International School on Planktonic Foraminifera: Oligocene to Middle Miocene Planktonic Foraminifera
Perugia, Italy 14 – 18 February 2005


The course will include lectures (taxonomy, biostratigraphy and palaeoceanography) on Oligocene to Middle Miocene Planktonic Foraminifera and practical sessions studying washed assemblages. The maximum number of participants is 30. Registration and payment deadline: 30th November 2004. Course organised by Prof. Silvia IACCARINO University of Parma (Italy) and Prof. Isabella PREMOLI SILVA University of Milano (Italy) with the collaboration of Drs Milena Biolzi, Luca Maria Foresi and Fabrizio Lirer. For further details, please contact Dr Roberto Rettori, Dipartimento di Scienze della Terra, Piazza Università, 1, I-06100 Perugia, Italy, e-mail <rrettori@unipg.it>, tel 00390755852664, fax 00390755852603, or visit the website at <<http://www.unipg.it/~denz/>>.




Geologic problem solving with microfossils
Rice University, Houston, Texas, USA 6 – 11 March 2005

The aim of the meeting is to draw together a diverse array of geoscientists to showcase the problem-solving power of microfossils in a variety of geologic settings. Call for abstracts via the website from 1st April to 14th October 2004. Registration begins 6th September 2004, via the website.

For details visit the conference website at <<http://www.sepm.org/microfossils2005.htm>>, or contact <thomas.d.demchuk@conocophillips.com>.



Tracking Dinosaur Origins: the Triassic/Jurassic terrestrial transition
Dixie State College of Utah, St. George, Utah 22 – 24 March 2005



Utah Friends of Paleontology Annual Meeting
Dixie State College of Utah, St. George, Utah 25 – 26 March 2005

The Triassic/Jurassic transition is a critical time in Earth history, recording the origins and early radiation of dinosaurs, pterosaurs, crocodylians, mammals, and several other significant


Mesozoic vertebrate clades. Additionally, a major interval of faunal stepwise extinction is recorded in both the marine and terrestrial environments that may be linked to impact events, setting the stage for the ascendance of dinosaurs to a position of dominance for the remainder of the Mesozoic. Current research in this area is dynamic, with important implications for a number of areas in palaeobiology and geology. A number of recently discovered fossil localities in a little researched area of southwestern Utah preserves a thick sequence of rocks spanning the Triassic/Jurassic interval. These localities are proving to be a catalyst for new studies on this time period. Many of these studies have centred on the basal Jurassic St. George Dinosaur Tracksite at Johnson Farm. This remarkable new site preserves an extraordinary series of track levels along the margin of a Hettangian lake ("Lake Dixie"); associated fossil plants, invertebrates, fish, and dinosaur remains make it particularly significant. These discoveries, along with a new interpretive centre slated to open in the Summer of 2004, provide an impetus to bring scientists together to discuss terrestrial faunas across the Triassic/Jurassic transition in a dramatic geologic setting unfamiliar to most attendees. A proceedings volume to be published by the New Mexico Museum of Natural History and Science, and a full colour overview volume, are planned by the Utah Geological Survey for initial distribution to attendees at the conference. This volume will include short review papers on areas of critical interest regarding the Triassic/Jurassic terrestrial transition in various areas of the world, summary papers on these rocks, and their preserved fossils in southwestern Utah.

Preliminary programme:

- March 22: plenary papers;
- 23: general conference papers;
- 24: field trip: Triassic/Jurassic geology and palaeontology in the St. George and Zion National Park areas.

Conference participants may fly into St. George, Utah directly, or speakers may fly into Las Vegas, Nevada and then be transported by volunteers to St. George. Conference participants are invited to remain for the Utah Friends of Paleontology Annual Meeting, which will include additional afternoon field trips on 25th and 26th March.

Information on the St. George tracksite may be viewed starting on page 4 of Survey Notes v. 34, no. 5. at <<http://geology.utah.gov/surveynotes/snt34-3.pdf>>.




10th Conference on Australasian Vertebrate Evolution, Palaeontology and Systematics (CAVEPS)
Naracoorte Caves, South Australia 29 March – 2 April 2005

The 10th CAVEPS will be held at the World Heritage listed Naracoorte Caves National Park and nearby Naracoorte township, approximately 360 km SE of Adelaide (capital city of South Australia), in the Limestone Coast region of South Australia. The Limestone Coast is well known for its Pleistocene vertebrate sites, with the most significant of these within the Naracoorte Caves World Heritage Area (serial nomination with Riversleigh, Queensland). The region is also famous for its high quality wine, wetlands and significant karst and geological features.

CAVEPS is a biennial meeting of vertebrate palaeontologists from around Australia and overseas. CAVEPS 2005 will consist of three days of general sessions including papers on all aspects of vertebrate palaeontology, culminating in a two-day symposium which will focus on Quaternary extinctions and dating applications. Included in the general sessions will be a special session on cave palaeontology which will be held in the historic Blanche Cave. In addition to the main sessions, a student forum is proposed where students can present their project proposals or work in progress and benefit from professional input. The conference will be held during the AVCC (Australian Vice Chancellors Commission) common vacation week and will commence on Tuesday 29th March (Monday 28th is Easter Monday), and culminate on Saturday 2nd April.

For further details visit <<http://www.environment.sa.gov.au/parks/naracoorte/events.html>>, contact Liz Reed <liz.reed@flinders.edu.au> or Steven Bourne <Bourne.Steven@saugov.sa.gov.au>, or write to CAVEPS 2005 c/o Naracoorte Caves National Park, PO Box 134, Naracoorte South Australia 5271, Australia.




The Palms: an International Symposium
The Linnean Society and Royal Botanic Gardens, Kew 6 – 8 April 2005

This international symposium aims to draw on recent advances by focusing on the current status of palm research both in evolutionary biology and in the environment. The symposium will take place at the Linnean Society and at the Royal Botanic Gardens, Kew. Invited papers will be given at the Linnean Society following the four main themes of Phylogeny & Evolution, Conservation & Sustainable Use, Structural Biology and Ecology.

A poster session will be held at Kew, as well as a series of workshops, including one on bioinformatics. Tours of Kew's extensive living collections of palms and the Herbarium and Library will be available. All participants will be encouraged to present posters at the meeting.

The symposium is held in honour of Dr John Dransfield, Head of Palm Research at Kew, in recognition of his outstanding contribution to global knowledge of palms over the past four decades. John is well known as co-author with Natalie Uhl of *Genera Palmarum*, the benchmark monograph of the palm family.

To receive further information, contact <janet@linnean.org> (subject: **Palms 2005**) or write to the Linnean Society of London, Burlington House, Piccadilly, London W1J 0BF.



Palynology Specialist Group of the Linnean Society of London:
Spring Meeting 2005
Linnean Society, Burlington House, Piccadilly, London 27 April 2005

This is to mark the retirement from the Royal Botanic Gardens, Kew, of former Palynology Specialist Group Secretary, Madeline Harley. Speakers so far include: Peter Crane (Kew), Bill Chaloner and Margaret Collinson (Royal Holloway University of London), Hannah Banks

(Kew), Raymond van der Ham (Leiden), Alan Hemsley (Cardiff), Michael Hesse (Vienna), Simon Owens (Kew). Offers of talks from people who know Madeline are welcome.

Please come along to wish Madeline well in her retirement (though I'm sure she won't be giving up palynology)! For further details, or if you wish to attend the meeting, please contact Carol Furness at, e-mail <c.furness@rbgkew.org.uk>, or at The Palynology Unit, RBG Kew, Richmond, Surrey, TW9 3AE, U.K, Tel: +44 (0)20 8332 5263.



**Palaeobotany Specialist Group of the Linnean Society of London:
Spring Meeting 2005**
The Linnean Society of London, Piccadilly, London 28 April 2005

This is a one-day open meeting with contributions welcome on **any area** of the **study of fossil plants**. Speakers who would like to make a presentation are requested to contact the Palaeobotany Group Secretary, who will compile the meeting programme. This should include the title of the presentation, name(s) of author(s), institutional affiliations (if applicable), and a short abstract of the presentation, preferably less than half a page of A4, and including images if required. This meeting will follow on from a one-day meeting of the Palynology Specialist Group of the Linnean Society of London.

For further details, and to submit presentations, contact the Palaeobotany Specialist Group Secretary, e-mail <webmaster@palass.org>.



Devonian vertebrates of the continental margins, IGCP 491 meeting
Yerevan, Armenia 22 – 27 May 2005

The meeting will be dealing with all aspects of taxonomy, biostratigraphy, palaeoecology and biogeography of early vertebrates as listed in the scopes of IGCP 491: "Middle Palaeozoic Vertebrate Biogeography, Palaeogeography and Climate." Special focus will be on the neritic to hemipelagic vertebrate faunas of the Middle to Upper Devonian on the northern edge of Gondwana and their relationships to contemporaneous faunas of Laurasia and the rest of Gondwana. An IGCP 491 business meeting and a post-conference field trip to Armenian Devonian–Carboniferous sites are also planned. The meeting will be hosted by the Institute of Geological Sciences, National Academy of Sciences, Marshall Baghramian Ave., Yerevan, Armenia on 22–27 May 2005. The scientific sessions are planned for 23–25 May. A post-conference field trip of two days is proposed, on 26–27 May, to visit four Upper Devonian–Lower Carboniferous sections (Erdich and Noravank in the east of Armenia; Sevakavan and Khor Virap in the west). The approximate cost for the field trip is about US\$70.

For further details, please contact the organisers, Dr Araik Grigoryan (Institute of Geological Sciences, Armenia), Dr Michal Ginter (University of Warsaw, Poland), e-mail <fiszbit@uw.edu.pl>, or Vachik Hairapetian (Islamic Azad University, Iran), e-mail <vachik@khuisf.ac.ir> or <vh_hai@yahoo.com>.



The Seventh International Congress on Rudists
Austin, Texas 5 – 11 June 2005

The International Congress on Rudists calls together Cretaceous sedimentologists, palaeontologists, stratigraphers, and explorationists every three years to pursue research goals set forth in 1988 by the Cretaceous Resources, Events and Rhythms project of the Global Sedimentary Geology Programme. The 2005 Congress theme is "Cretaceous Rudists and Carbonate Platforms: Environmental Feedback." This theme will be developed in three sessions, "Depositional Environments of Cretaceous Carbonates," "Origins, Events, and Demise of Rudist Paleocommunities," and "Towards Rudist Taxonomy, Biogeography, and Phylogeny." Oral and poster sessions are planned.

Before the meeting participants will enjoy a one-day field trip to see the Texas Hill Country geology. Following the meeting a three-day excursion will give participants the opportunity to examine rudist-bearing outcrops and collect from classic middle Cretaceous carbonate buildups. The Texas Memorial Museum has developed a new exhibit of Cretaceous life, and the Museum will give access to its extensive and important collections of Cretaceous fossils for qualified specialists.

The University of Texas and the University of Tulsa will co-sponsor this seven-day conference and field trip. The John A. and Katherine G. Jackson School of Geosciences has made a very generous grant to the Congress, and the Department of Geosciences of Tulsa University is providing financial and logistical support.

To register and submit abstracts contact Debra Sue Trinque, Treasurer, 7th International Congress on Rudists, PO Box B, Austin TX 78713-8901, USA, or see the website at <<http://www.tmm.utexas.edu/npl/rudist2005/>>.




TAPHOS-05
Barcelona, Spain 16 – 18 June 2005

The Facultat de Geologia of the Universitat de Barcelona and the Museu de la Ciència (Fundació La Caixa) are pleased to announce the celebration of the 4ª Reunión de Tafonomía y Fossilización / 2nd International Meeting TAPHOS-05 that will take place in Barcelona between 16th and 18th June 2005. The Reunión de Tafonomía y Fossilización will be celebrated for its fourth time, after the success of previous meetings in Madrid (1990), Zaragoza (1996) and Valencia (2002).

The growing number of participants in previous meetings and their variety of countries of origin show the increasing interest in this science and its utility in very different fields. The aim is to provide a periodically updated vision of the state of knowledge on the topic, which is achieved in two parallel ways: invited lectures by outstanding researchers, and poster/oral contributions by the rest of participants. In this meeting it is planned to give a great weight to participants' contributions through the organization of topic sessions coordinated by a specialist in the topic. Young researchers working in or having finished their Ph.D. concerning taphonomic aspects


are particularly encouraged to participate. To register for circulars concerning this meeting send an e-mail to <rosa.domenech@ub.edu> with the message 'preinscription Taphos05' in the 'subject' field and including your name and complete postal address. Otherwise, further information on the meeting can be obtained by contacting the meeting secretary, Rosa Domènech, at <rosa.domenech@ub.edu>.



American Association of Petroleum Geologists Annual Convention
Calgary, Canada 19 – 22 June 2005

At this meeting, the North American Micropaleontology Section of SEPM (NAMS) will sponsor a poster session on the 'Integration of Micropaleontology and Petroleum Exploration.' This session falls within AAPG Meeting Theme 5: 'Depositional Systems in Time and Space.' The NAMS session will be co-chaired by Dave McNeil (Geological Survey of Canada, Calgary) and Pete McLaughlin (Delaware Geological Survey/University of Delaware, NAMS President), who extend an open invitation to micropalaeontologists to submit an Abstract for consideration and come to Calgary to attend this major international conference. Our AAPG 2005 session topic was chosen so that virtually any aspect of micropalaeontology within any hydrocarbon basin from around the world could be included. The deadline for Abstract submission is 12th November 2004. We encourage you to present your data and interpretations, which we feel are important to understanding 'Depositional Systems in Time and Space.' Please note that last-day traffic for submissions is heavy and can cause 'gridlock.' If at all possible, don't procrastinate—submit before the last day!

Visit the meeting website for further details, at <<http://www.aapg.org/calgary/index.cfm>>.



North American Paleontological Convention (NAPC 2005)
Dalhousie University, Halifax, Nova Scotia, Canada 19 – 26 June 2005

The meeting will include field trips to Horton Bluff (Dev/Carb boundary: early tetrapod trackways), Wassen's Bluff (Trias/Jur: link fossil between dinosaurs and mammals), Joggins (Carboniferous: world heritage site), and Arisaig (a world class Silurian invertebrate site). Major field trips will include the Gaspé Peninsula (Quebec). The local organizer is David B. Scott (Centre for Environmental and Marine Geology, Dalhousie University, Halifax, Nova Scotia B3H3J5 Canada). The meeting website is at <<http://meguma.earthsciences.dal.ca/napc/napc.htm>>.



Sixth International Crustacean Congress
University of Glasgow, Scotland, UK 18 – 22 July 2005


The conference is organised on behalf of the International Crustacean Council by The Institute of Biomedical and Life Sciences, University of Glasgow. The Meeting will also host the 5th

European Crustacean Conference, the 4th Crustacean Larval Conference, and the 2005 Summer Meeting of the Crustacean Society. For more details see <<http://www.gla.ac.uk/icc6>>.




The Fifth International Brachiopod Congress
Natural History Museum, Denmark; University of Copenhagen 4 – 8 July 2005

The Copenhagen conference follows the successful meetings in Brest (1985), Dunedin (1990), Sudbury (1995) and London (2000). The main events, lectures and posters will be held in the Geological Museum of the University of Copenhagen. The Museum has a strong tradition in palaeontological research particularly in the Arctic and Baltic regions. The congress is being organised by David Harper (Chair), Lars Holmer, Sarah Long, Claus Nielsen and Nina Topp. There will be a pre-congress field excursion to Gotland and two post-congress field excursions, one to Jutland and one to Estonia. For more details see <<http://www.nathimus.ku.dk/geomus/index.htm>> or e-mail David Harper at <dharper@savik.geomus.ku.dk>.



Fourth International Symposium on the Cambrian System
Nanjing, China 18 – 24 August 2005

More than thirteen years after the successful Third International Symposium on the Cambrian System in Novosibirsk, former Soviet Union (1990), the time has come to focus on a new target and to create a platform for all scientists working on the Cambrian to meet and calibrate their information. This meeting will accumulate not only the most influential colleagues but create the intellectual guidelines for the next decades. The symposium will focus on (i) meetings to discuss latest research findings relating to the System, especially in the global context, (ii) discussions and workshops of the IUGS Subcommittee of Cambrian Stratigraphy and related geosciences, and (iii) field trips to examine the best exposed Cambrian rocks in China and South Korea. Further details can be found on the symposium website at <<http://www.nigpas.ac.cn/cambrian-conference.htm>>.



Algorithmic Approaches to the Identification problem in Systematics
Natural History Museum, Cromwell Road, London 19 August 2005

Sponsors: The Systematics Association and The Natural History Museum, London.

The automated identification of biological objects (individuals) and/or groups (*e.g.*, species, guilds, characters) has been a dream among systematists for centuries. Despite much preliminary work in the 1950s and 60s, progress in designing and implementing practical systems for fully automated object identification has proven frustratingly slow. However, recent developments in computer architectures, and innovations in software design have finally placed the tools needed to make the development of generalized, automated, specimen and/or group-



identification systems a practical reality. In order to summarize the current state-of-the-art in automated group-recognition systems, and assess their potential to make practical contributions to systematics and taxonomy both now and into the future, the Systematics Association and The Natural History Museum, London have agreed to sponsor a free, one-day symposium, to be held in the Flett Theatre of The Natural History Museum, London on 19th August 2005.

The purpose of this symposium is to provide leaders of research groups, researchers, post-doctoral research assistants, and students working or studying in any area of systematics with an opportunity to (1) learn about current trends in quantitative approaches to the group-recognition problem, (2) become familiar with the capabilities of various software systems currently available for identifying systematic objects/groups and (3) evaluate various applications of this technology to present and future systematic problems. Special attention will be paid to showing how different approaches to automated identification can be applied to various organismal groups and in various applied research contexts (*e.g.*, biodiversity studies, biostratigraphy, conservation, agriculture, curation). Ample programme time will also be provided for discussions of issues relating to how these approaches and technologies can play a larger role in meeting the needs of current and future systematists. This free symposium is being held in association with the Biennial Meeting of The Systematics Association which begins on Monday 22nd August 2005 at the University of Cardiff. Attendees of the Systematics Association meeting are encouraged to include attendance at this symposium in their Biennial Meeting plans.

If you would like to attend this symposium, provide a demonstration or contribute a chapter to a book of collected technical articles, please send your contact details to Norman MacLeod, Palaeontology Department, The Natural History Museum, Cromwell Road, London SW7 5BD (tel: +44 (0)207 942-5204/5295, fax: +44 (0)207 942-5546, e-mail: <N.MacLeod@nhm.ac.uk>. A symposium website is also available at <http://www.nhm.ac.uk/hosted_sites/paleonet/aaips_symposium/>.



6th Baltic Stratigraphic Conference

St. Petersburg, Russia 22 – 26 August 2005

The Conference will be held at the A.P.Karpinsky All-Russian Geological Research Institute (Sredniy prospect 74) and St. Petersburg University (Universitetskaya nab. 7/9 and 16 Liniya 29). The scientific sessions and workshops are planned on 23–25 August. Pre-conference field trips (Lower Paleozoic and Carboniferous) will take place on 19–21 August, post-conference field trip (Devonian) on 26–28 August. In parallel with the scientific session, a business meeting of the IGCP 491 Project 'Middle Palaeozoic Vertebrate Biogeography, Palaeogeography' will be held. The main issue of the Conference will be every kind of problem relating to the sedimentary basin stratigraphy of Baltic and neighbouring regions. The number of sessions and topics of symposia could be specified according to the preferences of registered participants. Participants are invited to submit abstracts of both oral and poster presentations that will be published in a special issue. For further details please contact Andrey Zhuravlev, e-mail <stratigr@mail.wplus.net>, or Alexander Ivanov, e-mail <aoi@AI1205.spb.edu>.



IGCP 491 meeting: Middle Palaeozoic vertebrates of Laurussia; relationships with Siberia, Kazakhstan, Asia and Gondwana
St. Petersburg University, Russia 22 – 26 August 2005

In conjunction with the 6th Baltic Stratigraphical Conference.

The meeting will deal with any aspect of research on Middle Palaeozoic vertebrates (taxonomy, morphology, palaeogeography, palaeoecology, biostratigraphy), with a focus on the vertebrate fauna of Laurussia and its relationship to the faunas of other palaeogeographic provinces. Participants are invited to submit abstracts for both oral and poster presentations (details below). An abstract volume will be published as a Special Publication of *Ichthyolith Issues*.

The IGCP meeting is organised by the Department of Palaeontology, St. Petersburg University. For further details, contact Dr. Alexander Ivanov Department of Palaeontology, St. Petersburg University, e-mail <aoi@ai1205.spb.edu>.



7th International Symposium on the Cretaceous
Neuchâtel, Switzerland 5 – 9 September 2005

The meeting will be held in the University of Neuchâtel.

For more details see <<http://www.unine.ch/geologie/isc7/>>.



15th International Symposium on Ostracoda
Freie Universität Berlin 12 – 15 September 2005

The First Circular can be downloaded from the Symposium website. We will offer a number of field trips with various contents (stratigraphically and ecologically), each also including an extensive touristic and cultural programme. Please have a look at our website at regular intervals, at <<http://www.palaeo.de/iso15/>>. We are perpetually updating and extending these pages, in order to inform you about congress, programme, excursions, accommodation possibilities *etc.*, to be as complete and up-to-date as possible.



Seventh International Workshop on Agglutinated Foraminifera
University of Urbino, Italy 2 – 8 October 2005

This Workshop will be held at the Scientific Campus of the University of Urbino. For further information please contact Rodolfo Coccioni <cron@info-net.it>.



Paleontological Society Annual Short Courses at GSA 2005
Salt Lake City, USA 16 – 19 October 2005

Organiser: Bruce Lieberman.

Paleobiogeography: Generating New Insights into the Coevolution of the Earth and its Biota.

For more details see <<http://www.paleosoc.org/futureprograms.html>>.



Palaeobotany Specialist Group of the Linnean Society of London
Autumn Meeting 2005: New discoveries in old collections
Linnean Society, Burlington House, Piccadilly, London 26 October 2005

This one-day meeting will focus on the continuing nature of discovery in historically collected fossil plant collections. Potential speakers are asked to contact the meeting organiser, Dr Chris Cleal, directly, by e-mail to <chris.cleal@nmgw.ac.uk> (National Museums and Galleries of Wales).



9th Symposium on Mesozoic Terrestrial Ecosystems
Manchester, UK July 2006

The 9th Symposium on Mesozoic Terrestrial Ecosystems (sponsored by the Palaeontological Association) will take place at the University of Manchester in July 2006. The scientific programme will run over three days, with a short pre-conference field trip to Lower Cretaceous localities on the Isle of Wight, and a longer post-conference field trip to explore the Mesozoic succession of southern England. Further details will be posted later and in the next issue of the PalAss Newsletter. Preliminary enquiries can be made to <ucgasue@ucl.ac.uk>.



Palaeobotany Specialist Group of the Linnean Society of London,
Spring Meeting 2006: A life of ferns and seed ferns
Montpellier, France July 2006

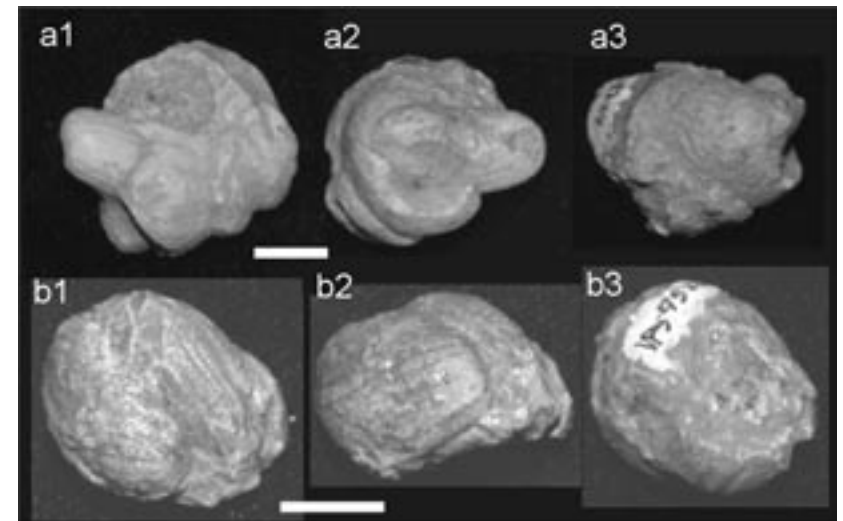
This is the initial announcement for a meeting to be held in Montpellier, the city where Jean Galtier has spent his academic life. Presentations will be on topics of special interest to Jean, specifically the early radiations of ferns and seed ferns. The meeting will (hopefully) be accompanied by an excursion to visit famous fossil plant localities in the south of France. Additional details will be made available shortly. Meeting organisers: Brigitte Meyer-Berthaud <meyerberthaud@cirad.fr> and Nick Rowe <nrowe@cirad.fr> (Montpellier, France).

Please help us to help you! Send announcements of forthcoming meetings to <newsletter@palass.org>.

MYSTERIOUS FOSSILS: Palass Newsletter mystery fossil 6

Mystery Fossil Numbers Two to Five remain mysteries, although Marc Philippe at the Université Claude Bernard Lyon 1 says he has seen an amazingly similar structure to Mystery Fossil Number Two in the basal Cretaceous of Byers Peninsula, Livingston Island, South Shetland Islands. He has a photo and will be happy to send a copy to anyone who is interested. Marc's e-mail is <philippe@univ-lyon1.fr>.

Mystery Fossil Six is actually two fossils with three views of each, collected by Xavier Panades I Blas from the Ludwig-Maximilians-Universität München and housed in the Museum Miquel Crusafont de Sabadell (Sabadell, Catalonia). Both specimens come from Eocene-aged continental lacustrine sediments in the Catalan Pyrenees and are formed of a white hard mineral. Fossil A (IPS 3738) was found at the Mas del Faro locality; fossil B (IPS9522) was found at the La Roca locality. The Miquel Crusafont de Sabadell Museum database has the fossils variously identified as snails, corals, claws, small mammal horns, coprolites, and unidentified. The photographs are courtesy of Drs J. Agusti and A. Galobart. Scale bars = 1 cm.



Please send insights to <c.little@earth.leeds.ac.uk>

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Book Reviews

Principles of Stratigraphy

Michael E. Brookfield (2003). Blackwell Publishing, Oxford, 340pp. ISBN 140511164X, £29.99 (paperback).

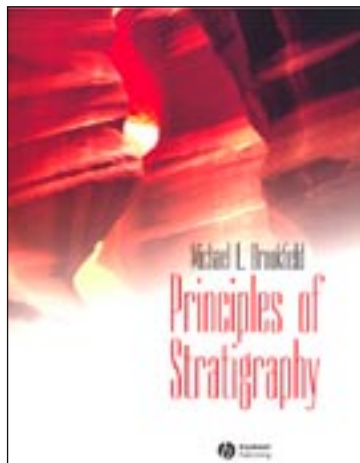
This book comprises 15 chapters divided into three sections of approximately equal length; part I 'Basics', part II 'Tracing environments in space and time', and part III 'Interpreting geologic history'. The book opens with a short introduction (chapter 1) covering the scope of stratigraphy and its historical perspective and origins.

Part I is a straightforward treatment of sedimentary rocks (chapters 2-4), competently describing the classification of rock types, weathering, sedimentary structures, fabrics and depositional settings.

Part II covers the main theme of the book, 'stratigraphic principles', although the emphasis is more sympathetic with 'environments through time' as acknowledged by the author in the preface. This part examines the vertical dimension (chapter 5), horizontal dimension (chapter 6) and temporal dimension (chapter 7). There then follows a chapter on basin analysis (chapter 8), (which includes the topic of sequence stratigraphy which is more fully dealt with in the next chapter). Chapter 8 might have been better included as the final chapter of Part I, focusing as it does on sedimentology. The concluding chapter 9 deals with cycle and sequence stratigraphy.

Part III concentrates more on the impact of tectonics and geophysical techniques on stratigraphy and interpreting geological history, which is a major driving force behind the book. Chapter 10 examines the role of tectonics on sedimentation, particularly the generation of sedimentary basins and their fill, and chapter 11 focuses on the effect of sea-level changes on sedimentation (transgressions, regressions and facies changes). Chapter 12 is a short study of the climatic effects on sediments and biota, as well as the controls on climate. Chapter 13 is an even smaller chapter (only five pages) on biology. This is a very brief nod to atmospheric evolution in the Precambrian, and biogeography (faunal provinciality in the Lower Palaeozoic Iapetus Ocean). Chapter 14 addresses some useful stratigraphic problems, and the book concludes with a short chapter (15) on extraterrestrial stratigraphy (featuring the topical sedimentology of Mars, and impact craters and eruptions on the Moon and Mercury, courtesy of NASA photographs).

It is questionable whether or not there is a market for this book, as despite its generalized title, it is in competition with many other sedimentology/stratigraphy text books, some from the same stable (e.g. Blatt *et al.* (1991), Reading (1996), Nichols (1999), and Leeder (1999), which cover most



of the material in chapters 2-4 (sedimentary rocks and depositional environments) of Part I, and chapter 8 (basin analysis) of Part II, and Chapter 10 (tectonics) and Chapter 11 (sea-level changes) of Part III, and to a much greater depth.

The visual aspect of the illustrations in the book is very mixed. The four colour plates in the middle of the book are very effective and informative, but several of the black and white field photographs are very dark and too small to be of much use. Moreover, many of them lack any scale and thus their impact is diminished.

It would have been very helpful to the reader to identify which geochronological time scale was used in Chapter 9 (Table 9.6) and in Appendix 3. Unfortunately, not all of the calibrated times for the base of the Periods are exactly the same in both tables: for example, the Cambrian is shown as both 545 and 544Ma, and the Silurian as 441 and 440Ma.

In summary, I think this is quite a useful addition to text-books on stratigraphy and is both a stimulating and an interesting read. It is primarily designed for undergraduate geology courses, with strong emphasis on the role of tectonics and geophysics on sedimentology, and ultimately, the rock record preserved for stratigraphical analysis and interpretation. The author sees one of the main aims of the book as to 'decodify' stratigraphy, *i.e.* to remove it from the bonds of terminology in which it appears to be entrapped. I think with his different perspective and emphasis he has made a noble effort. However, I do not think that many palaeontologists will be attracted to this book, because of the limited use of faunal/biological/palaeobiological examples. Furthermore, the author is not likely to endear himself to this group of geologists with his cavalier attitude in dispensing with most types of biozones and index fossils that he regards as either useless or redundant.

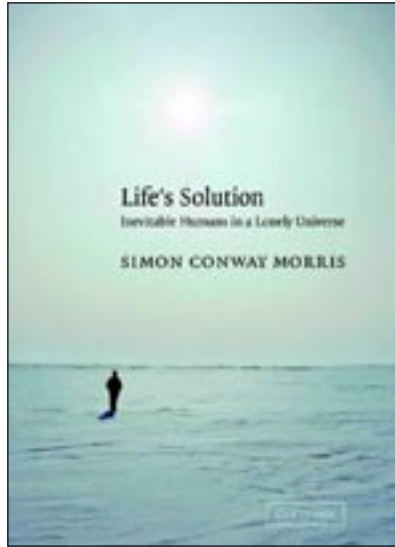
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- LEEDER, M. (1999). *Sedimentology and Sedimentary Basins: From turbulence to Tectonics*, Blackwell Science Ltd, Oxford.
- Ian D. Somerville**
Department of Geology, University College Dublin, Dublin, Ireland
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Life's Solution: Inevitable Humans in a Lonely Universe

Simon Conway Morris (2003). Cambridge University Press. ISBN 0-521-82704-3, £18.99, \$30.00 (hardback).

Within an appropriately numinous dust-cover (one of my colleagues remarked that it would not look out of place in Wesley Owen), Conway Morris presents a heuristic thesis: he asserts that the



apparent difficulty of generating life—based on the hitherto failure to replicate the process artificially—demonstrates that the origin of life is improbable (so it's a lonely universe) but given that life did evolve, humans, or more realistically sentience, were an unavoidable outcome (so we're inevitable humans). The latter statement contrasts with the late S.J. Gould's view that, if evolution were repeatedly replayed from the 'Cambrian explosion', each outcome would be substantially different and concomitantly the evolution of humans would not be guaranteed. In *Life's Solution*, Conway Morris argues why Gould's appeal to the dominance of contingency in evolution does not represent a true picture.

The first chapter of the book discusses the quintessence of life, DNA, and its striking

chemical stability and efficiency as a replicating information store. Conway Morris describes its origin as a 'one in a million' chance: although as it probably took many millions of years to develop, this probability seems somewhat unremarkable. Still, Conway Morris marvels at the ability of life to "home in" on adaptive solutions within the vast expanse of potential alternatives. He likens all adaptive solutions (further dealt with in chapter two), including the development of the DNA code, to the improbable navigation of the Polynesians to that remotest speck of *terra firma*, Easter Island. To carry this analogy further, I would note that Polynesians used not only astral navigation, but also scanned the monotonous horizon for seabirds as indicators of proximal land. Day by day, they would have steered towards the general direction in which the density of birds was highest and be virtually assured of landfall. Any deviation, and they would become lost in the watery wastes of the Pacific. One can envisage a similar process (albeit a blind one) operating as organisms navigate generation by generation with seeming precision through, for example, potential morphospace, towards the most adaptive solution, with selection culling those straying into the wilderness of maladaptation. Consequently, no astonishment need be engendered by the repeated navigation toward any particular morphotype.

Chapters three and four deal with the origin of life and its improbability. Despite the appropriate raw materials being present on primordial Earth, these still required abiotic assembly, and therein lies a perceived problem. Conway Morris briefly mentions one of the greatest difficulties: that the exact chemistry of Earth during this deepest of time is unknown and will be forever unknowable. Since only the generalities are constrained, and until we stumble across the precise mix within these broad boundaries, our Frankensteinian efforts will continue to be thwarted. As Conway Morris argues, it is because such precise requirements apparently need to be met before his "universal goo" will come alive, that the emergence of life is so unlikely. However, I think it likely that the primeval chemistry of Earth would have been in flux

for millions of years, providing a variety of conditions and ample time for a process of chemical trial and error to operate and produce successfully replicating molecules.

The apparent oddity of our solar system is discussed in chapter five: its structure and resultant suitability for life. Numerous other planetary systems have been identified (one hundred and thirty last time I checked), yet Conway Morris notes that there is a general feeling that all differ, some markedly, from our own. Based on this, he suggests that our system may be uniquely predisposed to the development of life. I think this conclusion is premature: although current telescopic resolution is insufficient to detect small rocky planets similar to the one we inhabit, this will change soon. We might then be in a better position to judge the clemency towards life in other systems.

In chapters six to ten Conway Morris reels off example after example of convergence to support his argument for the repeated and inevitable development of the same "biological properties," the same adaptive solutions, across a range of organisms. All the examples are interesting for anyone with a fascination for natural history and evolutionary biology (including myself), but although I appreciate the point he is trying to make, after a while (I think it was around example one hundred and sixty-four) I felt that this point had been sufficiently made. And that was before I discovered the dedicated convergence index. It at least allows easy location of your favourite example.

With chapter twelve forming a facetious coda, chapter eleven finishes the core of the book, considering some of the connections between religion and science; specifically those on which the topic of the book has bearing. This is the only section where the science plays second fiddle to more metaphysical considerations. The aim of the chapter seems to be to encourage a greater fusion of religious belief and scientific thinking, with Conway Morris arguing against the reductionist assertions of the ultra-Darwinists, incorporating some implicit anthropic thinking and hinting that humans were perhaps destined to appear. These are subjective and contentious issues, so make of this last chapter what you will.

Throughout the book, Conway Morris's writing is lucid; although it is liberally scattered with technical terms, most of these are explained. *Life's Solution* is not about palaeontology *per se* (although trilobites and spinosaurs get a brief mention) but everything within it is very pertinent to the palaeontologist. In terms of wider appeal as popular science, it is very much directed at the educated reader with some prior knowledge of evolutionary theory. One hundred and twelve pages of notes (to my shame I have not followed up on all the references) provide ample referrals to further reading on the topics discussed. As a point and counterpoint in an important debate, *Wonderful Life* and *Life's Solution* are worth reading and considering together.

So, are we inevitable humans? I would say that intelligence, and probably sentience, seems equally likely to evolve under the inexorable drive of selection as any other adaptation, be it sabre teeth or eusociality, given time and apposite conditions. However, contingency surely plays an important role in determining which organisms acquire a given adaptation. But do we inhabit a lonely universe? We've been attempting for less than a century to get those amino acids to club together and start replicating, yet it probably took millions of years first time round—I think we should give it time. And in the interim, we may still happen upon an obelisk to settle the issue.

EXTRASOLAR PLANETS ENCYCLOPAEDIA. 1995. *Extrasolar Planets Catalogue*
<<http://www.obspm.fr/encycl/cat1.html>>.

GOULD, S. J. 1989. *Wonderful Life: The Burgess Shale and the Nature of History*. Hutchinson
Radius, London.

David Jones

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The Great Ordovician Biodiversification Event

Barry D. Webby, Florentin Paris, Mary L. Droser and Ian G. Percival. (editors)
(2004). Columbia University Press, New York. ISBN 0231-12678-6, \$99-50
(cloth)

Over the past 30 years considerable effort has been put into understanding the mechanisms and evolutionary importance of major faunal turnover; scenarios have been developed but too few hypotheses tested. This book, the results of IGCP Project 410 of the same title, brings into focus how little we really understand about the evolutionary processes of faunal turnover, in this case radiations, adaptive or otherwise. The stated aim of the project was to provide a global and quantified evaluation of the Great Ordovician Biodiversification Event, arguably the second most important event in Earth history, after the advent of mineralised skeletons. To achieve this, the project included a number of sub-goals:

- 1) The development of a global and integrated time scale using conodont, graptolites and other zonal fossils, bio-events and graphical correlation methods. This was largely achieved by using CONOP9 software.
- 2) To analyse onshore to offshore biofacies profiles across palaeolatitudes; achieved only in a few clades.
- 3) To identify biotic responses to climate change, not achieved, largely because clade diversity data were not mapped into a framework of global change.
- 4) To identify extrinsic factors (e.g. plate tectonics, volcanism) which favoured biodiversification; not achieved.
- 5) To compare organic matter assemblages from economically important oil shales; not attempted?

These were ambitious tasks indeed, particularly within the structure of international collaborative teams. It is a credit to the project leaders that the impressive amount of data collected has finally emerged for public scrutiny. In essence this book provides the opportunity to judge whether the project objectives were fulfilled and summarise our current understanding of radiations.

The project was clearly an administrative success, gaining an additional non-funded year, bringing together groups of scientists from a wide range of backgrounds and increasing our knowledge of Ordovician faunas. However, in an age of hypothesis-driven science and limited

research funds, did the project answer some of the fundamental questions raised by the Ordovician radiation, not least why and how did it occur, and can any lessons be learnt from the Ordovician that will help us understand other radiation events? As readers of this book, you will undoubtedly be seeking the answers to these questions, as I was.

Seven regional teams and clade teams were formed to collate, analyse and interpret the primary data, taxonomic diversity change through time. The desire was to compile all relevant data on a regional-global web-based database, in effect a (Sepkoski Jr, 2002) by Ordovician specialists. Unfortunately a single database was not adopted across the project by all workers and two databases have emerged compiled by Arnold Miller and Alan Owen; each has a different focus and to date neither is web-based nor accessible to the wider scientific community. I guess hard-won raw data are too valuable for general release. This book provides the only widely available datasets from the project (see below). Declared scientific highlights of the project include a 19 time-slice subdivision of the Ordovician System tied to conodont, graptolite and chitinozoan biozones and calibrated by radiometric dates, progress on refining the global eustatic sea level curve (a new curve is presented for Baltica), development of global-regional databases for some groups and an assessment of the regional and global rate of diversity changes through time. Over 1,000 publications are attributed to the project, culminating and partly synthesized in the book under review.

First, we turn to the nitty gritty and the structure of the book. The book is divided into four sections. Section I comprises principles and methodology, including establishment and calibration of the time scale and a novel method for the measurement of diversity. Section II sets the Ordovician scene in a series of short review essays on various aspects of the Ordovician, palaeogeography; stable and radiogenic isotope patterns, an aspect of Ordovician science still in its infancy and plagued by geological over-prints; oceans and climate; Hirnantian glaciation and sea levels. A separate chapter is devoted to existence of a superplume event in the mid-Ordovician, which, if corroborated, would have implications for unifying many aspects of Ordovician earth systems and biodiversity. Most of these essays will be of use to those of you teaching Earth history modules and provide a reasonable set of illustrations and references. They do not however provide insight into a chronology of global events that may have moulded biodiversity.

Section III and the greater portion of this book (23 chapters) is devoted to documenting taxonomic richness at regional and global scales for groups as wide ranging as 'tube-shaped things' to 'plants' and vertebrates. I will not bore you with the details of each of these chapters, you will no doubt read the chapter on your pet group for yourself; suffice to say that each is stand-alone, datasets are comprehensive as far as the fossil record of each group allows, but data collection methods and analysis are not uniform across the chapters (some include species and genera counts, some only genera). The coverage is reasonably comprehensive with the exception of notable gaps in Middle and Upper Ordovician conodonts and foraminifera, and a few minor groups including the hyolithelminthes and hydroids. Unfortunately, inconsistency of approach is noticeable and leads to problems in trying to compare the diversity trajectories of each group; indeed no summary figure showing the diversity plots for all clades against timescale is included. Three chapters, bryozoa, brachiopods and trilobites, utilise cladistic

classifications; the rest do not. Some chapters include attempts to correct for sampling biases; the majority do not. Some chapters ignore the 19 time-slice framework, presumably because of poor fossil record or poor biostratigraphical resolution; why include these poorly known groups, what do they add to the understanding of the radiation? Few chapters include separate diversity patterns for different biofacies, provinces and/or palaeocontinents. Universally none of the chapters provides access to the original database beyond the inclusion of time–genera range charts (conodonts as an exception), or tables of diversity metrics (graptolites as an exception)—an omission I consider woeful when supplementary data are easily hosted on websites. Indeed from a geochemical standpoint, if original data cannot be accessed, checked and interpretations tested then papers are not published, a lesson that could be well learned in future IGCP projects. These differences of approach and an inability easily to synthesize diversity patterns across the groups means it is difficult to go beyond a very subjective and generalised discussion of patterns; quantitative testing of hypotheses of processes is impossible. The final section includes a chapter on trace fossils and a synthesis, with a view to the future.

The chapters on bryozoa, closely followed by the trilobites, brachiopods and graptolites, together provide a template of good practice. These are based on cladistic classification, have quantified assessments (by gap analysis) of completeness, provide generic and/or species patterns based on palaeogeography and/or biofacies and an evaluation of extrinsic (*e.g.* relative sea level, climate changes and oceanographic changes) and intrinsic factors (*e.g.* key evolutionary innovations) affecting the radiation of the clade. Depth of coverage of course reflects the maturity of study of the clades, and particularly those with biostratigraphical utility. The remaining compilations effectively include an analysis of non-monophyletic groups, which if you take the stand that only monophyletic groups are real identities then the diversity plots are meaningless. I do not take this view, preferring to think of diversity as the changing number of species through time, not an ordered sequence of branching events. If higher taxa can be taken as a proxy for a given number of species (many argue not) and these taxa reflect sets of related niches in an ecosystem, then all that matters is that species are sufficiently similar to one another to be reasonably thought of as ecologically related; it is irrelevant whether these taxa are monophyletic. But it is difficult to ignore the existence of recent papers on the cladistical classification of echinoderms, corals, conodonts and molluscs as a starting point for global syntheses of diversity and palaeogeography.

At the outset the study of radiations, and mass extinctions for that matter, is dogged by the fundamental problem: faunal turnovers measured by origination and extinction are a species-scale phenomenon. Interrogating the fossil record at this level is an intractable problem, clouded by the vagaries of taxonomy and preservation potential. (Hoffman, 1989) has argued that the probabilities and therefore the rates of extinction and origination varied randomly through time and the overall pattern of changing global diversity amounted to the summation of a myriad number of individual and largely independent individual to species level events—a double random walk of speciation and extinction. This view is not sustained by the results from Project 410; there are clear, parallel, multi-group trends. There is also little doubt that the rise in the number of species in the marine environment in the late Cambrian to late Ordovician is real and obeys a simple logistic relationship. That is, the rate of increase fell as diversity increased, reaching a plateau at ~25% present day diversity.

If time is short, and you can only read the chapters on clades with good fossil records, you will notice two fundamentally different diversity patterns:

- 1) Bryozoa, articulate brachiopods, echinoderms, corals, gastropods, bivalves, chitinozoa, ostracods and conodonts show a sustained increase in diversity until the late Caradoc or early Ashgill, independent of the timing of initiation. This was followed by a general decline through the later Ashgill and a varying impact of the end Ordovician mass extinction.
- 2) The planktonic groups exemplified by the acritarchs and graptolites reached peak diversity in the Arenig followed by a steady decline through the rest of the Ordovician.



Either the underlying mechanisms for radiation were different for plankton and benthos, or these groups responded differently to the same environmental cues. The fossil record of the terrestrial biosphere (trace fossils and spores) is too poor to deduce patterns beyond a gradual emergence through the Ordovician. Putative millipede tracks and the appearance of trilete spores in the Upper Ordovician suggest the existence of an incipient, low diversity, low complexity terrestrial ecosystem.

Multi-group parallels lend support to extrinsic factors being the cause of the radiation, for example changes in relative sea level, ocean chemistry and climate. These factors are all complexly inter-related and no single cause for the radiation should be sought. There is however no shortage of ecological explanations across the chapters, including habitat partitioning, trophic partitioning, increase in the number of guilds and escalation (probably best replaced by the more complex notions of multiple niche evolution). A correlation of productivity (*syn. orogeny*, (Connolly and Miller, 2002)) with diversity is evident only in the brachiopods. Intrinsic causes, such as the appearance of key evolutionary innovations, cannot be discounted but are difficult to test, requiring correlation of high resolution palaeo-environmental and diversity data, and certainly not with the data collected in this project.

Other published patterns and scenarios for diversification are present in some of the Ordovician data. The pattern of higher alpha diversity in nearshore environments and at low latitudes (Sepkoski Jr, 1991) is corroborated only by the trilobites and brachiopods which both show onshore originations followed by rapid offshore expansion. However, echinoderms show the reverse trend and bivalved molluscs originated in Gondwana and then expanded into low latitudes, first colonising the outer shelves of low latitude palaeocontinents. Plate tectonic partitioning of the marine ecosystem and the importance of island faunas as cradles of origination and refugia (hinted at by the brachiopods) are largely ignored.

IGCP 410 tried to discover non-random patterns in overall taxonomic turnover, to find common mechanisms that are statistically significant and can be used as explanations of evolutionary processes. The clear message from this book is that in groups with good fossil records, with resolved alpha taxonomy and deterministic palaeoecology, first order, non-random patterns can be demonstrated. As to discovering explanations, the how and why, and evolutionary mechanisms for radiations, this book does not provide the answers and there is much work still to be done. A synthesis of the patterns published in this book does hint at possible mechanisms. The Ordovician Radiation or, I think more correctly, the Ordovician Biodiversification, is characterised by the emergence of a more complex and more highly integrated marine ecosystem which extended from shelf to slope, and included ecologically diverse oceanic groups. At this point it is easy to become speculative and *ad hoc*, so I will.

Diversity peaks in planktonic groups may have led to radiation in the benthic fauna which started to include many new active suspension feeders with the ability to generate their own water currents. Changes in substrates led to an expansion in hard ground and reef communities. Simultaneously there was a rise in scavenging and predatory metazoans. This is a familiar scenario, also widely postulated for the Cambrian explosion. An additional feature of Ordovician shelf environments was the increase in the number of niches through time, the presumed result of trophic partitioning and other intrinsic factors operating at the individual and community level; these appear to have sustained the diversification. The jury is still out on the initial environmental and/or biological cues for the diversification. I would therefore suggest the study of radiations and mass extinctions have one thing in common; scenarios have been developed but too few hypotheses tested.

So should you buy this book? Would you buy Sepkoski (2002)? Ordovician workers will want this book on their shelves but, my feeling is this is a useful addition to your institutional library, you will use the information in teaching and I can see the scope for developing some interesting practicals on the nature of radiations. The compiled data are not accessible for research and this will of course retard progress on understanding radiations and in the follow up project IGCP 503, Ordovician palaeogeography and palaeoclimate.

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High-Resolution Approaches in Stratigraphic Paleontology

Peter J. Harries (editor) (2003). Kluwer Academic Publications, Dordrecht. ISBN 1402014430, £110 / \$175/ €159 (Hardback) (CD-Rom included).

This book (volume 21) is one of a series in 'Topics in Geobiology', and comprises 13 chapters involving 21 authors. As the editor remarks in the preface, one of its main aims is to 'offer a broad perspective on both theoretical and practical issues related to high-resolution paleontologic studies', and for the most part, this book achieves its objective. It also sets out to highlight the increasing need for integrating a variety of approaches while at the same time realizing the limitations of palaeontological databases.

This volume includes twelve chapters, excluding the last chapter, which features details of the CD-Rom, used for the studies in chapters 2 and 3. However, in the contents list, confusion is caused by two chapters being given the same number 11. Most of the chapters are case studies involving fossil data collected as part of field-based projects. As all but one of the authors are based in the USA, the studies are focused entirely on North America and include one study each on Cambrian, Ordovician and Devonian faunas and seven on Cretaceous faunas. The other two chapters which begin the book are more theoretical case studies, including the opening chapter by Kowalewski and Bambach which is a critical assessment of the limits of palaeontological resolution, followed by a study involving biostratigraphy and computer-assisted correlations (Sadler and Cooper) which uses, in part, a Cambrian trilobite database.

Lower Cambrian trilobites from southern California are the main focus of the chapter by Webster *et al.* that combines data collected by the authors from measured sections with material in museum collections. This study successfully increased the biostratigraphic resolution and added to the species database. One of the main reasons behind this achievement was the use of computer algorithms which are objective and reproducible.

The trace fossil *Zoophycos* is the basis of a study by Savrda who was able to recognize that marine depositional processes and events were recorded by *Zoophycos* spreiten, unlike most trace fossil activity that results in destruction by intense bioturbation.

The next two chapters feature ammonites as the main fossil group. Landman *et al.* studied scaphitid ammonites from the Upper Cretaceous of North and South Dakota. This involved a detailed statistical analysis of the variation in size range and class of adult species through four assemblage zones. Their results show a geographic pattern of change that appears to be ecophenotypic, but stasis was the dominant feature during the evolutionary history of species examined. The paper by Yacobucci on Upper Cretaceous acanthoceratid ammonites on the other hand involved a much greater study area: the Western Interior Seaway (WIS). Statistical analysis of the variation in thickness ratio (width versus shell diameter) of eight species of one ammonite genus showed that neither facies nor regional differences were involved. However these factors may well have affected the shell shape.

Inoceramid bivalves from the WIS are used by Harries to test the relationship between sea level in the Cretaceous and species richness. Trends of species richness, origination and extinction were analysed utilising well-documented sea-level curves and temporal resolution provided

by ammonite biostratigraphic schemes. Cross plots for the five major cyclothems showed that species richness is not solely determined by sea level at any given time, but a function of and response to ecologic, environmental and evolutionary factors that fluctuate through time.

Diversity patterns in non-marine Cretaceous vertebrates from the Western Interior Basin of North America are the basis of the study by Eaton and Kirkland. This is the first attempt at a meaningful compilation of data on sharks, fish, amphibians, reptiles and mammals from this period. It contains an extensive appendix (25 pages) for all occurrences of terrestrial vertebrates at order, family and genus level for the interval from the Barremian to Maastrichtian. This is very much a case of work in progress, but some tentative trends are suggested, with a notable increase in diversity through the Cretaceous and marked peaks in generic level at the end of the Albian and Campanian.

The study by Brett *et al.* on event beds and sedimentary cycles in Upper Ordovician rocks of Kentucky and Ohio best illustrates the importance of measuring sections at the centimetre scale. They were able to demonstrate that using high-resolution stratigraphy it was possible to correlate individual beds characterised by high concentrations of rare fossils, or ubiquitous fossil taxa marked by unusual morphologies. Even trace fossils and storm event beds could be correlated over tens of kilometres. This chapter contains many photographs of the important taxa and sedimentary features, but the vital cross-sections (figs. 13 and 14) summarizing the data lack a key to explain the symbols used as correlative markers.

The following chapter by Morrow and Sandberg on Upper Devonian conodonts from Utah and Nevada represents the integration of over 30 years of conodont biostratigraphic data with newer information on transgressive-regressive cycles and sequence stratigraphy. This chapter (by far the longest chapter in the book) consists of a detailed description and documentation of nine measured sections that contain summaries of important events. Focus is given particularly to the interpretation of the Frasnian-Famnenian boundary and event stratigraphy, set against a sound biostratigraphic framework provided by the conodonts. This chapter benefits from the presence of informative logs of sections, and outcrop photographs and photomicrographs.

A short chapter follows by Carpenter who has analysed the distribution of Cretaceous vertebrates from western Kansas. He documents the presence of Upper Cretaceous fish, reptiles and birds through seven biostratigraphic zones, defined using a combination of invertebrate macrofossils and vertebrates.

The final chapter is an unusual study by Kauffman of fossils recovered from limestone concretions from Cretaceous rocks of the Western Interior Basin. He has been able to recognize—from



collecting concretions for over 40 years—34 palaeocommunities, comprising mostly molluscs ranging from freshwater to fully marine. What is also interesting and novel about this study is that by using refined radiometric dates recorded from bentonites interbedded with the strata containing the concretions, that the latter are considered to represent almost isochronous surfaces. This is the only chapter in the book that has colour photographs of outcrops, but unfortunately, their benefit is somewhat reduced by a lack of scale in all eight pictures.

In summary, I think this is a very useful book on stratigraphic palaeontology that should appeal to many palaeontologists because of the use of the different techniques employed and the wide variety of fossil groups analysed. It certainly is a must for those who still value the prime importance of gathering fossil data by detailed field work, measuring and sampling sections, and making critical observations. The book also highlights the increasing need for palaeontologists to collaborate with other specialists, or at least integrate data from other disciplines in developing new palaeobiological approaches used for investigating environmental, ecological and evolutionary changes and patterns. Perhaps the only disappointing aspect of the book is the price. It is not by any means cheap and is probably beyond most individuals' budget. However, do ensure that your library acquires a copy.

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450 Millionen Jahre Beständigkeit in der Evolution endolithischer Mikroorganismen.

(Sitzungsberichte der wissenschaftlichen Gesellschaft an der Johann Wolfgang Goethe-Universität Frankfurt am Main, v. XLII, nr. 1: 3-42; 19 figs.)

Klaus Vogel and Ingrid Glaub (2004). Franz Steiner Verlag, Stuttgart. ISBN 3-515-08461-4, €22 (Softbound).

Klaus Vogel, retired Professor of Paleontology at Frankfurt University, in Germany, launched a programme 16 years ago to study *microendoliths*. As the term may not be familiar to everybody, a few introductory remarks are warranted.

Microendoliths are borings in carbonate substrates, mostly shells. They should not be confused with the borings that can be studied with a pocket lens: the chains of chambers made by the sponge *Cliona*; the amphora-like drillings of pholad bivalves; the ones of acrothoracican barnacles shaped like an apple seed; the spreite burrows of the polychaete worm *Polydora*; or the filigree patterns of endolithic bryozoans. Microendoliths are microscopic, and a number of special techniques were prerequisite to study them.

- (1) *Casting* with resins that are liquid enough to fill the minute cavities and catalyse into a rigid material enduring acid dissolution of the calcareous substrate as well as preparation for SEM studies.
- (2) The *scanning electron microscope* (SEM) allowing study and photography of the etched-out structures at the scale of microns.

- (3) *Experimental* methods to study modern forms by the same method. Clear crystals of sparry calcite are suspended in natural environments at different water depths over controlled periods of time.
- (4) Elaborate *morphological analysis* to distinguish highly variable bubbly or filamentous shapes that are controlled by general rules of growth rather than by rigid morphogenetic programmes.

Classification is another problem. By definition, these structures fall in the category of trace fossils. This means that they are labelled by Latin names different from those of the trace makers. Yet, there are important differences from traces produced in soft sediments. (1) While the latter reflect mechanical activities directed by behavioural programmes, microendoliths result from growth programmes. In this respect, they are more closely related to shells; only that growth steps correspond to overall dissolution, rather than localized accretion, of calcium carbonate. (2) As the morphological resolution of these trace fossils is also much higher than those in soft sediments, their external morphology corresponds directly to that of the trace maker. So the ichnological classification can, in principle, be concordant with the palaeozoological (or palaeobotanical) ones. As most taxa still exist, there is not even a discordance with the fossil record. (3) There is only one disadvantage compared to biogenic structures in soft sediments: microendoliths can be reworked from older layers, or transported into adjacent environments, together with their shelly substrates.

There is also an important general implication: the majority of the trace makers rely on photosynthesis. Thus, microborings provide potentially the most reliable proxy for palaeo-bathymetry, as long as those made by light-independent fungi and foraminifers can be singled-out. As shown by the authors, microendoliths made by red algae, green algae, and cyanobacteria extend to different water depths and can be used to subdivide the photic zone further, with the metric calibration changing with the luminosity (latitude) and clarity of the water column. This scheme was established in modern environments (Norway; Scotland; Mauretania; Bahamas; Great Barrier Reef). It has been successfully applied to ancient reef settings and basins of Middle Devonian, Permian, Middle Triassic, Upper Jurassic, and Upper Cretaceous age.

The present booklet by Klaus Vogel and his co-worker, Ingrid Glaub, is based on a public lecture, so it is not the comprehensive reference one might have hoped for. Instead, it carries a quite unexpected message: bacteria as the most ancient living fossils! Of course, one could argue that in these organisms speciation takes place at the level of their physiology rather than the poorly defined body morphology. But for practical purposes, some microendoliths found in Cambrian



shells (or in the Precambrian, emplaced into oolite grains) are identical to ones living today. As it appears, geologic life spans were orders of magnitude higher than the few million years observed in animal (or plant) species.

A few interesting questions remain open. For instance, one would like to know to what extent these borers are host-specific. Do they prefer certain taxa or, more likely, certain kinds of shell mineralogy and microstructure? Also, is their yield high enough to serve as food for larger shell borers and shell-rasping gastropods or echinoids?

In any case, this is a piece of palaeontological literature that is well illustrated (including colour plates), very enjoyable to read (provided you know some German), and well worth its price!

Adolf Seilacher

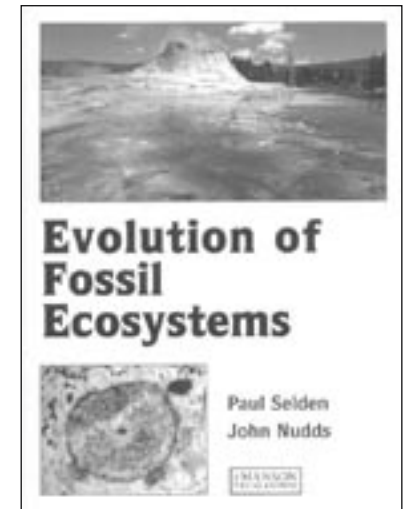
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Evolution of fossil ecosystems

Paul Selden and John Nudds (2004). Manson Publishing Limited, London
ISBN: 1-84076-040-0, £39.95 (hardback);
ISBN: 1-84076-041-9, £19.95 (paperback).

Despite the title, this book is first and foremost an introduction to the palaeobiology of 15 exceptional faunas. The Ediacara, Burgess Shale, Soom Shale, Hunsrück Slate, Rhynie Chert, Mazon Creek, Grès à Voltzia and Holzmaden Shale biotas, the fauna from the Morrison Formation, the Solnhofen Limestone biota, the biotas from the Santana and Crato Formations, the Grube Messel biota, the fauna and flora encased in Baltic Amber and, finally, the fauna recovered from the Rancho La Brea tarpits are each reviewed in a similar manner: there are successive sections describing the 'Background,' 'History of discovery' and 'Stratigraphic setting and taphonomy' of the biota, a 'Description' of selected taxa, a consideration of the biota's 'Palaeoecology,' and to conclude, a 'Comparison' of it with others of similar age. The book itself concludes with an Appendix providing details on museums with collections of material from each biota and information on access to specific sites associated with each biota.

The authors explain the reasoning behind their choice of title in the preface: these faunas "scattered throughout the geological record ... can provide a fairly complete picture of the evolution of ecosystems through time." Of course each fauna provides an invaluable insight



into the structure of a specific ecosystem (although not, by any means, a complete one, a point that could, perhaps, have been emphasised more). Further, by providing, in the introductory Background section of each chapter, a summary of the major evolutionary innovations in the interval preceding each fauna some temporal dimension is added; more usefully, it places each individual biota in a wider geological context. Nevertheless, these faunas are too scattered—both in time and in the variety of different environments represented—to provide by themselves a continuous narrative of how ecosystems have evolved. Joining up the dots or ‘windows’ (to use the authors’ phrase) provided by these and similar faunas involves the varied approaches and efforts of a much more diverse suite of biologists and palaeobiologists.

Although there are summaries of the entombment history and diagenesis of each individual biota, this text is not intended as a study of taphonomic processes. Thus, we have phrases such as “For a variety of reasons Plattenkalks often display exquisite preservation of soft tissues.” The brevity of the introduction illustrates this—two pages, of which most of the second summarises the contents of, and draws links among, the succeeding chapters. The first page is used to distinguish between Konzentrat-, and Konservat-Lagerstätten; taphonomic processes (biostratigraphy and diagenesis) are reviewed in a paragraph. Compounding this, the stratigraphic setting and taphonomy section for each biota emphasises the environmental and depositional conditions that favoured exceptional preservation (and is consistently reviewed well). The role of later, diagenetic, processes, including microbial activities, is, however, usually considered in less detail; there are, for example, no thin section or SEM photographs of, for example, the tissues preserved, or the minerals responsible. The only image in this vein is of musculature preserved within a Santana fish (image 201).

What about the choice of biotas? The inclusion of the biotas from the Morrison Formation and Rancho La Brea tar pits is an important contrast to the majority, the Konservat-Lagerstätten. There is little to quibble about with the selection of the latter; by and large, it is the ‘usual suspects’ that one would include in a book of this nature. Perhaps some mention of ‘Orsten-type’ Cambrian faunas as a source of information complementing that provided by Burgess Shale-type deposits, a chapter, or at least more detail, on the Chengjiang biota, and a chapter on the La Voulte-sur-Rhône biota (the fidelity of preservation of which is unparalleled in the fossil record) could have been included.

There can now be few exceptional faunas that have not had their geology, palaeontological content and taphonomy summarised for a wider audience. In fact most of the 14 faunas included here have been the subject of individual texts, and alternatively, or in addition, been reviewed in texts such as *Palaeobiology II* (edited by D.E.G. Briggs and P.R. Crowther, 2000) and *Exceptional Fossil Preservation* (edited by David J. Bottjer *et al.* 2003).

The obvious question is therefore: Is another book on exceptional faunas needed? The stimulus for this book was an undergraduate course taught by the two authors, and their recent collaboration on a new fossil gallery at Manchester University Museum. This is reflected in its scope, structure and target audience. And, yes, for the intended audience—the general reader, or as a text to complement an introductory undergraduate course—there is great mileage in this book. It’s well written—engaging, easily read, with a very nice balance between general content (history of research) and specific detail (*e.g.* of the faunal elements). It includes introductions

to individuals past and present who have been involved in the research on specific faunas and there are some great stories—exactly the sort of comment that adds a bit extra to any talk or lecture. Thus, for example, in an attempt to conceal locality information the discoverers of the Como Bluff locality within the Morrison Formation (William Edward Carlin and William Harlow Reed) identified themselves to Professor O.C. Marsh of Yale in their offer to sell him fossils under the pseudonyms Harlow and Edwards. All was going well until the cheque payable to Harlow and Edwards arrived, and couldn’t be cashed, ‘forcing them’ to come out of the woodwork!!

Unusually, and although the chapters are numbered individually, the images are numbered sequentially throughout the book (the format would lend itself to production of a set of 35mm slides or digital images to complement the hard copy, a move that would enhance its usefulness as a textbook). Image quality is consistently excellent, particularly the numerous photographs of actual specimens: check out the images of the suite of specimens from the Crato Formation on page 115, or the reproduction of the radiograph of the starfish *Helianthaster rhenanus* from the Hunsrück Slate.

The selection of photographs reflects the wider geological, scenic and cultural aspects of many of the biotas—a nice touch. This includes images of relevant field sites and the more general landscape, as well as Recent analogues for some of the biotas (*e.g.* images of Yellowstone National Park in the chapter on the Rhynie Chert) and fossils (*e.g.* images of the plant *Psilotum nudum*, to which Rhynie Chert plants are compared). Field photographs include local collectors with their ‘catch’ of Santana fish (image 198), slabs of Crato Formation limestone (200) stacked up like, as I heard David Martill once describe them, album or CD covers in racks that you thumb through. There are even images of cave art (40).

Different scales are used in the images of the fossils; for example, reference can be made to the length of the animal (*e.g.* 226, total length 750mm including tail which is 450mm), part thereof (300mm long carapace of turtle), or the height of the image or fossil slab (*e.g.* 144). Very occasionally no scale is present (*e.g.* 201) or the scale bar included in the image conflicts with the value stated in the figure caption (*e.g.* 213). Many of the superb pen and ink reconstructions lack scales—not a problem when an actual specimen is also illustrated, but this is not always the case.

Line drawings of locality maps and stratigraphic sections are in colour. Some of these are just the wrong side of ‘simplified,’ *i.e.* they are at the point where useful data has been omitted; they have the feel of a visually attractive, ‘clean,’ image designed to be projected with some further comments or explanation to be provided orally. More labelling on the image or an extended figure caption would have been useful in some of them. A single example, the locality map and cross section of Grube Messel (image 219), illustrates the sort of quibble I have in mind. The only scale on the maps is on the inset, not the more detailed map of the area around Grube Messel. The Messel Formation is shown in both green and salmon pink. In some—but not all—cases, confusion can be resolved or deduced from the text; although it’s not explicitly stated in the text, the colour difference in this case presumably separates the basal sands and gravels (green, based on the vertical sequence in the cross-section) from the succeeding oil shale facies.

The book could serve as an undergraduate-level teaching text on exceptional faunas, especially if it was supplemented by references providing more detail on the actual processes of fossilization that are involved. As a more general introduction to the subject of exceptional faunas, or just for anyone simply interested in the beauty of fossil material, this is an excellent book; it is much more than a pictorial guide to fossils, yet really does have that 'wow—look at that' factor.

And there's nothing wrong with that.

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Post-graduate opportunities in Palaeontology

This year we have migrated the listing of palaeontology and related PhD projects, from *Palaeontology Newsletter*, to the Association's website <www.palass.org> so that, as a community, we can make it as comprehensive as possible, and disseminate it as widely as possible. This list, of projects that are planned to begin in October 2005, should be complete by the end of November so please be sure to check it out to identify the projects that most interest you or your students. Descriptions of palaeontology, and palaeontology-related, MSc courses remain listed below. Careers advice is also available from the Palaeontological Association website <www.palass.org> and includes a series of biographies from Palaeo-MSc students through to Professors of Palaeontology, Museum Curators, Science Publishers, and workers in both Show- and Oil-business, all of whom have made their way through a career path in palaeontology. Learn from these luminaries and get yourself a career!

M.Sc. in Palaeobiology: University of Bristol, Department of Earth Sciences

The M.Sc. in Palaeobiology offers a broad-based overview of modern approaches in palaeobiology. Students study nine out of 16 possible options, and topics range from taphonomy and palaeoecology to mammalian palaeobiology, dinosaurs to trace fossils, systematic methods to macroevolution. Then there is a six-month independent project, and students are offered a wide range of topics. The programme is designed for students with a BSc in either a biological or earth sciences subject, and conversion courses in evolution, basic palaeontology, and sedimentology are offered. Students also receive training in writing scientific papers, creating websites, applying for Ph.D.s and jobs (both in Britain and overseas).

So far, 60 students have graduated, and many have gone on to rewarding careers in palaeontology and related scientific areas. Full details of the programme, of former students, and how to apply are available at <<http://palaeo.gly.bris.ac.uk/opportunities/MSc.html>>. Application forms may be downloaded from the website, or they can be provided by <shirley.sparks@bris.ac.uk>.

M.Sc. Micropalaeontology: University College London

The science of Micropalaeontology studies the microscopic remains of animals, plants and protists belonging to biological groups mostly of simple organisation and less than 1mm in size. These organisms were extraordinarily abundant and diverse in the past and continue to be so in modern environments, in many cases forming the primary elements in marine, lacustrine and terrestrial organic productivity cycles and food chains. The production of these organisms is a basic component of the global biogeochemical system, intimately linked to present and past environmental change. In this way microfossils are keys to palaeoceanography and palaeoclimatology and to understanding the evolution of the biosphere. Our ability to use the pattern of evolution of microfossil groups during the last 400 million years as a means of ascribing relative ages to sedimentary rocks and reconstructing their environmental histories is of great



value for understanding global sedimentary geology, and has especially important applications, for example, in the hydrocarbon industry.

The M.Sc. and Diploma course in Micropalaeontology was founded in 1959, was the first of its kind in Britain and was specifically designed to train professional micropalaeontologists. The importance of the subject for biostratigraphy and palaeoenvironmental interpretation is firmly established through its application to hydrocarbon exploration, and also as a key to understanding the history of the continental shelf and oceanic basins. A high proportion of graduates have entered the oil industry, either following the M.Sc. course or after further research. Close links are maintained with the hydrocarbon industry.

The course is broadly based and covers calcareous, organic-walled and siliceous microfossils. Great emphasis is placed on the biostratigraphy and spatial distribution of the organisms and their application to problems of zonation and correlation and to environmental analysis. All major post-Palaeozoic microfossil groups are covered in the M.Sc. curriculum. Individual and team project work forms an important part of the course.

The entry qualifications for the M.Sc. in Micropalaeontology are: at least a Lower Second Class Honours degree in Geology, although joint combinations with Geography, Biology and Oceanography may be acceptable. We welcome enquiries from graduates with experience in oil companies who wish to obtain further qualifications.

Further details and application forms are available from:

Professor A.R. Lord

Department of Geological Sciences, University College London, Gower Street, London WC1E 6BT, UK. Tel: (44) 020 7679 7131; Fax: (44) 020 7388 7614
<micropal@ucl.ac.uk>

M.Sc. Advanced methods in taxonomy and biodiversity: Imperial College London

Imperial College of Science, Technology and Medicine and The Natural History Museum are jointly offering a Masters degree course in Advanced Methods in Taxonomy and Biodiversity.

The one-year full-time M.Sc. course provides essential skills for all concerned with taxonomy and biodiversity. The course is composed of ten taught modules followed by a four-month research project. The series of modules seeks to provide as wide as possible an overview of the theory and practice of modern taxonomy and systematics, with associated biodiversity studies. During their four-month research project, students can specialise in their chosen area.

The course is based at The Natural History Museum, London, one of the world's premier institutions for research on the diversity of the natural world. The collections include over 68 million specimens, 800,000 of which are type specimens, and the Museum houses a world class library covering all areas of taxonomy and systematics. The Museum is situated next to the main South Kensington campus of Imperial College, and there are close research and teaching links between the two establishments. Students will therefore be situated in the heart of London, and are able to make full use of the facilities at both institutions.

Students are trained to a high level of competence in systematics and a detailed understanding of the various uses and problems involved. The course provides methodological background,



including quantitative skills, computer applications and practical skills in morphological and molecular techniques of taxonomy and systematics. The most up-to-date ideas and research in taxonomy and biodiversity are taught, to a large extent from primary literature. Hands-on training in conducting research in this area will be provided by project supervisors, with specialisation in the student's field of choice. After completing the course, students will be able to:

- apply a wide range of techniques to the study of systematics, including collections management, identification, key construction, taxonomic revision, phylogeny reconstruction and comparative methodologies;
- understand the diversity of living organisms in space and time, and be familiar with methods for measuring this diversity and monitoring changes due to both anthropogenic and natural factors, and in Earth history;
- select appropriate methods to solve taxonomic and biodiversity problems, and be able to acquire and analyze taxonomic data, including both traditional and molecular data;
- understand fully the conceptual basis of taxonomy and phylogenetics and in particular, cladistics, and to understand "biodiversity" within this framework;
- apply these concepts to issues of biodiversity and conservation management and research, to set priorities for sustainable development, environmental assessment and inventories; apply these concepts to other areas of biology such as parasitology and epidemiology.

Who is this course aimed at?

The course is aimed at anyone concerned with taxonomy and biodiversity. It is relevant to those involved with biodiversity assessments, conservation and sustainable development, from biomedical sciences to agriculture and fisheries, as well as to those intending to pursue academic careers in systematics and related fields.

Entry requirements

Applicants should normally either have or expect to gain at least a lower second class honours degree (or equivalent) in a biological or environmental subject (e.g. zoology, botany, microbiology, agriculture and veterinary science). Exceptionally students with different backgrounds or with related work experience will be considered.

Further details are available from:

Ms Amoret Brandt

Department of Entomology, Natural History Museum, London SW7 5BD, UK tel: +44 (0)20 7942 5036; fax: +44 (0)20 7942 5229
<a.brandt@nhm.ac.uk>

Royal Holloway, University of London: Department of Geology

MSc Geology by Research

This programme is offered to prospective students who wish to pursue research in a selected field of the Geological Sciences for a period of one calendar year full time or two calendar years part time and be awarded a Masters degree. Students will receive training in research skills, including data collection, data handling and analytical techniques as well as transferable and presentation

skills. Students will take a course in a subject area closely related to the chosen field of research, selected from a menu of masters level courses offered by the department. The main outcome of the programme is a piece of independent research presented in the form of a dissertation. Upon completion of the programme students will have gained experience of research and presentation of material in the geological sciences which equips them to publish work in international scientific journals.

Prospective students should contact individual members of staff in the department in the first instance to discuss potential research projects. The research interests of staff are available on the department website <<http://www.gl.rhul.ac.uk/staff/acad.html>>.

University of Plymouth,

School of Earth, Ocean & Environmental Sciences

M.Res. in Micropalaeontology

This programme in Micropalaeontology operates within a scheme involving a range of M-level subjects in the Earth, Marine, Environmental and Biological Sciences. In the first term a range of taught courses are offered including both subject-based topics and skills training. After this is completed satisfactorily students then pursue a major research project through from January to mid-September. This may be based on field samples collected by the student, samples provided by an industrial sponsor, samples requested from the Ocean Drilling Program or other samples in the collections of staff. Projects undertaken by students in the last academic year include estuarine foraminifera, sea level change in S.E.Italy, foraminifera of the Cambridge Greensand and the use of foraminifera and stable isotope stratigraphy in dating volcanic activity on Montserrat, Caribbean Sea. During this period of research students have to generate assessed reports and give a full seminar presentation on their research.

For further information contact the Course leader Prof Malcolm Hart <mhart@plymouth.ac.uk> or use the University website <www.plymouth.ac.uk>. Application forms for postgraduate study can be downloaded from this website. Some University bursaries may be available.

University of Plymouth,

School of Earth, Ocean & Environmental Sciences

M.Sc./M.Res. Global Environmental Change

The M.Res. programme in Global Environmental Change follows the pattern outlined for the M.Res. in Micropalaeontology. Students following the M.Sc. pathway undertake a second term of subject-based courses, with examinations just after the Easter break. The research project for the M.Sc. students runs from Easter until mid-September and is assessed by interim reports, a seminar presentation and a final dissertation. Recent projects have involved investigation of climate change, sea level changes using foraminifera, palynological change in S.W.England and upland habitats in S.W.England.

For further information, contact Course Leader Dr Gregory Price <gprice@plymouth.ac.uk> or use the University website <www.plymouth.ac.uk>. Application forms for postgraduate study can be downloaded from this website. Some University bursaries may be available.

Discounts available to Palaeontological Association Members

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Individual US\$75 (compared with the normal price of US\$115). Please pay your subscription directly to the publishers.

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£25 reduction on a personal subscription. Contact *Blackwells* Journal subscription department for further details.

Paleobiology

2004 subscription with back issues to date: \$43 to ordinary members, \$24 to student members, plus an additional \$10 for an online subscription as well as the paper copy.

2005 subscription: \$45 to ordinary members, \$25 to student members, plus an additional \$10 for an online subscription. Payment to the Paleontological Society's Subscription Office in the normal way (*not* to the Palaeontological Association). Download the form from <http://www.paleosoc.org/member.pdf>

Please mark the form "PalAss Member" and provide evidence of membership in the form of a confirmatory email from the Executive Officer, or the mailing label from a current issue of *Palaeontology*, which bears the PA member's name and membership status. It will be possible to subscribe and renew on-line by January, 2005.

Palaeontological Association Publications

Don't forget that all PalAss members are eligible for a 50% discount on back issues of the *Special Papers in Palaeontology* monograph series. Discounts are also available on PalAss field guides and issues of the *Fold-out fossils* series. See the Association website for details of available titles, discounts, and ordering.

Trilobites and Their Relatives

Special Papers in Palaeontology No 70, 397 pp. ISBN 0 901702 81 1. £80 (£40 to members)

Edited by Philip D. Lane, Derek Siveter and Richard A. Fortey

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Introduction. *By Richard A. Fortey, Philip D. Lane and Derek J. Siveter.*

An Early Cambrian Phosphatocopid Crustacean With Three-Dimensionally Preserved Soft Parts From Shropshire, England. *By David J. Siveter, Dieter Waloszek and Mark Williams.*

Tagmata and Segment Specification in Trilobites. *By Alessandro Minelli, Giuseppe Fusco and Nigel C. Hughes.*

Phylogeny of Early Cambrian Trilobites. *By Peter A. Jell.*

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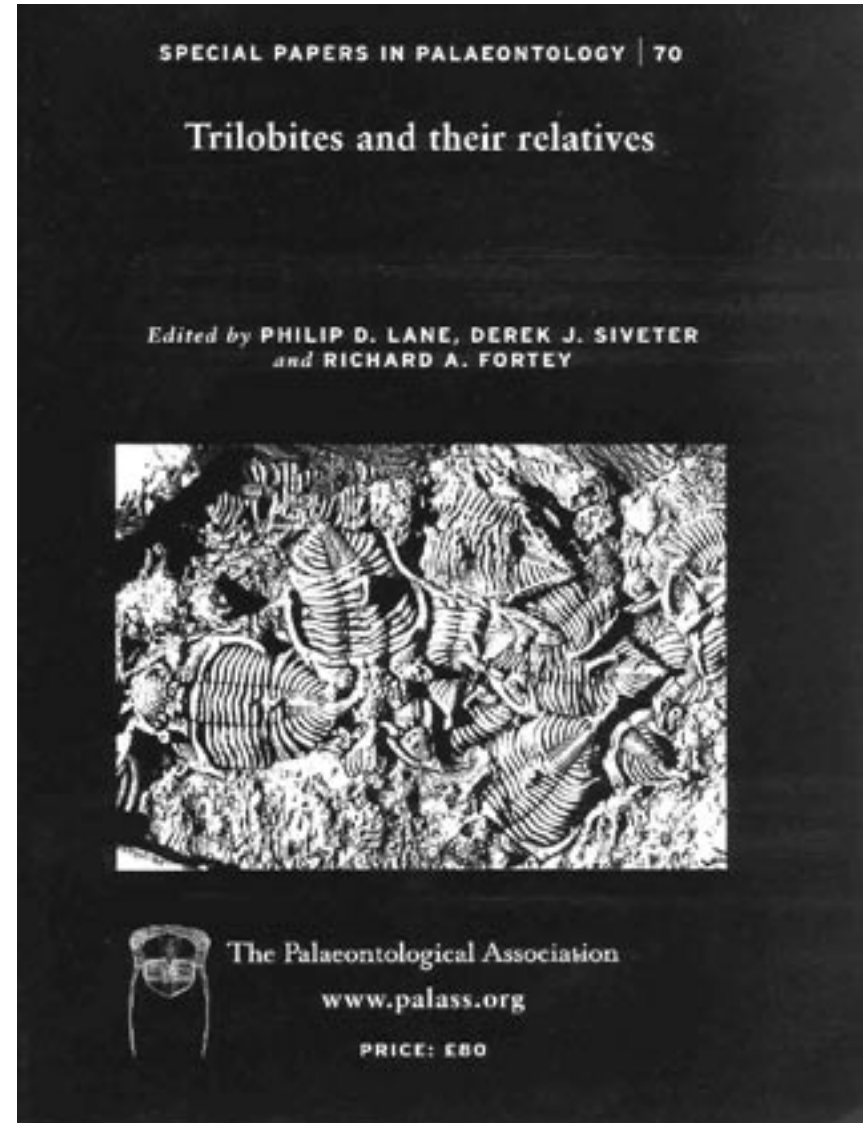
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Fossils of the Miocene Castillo Formation, Venezuela: contributions on neotropical palaeontology

Special Papers in Palaeontology No. 71. 112 pp. ISBN 0 901702 82 X. £48 (£24 to members)

Edited by Marcelo R. Sánchez-Villagra and Jennifer A. Clack

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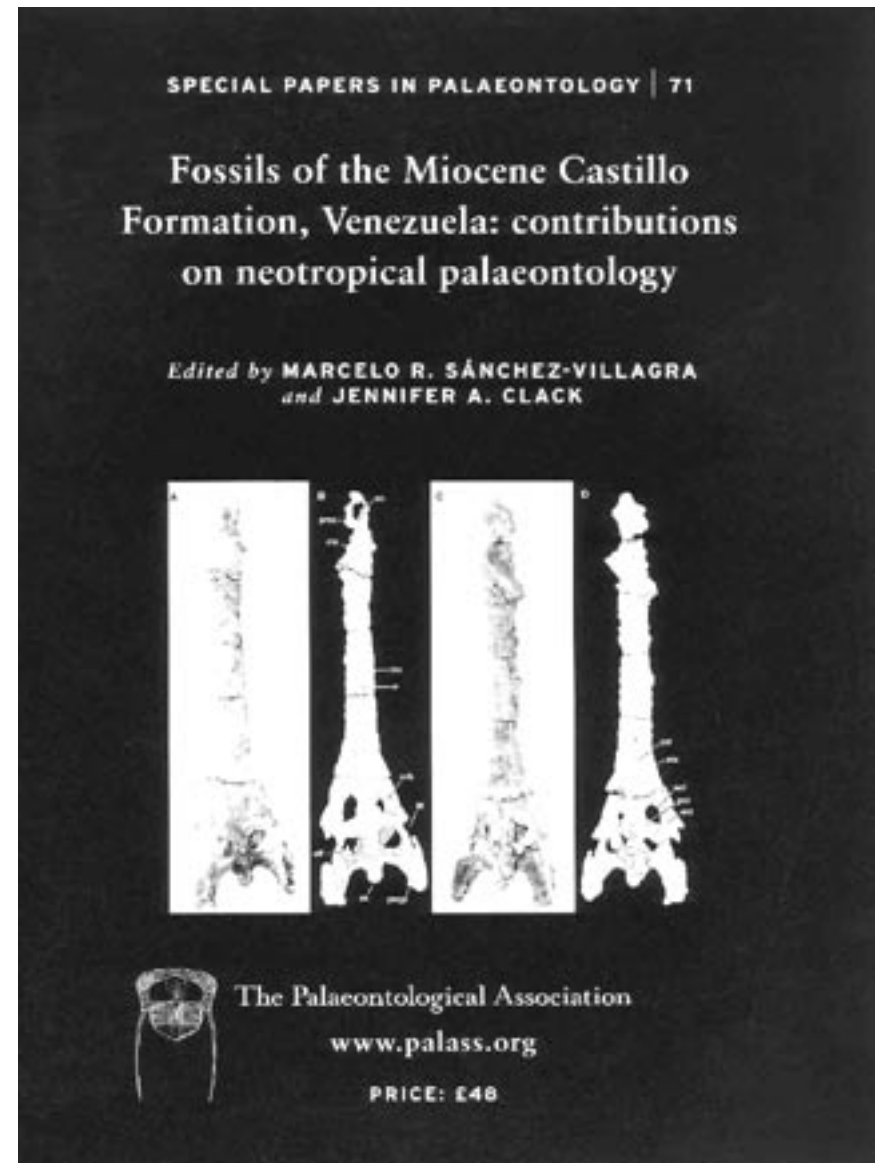
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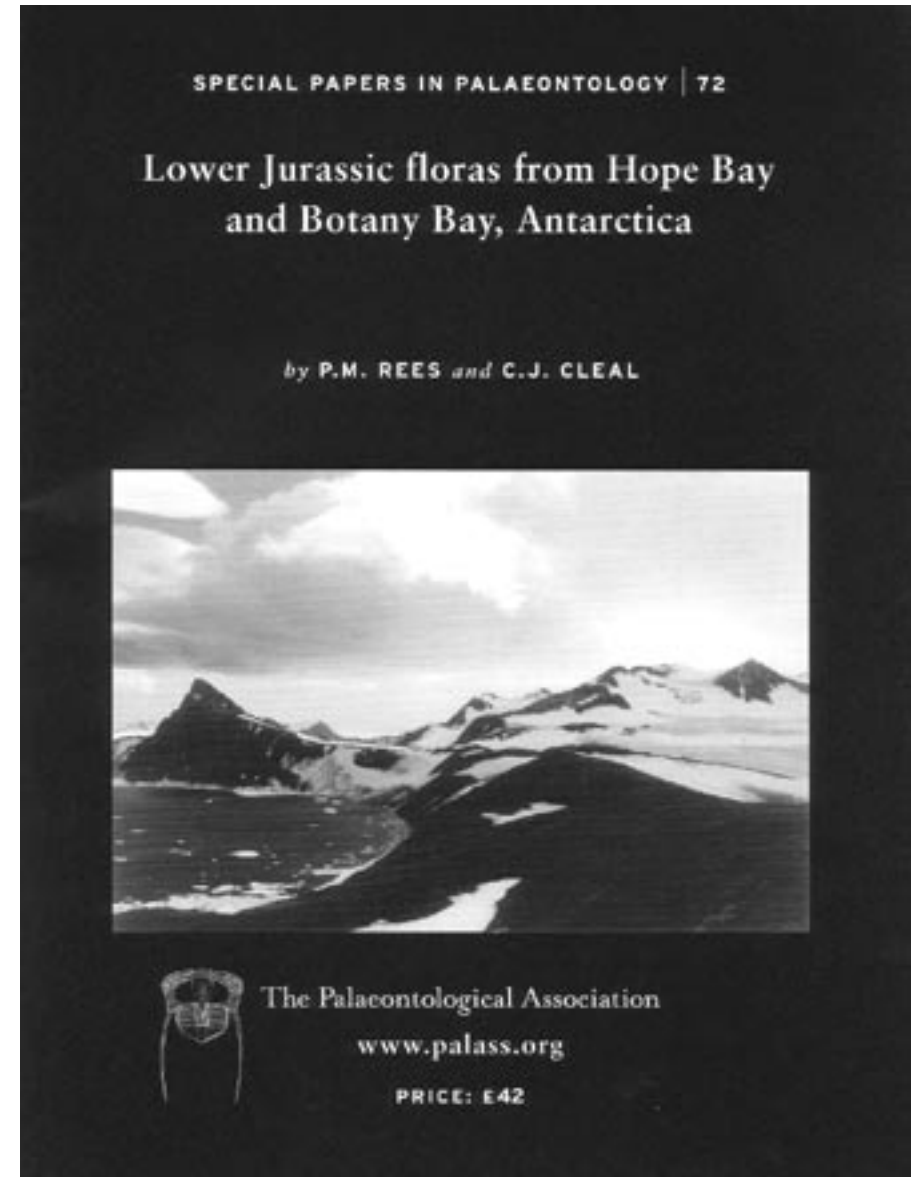
Lower Jurassic floras from Hope Bay and Botany Bay, Antarctica

Special Papers in Palaeontology No. 72. 90 pp. ISBN 0 901702 83 8.
£42 (£21 to members)

By P. M. Rees *and* C. J. Cleal

Abstract

Hope Bay and Botany Bay, Graham Land, Antarctica have yielded two of the most diverse floras known from the Jurassic. Because of its high diversity, as well as its early discovery and description (by T. G. Halle in 1913), the Hope Bay flora has served as a taxonomic standard for studies of other Mesozoic floras from Gondwana. This paper presents a major revision of the Hope Bay flora, based on extensive subsequent collections. A nearby flora from Botany Bay is described for the first time. Thirty-seven species are now recognised in the Hope Bay flora and 32 from Botany Bay. The floras are closely similar; 80 per cent of the Botany Bay species also occur at Hope Bay. They are shown here to be Early Jurassic, which contradicts the results of previous studies that suggested a Late Jurassic or earliest Cretaceous age. The revision of their age has special significance for our understanding of the Mesozoic geological history of the Antarctic Peninsula. It also highlights the need for reappraisal of a number of other Mesozoic Gondwanan floras that had been dated mainly on their close similarity to the Hope Bay flora. The taxonomic work has resulted in establishment of a new combination, *Taeniopteris taeniopteroides*, and emendation of the diagnoses of *Cladophlebis oblonga*, *Sphenopteris nordenskjöldii*, *Sphenopteris pectin* and *Komlopteris indica*.





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