

1. Welcome to members and guests.
2. Admission of Fellows.
3. Apologies for absence.
4. Minutes of meeting held on 19–20th April 2002, which have been posted in the Society's Rooms.
5. Third Reading of Certificates of Recommendation for two Foreign Members (FMLS): *Prof. DW Stevenson (USA)*, *Dr KM Matthew (India)*, (both botanists).
6. Appointment of three scrutineers.
7. (a) Ballot for Members of Council (blue: see Council nominees overleaf)
(b) Ballot for a Fellow *Honoris causa* and three FMLS (pink: see 5 above)
(c) Ballot for Officers (yellow: the current honorary Officers offer themselves for re-election; additionally *Prof. G McG Reid* has been nominated as President-Elect.).
(d) Ballot for Fellows and Associates (white).
8. Citations and Presentations of Medals and Awards.
Linnean Medal for Botany *Dr. S Carlquist FLS*.
Linnean Medal for Zoology *Prof. WJ Kennedy*.
HH Bloomer Award (2001) for an amateur who has made a notable contribution to science *Dr. H Hess*.
Dr. Hess was unable to receive the medal in 2001.
HH Bloomer Award (2002) *Mr. TL Blockeel* and *Sir Anthony C. Galsworthy*.
Bicentenary Medal for a biologist under 40 *Dr. PE Ahlberg FLS*.
Jill Smythies Prize for botanical illustration *Mrs. J Brasier*.
Irene Manton Prize for a PhD thesis in botany – *tba*.
9. Treasurer's Report for 2001.
10. Motion to Accept Accounts for 2001.
11. Appointment of Auditors for 2002.
12. Appointment of Bankers for 2002.
13. Contributions 2003.
14. President's address.
15. Vote of Thanks.
16. Result of Ballots and any casting votes.
 - A. Council.
 - B. Fellow *Honoris causa* and Foreign Members.
 - C. Officers, including President-Elect –
 - i President
 - ii Treasurer
 - iii Zoological Secretary
 - iv Botanical Secretary
 - v Editorial Secretary.
 - D. Fellows and Associates.
16. Names of the Vice-Presidents.
17. Any other valid business.
18. Close.

Council Nominations

For President-Elect

Gordon McGregor Reid (1978). Director and Chief Executive of Chester Zoo (The North of England Zoological Society). The Zoo occupies more than 150 hectares, and is the leading wildlife attraction in Britain with about 5000 animals in 500 species (about half of which are on the IUCN Red List). The Zoo attracts more than one million visitors each year and under the present Directorship has gained more than 70 international, national and regional awards and distinctions in conservation, education, science and business excellence – culminating in 2002 with the *Queen's Award for Enterprise* in the category of Sustainable Development. Gordon is a graduate of the University of Wales, Cardiff (BSc Zoology, 1974). In 1978 he gained a PhD in comparative anatomy and systematics (labeine cyprinid fishes) from the University of London via the British Museum (Natural History). The author of more than 100 published works, his research interests include taxonomy, zoogeography and conservation biology. For this contribution Gordon was awarded a Fellowship of the Institute of Biology (1994). He has acted as a consultant for the World Wide Fund for Nature, Conservation International, Fauna and Flora International and other agencies and has extensive field experience in Africa, Central America, India and the Middle East. Gordon has worked for Voluntary Service Overseas (Nigeria, Botswana) and British Executive Service Overseas (Hungary). He is a Visiting Professor in the Department of Veterinary Clinical Science and Animal Husbandry, University of Liverpool and a Zoo Inspector on the Secretary of State's List (DEFRA). Previous appointments include Keeper of Natural History, Horniman Museum, London (1985–91) and Keeper of Natural History (Conservation), Liverpool Museum (1982–84). In 1979, Gordon resigned as caretaker of the Linnean Society to take a post as Lecturer in the University of Sokoto, Nigeria! He has served as a Member of Council of the Linnean Society (1990–1993 and 1995–1998) and co-organised regional meetings. He is currently a Trustee of the National Museums & Galleries on Merseyside, a Council Member of the World Association of Zoos and Aquaria (and Chair of the Marketing & Public Relations Committee), Council Member of the European Association of Zoos and Aquaria (and Chair of the Aquarium Committee), and he serves on the Editorial Board of the *International Zoo Yearbook*.

For Council

Stephen Blackmore FRSE (1976). Member of the Council of the Linnean Society 1986–1989 and Bicentenary Medal Winner in 1992. Regius Keeper of the Royal Botanic Garden Edinburgh. A plant taxonomist specialising in systematic palynology who is strongly committed to the objectives of the Convention on Biological Diversity. During the late 1970's he worked at the Royal Society Aldabra Research Station, Seychelles and the National Herbarium of Malawi before taking up a research post in the Department of Botany at the Natural History Museum, London. From 1990 to 1999 he was the Keeper of Botany at the Museum and during this time he chaired the UK Systematics Forum. He is a Visiting Professor at the Universities of Glasgow and Reading.

Paul Kenrick (1998). Bicentenary Medal Winner in 1999. Henry Allan Gleason Award of The New York Botanical Garden in 1998. Palaeobotanist at the Natural History Museum, London. Prior to this he held research posts at several other institutes in Europe and the US, including the Swedish Museum of Natural History, Stockholm, and the Field Museum, Chicago. His main research interests include the origins and early evolution of the land flora and the systematics of ferns and fern allies. He is the author of a book on land plant origins.

Alex David Rogers (1993). Principal Investigator, Antarctic Biodiversity, Past, Present and Future, British Antarctic Survey. Dr. Rogers maintains a wide interest in natural history. His first degree was in marine biology, his PhD on the systematics and population genetics of marine nemertean worms, both at the University of Liverpool. He has completed two research fellowships on the molecular ecology and systematics of marine animals. In his current post, he leads the Antarctic Biodiversity Programme, with research interests in the systematics, evolution and molecular ecology of marine organisms. Dr. Rogers is also applying genomic and proteomic methods to study the stress-related physiology of polar organisms.

David A. Simpson (1986). Graduate of the Universities of Wales (Bangor), Reading and Lancaster. Following the completion of a PhD in 1983 investigating the systematics and ecology of the waterweed genus *Elodea*, he took up a postdoctoral Fellowship at Trinity College Dublin. He was appointed as a taxonomist in the Herbarium at Kew in 1985 where he is currently Head of Commelinoid Monocots. David's main research interests are in the sedge family Cyperaceae, particularly the tropical genera. He also has broader interests in SE Asian floristics, particularly in Thailand, and is a member of the Editorial Board of *Flora of Thailand*.

Editorial

This issue of *The Linnean* contains a paper entitled "Evolutionism and Creationism" read by the late Colin Patterson to the American Museum of Natural History, New York in 1981. In this paper Colin contrasted the approach of the evolutionary taxonomists with that of cladists, pointing out that there is a natural heirarchy which derives from the orderliness of ontogeny, while the explanatory value of a hypothesis of common ancestry is nil.

Elsewhere in this issue I have included my 1996 Presidential address since not only does it epitomise the cladists' approach, but more importantly, because it was Colin who convinced me that hair and feathers were part of the same developmental process.

In a subsequent issue we shall deal with adaptive radiation and convergence and the sad status of evolution education in American schools, then in the October issue we introduce the "Crucible of Creation", otherwise known as the Burgess Shales.

BRIAN GARDINER

Society News

On 7th September we will commemorate the late **Sir Cyril Clarke Hon FLS FRS**. Sir Cyril, who is chiefly remembered for solving foetal incompatibility problems in *rh*-mothers, was one of a number of eminent biologists in Liverpool in the latter half of the last century, most of whom are no longer with us. A lecture by Dr. Laurence Cook Hon FLS on Clarke's non-medical genetic work, much of which was published in the Society's journals will be given in Liverpool at 6pm on Saturday, 7th September in the Liverpool Museum, John Brown Street, near Lime Street Station. All are welcome.

Others who have shuffled off this mortal coil more recently include **David McClintock MBE** on 23rd November 2001 who was the HH Bloomer Award winner in 1993, Council member for most of the 70s, Vice-President 1971–74 and Editorial Secretary 1974–78; an obituary appeared in *The Times* on 13th December 2001. He was 88 and his MBE was part of the 2002 New Year's Honours. The Society was represented at his memorial service by the Executive Secretary. In his will, David left the Society books to the value of £1000 for the Library. At 96, **Barton Worthington** was our longest serving Fellow and Fellow *Honoris causa*. First elected in 1929, he served on Council from 1935–38 and was elected a Fellow *Honoris causa* in 1992. A brief obituary appeared in *The Times* on 1st January 2002. He died on 14th December 2001. **Kenneth Alvin** was also a long-serving Fellow of the Society, elected in 1958, who died on 28th December after a long illness. The Society was represented at his funeral by Dr. Joan Watson FLS.

The Society acknowledges with gratitude a bequest of £20,000 in the will of **Alexander Cave FLS FRS**, President of the Society 1970–73, who died earlier in the year. Professor Cave was 102; despite infirmity in the past decade which prevented his participation in Society affairs, he invariably acknowledged invitations in an extremely neat and minute hand. Owing to postal problems in this area of London, the Society unfortunately did not receive sufficient notice to be represented at his funeral. His main area of distinction was evolutionary anatomy and his scalpel was wielded to useful effect on animals which died at the Regent's Park Zoo. Whilst on the Council of the Zoological Society of London, a lady was so overcome by the intensity of the proceedings (even then!) that she fainted. As a medical man, Professor Cave attended to her and some while later rejoined the meeting. To the inevitable question as to the health of the lady, the professor is said to have remarked that she was no good to him, as she was still alive!

We also acknowledge with gratitude an interim bequest of £12,000 from **Dr. James Samuel Challis FLS**. Further smaller sums await the maturing of two financial instruments in the coming year.

In the land of the living, our newest Fellow *Honoris causa* (2001), **Ray Desmond**, also became an MBE for services to garden history at the beginning of 2002. It is nice to have the Society's decision – for services to botanical history – confirmed so speedily by so august an authority as our Patron.

JOHN MARSDEN

Library

The Linnean Society took part in London Open House on Saturday 22 September 2001 with over 400 people visiting the Rooms. New leaflets on the Rooms and the Collections were given to visitors as they arrived. The Meeting Room, Hall and Library Reading Room were open with volunteers "guides". Most visitors were very appreciative of the opportunity presented by the occasion, some had prior knowledge of the Society, others came by chance as many other buildings were closed due to security measures. A number of other visiting groups have had pre-booked tours of the Library, the Rooms and the Collections, these ranged from students to special interest groups. This is usually possible given advance warning but it is necessary to book well ahead.

Cathy Broad was appointed Assistant Librarian, from 19 November 2001, and has been working on proposals for an electronic catalogue. Recommendations will go to Council in May. Work on stores in the East Basement has continued, with the need to remove all remaining journals from the newly painted room to enable the floor to be levelled and lino laid. This has now been done and most of the journals replaced thanks to a team of sporadic student helpers, who are still continuing to sort out both shelving and journals.

The Library gains considerably from the work of volunteers. NADFAS volunteer Pat Bratton has recently has a replacement knee operation so has been absent for a while but Melba Coombes continues to clean Linnaean manuscripts. Other volunteers working on various projects include Diane Furley and Iris Hughes who have now almost completed indexing the contents of Miriam Rothschild's reprint boxes. Edna Clifford files their completed cards and has been assisting with processing new accessions by pasting in the Library Regulations. Dr Alan Brafield continues to help with cataloguing. Margot Walker is making a new calendar of Smith's correspondence with T.E. Cullum and Enid Slatter has continued to work through listing the illustrations in the Gunther albums. Charles Hutt is a regular Friday visitor, listing Society files and archives at home on his computer. Jenny Brasier, also working on the Gunther albums, has not been able to visit since the summer.

Donations

Notable donations listed here include Edward Tyson's, *Orang-Outang or the anatomy of a pygmie...* which should have come to us after the death of Prof. R.J.G. Savage but has only just been delivered. The copy of the 5th edition of P. Miller's *Dictionary* listed last time has now been repaired and to it has been added a single volume of uncoloured plates from Miller presented by Margot Walker FLS. A gift, of recent publications of the Wildlife Trust of Sri Lanka, by Rohan Pethiyagoda during his visit in November also included recent issues of the *Journal of South Asian Natural History*, now re-named *Zeylanica*.

Dr W. Backhuys Valkenberg, J.L.C.H. van & Bunyaphrathatsava, N. eds., *Plant resources of S.E.Asia, Medicinal and poisonous plants 2*. 782 pp., illustr., map., Leiden, Backhuys, 2001.

- J.D. & J.M. Chapman Chapman, J.D. & Chapman, H.M., *The forest flora of Taraba & Adamawa states*, Nigeria. 146 pp. + 75 pp., illustr., maps. Christchurch N.Z., Univ. of Canterbury, 2001.
- Prof. M. Dick Dick, Michael W., *Straminipilous fungi... systematics of the Peronosporomyces*. 670 pp., illustr., figs, maps, Dordrecht, Kluwer, 2001.
- Hunt Institute Pittsburgh, Hunt Inst. of Botanical Documentation, *International exhibition of botanical art, October 2001–February 2002*. ed. by James J. White & Luene B. Bruno, 183 pp., illustr. some col., Pittsburgh, Hunt Institute, 2001.
- Hilary Lees Lees, Hilary, *So runs my dream, the story of Arthur and Keble Martin*. 144 pp., illustr., some col., Tiverton, Halsgrove, 2001.
- Sir C. Lever Lever, Christopher, *The cane toad, the history and ecology of a successful colonist*. 230 pp., Otley, Westbury, 2001.
- Cees Lut Van Balgooy, M.M.J., *Malasian seed plants. 1: Spot characteristics* 154 pp., illustr., Leiden, Rijksherbarium, 1997. 2: *Tree families* 307 pp., illustr. Leiden, Rijksherbarium, 1998. 3: *Non-tree families*. 260 pp., illustr., Leiden, Rijksherbarium 2001.
- Dr F.K. McKinney Jackson, Jeremy B.C., Lidgard, Scott & McKinney, Frank K., *Evolutionary patterns, growth form and temperature in the fossil record*. 399 pp., illustr., maps, Chicago, Univ. of Chicago Press, 2001.
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- Royal Botanic Gardens, Kew Schatz, George E., *Generic tree flora of Madagascar*. 477 pp., illustr. map, Kew, Royal Bot. Gardens & Missouri Botanic Garden, 2001.
- Russian Academy of Sciences Moiseev R.S. & Tokranov, A.M. eds., *Catalogue of vertebrates of Kamchatka and adjacent waters*. 165 pp., Petropavlosk-Kachatsky, Kamchatskiy Petchatniy Dvov, 2000.
- The estate of R.J. Savage Tyson, Edward, *Orang-Outang or the anatomy of a pygmie...* 108 pp., illustr., bound with *A philological essay concerning the pygmies*. 58 pp., London, Thomas Bennett, 1699.
- South Georgia Assoc. Burton, Robert and Venables, Stephen, *Shackleton at South Georgia*. 24 pp., illustr. map, Towcester, privately, 2001.
- Headland, R.K., *South Georgia, a concise account*. 28 pp., map, Cambridge, British Antarctic Survey, 1982.

- A.H. Swann Swann, Tony, *Great Zoological Books, a bookseller's perspective* (talk to the Zoological Society). 32 pp., col. illustr., privately, 1996.
Swann, Tony, *Great Botanical Books, a bookseller's perspective* (talk to the Natural History Museum). 32 pp., col. illustr., privately, 1997.
Swann, Tony, *Conchological Books, a bookseller's perspective* (talk... January 1992). 13 pp., privately, 1992.
- Prof. P. Thrower Walters, Heinrich, *Die vegetation der Erde*, Bd.I 2nd ed. 592 pp., illustr., some col., maps, Jena, Gustav Fischer, 1964.
- Dr S.M. Walters Walters, S.M. & Snow, E.A., *John Stevens Henslow, Darwin's mentor* 338 pp., illustr. some col., Cambridge, Cambridge University Press, 2001.
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Yen, Douglas E., *The sweet potato and Oceania, an essay in ethnobotany*. 389 pp., illustr., 2 col. pl., map, Honolulu, B.P. Bishop Museum 1974 (B.P. Bishop Museum Bulletin No. 236).

GINA DOUGLAS

Picture Quiz

Alcide d'Orbigny (1802–1857)

The bicentennial of the French naturalist Alcide d'Orbigny is commemorated in 2002, with conferences planned to take place in Paris and La Rochelle, France, and in Santa Cruz, Bolivia (<http://www.orbigny.org>). Although d'Orbigny is a well-known name to palaeontologists interested in Mesozoic fossils, and also to students of the Foraminifera, his broader reputation among natural historians does not match his considerable achievements in zoology, geology and palaeontology.

Alcide Charles Victor Marie Dessalines d'Orbigny, whose portrait appeared in the last issue of *The Linnean*, was born on September 6th 1802 at Coueron (Loire Inferieure) to a family whose origins can be traced back to 15th Century barons in the service of King Louis XI. The family



subsequently migrated to San Domingo in the West Indies where Alcide's grandfather was a planter. Most of the d'Orbigny family were massacred during a slaves' revolt except for two sons who were in France receiving their education at the time. One of these sons, Charles d'Orbigny (1770–1856), married Marie-Anne Pipat in 1799 and fathered Alcide. Charles had trained as a naval surgeon and later practised medicine in France. From 1815 until 1820 the d'Orbigny family lived at Esnandes, a coastal village 13 km north of La Rochelle. Here Charles nurtured his interest in marine life, particularly foraminifera, an important group of protists at that time classified as cephalopod molluscs. As his own eyesight deteriorated, Charles employed his elder son Alcide to observe and draw these tiny animals. Heron-Allen's (1917, pp. 6–7) translation of a letter written by Charles d'Orbigny in 1819 tells of how he and Alcide observed living foraminifera swimming in a bowl and clinging to the zooids of a bryozoan colony. Alcide d'Orbigny's younger brother Charles (1806–1876) also became a naturalist, assisting Cordier, Professor of Geology at the Museum d'Histoire Naturelle, Paris, and is best known for his editorial work on the *Dictionnaire universelle d'Histoire Naturelle*.

Whereas Heron-Allen (1917) attributed the origin of scientific study of foraminifera to the elder Charles d'Orbigny, Le Calvez (1974, p. 264) accorded Alcide d'Orbigny the honour of being "the first to recognise the importance of foraminifera and to foresee the place they would one day occupy in stratigraphical geology." Alcide began serious research on foraminifera after the family had settled in La Rochelle from where he visited Paris to examine the collections of DeFrance and Lamarck in 1825. He first came to scientific prominence with the publication of the *Tableau méthodique de la classe des Céphalopodes* in 1826. Although regarded by Alcide d'Orbigny as only a preliminary study, the *Tableau* thoroughly overhauled the existing classification, erected 552 species, both living and fossil, and proposed the name Foraminifera for this group of minute 'cephalopods'.

While working on foraminifera for the *Tableau*, d'Orbigny had the idea of making enlarged models in order to bring these tiny animals to the attention of those without the means of observing them directly. The models, at scales of 40–100 times the original animals, were carved by d'Orbigny himself, moulded, and cast in white plaster. A total of 100 models were produced. These were issued in 4 installments (1823–1826) of 25 models, priced at 20 francs per installment.

In late 1825 d'Orbigny was appointed "Naturaliste-voyageur du Muséum d'Histoire Naturelle", having been proposed for this post by Geoffroy-St Hilaire, Cuvier, Brogniart and others who were impressed by his work on foraminifera. On July 29th 1826 he set sail from Brest on board the corvette 'La Meuse' bound for South America via the Canary Islands. During the next seven and a half years, he was to visit Brazil, Argentina, Paraguay, Chile, Bolivia and Peru. His adventures were described recently by Laborde Pédelahore (2000). In Montevideo he and a companion naturalist called Trion were imprisoned and had to bribe their way to freedom. In Patagonia he exchanged a scythe, a hatchet and a knife for a native Guarayo Indian boy who he subsequently brought back to France. By the time d'Orbigny departed from South America in September 1833, he had accumulated a huge collection of natural history specimens which included, for

example, some 4000 insect and 600 mollusc species. The full narrative account of d'Orbigny's diverse adventures and research in South America fills 7 folio volumes and 2 atlases. Preparing this *Voyage dans l'Amérique Méridionale* for publication occupied most of his time from 1834 until 1847. The content ranged from geological and geographical maps, to the ethnography of the American races of Man and the archaeology of the Inca.

While writing his South American memoirs, d'Orbigny also worked on fossil foraminifera, especially from the Cretaceous of the Paris Basin and the Tertiary of the Vienna Basin. From this time onwards invertebrate palaeontology became the main focus of his interest. He amassed a collection of more than 100,000 fossils, a great many of them apparently collected personally in the field. These fossils were described in two major works: the *Prodrome de Paléontologie Stratigraphique Universelle des Animaux Mollusques et Rayonnés faisant suite au Cours Élémentaire de Paléontologie* (1850–1852) and the *Paléontologie Française* (1840–1858). The *Prodrome*, published in three volumes, lists some 18,000 fossil species, arranged in stratigraphical order. Unfortunately, however, it is unillustrated. The 8 volumes of *Paléontologie Française* authored by d'Orbigny contain 1000 plates and describe approximately 3000 species of which 2500 are new. Further volumes of the *Paléontologie Française* were written by Cotteau and others after d'Orbigny's death. Most of d'Orbigny's formidable collection is today stored in the 'Salle d'Orbigny' on the first floor of the 'Galerie d'anatomie comparée et de paléontologie', Paris. Now closed to the public, this room, overflowing with specimens, is a testament to d'Orbigny's veracity as a collector and monographer, as well as a monument to 19th century museology. More than 100,000 specimens were in the d'Orbigny Collection when it was purchased from his widow in July 1858 for the sum of 55,000 francs. It took M. Hupé almost two years to catalogue the entire collection.

In 1853 d'Orbigny was appointed to the newly created Chair of Palaeontology at the Muséum d'Histoire Naturelle. However, his time in this post was brief. Following a year of illness, he died on 30th June 1857 at the age of 54 in the small town of Pierrefitte-sur-Seine and was buried there in the municipal cemetery.

Close to d'Orbigny's grave in Pierrefitte-sur-Seine is a small street named in his honour. There is no equivalent Rue d'Orbigny in Paris itself, despite the fact that the French capital has a noble tradition of naming streets after natural historians. Although Alcide d'Orbigny was made a Knight of the Legion of Honour and given numerous other honours during his lifetime, he died disappointed after failing to be elected to the Institut de France to which his name was presented no fewer than eight times. For several reasons, he was never fully accepted among his peers, partly because they were jealous of his productivity, but also perhaps because of his humble origins as a 'naturaliste-voyageur'. His association with the late Cuvier, whose reign at the Muséum d'Histoire Naturelle was remembered without regret, did not help matters. Furthermore, the professors of Geology, Zoology and Botany at the Muséum believed there was no need for a Chair of Palaeontology as the subject was already sufficiently covered by them.

Alcide d'Orbigny threw himself into projects vast in scope. As Heron-Allen (1917, p. 68) remarked, any one of half a dozen major works produced by d'Orbigny might have constituted the lifetime output of a more ordinary scientist. He was a prodigious namer of taxa. Unfortunately, many of his species are unillustrated (as in the *Prodrome*), and much revisionary work remains to be completed on his type material. Two of d'Orbigny's procedures have caused nomenclatorial confusion. The first was ostensibly to attribute to himself the authorship of any established species which he assigned to a different genus than that used by the original author, e.g., *Pustulopora pseudospiralis* Michelin became *Peripora pseudospiralis* d'Orbigny when d'Orbigny transferred this species to his new genus *Peripora* in 1853. By adding his own name, d'Orbigny was actually indicating that he was the author of the new combination – he considered it unfair to the original author to retain their name given that they might disagree with the new generic placement. The second problem arises from d'Orbigny's convention of appending the year in which he wrote the description of a species to the epithet, e.g., *Idmonea elegantula* when first published in the *Prodrome* in 1850 was followed by the date 1847 which was when he first drafted the description (in this case the three year delay in publication was caused by the abdication in 1848 of King Louis-Philippe and declaration of the Second Republic).

From a taxonomic standpoint, d'Orbigny's work has been often criticised because he paid scant attention to internal structures and was careless in transcribing information from earlier publications. He also tended to use the same specific names over and over again; for example, at least 18 new species of fossil bryozoans were given the name *elegans* by d'Orbigny. His lithographed drawings are very stylistic – they are referred to as “synthétogrammes” by Gauthier (1993) – and can be difficult or impossible to match with individual specimens, a major problem in cases where the type material is disputed. Furthermore, d'Orbigny's theories on the distribution of species in space and time compelled him to be a taxonomic splitter. For example, a foraminiferan species found on the two sides of the Atlantic would be given two names even if there were no detectable morphological differences. More interestingly, d'Orbigny believed in multiple creations – 27 in all – and would give separate species names to what we can now recognise as conspecific specimens if they came from different geological stages. D'Orbigny divided fossiliferous strata into six periods (terrains) which were then subdivided into stages (étages). The fossils contained in the rocks belonging to a particular stage were viewed by d'Orbigny as representing the fruits of a separate act of creation, subsequently to be erased by the cataclysmic extinction that occurred at the end of that stage. Supraspecific taxa, however, were permitted to cross from one geological stage to the next. D'Orbigny gradually hardened his stance on multiple creation from that of a hypothesis to a statement of fact. Although such Cuvierian ideas are today largely the domain of historians of science, d'Orbigny's methods have left a contemporary geological legacy in that the stages he proposed (e.g. Bathonian, Cenomanian) are still used by stratigraphers as divisions of geological time that can be recognized by the fossils they contain.



Clue: He studied South American flora;
but failed to recognise the importance of Chilean copper.

While present day systematists might despair at some of the nomenclatorial and taxonomic problems created by d'Orbigny, few can fail to admire his tenacity in describing so many taxa and skill in organizing them into logical schemes of classification. He was an excellent observer of small-scale details at a time when microscopy was in its infancy, and had the ability to distinguish between morphological variations which are of taxonomic importance and those which are not.

In the foyer of the magnificent new 'Grande Galerie de l'Evolution' in Paris hangs a large impressionist painting by Raoul Dufy called *Les Savants*. The canvas depicts an imaginary meeting of famous naturalists including Darwin, Cuvier, Linnaeus and Haeckel. Taking his place among these celebrities of early natural history is Alcide d'Orbigny. His bicentennial in 2002 provides a reminder of his achievements and an opportunity to revive the name of this unsung giant of early 19th century natural history.

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PAUL D. TAYLOR FLS

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[Visiting Professor, Muséum National d'Histoire Naturelle,
Paris, March–April 2001]

There was only one correct answer to the October Picture Quiz, from Dr Arthur Hollman. Ed.

Correspondence

Dear Brian

Centenary Celebrations at Glasgow University

On the afternoon of 13th June 1901, the new Botany Building of the University of Glasgow was officially opened by Sir Joseph Hooker in the presence of a distinguished company, (see *The Linnean*, Vol. 13(4), pp. 15–37, *The Establishment of an Institute of Botany*). For the Regius Professor, F.O. Bower, this event marked the culmination of 16 years of planning, waiting and often frustration. His reward was a purpose-built institute designed to his own requirements. At the time he claimed that this was the first building in a University in Britain solely for botanical teaching and research. It was also the first separate building for a scientific discipline to be erected in the grounds of the University.

On the afternoon of 13th June 2001 the centenary of this opening ceremony was remembered and celebrated in the same building, now named after Bower. Speakers were Emeritus Professor A.D. Boney and Regius Professor M. Blatt. An evening reception was held in the Kibble Palace of the Glasgow Botanic Garden.

The event was also part of the University's celebration this year of its 11th Jubilee, the 550 years since its foundation in 1451. During this span of time botany has been taught in every academic session in the past 297 years. Teaching and research continue with the many attributes now necessary for the present day study of 'the science which treats of plants' (OED).

Yours sincerely

A.D. BONEY FLS

Systematics and creationism

In this issue of *The Linnean* we publish the transcript of a talk given under the title “Evolutionism and Creationism” by Colin Patterson FRS FLS (d. 1998) to the *Systematics Discussion Group* at the American Museum of Natural History on Guy Fawkes night 1981. Patterson used to write his lectures out verbatim, often with marginal marks, so the job has been relatively easy. We are grateful to Drs David Williams (The Natural History Museum, London) and Gareth Nelson (School of Botany, University of Melbourne) who carried out this task and added supplementary notes.

Why should we print this 21 years later? Well, just log on to the Google web search engine (www.google.com) and type in “Colin Patterson” and wait for hundreds of sites to appear. After about one hour you may have scratched the surface and read countless quotes and innumerable misquotes taken from this talk, seemingly supporting the case for creation ‘science’ and excavating the bedrock of evolutionary theory. These web sites are continuing to appear – perpetuating this message. Some are more faithful to the handwritten lecture than others, some are even for sale with a tape, but in all cases they hardly have the blessing of the author: “It is a pity that the tape-recorder allows others to give the permanence of print to verbal explorations.I would have preferred a chance to approve what is published” (Patterson, 1982:285). Creation ‘scientists’ are well-known for their selected extracts of talks and meetings and might be dismissed as non-scientific (by definition) or eclectivists (by intent?). But such extractions are singularly unhelpful to those who defend evolutionary theory, sometimes in courts of law. Patterson himself was much troubled by the reaction this talk generated amongst his fellow scientific colleagues. Such reaction from a distinguished scientific colleague may be illustrated by an account of that evening by Wayne Friar – a creationist herpetologist and one of an unknown number of the audience who taped the talk: “I was sitting in the front row next to an AMNH curator of mammals, Karl Koopman, who, obviously very agitated kept slamming his pencil down in front of him. Niles Eldredge in the Department of Invertebrates at AMNH was standing by the left wall (as one looks toward the speaker). Beside Eldredge stood a high school biology teacher, Roy Slingo, from the prestigious Scarsdale NY district. Slingo later informed me that at one stage of the talk Eldredge (well known for his anti-creationist perspective) grabbed his forehead and slid down the wall proclaiming, “My God, how can he be doing this to us.” (<http://www.creationequation.com/EvolutionistSeeksAnswers.htm>).

So what exactly was Patterson doing? The reader must make up his or her own mind but it needs stressing that he was talking about systematics – a point he repeatedly made (e.g. Scott, 1992). But a little background may be in order. Throughout the latter years of the 1970s Patterson was deeply interested in the theory of cladistics and, in particular, in the realisation sparked by Gary Nelson that cladistic classifications of biological taxa were more concerned with expressing hierarchical distributions of characters than with theories of character transformation through concepts of ancestry and descent (Platnick, 1979). Patterson saw that the idea of considering evolutionary transformations from ancestors to descendants in our attempts to classify organisms

could only be justified by authority and he wrote a great deal on this subject. In the 1970's the authority was Ernst Mayr who supported the school of evolutionary taxonomy.

Stimulated by Morris Goodman (Wayne State University) and Walter Fitch (University of Southern California) Patterson was also becoming increasingly interested in molecular sequences as additional data to be used in classification. In those early days he used large sheets of squared paper to write out amino-acid sequences of genes (myoglobin and hemoglobins were the usual stock in trade then), align them by eye and carry out the cladistic analyses by hand. Patterson's interest in molecules was not without a specific purpose. In 1974 Mayr had published a fierce defence of evolutionary taxonomy in the face of mounting literature promoting cladistic methodology. Mayr's argument was phrased in the context of shared similarities in genotype – but it was a genotype which at that time had been unexplored. Now, in the late 1970's, with sequences appearing, Patterson was able to test the predictions that Mayr had made. He found them wanting. So he went on to explore the reasons for the mismatch – was there something wrong with the molecules, or something wrong with the theory that gave rise to the predictions? This talk recounts one of his early verbal explorations.

Exploration is fraught with danger attracted by going down paths not previously explored. Patterson was deluged with mountains of mail, phone calls, requests for interviews and comment, especially surrounding the school educational creation vs evolution debates played out interminably in State legislatures. He dutifully carried out all these responsibilities but it must surely have taken its toll on his health. Recalling this talk some 12 years later he wrote .. “Unfortunately, and unknown to me, there was a creationist in my audience with a hidden tape recorder. A transcript of my talk was produced and circulated among creationists, and the talk has since been widely, and often inaccurately, quoted in creationist literature” (Patterson, 1994). In an attempt to put that right we publish the transcript for all to read. Sadly we are unable to give Patterson the chance to approve what is published, but we do have the authority of his own handwritten lecture (The Natural History Museum Archive, Accession number 2002/6) rather than transcript of a tape(s). Appendix notes have been added where we consider there to have been a discrepancy between spoken quotes from others work and that written in the cited papers. Bold text relates either to obvious grammatical laxity slipped into conversational speech or to references inserted to amplify a point.

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Evolutionism and Creationism

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American Museum of Natural History, 11/81

(from the script)

I should warn you that this title was laid on me by Donn Rosen: I'm speaking on it to gratify an old friend. I've never spoken on it before, but I have been kicking non-evolutionary or anti-evolutionary ideas around for 18 months or so. Usually when I get up to talk on some subject I'm confident of one thing – that I know more about it than anyone in the room. I don't have that confidence today: I'm tackling two subjects about which I feel I know nothing at all. One of the reasons I started taking a non-evolutionary view was my sudden realisation after working, as I thought, on evolution for 20 years, that I knew nothing whatever about it: It was quite a shock to learn that one could be so misled for so long. Either there was something wrong with me, or something wrong with evolution, and naturally I suppose there is nothing wrong with me. Over the last few weeks I've tried putting a question to various people and groups of people. The question is "Can you tell me anything you know about evolution – any one statement that seems to be true"? I tried that question on the Geology staff in the Field Museum and got no answer. I tried it on the members of the evolutionary morphology seminar in the University of Chicago. After a long silence, one person said "I know it ought not to be taught in high school." I wonder if anyone here has a better answer? The only other answer I've got from anyone – and I've had this from several people like Ken Campbell and Jim Hopson – is "Convergence is rampant – it's everywhere". I'll come to convergence later: but the level of knowledge about evolution seems to be pretty low – we know it ought not to be taught in high school and that's about all. What about creationism – I suppose we know the same fact about it – that it ought not to be taught in high school – and that's about the lot.

Well, I'm not interested in the controversy over high school teaching, and if any militant creationists have come here looking for political ammunition, I hope they will be disappointed. As an aside, I think the high school evolution-creation controversy is easily solved – all you need is an established religion, which is automatically taught in schools as the Church of England is, and creationists have no ground for complaint. But it's 200 years too late for that solution here. Anyway, I'm not talking about that controversy – this is a systematics discussion group, and I shall talk about evolutionism and creationism as they apply to systematics. And since it's a discussion group, I only want to be outrageous enough to get a discussion going. So I'm not here to sell you tracts. Instead, I shall take my texts from this book – Gillespie's "*Charles Darwin and the problem of Creation*" [1979] – a first rate book, and I want to consider the ways in which two alternative world views – creationism and evolutionism – have affected or might influence systematists and systematics.

Gillespie's book is a historian's attempt to explain the amount of space that Darwin and his colleagues gave to combating the creationist argument. Gillespie shows that

Darwin was trying to replace the creationist paradigm by a positivist paradigm – one in which there was no need for final causes, and no room for them. Of course, Gillespie takes it for granted that Darwin and his disciples succeeded, and that a rationalist view of nature has replaced an irrational one. And of course, I took that view for granted until 18 months ago, when I woke up, and realised that all my life I had been duped into thinking of evolution as revealed truth. From my new viewpoint, some of Gillespie's comments seem very apt, when I transpose them from the period at which he is addressing, the 1850s, to the 1970s and 80s.

Here's a quote from Gillespie:

"The old scientific episteme had sanctioned, or so it appeared from the new perspective, a pseudoparadigm that was not a research-governing theory, since its power to explain was only verbal, but an antitheory, a void that had the function of knowledge but as naturalist's increasingly came to feel, conveyed none." [Gillespie, 1979, p. 8]¹.

Here, Gillespie is characterising the pre-Darwinian creationist paradigm, but I feel that what he says can just as well be applied to evolution as we understand it today: let me repeat part of the quotation – "not a research-governing theory, since its power to explain is only verbal, but an antitheory, a void that has the function of knowledge but conveys none". Of course, you will feel that I am merely misguided or malicious to characterise evolution in that way, but I hope to show you that there is something in it, at least where systematics is concerned.

Gillespie says that creationism can't be a research-governing theory since its power to explain is only verbal. Evolution certainly seems to be a research-governing theory, in systematics as in the rest of biology, but is its explanatory power any more than verbal? In systematics, the research-governing element in evolution is common ancestry, or descent with modification and divergence. Those of you who were at the Hennig meeting in September may remember that Ron Brady and I both quoted, without collusion, the same statement – "the explanatory value of a hypothesis of common ancestry is nil", a statement Brian Goodwin attributed to E.S. Russell in his 1916 book "*Form and Function*"². Thinking about it since then, I feel that the effect of the hypotheses of common ancestry in systematics has not been merely void, as the quotation suggests, but actively misleading. I'll come back to that later. Gillespie also says that creationism is "an antitheory, a void that has the function of knowledge but conveys none". What about evolution – it has the function of knowledge but does it actually convey any? We are back to the question I have been putting to people – "Can you tell me one thing about evolution?" The absence of answers seems to show that indeed no knowledge has been conveyed. Here we all are with shelves full of books on evolution, and yet we seem to have learned nothing from them, as I learned nothing from what I thought was over 20 years work on evolution. Gillespie's comment – "A void that has the function of knowledge but conveys none" [Gillespie, 1979, p. 8] seems very apt.

In systematics, there are pieces of evolutionary knowledge that all our heads are stuffed with, from the most general – statements like "eukaryotes evolved from

prokaryotes”, or “vertebrates evolved from invertebrates”, down to lower-level statements like “man evolved from apes”. I imagine that by now you all appreciate that such statements exactly fit Gillespie’s description – “voids that have the function of knowledge but convey none”. Because when analysed, all such statements say that there is a group – a real group with characters – eukaryotes, vertebrates, *Homo sapiens* or whatever, opposed to a non-group – prokaryotes, invertebrates, apes, or whatever – abstractions that have no characters, no existence in nature, and that cannot convey knowledge, although they appear to.

So in general, I want to suggest two themes. The first is that evolutionism and creationism seem to have become very hard to distinguish. I’ve just been showing how a vicious characterisation of creationism seems to apply just as well to evolutionism. As you all know, there is some sort of revolution going on in evolution at the moment – it concerns not the fact of evolution as a general theory, but the mechanism – the means by which change occurs. Natural Selection is under fire and there’s a rash of new and old alternative theories. Here’s Gillespie again on creationists in the 1850s –

“Frequently, those holding creationist ideas could plead ignorance of the means and affirm only the fact.” [Gillespie, 1979, p. 21³]

That seems to summarise the feeling I get in nattering to evolutionists recently – they plead ignorance of the means by which evolution occurs, but affirm only the fact that it does occur – another instance of creationism and modern evolutionism converging or being hard to distinguish. Here are a couple more quotes – Gillespie on creationism – saying things that seem to me to apply just as well to evolutionism today – “the supposed creditability of the theory was merely the result of familiarity” [Gillespie, 1979, p. 34]⁴. Another quote:

“Too much of the content of the old science was the result of intuition that was in principle unverifiable, either directly or indirectly” [Gillespie, 1979, p. 54].

Another, on the change in worldview associated with the spread of evolutionary thinking –

“Just as science shifted from a theological ground to a positive one, so religion among many scientists and laymen influenced by science shifted from religion as knowledge to religion as faith” [Gillespie, 1979, p. 16]⁵.

I think many of us here would acknowledge that during the last few years, if we’ve thought about it at all, have experienced the shift from evolution as knowledge to evolution as faith. So that’s my first theme – that evolutionism and creationism seem to me to be increasingly difficult to tell apart.

My second theme is that evolution not only conveys no knowledge, but seems to convey antiknowledge, apparent knowledge which is actively harmful to systematics. I want to illustrate that with a couple of parables. Both concern a diagram that I expect to be immediately familiar to you when I put it up (Fig. 1). Recognise it? It’s the diagram that Ernst Mayr has used repeatedly in his explanation of the true method of systematics;

what he calls evolutionary systematics. My first parable concerns a version of the diagram that looks like this. The marks along the lines are autapomorphies, except for this one x [b in original figure] which is a synapomorphy of B and C. This comes from Mayr's paper in *Science* last week [Mayr, 1981] – C is man, B is chimpanzee, our sister-group according to Mayr, and A is not named, but I assume it is gorilla. Here is what Mayr says:

"The main difference between cladists and evolutionary taxonomists is in the treatment of autapomorph characters. Instead of automatically giving sister groups the same rank, the evolutionary taxonomist ranks them by considering the relative weight of their autapomorphies... [then he refers to this diagram]" [Mayr, 1981, p. 514]⁶.

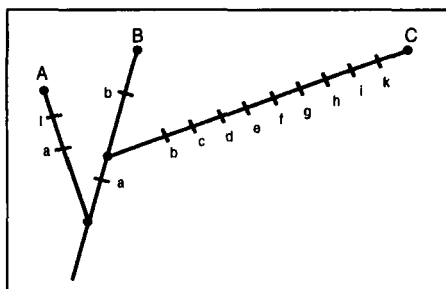


Figure 1. Diagram from Mayr (1981: Fig. 1)

He goes on:

"one of the striking autapomorphies of man in comparison to his sister group, the chimpanzee, is the possession of Broca's centre in the brain, a character that is closely correlated with man's speaking ability. This single character is for most taxonomists of greater weight than various synapomorphies between man and apes. The particular importance of autapomorphies is that they reflect the occupation of new niches and new adaptive zones that may have more biological significance than synapomorphies" [Mayr, 1981, p. 514]⁷.

Well, there are several things one might say about that statement. Not all of them would be polite so I'll just point out that both the statement and the diagram are intended to imply knowledge of evolution – the diagram in the different angles of these lines, and the statement in referring to things like new niches and adaptive zones, biological significance of certain characters, and so on. But when I first read that passage, it immediately reminded me of something, an episode in the history of evolution that many of you will recall, called 'the great hippocampus question'. The great hippocampus question is recorded in fiction in Charles Kingsley's children's book *The Water Babies* [Kingsley, 1863]: in fact, not fiction, it was a controversy that lasted through 1861 and 1862 between T.H. Huxley, the evolutionist, and Richard Owen, the creationist [Kingsley, 1861, 1862; Gross, 1993; Rupke, 1994, p. 270–282; Desmond, 1994, p. 292–311]. Owen insisted that man was quite distinct from the apes, and couldn't be related to them by descent or any other physical link, because the brain of man contained a centre, the hippocampus, that was absent in apes. T.H. Huxley insisted that man was related to

apes, and that apes had a centre homologous to the hippocampus in their brains. The row went on for two years, and in the end and as usual, Huxley won. But here we are, 120 years later, and we have Mayr, the evolutionist, insisting that man is quite distinct from the apes – because the brain of man contains Broca's centre, that is absent in apes. Mayr goes on to cite J. Huxley's kingdom *Psychozoa* [Mayr, 1981, p. 514]. Sounds familiar, doesn't it. And yet notice how the roles have become reversed. The part of Owen, the creationist, is now taken by Mayr, the evolutionist, citing Julian Huxley, the grandson of T.H. Huxley. And the part of T.H. Huxley, the evolutionist, is now taken by the cladists, who are now often cited as anti-evolutionists. Halstead, who needs no introduction here, has published a paper called "In bed with the creationists", in which he calls me "a devoted disciple of Sir Richard Owen" [Halstead, 1981, p. 159]. So be it.

The wheel has come full circle – the evolutionist is now taking just the stand that the creationist took in 1860 – Broca's centre equals the hippocampus. This parable reinforces the point I was making earlier, that evolutionists and creationists are now hard to distinguish. But I want to use it to make another point, about evolution being an antitheory that conveys antiknowledge, that is harmful to systematics. Look what Mayr is recommending – he recommends that man be maintained in a taxon of high rank, distinct from the apes. And look at what prompts him to that recommendation – it is his apparent knowledge of evolutionary theory and fact, which tells him that man has evolved at an exceptional rate, and in an exceptional adaptive direction, justifying the use of the taxon of high rank. And then look at the consequence of his recommendation – man is removed in a taxon of high rank, and the apes are left as a paraphyletic group – a group without characters, and therefore with no individuality or reality, and therefore an abstraction that is beyond criticism. And what can one then say – that man evolved from the apes, a statement that has the appearance of knowledge but in fact contains none – a piece of antiknowledge, derived from evolutionary theory. Rather than comment, I will quote what T.H. Huxley said of Owen in 1861 – Here is Huxley writing to Hooker, April 27th 1861, on Owen and the hippocampus question:

"I do not believe that in the whole of history of science there is a case of any man of reputation getting himself into such a contemptible position" [Huxley, 1913, p. 277].

Well, the position may or may not be contemptible, and the pleasure merely momentary, but what is more interesting is to go on to my second parable – To return to this diagram, and think of the way in which Mayr previously used it. If you remember, in his 1969 book [Mayr, 1969, p. 256, Fig. 10.17] and his 1974 anti-cladist paper, the diagram looked like this (Fig. 2). Mayr was trying to put some precision into evolutionary systematics, and said let A be the common ancestor of BCD, and suppose that the genome of B has deviated by 15% from the ancestral genome A, C has diverged from A by 10%, and D has evolved rapidly and diverged from A by 70% of the genome, entering a new adaptive zone. Then, he said, we should be quite wrong to classify C with D, its nearest relative by common ancestry, because relationship means the inferred amount of shared genotype, not the inferred recency of common ancestry – here B + C share 75% of the ancestral genotype, whereas C and D share only 30%.

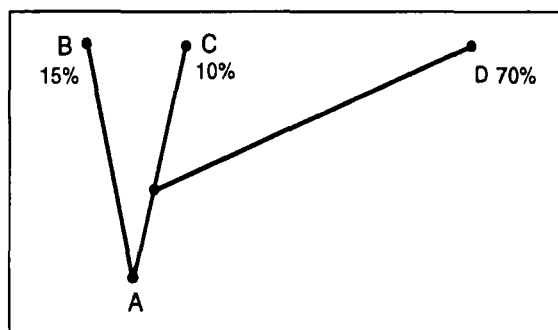


Figure 2. Diagram redrawn from Mayr, 1974: fig. 1 left.

To show the kind of mistakes that might be made, Mayr said that a foolish systematist might group crocodiles with birds, rather than with other reptiles, or might group African apes with man, rather than with the orang utan. So Mayr, from his knowledge of evolution, was making predictions about the genotypes of crocodiles and of African apes. In the first example, crocodiles, he predicts that the proportion of genotype shared by C, crocodiles, and B, other reptiles will be greater than the proportion of the genotype shared by C, crocodiles, and D, birds. So, in terms of genotype, $BC > CD$. Notice here that Mayr is demonstrating the explanatory power of a hypothesis of common ancestry, something I suggested had zero explanatory power, and that he is demonstrating his knowledge of evolution. Right – here is a theory that makes a prediction, and we can test it. Last month in Ann Arbor a student of Morris Goodman's gave me the amino acid sequences for α and β haemoglobin of three crocodiles, a caiman, the Nile crocodile and the Mississippi alligator. We know the α and β haemoglobin sequences of two birds, chicken and goose, but unfortunately we're still very short of other reptiles – the only one I know of is the sequence for a viper. α haemoglobin is 143 amino acids long – it represents $3 \times 143 = 429$ nucleotides – a tiny fragment of the genotype but worth checking. The prediction is that the amino acids common to B, the viper, and C, the crocodile, will be more numerous than the amino acids common to C, the crocodile, and D, a bird. If B – viper, C – caiman, D – chicken we get $BC = 8/143 = 5.6\%$, $CD = 25/143 = 17.5\%$ (Same amino acid at a given position in viper and chicken). And the third possibility – same amino acid in viper and bird – $BD = 15/143 = 10.5\%$. So Mayr's prediction is falsified, decisively. Something was wrong with it. But of course, falsification is never absolute, because you are never sure what it is you have falsified. Here, there seem to be only three possibilities. 1st – you've falsified the data – there is something odd about haemoglobin. 2nd, you've falsified the diagram – there was something wrong with the scheme of relationships. And 3rd, you've falsified Mayr's knowledge of evolution, either the particular stuff about rates and adaptive zones, or something more general.

Well, we can check one of these three – the data, by taking another sample of the genome. We know the myoglobin sequence for a crocodile, and for two birds, chicken and penguin, and for two other reptiles, a varanid lizard and a turtle. Myoglobin is 153

amino acids long – another 460 nucleotides. If B is the lizard, C is crocodile, D is chicken, for myoglobin, we get $BC = 16/153 = 10.5\%$, $CD = 13/153 = 8.5\%$ and $BD = 16/153 = 10.5\%$. This looks better for Mayr – the BC proportion of the genome is greater than the CD proportion, as he predicted – but hang on a moment – what’s this BD, shared by the lizard and the bird – according to the diagram, that should be next to nothing, but here it is exactly the same as the lizard/crocodile proportion. Something’s gone wrong again – as before, is it the data, or the diagram, or the knowledge of evolution that’s wrong?

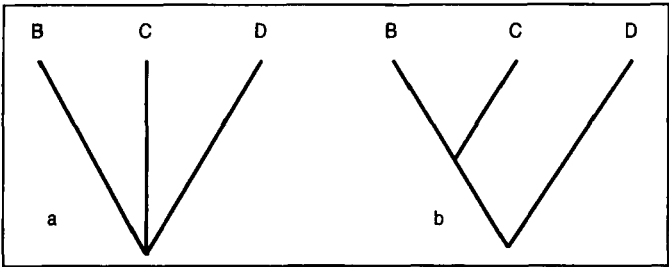


Figure 3.

These [this] data suggest a different diagram – a trichotomy (Fig. 3a). Let’s check again with turtle myoglobin. As before, C is crocodile and D is chicken but now B is turtle. We get $BC = 18/153 = 11.8\%$; $CD = 8/153 = 5.2\%$; $BD = 9/153 = 5.9\%$. Here, at last, is the data Mayr wanted – the BC proportion of the genome is more than twice as large as the CD proportion. But, again, there is a problem. The BD proportion, which should be tiny, is also larger than the CD. So again, something is wrong – is it the data, the diagram, or the knowledge of evolution – **these** [this] data suggest a third diagram (BC)D (Fig. 3b). Either we get the right diagram but the wrong proportions, as here with viper, or the right proportions but the wrong diagram, as here with turtle, or agnostic data and diagram, as here, with lizard. There are no more crocodile or lizard sequences available to check with, so what do we do. There are two things we might do. One is to accept Mayr’s assurance that reptiles are a group, and sum the data [**table constructed from notes**]:

	BC	CD	BD
viper α Hgb	5.6	17.5	10.5
lizard Mgb	10.5	8.5	10.5
turtle Mgb	11.8	5.2	5.9
Totals	27.9	31.2	26.9

I suggest that only a parsimonious maniac would attach any importance to the difference between these numbers – effectively, they are all the same, and the diagram you get is this (Fig. 3a):

The other thing we might do is accept Gary Nelson’s optimistic view that every set of data is a glimpse of the truth [Nelson, 1979], and combine the 3 diagrams. This is what we get:

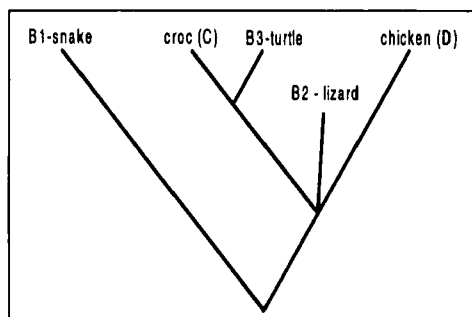


Figure 4.

Do you like that better? I assume not. So what do we do? I'll play one more of these games, and then drop it. We have no more reptile sequences but we have plenty of mammal sequences – let's try one. If we put mammals into Mayr's diagram instead of non-crocodilian reptiles, I hope I do him no injustice if I guess that the diagram should look like this:

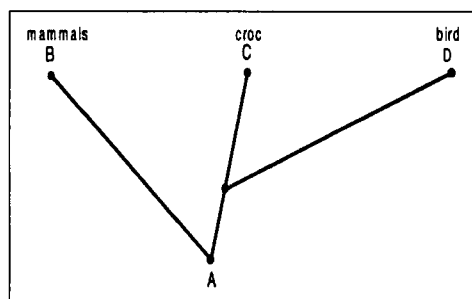


Figure 5.

– both mammals and birds should have diverged strongly from the common ancestor A entering different adaptive zones, whereas crocodiles will have stayed relatively close to A, as before. OK – with haemoglobin, if B is man, a mammal, C is caiman, a crocodile, and D is a chicken again, this is what we get:

BC	Man + crocodile	11/143	7.7%
CD	Crocodile + bird	11/143	7.7%
BD	Man + bird	21/143	14.7%

What does that mean – BC should be smaller than CD, and BD should be minute, yet BC and CD are equal, and are far outweighed by BD. Well, I don't know what an evolutionist would do with this, but I can guess – the only answer I got from people when asking them what they knew about evolution was “convergence is rampant” – and this would surely be treated as another example – mammals and birds have entered the endothermic adaptive zone, after all, so their haemoglobins might converge – and you redraw the diagram like this:

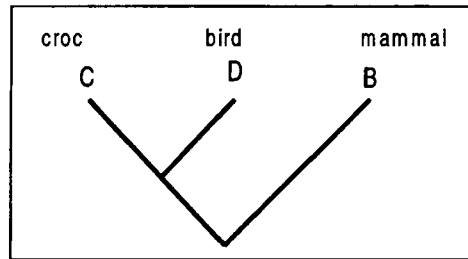


Figure 6.

Yet that won't do, for the BC and CD proportions of the genotype are the same – you have to draw it like this:

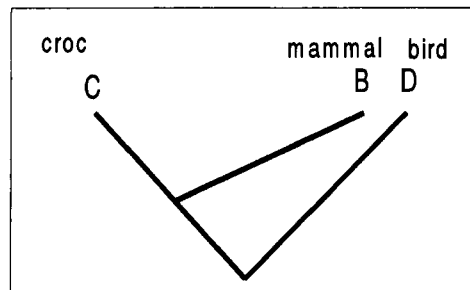


Figure 7.

Yet that won't do either, for in the case B + D should have converged so close that they are identical, yet they each differ from the turtle by a different 7.7% of the sample, and so have to be at least 15% apart. The question seems insoluble. We can, however, see that **these** [this] data give[s] a different cladogram: not ((C D)B) but ((B D)C) and can add mammals in the appropriate place here as the sister group of birds. Do you like that? No I thought not. So what do we do? Well luckily I don't have to keep asking these rhetorical questions, because I'm discussing Mayr's example, and I know what he did. If you remember, Mayr published this diagram, with proportions of genome in it, in 1974, when there were very few samples of genomes available in the form of protein or nucleic acid sequences, and he offered no examples that should conform to it – the bird crocodile one, and the man ape one [Mayr, 1974]. Well, since 1974 we've had abundant samples of the genome in man and apes, and far from matching this diagram, it turns out that chimps and people differ by only about 1% of the molecular genome. So Mayr's prediction was falsified there. As I said before, falsification is never absolute, and in this case I suggested that there are three possible things that might be false here – the genome data, the diagram, or the claims about evolution. Well, Mayr still believes the diagram is correct, and that his knowledge of evolution is correct, so all that can be wrong is the data, so he dropped the genome and returned to morphology, and to Broca's centre and the hippocampus question, or its modern equivalent. There are two points to

be made here. The first concerns another of the parallels between evolutionism and creationism: when, back in 1974, Mayr appealed to the genotype as the holder of true knowledge, the genotype was still very much a mystery. Now that samples of the genotype are available in a wide variety of organisms, so that it is no longer quite so mysterious, it is dropped and a new mystery is introduced, Broca's centre and a long chain of unspecified morphological autapomorphies – just like creationists, evolutionists are liable to appeal to mystery.

The second and more important point concerns the levels at which we can investigate characters. The traditional level is morphology. We are all pretty familiar with morphological data, and feel competent to handle it, despite its complexity. We have a good grasp of what homology means at the morphological level, and we have the transformations of ontogeny as a guide in ordering characters into transformation series. Back in 1978 Gary Nelson suggested that – I quote “the concept of evolution is an extrapolation, or interpretation of the orderliness of ontogeny” [Nelson, 1978, p. 336]. So far as I know, at the morphological level that is true, and as Gary said, it is von Baer's law that ontogeny moves from the general to the particular, that is behind the transformations we invoke in morphology, and the systematic hierarchy we have built on homologies ordered in that way. Of course, not all transformations we invoke are directly observed in ontogeny, but all have to be congruent with the ontogenetic transformations – the only exception is inferred secondary loss, which can be regarded as truncated ontogeny. So at the morphological level, we have a sound concept of homology, and ontogeny to guide us in ordering polarity of homologies. Morphology or in more general terms, the phenotype, is the highest level of investigation in systematics.

The next level down is the level of gene products – proteins. There the concept of homology becomes vaguer. In the first place, it is beset with the problem of paralogy. Paralogy is what people who play with protein sequences call the relation between gene products that they think are the result of gene duplication. Paralogy is the molecular version of serial homology in morphology. The difference is that in morphology you can be fairly sure whether two structures are serial homologues, because you have ontogenetic evidence of duplication. But at the level of protein sequences, the inferred gene duplication is lost in the past, and paralogy seems sometimes to be invoked to explain away awkward data. In any case, when comparing two or more protein sequences as a whole, rather than amino-acid by amino-acid, homology for molecular biologists is a purely statistical concept – if two sequences have more amino acid matches than would be expected by chance, they are homologous, and there's the end of it – a paper by Doolittle in *Science* last month explained the concept [Doolittle, 1981]. Having decided that two sequences are homologous, they can be aligned and compared position by position. A match at any one position, that is, the same amino acid, is a homology at a finer level, but here the problem is whether the amino acid is really “the same” or not. Because of the redundancy of the genetic code, there are only 2 amino acids, tryptophan and methionine, that are coded by a single triplet, and are therefore always “the same”. These are amongst the rarest amino acids, making up less than 2% of the average

sequence. All the other amino acids are coded by two or more triplets, so that at the gene product level, a match can hardly ever be treated as a homology at the DNA level. So at the gene product level, homology becomes a pretty vague concept, and we have no ontogeny to order transformation series. I used to think that because there is no ontogeny in proteins, yet we seem to need the notion of transformation to order protein sequences, they provided some sort of proof of evolution [Patterson, 1980, p. 236]. I'm no longer sure that follows, because the homologies we infer, and the transformations we infer in ordering them into transformation series, are based on ambiguity – the ambiguity of the genetic code, and the statistical nature of molecular homology. The real molecular homologies must lie at the third and lowest level, DNA.

At the DNA level, we still know very little, because there is still hardly any comparative data, in the form of sequences that can be aligned and compared. But last Saturday, at Ann Arbor, through the kindness of Arnold Kluge, I laid eyes on what I think is the first such data set, sequences of mitochondrial DNA of man, chimp, gorilla, orang and gibbon, done by Prager and Wilson's group at Berkeley [Brown *et al.*, 1982].

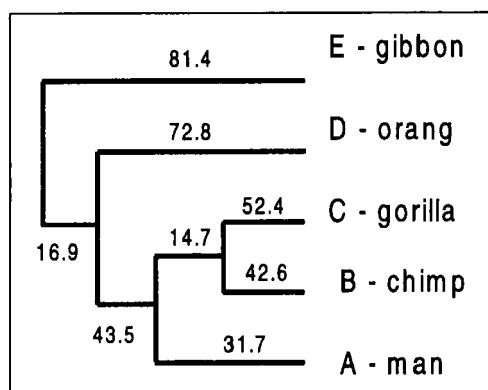


Figure 8.

The sequences are 896 nucleotides long, and include parts of two genes. When I saw these, there was much more order than I expected but of course you expect more order in the form of matches in nucleic acids than in proteins because there are 20 amino acids in proteins, so you only get 5% chance matches but there are only 4 nucleotides so you expect 25% chance matches. But I'm talking about systematics, so let me ask you what evolutionists and a creationist systematist make of them. Amongst these 896 nucleotides, 612 are invariant and 284 show some variation. This is the most parsimonious tree the Berkeley people got (Fig. 8) where A – man, B – chimp, C – gorilla, D – orang, E – gibbon. The numbers are evolutionary events per lineage, and the tree says a lot about evolution – for example, that gorilla has evolved fastest, man slowest, that mt DNA evolves about 10 times as fast as nuclear DNA, and that silent substitutions are several times as likely as coding substitutions, which is a conclusion noted in every comparison of DNA sequences that's yet been made.

OK – that’s what evolutionists make of it. Here’s what a creationist makes of it. There are five species, ABCDE, and the first class of characters is those picking out four of the five: here is how they stack up:

Nucleotides shared among four of the five species⁸.

ABCD	66
ABCE	53
ABDE	21
ACDE	19
BCDE	14

Nucleotides as autapomorphies.

E	66
D	53
C	21
B	19
A	14

There are two strong signals here – ABCD and ABCE, each of them far outnumbers the others. But the two groups they form are incongruent, so they both can’t be true. Thus there is no useful grouping to be found here, so it’s better to treat these as indications of the autapomorphies of the five species. Notice that man, A, has the fewest autapomorphies, despite Mayr’s expectation.

The next class of characters is those picking out three of the five. There are ten sets, and this is how they stack⁹:

ABC	30
ABD	11
ACD	10
CDE	10
ADE	9
ABE	7
BCD	7
BDE	4
ACE	3
BCE	3

There is one strong signal here, that A, B and C make a group. Since there are ten ways of picking three taxa out of five, the probability of a repetition by chance is one in ten – this group has 19 repetitions more than its nearest rival, so the probability of this signal being due to chance is 10^{-18} . Beyond that grouping, the rest of this has nothing to say. The last class of characters is those picking two of the five species – Again there are ten sets here’s how they come out¹⁰:

DE	31
AB	14
BC	12
AC	9
CE	8
CD	7
BE	6
AF	5
BD	4
AD	3

Once again there is one strong signal – D and E form a group, with a probability of the distribution being due to chance of 10^{-16} . Amongst the rest of this, there is a weak signal that A and B go together but since BC, which is incongruent with AB, is next in rank, and the difference in probability is only 1 in 10, I think only a fanatic of parsimony would think it significant.

So the information that's in this DNA, to me, is that there are two groups ABC and DE and together they form a larger group. If you prefer it in cladogram form [Fig. 9]. And amongst the autapomorphies, they are ranked in this sequence – E down to A, which is incidentally the reverse of Mayr's prediction.

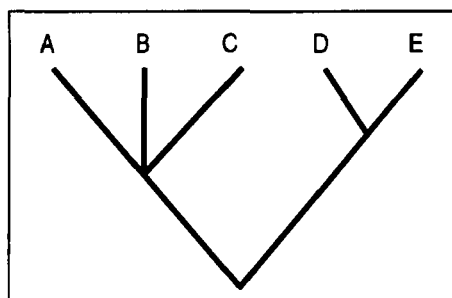


Figure 9.

I don't know if you will all agree, and by all I mean Steve Farris, but as far as I can see that's all the information that's in the data – there isn't any more. So what about this tree and the numbers on the branches, and so on – where do they come from? Well, they don't pop out of the data, so I suppose they come from massaging the data with a theory – or with a computer programme based on a theory; and the theory is evolutionary theory, descent with modification. So what does the tree tell us about – is it telling us something about nature, or something about evolutionary theory – I'll leave you to decide.

At this level, of DNA sequences, there is the question of homology, just as there is at the level of morphology and of gene products. What does homology mean at the DNA level? The procedure is the same as at the gene product level – sequences are homologous

if they can be aligned to match at a level that passes some statistical test, but with only four possible nucleotides at each position, you expect a match of 25% by chance alone, and among these 5 very closely related species there is only a 70% match, so that leaves 45% of the possible matches to accommodate the mitochondrial DNA of all other eukaryotes. Obviously, as DNA sequences begin to come in in quantity, there are going to be severe problems with decisions on sequence homology. Thus, at the level of individual nucleotides, I suppose that a match, say adenine at one position in two or more organisms, is a homology, and a mismatch is a non-homology.

Bearing that notion of homology in mind, lets look again at this tree and **these** [this] data – the tree tells us that the BC matches – these 12 – are the homologies, and that the matches that are incongruent with them – the 14 ABs and the 9 A[B]Cs, are non-homologies. So we have 12 homologies here, and $12 + 9 = 23$ non-homologies among these 3 taxa. No wonder convergence seems to be rampant. But what does it mean to say that these 12 matches are homologous and the 23 others are not? It means that somehow these 12 adenines and cytosines and so on are the same, and these other 23 are not the same, but what does that mean? Presumably all adenine nucleotides are identical and so are all cytosines – it seems nonsensical to say that 12 of these identical nucleotides are the same and 23 are not the same, because that is what the tree demands. But so it goes. It seems that evolutionary theory makes us throw away more than half the DNA data as non-homology, but that the non-homology is only specifiable in some metaphysical way, as identity that is not real identity – do you see the point I am trying to make? Evolutionists seem forced to regard a large part of the data as not merely neutral, but positively misleading, and misleading data, bad data, is given a bad name – convergence. I don't think the creationists have the same problem. To a creationist, all the matches here are matches, they are all really the same, and they give a clear message – there are two groups – ABC and DE – I don't have to throw away a large part of the data as misleading.

So far as economy of hypothesis is concerned, I think the creationist approach wins hands down – it asks that there be some groups, that the groups be non-overlapping, and that the groups have characters. OK, here are the groups, and here are the characters.

The evolutionist approach is that there are some groups in here, and some information about their historical relationships – some information about evolution. The extra part of the hypothesis introduces extra problems with the data, and the inference that much of it has been produced by a sort of anti-evolution, what is called convergence or parallelism.

There is one more problem with homology at the DNA level. You recall that at the level of gene products, of protein sequences, there was the problem of paralogy, or presumed gene duplication – the equivalent of serial homology, as against what molecular biologists call orthology, the equivalent of true homology at the morphological level.

There is a similar problem in sight at the DNA level – a couple of weeks ago Roger Lewin had a piece in *Science* on globin genes, and in it he touched on the model of DNA that is becoming fashionable amongst molecular geneticists [Lewin 1981]. He called it

the ‘Vesuvius’ model, and the short description of it in that article and in others is that every gene is constantly bombarding the genome with pseudogenes – more or less perfect copies of itself. If this is so, and the model has empirical support in the work of people like Gabriel Dover, then the problem of paralogy at the DNA level is a real one. DNA sequencing is done by mincing up the genome, and cloning a bit that you hope is the right one. But if each gene is constantly throwing out pseudogenes, you may well clone something that is not the right thing. I don’t want to dwell on this, because I know nothing about it; all I want to say is that there is an equivalent of paralogy at the DNA level, and that all in all the concept of homology seems to get vaguer as you move down from the phenotype level to the gene product level, and then to the DNA level. What is the significance of that? Well, it brings me back, by a roundabout route to evolution. There has been a good deal of discussion over the last few years on whether evolution is a testable theory. Here I mean the general theory of evolution, that species are mutable and related by descent, rather than any specific theory about the mechanism that is responsible for transformation of species.

If the general theory of evolution is testable, it must have some consequences that can be confronted with reality – in other words, it must make some prediction. So far as I know, only one sensible prediction has been offered. Niles Eldredge put it like this in a letter to *Science* –

“If evolution is descent with modification, a hierarchical array of organisms, defined by nested sets of evolutionary novelties *must* result – this is evolution’s grand prediction” [Eldredge, 1981, p. 737]¹¹.

Then Niles went on to say that whatever organisms you look at, and whatever aspect of them you study, you find the same hierarchy. I’ve heard the same point made repeatedly at meetings – that there really is a hierarchy, and there can be no hierarchy without history, and therefore the prediction of evolution is met.

There are several things one might say about this. One is that it seems to imply that evolution is a deductive inference from the systematic hierarchy – that Linneaus and Cuvier, and Agassiz, and Von Baer, and Johannes Müller, and Hooker, and a thousand other pre-Darwinians were merely poor thinkers if they failed to see the inevitable consequences of their observations. That seems to me improbable.

The second thing one might say concerns the prediction that whatever aspect of organisms you look at you find the same hierarchy. Not everyone seems to agree. Here is Ernst Mayr in *Science* last week:

“Different types of characters – morphological characters, chromosomal differences, enzyme genes, regulatory genes and DNA matching, may lead to rather different groupings. Different stages in life cycles may result in different groupings” [Mayr, 1981, p. 511]¹².

And here’s Arnold Kluge, the conclusion to his cladistic study of apes and man: He says it “denotes a clear lack of congruence between molecular and other, more traditional kinds of data.” [Kluge, 1983, p. 173]¹³.

Now the prediction of evolution is hierarchy, and congruent hierarchy no matter what aspect of species you look at. The experience of Mayr, and of Arnold Kluge is that there is no such congruence – in particular the molecular data are not congruent with morphology. Is that so? I'm not sure, but there are other signs of it amongst the protein sequences, for example, the cladogram of amniotes that I was building up earlier which was widely incongruent with the accepted morphological cladogram, and there are published globin cladograms that have birds as the sister group of mammals, not of crocodiles, and snakes as the sister-group of other amniotes. Never mind that – let me put the question at a more basic level – how we recognise the hierarchy. At the level of the phenotype, I think there is no real problem. We have a fairly rational concept of homology, and I agree with Gary Nelson that the organising principle is ontogeny, and Von Baer's laws in particular. As Gary suggested, and as I have shown, at least to my own satisfaction, we can analyse phenotypic data in that way and get a hierarchy by a method that has no evolutionary implications at all: there is history in there, but the history is ontogenetic, and there is hierarchy in there, but the hierarchy is tied to the hierarchical nature of ontogeny.

What about the molecular level, the level of proteins and DNA sequences: how do we recognise hierarchy there, where homology is more vague and we don't have ontogeny and Von Baer to guide us? I suggested, in my comments on the DNA sequences, that the hierarchy is recognised by massaging the data with evolutionary theory, which obviously prevents it being used to corroborate that theory. Is the data hierarchical without massage of that sort? I don't know. At the protein sequence level, where I have played about a lot, my impression is that it is strongly hierarchical when all you have is a few sequences, or if you just select a few sequences and play with 4 or 5 taxon problems. But when you take a big set of data, like all the myoglobins now available, my experience is that the hierarchy melts away unless you force it by massage with descent theory.

At the DNA level, nobody yet knows. All we have is the 5 short sequences I talked about: they are hierarchical, without massage, to this extent ((ABC)(DE)). My prediction is that as the DNA sequences come in, the hierarchy will melt away much faster than it does in the proteins, because there are only 4 variables instead of 20. If that is so, what is it that puts the hierarchy into the phenotype data, if it isn't there at the protein or DNA level? Plainly it is ontogeny, which is itself hierarchical, and Von Baer's law. So what of the prediction of evolution – that there is a natural hierarchy. In one sense, there is, but it is at the phenotypic level and derives from the order of ontogeny, and can be investigated without any preconceptions about evolution. Then why is evolutionary theory necessary in systematics? I end up with the odd conclusion that it is necessary to impose hierarchical order on recalcitrant data – it is a massage machine to impose hierarchy where none may exist. If that is so, don't you think systematics would be better off without the theory?

I'll end with 4 quotes. The first is from A.J. Cain, his paper at the 150th anniversary of the British Association reported in *Nature* – you may remember this as the paper in which he called cladistics “this ridiculous scheme” [Cain, 1981, p. 16]. Well, he also said:

“If there is a problem left over from 1831, and still unsolved, it is indeed the recognition of true affinity” [Cain, 1981, p. 16].¹⁴

I suggest that the reason he finds it hard to recognise is that he has to look for it through the evolutionary telescope.

The second quote is from Arnold Kluge’s paper on man and apes:

“My view of hominoid relationships is that much research remains to be done, and that we are little closer to the natural system of classification of man and his ancestors than when Darwin wrote” [Kluge, 1983, p. 174].

Whereas Cain can’t recognise true affinity, Arnold can’t recognise the natural system – the right data hasn’t yet come along. As his use of the word ancestors implies, his problem is the same as Cain’s – theory of evolution has got between him and the data.

The third quote is from Darwin, in the *Origin*.

“When the views entertained in this volume are generally admitted, systematists will be able to pursue their labours as at present” [Darwin, 1859 [1964], p. 484].

By “at present”, Darwin means as in pre-Darwinian times, as in pre-evolutionary biology – he is saying – don’t let my theory get in your way. People seem not to have listened to him.

The final quote is from Gillespie again, and it concerns J.D. Hooker, the botanist. If you think about it, Hooker was the only professional systematist amongst the Darwin coterie. Hooker was also Darwin’s oldest confidant, and had been fully aware of his work and had read all his manuscripts since the early 1840s. Yet Hooker remained unconverted to evolutionism, until he finally succumbed in 1858. Here is Gillespie on the reason for this:

“Hooker adopted the view that species were immutable, and each descended from a single pair. This was not necessarily his belief, but a methodological postulate to make classification possible” [Gillespie, 1979, p. 49].¹⁵

Gillespie makes the point again later in his book like this:

“Hooker believed that the taxonomist who was an evolutionist must ignore this theory, and proceed ‘as if’ species were immutable” [Gillespie, 1979, p. 65–66].

In other words, evolution may well be true, but basing one’s systematics on that belief will give bad systematics. Since so-called ancestral groups are the invention of evolutionists, as is the belief that trees not cladograms are the proper level of analysis, I think Hooker was right.

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Notes referred to by numbered superscripts in the text:

1. "The old scientific episteme, creationism, which mixed the Newtonian nomothetic and the Baconian inductionist traditions from the physical sciences with biblical theology and a type of philosophical idealism, had sanctioned, in the idea of special creation, or so it appeared from the new positive perspective, a pseudoparadigm that was not a research governing theory (since its power to explain was only verbal) but an antitheory, a void that had the function of knowledge but, as naturalists increasingly came to feel, conveyed none." Gillespie, 1979, p. 8.
2. Webster and Goodwin. 1981, p. 42: "While it may be true, as a matter of fact, that different organisms do have a common ancestor, the explanatory value of this fact is zero, a point made clear by a number of writers at the beginning of this century [5]." The reference [5] is to Russell, 1916.
3. "More frequently, those holding creationist ideas could [would] plead ignorance of the means and affirm only the fact." (Gillespie, 1979, p. 21) "would" not "could" in original.
4. "...its supposed creditability was merely the result of familiarity, a residue of early religious training" (Gillespie, 1979, p. 34).
5. "Just as science shifted from a theological ground to a positive one, so religion – at least among many scientists and laymen influenced by science – shifted from religion as knowledge to religion as faith" (Gillespie, 1979, p. 16).
6. "The main difference between cladists and evolutionary taxonomists, thus, is in the treatment of autapomorph characters. Instead of automatically giving sister groups the same rank, the evolutionary taxonomist ranks them by considering the relative weight of their autapomorphies as compared to their synapomorphies (Fig. 1)" [Mayr, 1981, p. 514].

7. "For instance, one of the striking autapomorphies of man (in comparison to his sister group, the chimpanzee) is the possession of Broca's center in the brain, a character that is closely correlated with man's speaking ability. This single character is for most taxonomists of greater weight than various synapomorphous similarities or even identities in man and the apes in certain macromolecules such as hemoglobins and cytochrome c. The particular importance of autapomorphies is that they reflect the occupation of new niches and new adaptive zones that may have greater biological significance than synapomorphies in some of the standard macromolecules." [Mayr, 1981, p. 514].
8. These counts differ from those recorded in Brown *et al.* (1982, legend to figure 3). They give: ABCD 64; ABCE 55; ABDE 22; ACDE 19; BCDE 17. Patterson (1988, p. 74 and Fig. 4.2) gives different figures: ABCD 64; ABCE 55; ABDE 22; ACDE 19; BCDE 15
9. Patterson (1988, p. 74 and Fig. 4.2) gives the following: ABC – 31; ABD – 11; ACD, CDE, ADE – 10; ABE, BCD – 7; BDE – 5; ACE, BCE – 3
10. Patterson (1988, p. 74 and Fig. 4.2) gives the following: DE – 32; AB – 14; BC – 12; AC – 9; CE – 8; CD – 7; BE – 6; AF – 5; BD – 4; AD – 3
11. "If evolution is 'descent with modification', as Darwin so elegantly phrased it, a hierarchical array of organisms defined by nested sets of evolutionary novelties (modifications) must result. This is evolution's grand prediction..." [Eldredge, 1981, p. 737].
12. "Different types of characters – morphological characters, chromosomal differences, enzyme genes, regulatory genes and DNA matching -- may lead to rather different grouping. Different stages in the life cycle may result in different groupings" [Mayr, 1981, p. 511].
13. Patterson's reference is to the unpublished manuscript that was published in 1983. "Such a lack of phylogenetic history denotes a clear lack of congruence between molecular and other, more traditional, kinds of data." (Kluge, 1983, p. 173).
14. "If there is a problem left over from 1831 and still unsolved, it is indeed the recognition of 'true affinity', now usually called phylogenetics" [Cain, 1981, p. 16].
15. "Hooker adopted the view that species were immutable (save for local variation) and each descended from a single pair. This was not necessarily his belief, but a methodological postulate to make classification possible" (Gillespie, 1979, p. 49).

Crocodile relationships

The Presidential Chair, emblazoned with the Society's Arms and covered by crocodile skin was inaugurated on 5th November 1925 by Dr A.B. Rendle who proposed a vote of thanks to the anonymous donor. The donor later turned out to be Reginald Radcliffe Cory (1871–1934) a brilliant oarsman, benefactor of the Cambridge Botanic Garden and a strong supporter of the temperance movement. Cory was also a Director of a shipping firm and consequently had access to some of the larger Nile crocodiles coming out of East Africa. Our chair is covered by belly skin from a 22' Nile crocodile (*Crocodylus niloticus*).

The skin of crocodiles, like that of lizards and snakes, cannot grow and is shed at intervals but, unlike lizards and snakes, it is not sloughed off,





Sabek, the crocodile god of Ancient Egypt
(from Ross, Garnett & Pyzakowski, 1989)

instead crocodiles merely loose isolated horny scutes from their armour. The skin of crocodiles is also protected by bony plates or osteoderms; which are arranged mainly along the back, making this area difficult to process. Nile crocodiles from Tanzania and Madagascar are the most sought after since they are medium sized or small-scaled and have no osteoderms in either the flanks or belly, and so produce neater patterns for shoes and handbags. Commercial utilisation of American alligators started in the late 1700's and Audubon commented on the use of alligator skins for saddlebags, boots and shoes. The systematic slaughter of the alligator reached its peak during and immediately after the American Civil War. Nevertheless, they have survived in Florida since the Eocene, and the greatest pressure on them today is habitat destruction.

During the mid 1950s nearly 60,000 Nile crocodile skins were exported from East Africa each year. Today, despite CITES regulations it is calculated that at least 2 million crocodiles, alligators and caimans are killed each year to supply the international skin trade which converts hides into expensive footwear, handbags, wallets and belts.



Embalmed crocodiles
(from Ross, Garnett & Pyzakowski, 1989)

Crocodiles and humans: The ancient Egyptians had some 400 deities of which the most revered, was Sabek the crocodile God. Various temples were erected in his honour and sacred crocodiles were looked after in special pools. When they died they were embalmed and placed in elaborate coffins.

Crocodile evolution spans more than 200 million years beginning in the late Triassic with the earliest forms the Protosuchia. Eventually, in later forms a secondary palate developed, allowing crocodiles to breathe under water. The development of a secondary palate in mammals is presumed to be homoplasy since it is not developed in birds. Some crocodiles (*Geosaurus*) went to sea and developed a tail fin and paddles. The fish-eating habit they adopted is seen today in the gharial or Gavial with its narrow snout and numerous teeth, which is confined to the Indian subcontinent.

Although crocodiles have a worldwide distribution they are mainly found in the Southern Hemisphere. Two exceptions are the alligators from the Everglades and the Chinese alligators found in the lower Yangze River. Although nearly all crocodile species have salt glands, those from the Indopacific appear to have exploited this mechanism to the greatest degree. Their salt glands are on the tongue and appear to be modified salivary glands. The American alligator and caiman have no salt glands.

Life History: Crocodiles build nests out of vegetable material as do birds and monotremes. Moreover, as in birds and monotremes, the eggshell is lime impregnated. Unlike birds and mammals, however, the sex is not determined at the moment of fertilisation. The embryo within a newly laid crocodile egg is without gender. The temperature at which the egg is incubated during the first few weeks in the nest determines if the embryo will develop into a male or female. The critical temperature varies among species; although all are incubated close to 30°C (86°F) it is not clear how they manage to make nests that function as fairly precise incubators. Higher temperatures, 32–34°C, result in male alligators while lower temperatures, 28–30°C, yield females. Likewise, in many turtles and some lizards, sex determination is temperature dependent.

The crocodile female carries the eggs and hatchlings to the water in a pouch in her mouth. Both parents guard them from predatory birds, monitor lizards, etc. The hatchlings gather in crèches as they bask in the sun and maintain contact by vocalising when they disperse to hunt in the evenings. The young produce distinct yelps and grunts. The most vocal singer and talker is the American alligator whose behaviour is designed not only to impress his lady friend but also to establish his territory. Although vocal cords occur in all amniotes (apart from birds, which have a syrinx), parental care in amniotes only occurs in crocodiles, birds and mammals.

Relationships: Although we know a great deal about crocodiles the real problem is who are their closest living relatives? Most people believe that the crocodiles' nearest living relatives are the birds, whereas I believe they shared ancestry with both birds and mammals.

The reason why most people accept crocodiles and birds as sister-groups goes back to Haeckel who, in 1866, regarded mammals as a sister group of the rest of the amniotes. Huxley (1870) and then Lankester (1870) threw their weight behind Haeckel and today the crocodiles are regarded as the birds' most closely related, living relative. However, in one of the earliest dichotomous trees or systematic tables ever produced John Ray (1693) combined birds with mammals on the grounds that both were warm blooded and both possessed two ventricles – that is, a completely divided heart. Since Ray's time it has been shown that crocodiles also have two ventricles and a completely divided heart (apart from the Foramen of Panizza, a secondary perforation formed late in development) just like birds and mammals. Moreover, there is a complete division or segregation of oxygenated and deoxygenated blood.

Crocodiles, like diving mammals and birds, can decrease their peripheral circulation during diving and this reduces blood flow to the muscles while maintaining an

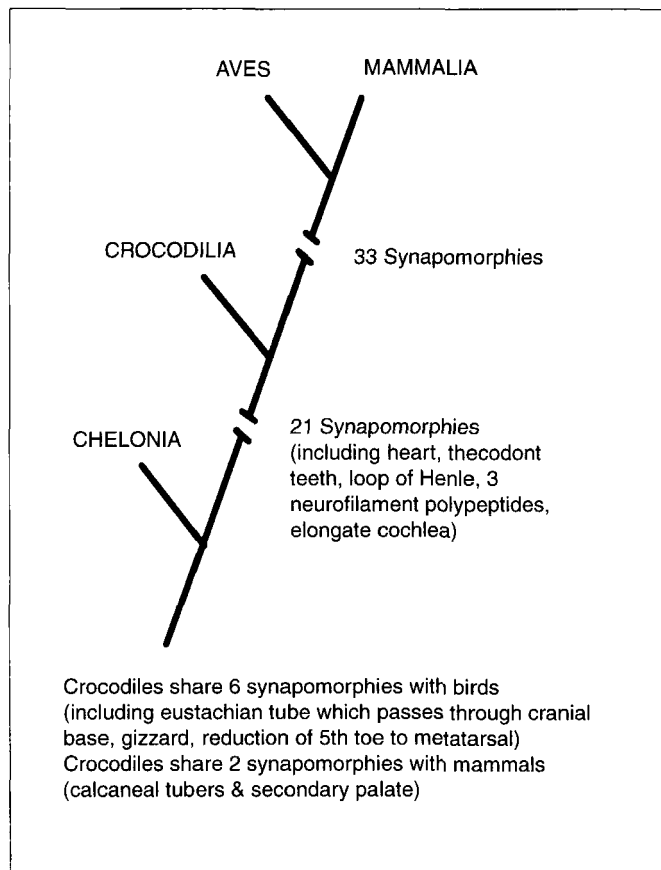


Hypothetical bird ancestor
(from *La Recherche*)

oxygenated blood supply to the heart and brain. Just as in mammals and birds, crocodiles can also adjust their peripheral blood circulation in response to temperature, movement or even fear. Thus, when crocodiles sun themselves, the peripheral blood flow increases and therefore maximises heat transfer from the exterior to the body core. Conversely, a decrease in peripheral blood flow, through slowing down of the heart rate (bradycardia) and constriction of blood vessels (vaso-constriction), reduces heat loss in cool periods. Not only are these thermo-regulatory devices common to birds, mammals and crocodiles but, more importantly, the inter-ventricular septum of the heart has an identical developmental sequence in all three groups, from the endothelial cushion of the atrio-ventricular canal and the bulbar ridges. Further, in all three the left ventricle is thicker and more muscular than the right, which partially surrounds it. Other characteristics of the heart shared by all three groups include the pulmonary artery and aorta, both with three semi-lunar pocket valves (two in all other amniotes).

Teeth: Another unique feature of mammals, crocodiles and fossil birds, such as *Archaeopteryx* and *Parahesperornis*, is thecodont teeth in which there is a true peridental membrane between the tooth and alveolus. Individual teeth are set in deep sockets and replacement teeth develop from below. The life span of the teeth in crocodiles is up to two years for an individual tooth and about one year in a functional position. The front teeth of the American alligator fit into sockets whereas the lower teeth of crocodiles fit outside the upper jaw. The teeth at the front are replaced more frequently than those at the back and there is a pattern of waves of replacement. In mammals the dentition is diphydont, alteration is absent, and a single, partial replacement involves all the teeth in an antero-posterior sequence. In recent birds, teeth are absent and are replaced by a horny beak, but in *Archaeopteryx* teeth similar to those of a crocodile are present; with replacement teeth lying inside their bases. Neither of the monotremes have true teeth in the adult, but the teeth of the fossil monotreme *Oburodon* are typically thecodont.

Lungs: Crocodiles have lungs which are spongy, as in mammals, while they also have a diaphragm with its own muscles, as in mammals and birds.



Amniote relationships (see also Gardiner, 1993)

Walking: Crocodiles, like mammals and birds, can hold their limbs nearly vertical beneath their body, whereas all other amniotes have a sprawled posture. Crocodiles also have a stately high walk and have a true heel bone or *tuber calcanei*. Thus, crocodiles not only run like mammals and birds but have a true heel bone as in mammals. Elsewhere, a true heel bone occurs in stem group birds (coelurosaurs) and most stem group mammals (therapsids).

A considerable external auditory meatus is also found only in crocodiles, birds and mammals while all three have a movable integumentary valve or pinna.

Finally, before summarising the specialisations shared by crocodiles, birds and mammals, I would like to mention the gizzard. Crocodile teeth are designed for seizing and holding prey, not for chewing, so the stomach is in two parts, a muscular gizzard that grinds food and a digestive section. Crocodiles gulp their food (their young eat aquatic insects, tadpoles, frogs, snails, crustaceans etc.). Adult Nile crocodiles consume a full meal every 24 hours, mainly fish. Research has shown that 30% of the stomach is empty, apart from a huge quantity of stomach stones. These act as ballast and allow the crocodile to lie submerged on the bottom of the water. A crocodile 3.8 metres long and weighing 240kg, had 45kg of stones in its stomach, up to a quarter of its submerged weight, which raised its specific gravity considerably.

The Nile crocodile consumes a wide range of mammals in its diet, ranging from cane rats to buffaloes, wildebeest and antelope. Gizzards are also present in many birds – though highly variable – being thin and bag-like in owls and cormorants, for example. Gizzards of graminivorous birds contain numerous small stones. Opponents of the view that crocodiles are the sister group of mammals and birds seized on this evidence, that only birds and crocodiles have a strongly developed gizzard and swallow gastroliths, to deny my proposed relationship (Gardiner, 1982) between crocodiles, birds and mammals. In 1993, as the result of an exhaustive cladistic analysis, I concluded that birds and mammals are indeed sister-groups to crocodiles (see Table 1 in Gardiner, 1993 which lists some thirty-three synapomorphies).

So my final conclusion is that the presidential chair shared an ancestry with both the fellows here present and the pigeons in Trafalgar Square.

This lecture formed the basis of my 1996 Presidential Address.

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APPENDIX

Melanin extruded into special epidermal processes – Analysis

(from Gardiner, 1993)

Neither Kemp (1988a) nor Gauthier *et al.* (1988) were prepared to accept my hypothesis that feathers and hair are part of the same developmental process, while Gauthier *et al.* (1988) even refused to accept the concept of an epidermal-melanin unit, hence the rephrasing of this synapomorphy.

There are strong resemblances between hair and feathers. Both are formed by three morphologically and chemically distinct types of cells with identical patterns of arrangement [cf. the distribution of acid phosphatase in the keratogenous zone and non-specific esterase in the outer root sheath (Spier and Martin, 1956; Braun-Falco, 1958)]. Moreover, although hairs are said to contain only α -keratin (unlike feathers which have β -keratin) the quill medulla of the American porcupine and the pangolin's scales are comprised solely of β -keratin.

The classical distinction between hair and feathers given in most text books is that the feather follicle germs develop as (hollow) outpushings of the epidermis and have a dermal core, in contrast to hair follicles, which arise as solid down-growths from the epidermis into the dermis (Spearman, 1966). In *Ornithorhynchus*, however, the hairs develop as open tubes which then sink deep into the dermis just like feathers.

Hairs, like feathers, are arranged in groups and both are used as organs for sensation of touch. Nerve fibres are wound round the base of the papillae and filoplumes (hair-like structures accompanying the contour feathers) and these transmit pressure changes in birds from contour feathers to Herbst's corpuscles. Mammals and birds also uniquely possess sinus hairs.

Finally, though feather and hair raising are used to control heat loss they are also used especially in association with exaggerated crests or hackles – to signal aggression.

BRIAN GARDINER

Book Reviews

Wimbledon Common and Putney Heath - A Natural History, edited by Tony Drakeford & Una Sutcliffe, Wimbledon and Putney Commons Conservators, London, 2000. x+200pp., £25.00. ISBN 0 95018875 1.

Since moving to London almost 30 years ago I've developed a particular fascination with the natural history of the capital, especially its bird life and certain aspects of urban entomology (in particular its moth fauna). This has not been just for recording purposes, although that is an important aspect, but also for the sheer enjoyment of 'discovering' areas where wildlife can still find a niche in an ever expanding metropolis. However, I must confess that, until now, Wimbledon and Putney Commons did not figure high on my list of localities to visit. Part of the reason for this was that they were

already well covered by the interests of others, many of whom are authors of the various chapters of this book. Reading through this book, I now feel that my decision to look elsewhere may well have been a mistake. It is quite clear that the commons, their history and natural history are worthy of study, and not just that which they have already received. There is a pressing need to continue to discover more about the fauna and flora so that the Conservators, Rangers, Keepers and others who are empowered with its protection, can be kept fully informed and will thus be able to ensure the area's survival for future generations to enjoy.

Although the title-page states "edited" by Tony Drakeford and Una Sutcliffe, it is clear that they are much more than just editors. Between them, they have authored almost half of the text and are also responsible for many of the original photographs for the 200 plus colour illustrations (many of the remaining were taken by Una's husband, Anthony!).

Both authors have had a long association with the commons and, in fact, both now serve on the Board of Conservators. After an initial acknowledgement by the editors and a foreword by David Bellamy the book opens with an *Introduction* by Tony Drakeford. This is followed by 15 chapters in which are described the *Geology and Drainage*, *Six Thousand Years of History preserved in Farm Bog*; *A Disturbed Landscape*; *Wimbledon and Putney Commons Management*; *Terrestrial Habitats of the Commons*; *Fungi and Lichens*, *Aquatic Habitats of the Commons*; *Special Studies of Selected Habitats*, *Butterflies*, *Grasshoppers*, *Bush-Crickets and Dragonflies*, *Beetles and Bumble Bees*, *Fish*, *Amphibians and Reptiles*, *Birds*, *Mammals on the Common*, *Extracts from a Keeper's Diary* and *Habitat Creation and Habitat Restoration* (this final topic is actually concerned with Fishpond Wood and Beverley Meads, an area officially outside the boundaries of Wimbledon Common that is a reserve managed by the London Wildlife Trust). The volume concludes with an *Epilogue*, *Further Reading* and an *Index*. The inspiration leading to the production of this book was to provide, as we enter into the 21st century, an up-date to the previous, definitive publication about the common, Walter Johnson's *Wimbledon Common: its Geology, Antiquities and Natural History* (Fisher Unwin, London, 1912). As Tony Drakeford states in the *Introduction*, this 'classic' was "perhaps surprisingly, the only major work to be published, devoted entirely to the natural history of the Commons". The present volume catalogues the sometimes none too subtle changes that have happened to the common and its natural history over the intervening ninety years and addresses how the area will survive into the future, especially with the ever increasing demands of public access.

Whether it be through reading details about the management of the commons, where one learns how the problems caused by professional dog-walkers (with packs of up to 40 dogs!) had to be resolved by introducing new bye-laws; or through the extracts from the diary of a head keeper, where one get a unique insight into the day-to-day problems (and benefits) encountered by those actively involved in the commons and their upkeep, this book provides a wealth of information about the two commons, that, to me, opened up a whole new appreciation of the area.

As far as the natural history is concerned, it does not provide a comprehensive treatment of all groups. With the exception of the flora, the writers have chosen to concentrate on those groups of organisms that are most obvious to the general public. It may seem odd that the plants receive no special coverage but are included only in relation to specific habitats (e.g. in the chapters on aquatic or terrestrial habitats, or the chapter dealing with a special study of 7 Post Pond). I was personally slightly disappointed with the coverage given to one of my own specialist groups, the Lepidoptera. Although the day-flying moths are covered, as are a few of the more spectacular night-flying species, the majority are omitted. Perhaps the reason for this omission is that they are mainly nocturnal and are therefore not obvious to the casual observer. However, what I find most surprising is that no reference is provided as to where to look for information about them. Although mention is made of a list of some 200 species of moths that have been recorded from the commons, no reference is provided. Nor is any reference made to any of the standard works covering the area (e.g. Colin Plant's *Larger Moths of the London Area* or the *Larger Moths of Surrey* by Graham Collins). A similar comment can be made about the butterflies. On p. 124 is the statement "We have to wait for the 1950s for the first positive records", referring to a report by A.W. Jones to the London Natural History Society. It would have been a simple matter to include that as a published reference (Jones, A.W. 1955 Notes on the butterflies of Wimbledon Common. *London Naturalist* 34: 108-114), especially as it is the first 'list' of the butterflies and would therefore provide a useful comparison with the modern records provided here. In that respect, this book is very much a personal account by the author Tony Drakeford.

These minor complaints aside, I enjoyed reading the book, as I hope do others. Perhaps by understanding a little more about the commons and the difficulties faced by those concerned with its survival they will also enjoy the common more. As David Bellamy states in the *Foreword*, "love it *not* to death" but rather help preserve its haphazard patchwork for others and perhaps help return parts to some of its former glory.

MARTIN R. HONEY

Finding Order in Nature: The Naturalist Tradition from Linnaeus to E.O Wilson by Paul Lawrence Farber. John Hopkins University Press, Baltimore and London, 2000, x + 136 pp., monochrome line drawings and photographs, P/B ISBN 0-8018-6390-2, £12.50.

This slim volume, as is explained on the back cover, is intended "for the general reader and student alike", and in this has probably mostly succeeded insofar as it is easy to read, entertaining, and attractively presented with numerous illustrations organized in 'boxes' containing extra information, as with many good textbooks. But it is immediately obvious why this book should be described as "exciting and innovative", as it is in a pre-

publication reviewer's commendation also on the back cover. Much, probably most, of what the author has to say can be found elsewhere (indeed, in many of the titles he lists in a 'Suggested Further Reading' section in the back of the book), and readers with a high-school education or better will surely not usually require the frequent explanations of terms such as "taxidermy – preserving animal specimens" (p.48), "breach loading shotguns (i.e. firearms designed to be loaded on the part of the gun behind the barrel)" (p.51), "physiology, which explores the functions of organisms, including physical and chemical processes" (p.72) and "in [an] anthropomorphic manner, which projected human motivations onto animals" (p.94), etc.

The back cover also advises that the book "explores the importance of socio-cultural contexts" and "institutional settings" in the shaping of the natural history sciences, but for this reviewer the author's endeavours in this regard are forced and not very convincing. Thus, at the beginning of a chapter on 'Darwin's Synthesis: The Theory of Evolution, 1830-1882', where the reader is told that Darwin, during the *Beagle* voyage, on learning that there may be two species of Rhea in South America, determined to obtain a specimen of the rarer species because of his "sharp sense of competition" (which, it seems to be implied unduly influenced Darwin's perceptions of the natural world). Similarly, Malthus's *Essay on Population*, Dr Farber informs us, was written in "the context of the social issues of the day" (p.60), and was accordingly coloured by Malthus's ideological stance on these issues.

On Linneaus, Farber is better, though his discussion of the great naturalist is necessarily truncated due to the thinness of the book. It was interesting to read about Linneaus's views on "balance" in nature, and how (as Malthus was to similarly observe), "carnivores...daily destroy animals that is unchecked, would reproduced so quickly as to outstrip their sources of food" (p.11). I'm not sure, though, what Dr Farber means when he says that Linneaus acknowledged, "that his method [of classification] did not reflect any 'real' order in nature." Did Linneaus actually believe this? And did he write it somewhere? – unfortunately Dr Farber's list of (secondary) works in his further-reading supplement doesn't allow us to check this easily. Farber is also interesting, and readable, on E.O. Wilson (the subject of his last chapter – 'The Naturalist as Generalist: E.O. Wilson, 1950-1994), especially on Wilson on biodiversity; as Farber cogently remarks, "Lack of knowledge of the organisms that inhabit the planet hinders our understanding of the actual loss" (p.112). I was disappointed, however, that Farber does not mention Wilson's energetic promotion of the All Species Inventory, a project on the scale of the Human Genome Project and described in *The Times* (30 July 2001) as the culmination of the "time-honoured dream of taxonomists and natural historians." More than likely Farber's book was completed before canvassing of the project.

JOHN LAURENT

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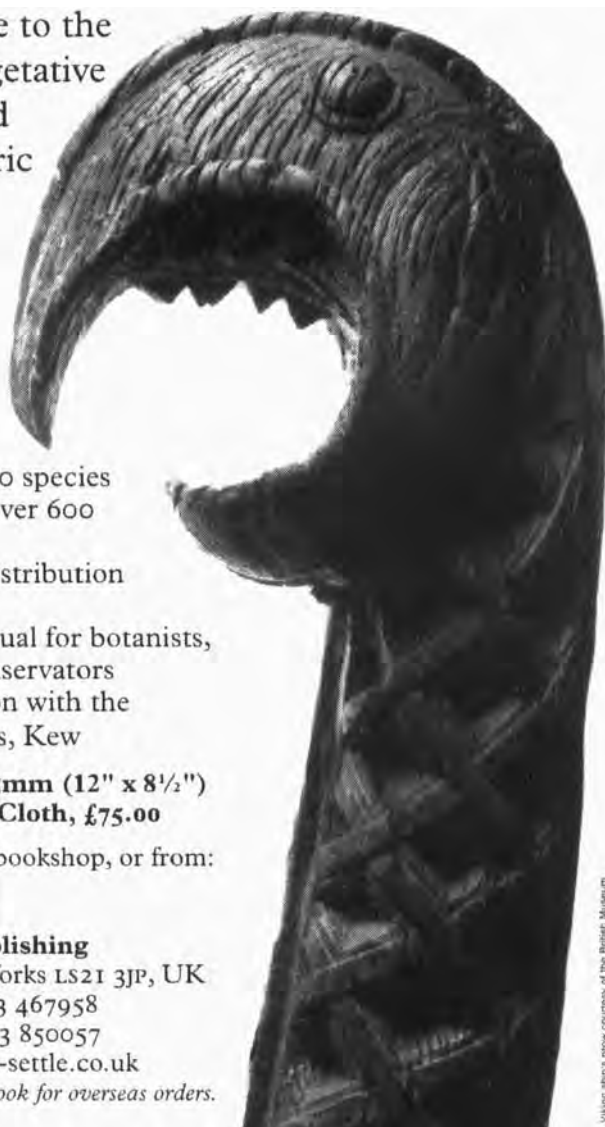
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Walking stone's arrow courtesy of the British Museum

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