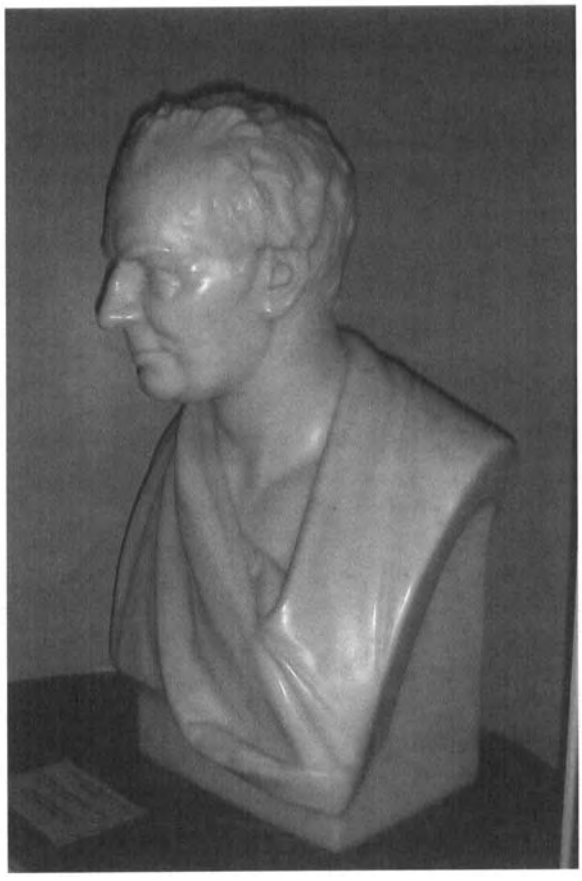
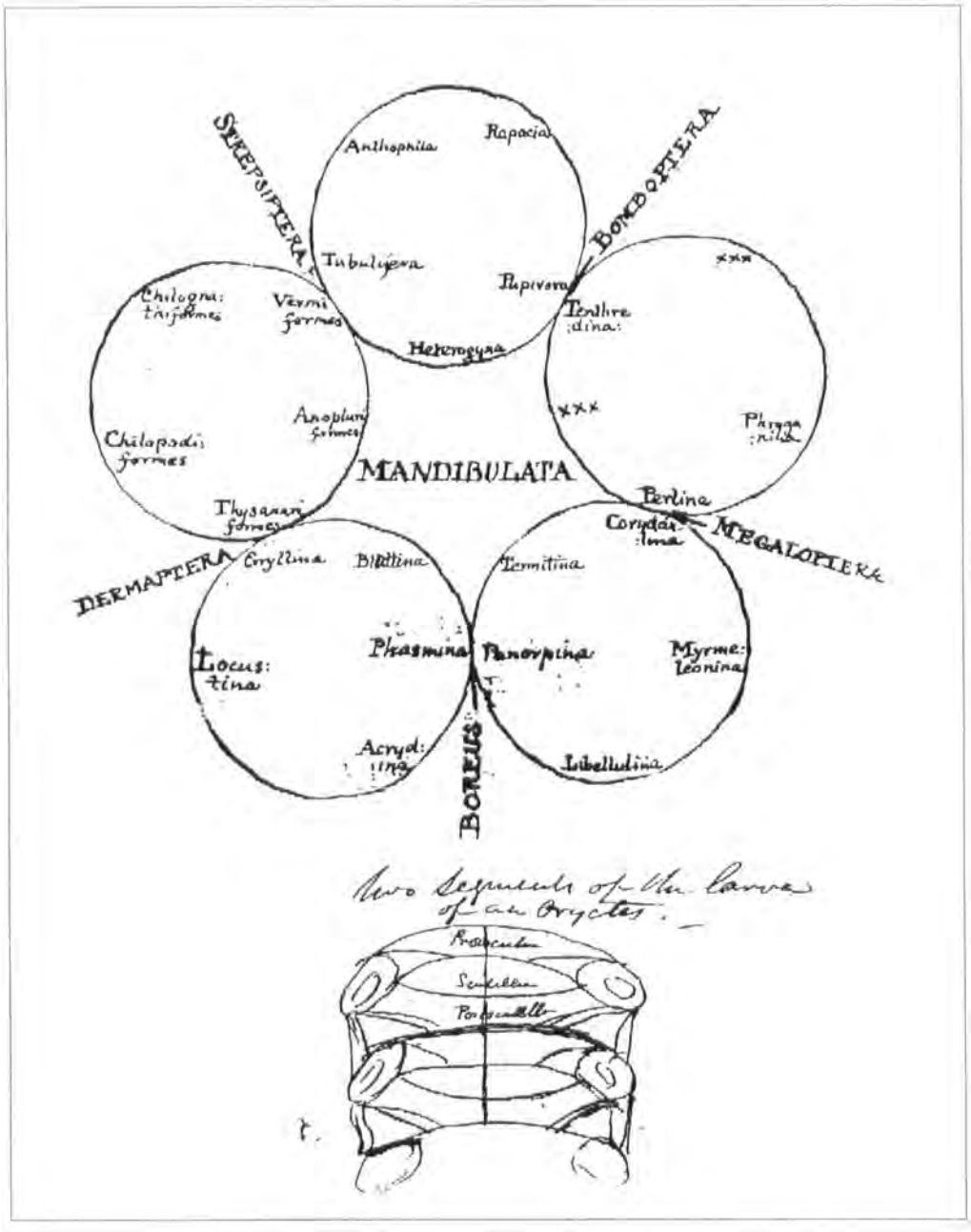
Editorial

This issue contains an historical article on the subject of the last issue's quiz, William Swainson. It describes a classification system of which Swainson was one of the chief exponents. This was the Quinary system, first proposed by William Sharp Macleay (1792–1865) in his *Horae Entomologicae* (1819 –1821). Today, a bust of Macleay resides in the alcove to the left of the meeting room. He was the son of Alexander Macleay (1767–1848) our first secretary who held the Society together for some 30 years after the departure of James Smith to Norwich. William acted as his father's treasurer/banker after his return from Paris, where he had been an attaché at the British Embassy during the period 1814–1819. It was in the family home (12 Queens Square) that William wrote *Horae Entomologicae.* He was encouraged by the Reverend William Kirby, who later enlisted his support for a zoological society which was to be part of the Linnean Society. According to Swainson (see *The Linnean* **1**(5): 11) *Horae Entomologicae* was the distillation of all Macleay had thought and perceived since his boyhood in Caithness, where he avidly made collections of both insects and crustaceans, stimulated by the comments of scientists he had met in Paris (De Geer,



William Sharpe Macleay
Posthumous marble bust by Charles Summers, Rome, 1870.



An original illustration for *Horae Entomologicae* annotated in Macleay's hand.

Lamarck, Cuvier, Latreille) and helped by his father's Linnean friends and remarkable natural history collection. As he gave reasons for grouping species into a relationship of five he did not lose sight of the origin of creation and God's purpose.

Some subsequent 19th Century authors (i.e. his friends Kirby and Newman) presumed Macleay's quinaries were resolvable into septenaries; nevertheless, the

circular system was to hold sway in our Society throughout the 1840s.

Swainson used the Quinary System in order to establish a uniform system of arrangement for the diverse groups of animals treated in his encyclopedias.

Following the reading of the 1858 Darwin-Wallace papers and the publication of the *Origin* in 1859 one might have imagined that the influence of this system would have ended. However, both Macleay and Murchison considered Darwin's facts were not always sound, while several reviewers of the *Origin* used Swainson's doctrine of analogies, or Quinarianism, to challenge natural selection (eg. Anon, in *Future*, 1860 = Collingwood). Moreover, the influence of this system did not end there, for in the second volume of the *Cabinet Cyclopedia* and in *Murrays Encyclopedia of Geography*: *The geographical distribution of man and animals* (both 1835), Swainson divided the world into five geographical provinces. Although Sclater, in his Linnean Society paper of 1858, dealing with bird distribution, divided the world into six regions, as did Wallace in his *Geographical Distribution of animals* (1870) both had to all intents and purpose adopted Quinarianism.

Society News

In late July, the President, on behalf of the Society, sent a message of congratulations to Her Majesty the Queen Mother for her centenary. We received a telegram on 3rd August which read:

> "I greatly appreciated your good wishes on my hundredth birthday and send my very sincere thanks to all who joined in your kind message.
>
> Elizabeth R
> Honorary Member"

Mailing overseas Members with *The Linnean* is not the straightforward exercise it should be. We have been aware for some time that Members in Africa have difficulty getting *The Linnean* promptly, despite all copies to Africa being despatched by airmail. Hopefully our www site and Mailbase will marginally improve matters and we can but apologise for the delays. More worryingly, there is evidence that US Members are suffering delays in obtaining *The Linnean* and we are looking into the reasons for these. Again we must apologise to Members who have been inconvenienced.

In our last issue, new addresses and phone numbers were noted. Some caution needs to be exercised by senders of both faxes and e-mails. Considerable numbers of junk messages are to be found with both these methods of communication; these are mainly sent at night (when phone calls are cheap) and await our arrival in the morning. Much fax junk mail encourages us to fax back the answers to some apparently relevant questionnaire; the (very) small print indicates that this will be at rates of up to £4 per minute. We don't. Sadly, more relevant material can accompany junk mail and *even be interleaved with it.* It may be directed to the wpb without the realisation that it is not junk. Similarly, if you have the e-mail address pegasus@augean.com, your message is likely to be deleted without ceremony and certainly without perusal. And whilst on the

subject of questionnaires received by the Society, please note that they invariably go straight into the wpb. Life is (relatively) short.

The Society gratefully acknowledges two further donations totalling £4500 from the Golden Bottle Trust.

The **British Ecological Society** is holding its annual symposium on 3–5 April 2001 on Dispersal. Details are to be found on www.demon.co.uk/bes or write to James Bullock, NERC Centre for Ecology and Hydrology, CEH Dorset, Winfrith Technology Centre, Dorchester, DT2 8DZ, jmbul@ceh.ac.uk.

Dr Ole Seberg FLS has asked us to draw Members' attention to the **Copenhagen Biosystematics Centre (COBICE)**, which offers supported access to its collections and other facilities. Through the European Commission's Fifth Framework Programme 'Improving human research potential, Enhancing access to research infrastructures', funds have been made available to provide transnational access to researchers from *member and associated states of the European Community* to utilize the collections and other facilities of COBICE. Access to COBICE will be provided free of charge for visits of up to 3 months. Travel and living costs for visiting researchers under the programme will be covered. Applications for support for a visit to COBICE are herewith invited. Please obtain the application form and other documents from COBICE's website at http://www.zmuc.dk/commonweb/COBICE.htm or request the material from: COBICE, c/o Zoologisk Museum, Universitetsparken 15, DK-2100 Copenhagen Ø, phone: +45 35 32 10 95 (Ada Kramer), fax: +45 35 32 10 10, email ajakramer@zmuc.ku.dk.

COBICE's contract with the European Commission runs for 3 years. During this period, six calls for applications will be made. The third deadline (to be determined) will be for visits scheduled to take place during the second half of 2001 and the first half of 2002. Please note that this offer only applies to researchers based in member and associated states of the European Community.

COBICE is a powerful centre of biosystematic research at the Faculty of Science, University of Copenhagen comprising the Zoological Museum (ZM), the Geological Museum (GM), the Botanical Museum (BM), the Botanical Laboratory of Molecular Systematics (BLMS), the Department of Evolutionary Biology (DEB), and the Department of Zoomorphology (DZ). The scientific collections are among Europe's largest and most well-organised, comprising a total of about 20 million specimens. These include about 75,000 type specimens and particularly important collections of Arctic (especially Greenlandic) and Danish animals, deep-sea animals, whales and South American Quaternary mammal fossils. COBICE also possesses the World's largest databases on African terrestrial vertebrates. There are extensive tissue collections for DNA analysis, including birds (15,000 samples representing 3,000 species), African mammals (8,000 samples), fossils from Greenland, mo-clay Tertiary fossils, plants from Greenland and Denmark, flowering plants from Thailand and NE Africa, and seeds of Triticeae (wild relatives of wheat, rye and barley – more than 10,000 samples).

Equipment available at COBICE includes light, fluorescence, transmission electron and scanning electron microscopes, DNA sequencing facilities, X-ray apparatus, and image analysis systems.

The Botanical Research Fund is a small trust which annually, in May, makes modest grants to individuals to support botanical investigations of all types and, more generally, to assist their advancement in the botanical field. It is available to amateurs, professionals, and students of any nationality who are sponsored by a British botanist and who are unable to obtain support from the major grant bodies. Where appropriate grants may be awarded to applicants in successive years to a maximum of three. Applications should be made in writing (there are no forms) to the Hon. Secretary. Professor Keith Jones, 57 Marksbury Ave., Richmond, Surrey TW9 4JE.

Units...

The new tests involve taking a handful of cells to map the genetic make-up of an embryo's 23 chromosomes to make sure they contain no abnormalities.
Metro, Monday, 23rd October 2000.

JOHN MARSDEN

Picture Quiz

William Swainson

The October Quiz (**16** (4):10) featured the zoologist William Swainson (1789 –1855). His family originally came from Hawkshead in the Lake District, but by the time William was born on 8 October 1789, at Newington Butt, London his father was a collector of customs in Liverpool.

After an initial education at a local school in Newington, where he apparently showed not the least aptitude, his father got him appointed, at the age of 14, as a junior clerk in the Liverpool Customs (salary £80 per annum), thus following in the footsteps of both father and grandfather. It was here during his three years as a clerk that Swainson developed his great interest in Natural History through visits to William Bullock's 'Liverpool Museum' – which included much biological and ethnographic material. Furthermore, at the request of that museum he drew up *Instructions for Collecting and Preserving Subjects of Natural History* which was first printed in 1808 and subsequently expanded in 1822 into the *Naturalist's Guide.*

In 1806 his father, realising his ambitions to collect in the tropics and his lack of concentration on official duties, arranged for him to join the Mediterranean army!

After a short stay at Malta he was sent, the following spring, to Sicily, where the British army was garrisoned and remained there for the next eight years. Luckily there

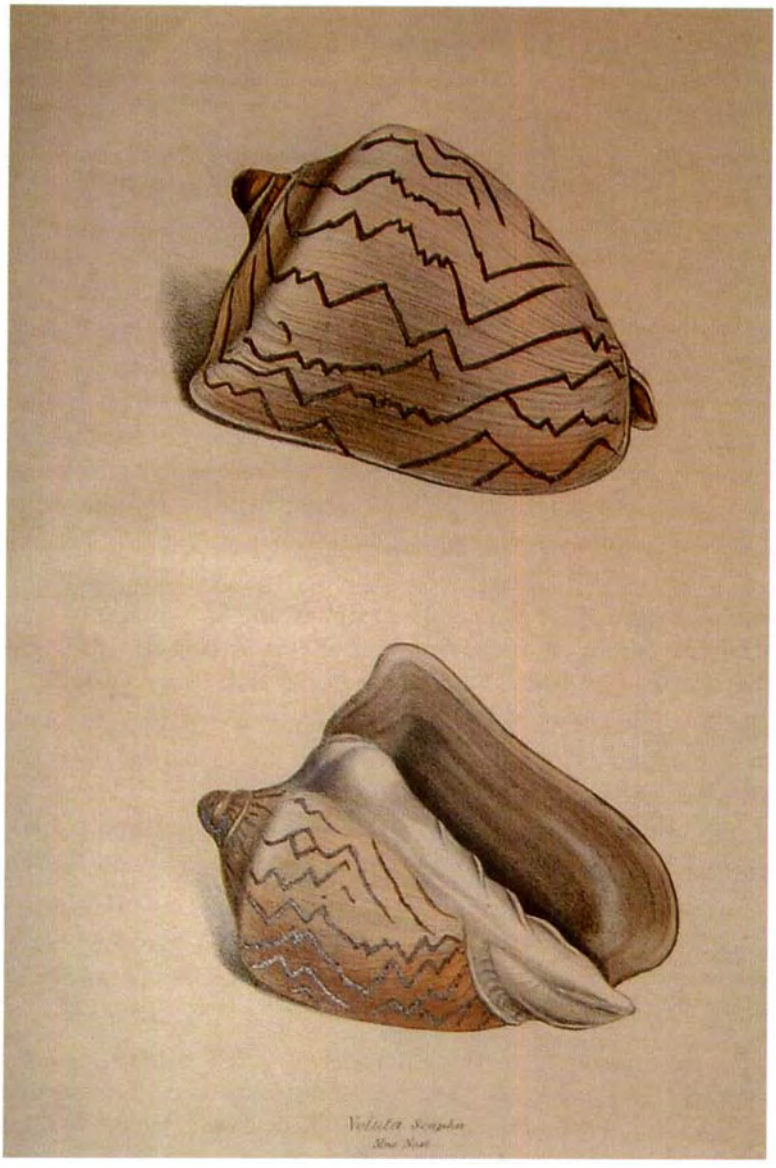
were no serious military operations and Swainson was able to spend much of his time collecting plants, insects, molluscs and ferns.

Following the publication of Sibthorp's *Prodromus*, he spent his first annual leave in Greece where he collected numerous plants which he eventually donated to the Liverpool Botanic Garden. The next year's short leave, spent back in the garrison headquarters on Malta, was prolonged due to an outbreak of the plague. This quarantine period enabled Swainson to arrange his collections and finish many of his Sicilian and Grecian sketches. Subsequently the army was sent to Naples, Genoa and Tuscany. These sojourns enabled him to not only make extensive natural history collections but also to collect pictures, sketches and etchings – particularly those of the Genoese school, all of which he eventually donated to the Liverpool Museum. Then, due to ill health it was deemed necessary for him to return to England, which he did in 1815 aged 26, when he retired on half pay. Since he had risen to the rank of Assistant Commissary-General his pension was certainly sufficient to keep the wolf from the door. He joined the Linnean Society in 1816 and his sponsors included Alexander Macleay and W.G. Manton. His desire to collect in the tropics, which had been stimulated initially by reading such tomes as le Valliant's *Histoire naturelle des oiseaux d'Afrique* and Drury's *Illustrations of Entomology,* was finally realised when, in the autumn of 1816, in the company of Henry Koster, he set sail for Brazil (financed by his father!). Sadly, a revolution initially prevented them from penetrating far into the interior so Swainson was confined to collecting, mainly birds, in the vicinity of Olindo. When the insurrection had been put down, Swainson visited Bahia, Rio de Janeiro and then made several excursions inland. He commented:

"In four months I so enriched my collections that I became satiated."

On his return in 1819 he sent a short abstract to the *Edinburgh Philosophical Journal* (**1**:369 – 373) in the form of a letter to Professor Jameson entitled "Sketch of a Journey through Brazil in 1817 and 1818".

At about this time, the technique of lithography started to become better known in England. Encouraged by his friend at the NHM, William Elford Leach, he engraved a series of plates under the title *Zoological Illustrations.* The first series consisted of three volumes (1820–23 with 182 coloured plates; 2nd series, 3 vols. 1832–33). Then, in order to superintend the publication both of the *Illustrations* and of his subsequent *Exotic Conchology* (1824–25), he took lodgings in Surrey Street, Strand. He was elected FRS in 1820. In 1825 he married the daughter of John Parkes of Warwick taking up residence in his father-in-law's house near the town centre. On the death of his father in 1826, and with it the cessation of his annual allowance of £200, he decided to take up authorship in earnest and entered into an engagement with Messrs. Longman for the publication of an Encyclopedia of Zoology. This was intended to form a companion volume to Loudon's two *Encyclopedias*, (*Agriculture* and *Gardening*). In the event, Longmans remodelled the intended Encyclopedia and merged it into Lardner's *Cabinet Cyclopedia* to which Swainson contributed eleven volumes between 1834 and 1840. A twelfth volume, written in conjunction with W.E. Shuckard, entitled *On the history and natural arrangement of insects* was also published in 1840. In order to establish a uniform system of arrangement for the diverse groups of animals treated



Voluta scapha from Swainson's personal collection, lithographed and hand coloured by himself.

in his encyclopedias, Swainson adopted W. Macleay's Quinary System (see *The Linnean* **1**(5): 1, 11–18 and this volume pp.1–3) and soon became its principal exponent, particularly for vertebrates such as birds. Despite initial success the system had few serious supporters, apart from Macleay and Vigors. Moreover, Swainson not only introduced some peculiar modifications of his own, but he also used it to divide the world into five geographical provinces.

Meanwhile, in order "to be out of the reach of morning visitors" while at the same time having reasonable access to his London publisher and to the various London museums and libraries, he moved in 1828 from Warwick to Tittenhanger Green, a little village near St. Albans. Later that same year, in preparation for the series of works

mentioned above, he spent the summer in Paris studying the collections at both the Muséum d' Histoire Naturelle and the Jardin des Plantes, where he was helped by Cuvier and Geoffroy St. Hilaire.

Most of Swainson's prodigious output during this period was written in Tittenhanger Green, surrounded by his immense collections and large library. Here he prepared and published his second series of *Zoological Illustrations, Ornithological Drawings*; *the Birds of Brazil* (6 parts London, 1834) and contributed the plates, classification and synonymy to John Richardson's *Fauna Boreali-Americana* (1829–31). He also wrote 3 volumes for William Jardine's *Naturalist's Library*, published in Edinburgh: *The Birds of Western Africa* (2 vols, 1837) and *Flycatchers* (1838).

His wife, by whom he had four children, died in 1835. He remarried in 1839 and by his second wife he had three daughters. In 1840, for a variety of reasons, he decided to emigrate to New Zealand. On the voyage out he lost most of his collections (due to the unseaworthiness of the vessel) but he took advantage of a refitting stop in Rio de Janeiro to collect a plentiful supply of plants which he took to his new home to naturalise.

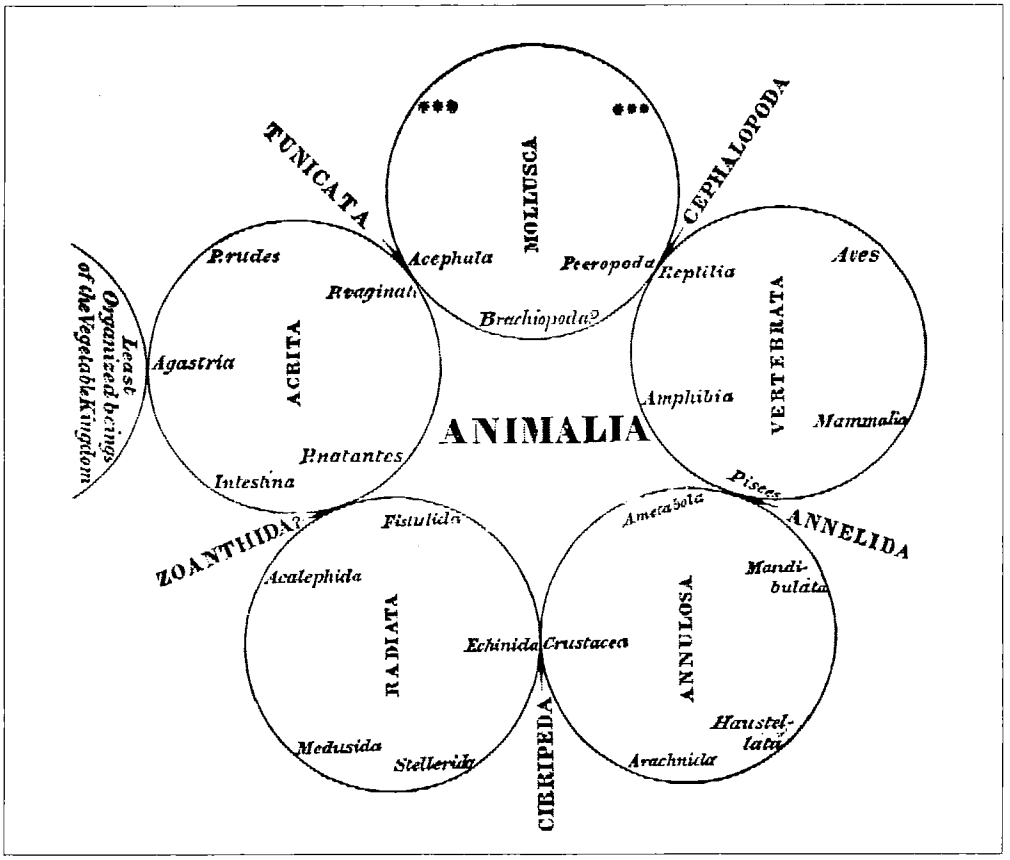
In 1853 he was commissioned to make reports on the timber trees of the colonies of Van Diemen's Land and Victoria. He returned to New Zealand in 1855, where he died on 7 December at his residence, Fern Grove in the Hutt River Valley.

He will be remembered particularly for his animal illustrations, which display the skill of an accomplished artist combined with the accurate observation of an experienced naturalist.

His publications number more than forty. His last two papers, published by the Royal Society, describe the genus *Melampas* from New Zealand and Australian Haliotidae.

John Edmonson from the Merseyside County Museums, whom I asked to look for Swainson material in his collections, has informed me that they have some 10 Swainson herbarium sheets which contain Brazilian and UK material. He also sent me the following notes on William Swainson.

> "We have at the Museum the cover of a folder of New Zealand ferns collected by Swainson and dated 1846; this indicates that he subsisted at least partly through the sale of exsiccata. The set of 40 specimens sold for 15/-, this despite the drawback that "the scientific names are provisional, until the Author receives certain books from Europe" – according to the prospectus. This was the time of the Victorian Fern Craze, and one hopes that sales of the fern albums were sufficiently brisk for him to afford to acquire a satisfactory botanical library.
>
> In 1851 Swainson went to Australia and carried out fieldwork in Tasmania and Victoria, including a survey of the forest trees. It is perhaps surprising that the British Government was obliged to rely on the services of an amateur in assessing the timber resources of one of its colonies. One must bear in mind that many people in Victoria were more interested in joining the gold rush in 1851 than in pursuing the study of botany. Only in 1853 was a government botanist appointed. This was Dr Ferdinand Von Mueller, a formidable figure who soon established Melbourne Botanic Garden as a centre of international importance.
>
> The Director of Kew Gardens, Sir William Hooker, was unimpressed by Swainson's report on the gum trees; he wrote to Von Mueller on 9 April 1854:



Macleay's Quinary System.

"If I were pleased with your report, I cannot say that I gave to our Secretary for the Colonies an equally flattering account of Mr. Swainson on the Gum Trees!!! In my life I think I never read such a series of trash and nonsense. There is a man who left the country with the character of a first-rate naturalist (though with many eccentricities), and of a very first rate natural history artist, and he goes to Australia and takes up the subject of botany, of which he is as ignorant as a goose."

In view of the fact that Swainson was entirely self-taught, this judgement is perhaps a little harsh. He did not remain long in Australia; for a man of sixty, the rigors of travel in the Australian bush must have taken their toll."

Darwin, Swainson and the Quinary System

The first recorded mention of Swainson in Darwin's correspondence is in a letter to Susan Darwin (6 September 1831) asking her to look for a book on taxidermy written by Swainson in 1822.

That Darwin had already been introduced to Quinarianism while an undergraduate at Cambridge is clear from his comments to Henslow in a letter from Buenos Aires (24 November 1832) in which he remarked:

Voluta symbium, lithographed and hand coloured by Swainson – from *Exotic Conchology*.

"There is a poor specimen of a bird which..... appears to be a happy mixture of a lark, a pigeon & snipe. Mr. Macleay himself never imagined such an inosculating creature"[1]

In 1836 Dr. Boott the then secretary of the Linnean Society introduced Darwin to Macleay over dinner at the Athenaeum. In a subsequent letter to Leonard Jenyns Darwin remarked:

"Mr. Macleay has taken a great deal of interest in the subject of the publication of the Zoology of the *Beagle's* voyages on some uniform plan. He maintains such a publication

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1 Macleay's Quinary System (*Horae Entomologicae* 1819–1821) proposed that the five main animal groups are represented by circles of affinity in which each is inosculant or intersects with two others (see *The Linnean* **1**(5): 1,11–18)

is very desirable, because it keeps together a series of observations made, respecting animals inhabiting the same part of the world."

Then, much later in a letter to T. Eyton (30 November 1839) who was examining some of the birds collected on the voyage of the Beagle, Darwin commented:

"*Trochilus gigas* – Mr. Blyth has some notion about humming birds belonging to a very different type in their internal structure..... In Swainson's nonsensical language they might be called the gallinaceous type in the thrushes."

In a letter to Hooker (31 March 1844) dealing with the relationship between the ranges of genera and individual species he notes:

"Swainson has remarked (& Westward contradicted) that typical genera have wide ranges. Waterhouse (without knowing these previous remarks) made to me the same observation[1]. I feel a laudable doubt and disinclination to believe any statement of Swainson's, but now Waterhouse remarks it, I am curious on the point."

In reply, Hooker (5 April 1844) said he believed that there was a great relation between the ranges of genera and individual species:

"With regard to typical genera having wide ranges Swainson is an instance of the type of a certain class of Naturalists wandering very far indeed both mentally & bodily. I hardly know what is always meant by a typical form. The character of a group should be founded on the most important objects it contains in the oeconomy of nature. The most important genus of a class is surely generally either the largest or the most widely diffused; if the largest genus is the type, we have already seen that large genera are generally the most widely diffused. The type of a group often turns out (on extended knowledge of that group) to be the most aberrant form in it. – Perhaps Swainson has put the cart before the horse & should have said, "a typical group or genus is that which is the most widely diffused."

Not only did Swainson introduce some peculiar modifications of the Quinary System of his own, but he also committed the cardinal taxonomic sin of changing the names of several taxa. Darwin to H.E. Strickland (31 January 1849):

"One man has a fancy for geographical names, another (like Swainson) repudiates them & substitutes others of his own invention, with the delightful '*mihi*' attached. Were this permitted, the multiplication of names would be wanton and perpetual, and I conclude therefore that such a principle will never be sanctioned."

In his reply to Darwin, Strickland (15 February 1849) commented:

"Of course you will understand that by *type-species* I only mean a conventional distinction, referring only to *words*, not to *things*; and like human titles, only used as a matter of convenience. Nature knows no more of *type-species* or 'typical groups' than she does of Dukes and Marquesses. Swainson indeed & other Quinarians talk very mysteriously about '*types*' as if the latter got their coronets from nature & not from Man, but all that appears to me to be *Fudge*.

Yours very truly,
Hugh E Strickland"

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1 In 1832–3 Swainson had argued that typical or type genera had a wide geographical distribution whereas osculant genera were more restricted.

In his correspondence with Darwin, Blyth makes reference to Swainson on several occasions. In April 1855 he discusses Swainson's genus *Crithagra* in relation to the domesticated races of canary. In October 1855 he tells Darwin that Swainson's *Papilio nomious* of Brazil is indistinguishable from the Indian race (!) and that several of the African partridges are inseparable from Asiatic Francolins and should be referred to as Swainson's genus *Chaetopus*.

In January 1856 Blyth commented in reference to bantams and canaries that the generic name *Amadina* Swainson had probably been derived from 'amadavat' or the Indian vernacular for a small bird.

Darwin in a letter to Hooker (3 January 1860) noted that Richardson, who had originally collaborated with Swainson on the description of the birds collected on Franklin's second polar expedition of 1824 (see *The Linnean* **13**(2):7), had used the Quinarian System but had, after discussing the *Origin* with Hooker, apparently changed his mind. As Darwin noted:

> "Sir J. Richardson might have added to his exploded fallacies the Quinarian System in which he formerly believed."

Despite Richardson's change of mind the Quinary System still had its adherents, with Swainson a leading advocate. Swainson added his own interpretation and his own idealistic system of classification and analogies. With the publication of the *Origin* one reviewer (probably Collingwood) used Swainson's analogies to challenge "natural selection". The review in question was published in *Future* (see letter to Charles Lyell, 6 June 1860)

> "I have read the Future: how curious it is that several of my reviewers should advance such wild arguments as that vars. of dogs and cats do not mingle; & should bring up the old exploded doctrine of definite analogies or Quinarianism. I am beginning to despair of ever making the majority understand my notions."

When, however, Robert Lowe wrote to William Macleay asking for his opinion of the *Origin*, he replied:

> "Darwin like his predecessor Lamarck is a most able naturalist, although I agree with Sir Roderick Murchison that his facts are not always sound. I think they may be interpreted another way, and he had not stated many things which bear on the subject. I am myself so far a Pantheist that I see God in everything; but then I believe He is the constant and active sole creator of the world. – Nevertheless Darwin is an old friend of mine and I feel grateful for his work and hope it will make people attend to such matters."

Finally it is in a much earlier letter to G.R. Waterhouse of December 1843, that Darwin succinctly sums up his real opinion of Swainson's system:

> "I have one criticism to make about your circles – that is that I think you are bound to state that they do not necessarily represent (without you think they do) groups of equal value & though all touching, the affinities are not necessarily equally strong. – I believe infinite harm has been done by these circles, which catch the eye as of equal size, & inevitably lead the mind to suppose they are of equal value – it is by this artifice, as I believe, the possibility of making the Quinarian system appear probible has chiefly rested: Moreover it should be stated by everyone, I think, who indulges in these *vicious* circles, that

confessedly there is no standard to judge of the value of groups. – Who can *prove* that the woodpeckers are not a group of equal value with the Hawks. – I suspect that number of species, ie amount of variation of one common type does silently come into play in estimating the value of groups."

Swainson's Correspondence

This enormous correspondence, comprising some 934 letters from 236 individuals addressed to Swainson between the years 1806–1840, was catalogued and arranged by Albert Guenther during his term of Presidency (1896–1900)[1]. The correspondence is unique in that it throws light on the character and work of those natural historians to whom we are most indebted for the progress made in the description of the vast numbers of new species during the first 40 years of the 19th century, among whom Swainson took a prominent position.

The collection includes 53 lengthy letters from his friend Rafinesque Schmaltz, one of the pioneers in the investigation of the natural products of the USA, who introduced Swainson to the fish fauna of the Sicilian coast. Another American correspondent was the entomologist John Abbot from whom Swainson bought a set of 104 drawings of lepidopterans and arachnids.

As a consequence of his visit to Brazil in 1819, Swainson later corresponded with the Russian Consul-General in Rio, who sent him both insects and plants (the latter deposited at Kew). The publication of his *Exotic Conchology* brought him into contact with William Broderip who revised and corrected the proof sheets for his subsequent malacological publications.

In 1822, following the resignation of Leach from the Keepership of Natural History at the British Museum, Swainson applied, unsuccessfully, for the post. Swainson's correspondence reveals that among his supporters were W. Hooker and Thomas Traill of Liverpool. Traill apparently was so upset at his friend's rejection that he wrote anonymous articles to both the *Edinburgh* and *Westminster Review* pointing out the miserable conditions under which Natural History Collections were housed at Montague House, and drawing attention to the near total loss of the Sloane collection. In so doing, he performed a public service, which according to Guenther (President's Anniversary Address *Proc. Linn Soc.* 1938:20) paved the way towards the creation of a separate Department of Zoology at the BM. Despite this rejection by the British Museum, before his departure to New Zealand Swainson offered that establishment both his collection of specimens and drawings. Sadly, the offer was rejected and his collection went to the Cambridge University Museum.

Other interesting correspondents include the ornithologists Audubon and Prince C. Lucien Bonaparte.

Guenther concluded his Presidential Address on Swainson's correspondence with a sly dig about Swainson's systematic attempts in ichthyology:

"I regard his work on fishes as a literary curiosity, the appearance of which was a misfortune to a man who, by his indefatigable industry under by no means favorable

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1 The correspondence was purchased partly courtesy of the Bentham Fund and partly by the generosity of three Fellows who between them contributed £50.

circumstances, has contributed as much as any of his contemporaries to the advances of zoology and its diffusion among the people."

Swainson's *Natural History of Fishes,* with its attempt to classify them on Quinarian lines, had clearly upset Guenther!

Other published correspondence relating to Swainson is that between the eccentric Charles Waterton (author of *Wanderings in South America, The North-West of the United States and the Antilles, in the years 1812,1816,1820 and 1824 with original instructions for the perfect preservation of birds and for cabinets of natural history* [1825]) and the American naturalist George Ord.

According to Waterton's testimony he had written to Swainson on ornithological matters (10 March 1837) and evidently this had "galled him tremendously, and pointed out to the public pretty clearly his lamentable ignorance of real ornithology." Writing to Ord again (1 July 1839), Waterton remarked that the specimen of the goatsucker mounted in the museum of the Zoological Society had *one very long* and broad feather in the middle of each wing "which at once floors Swainson's alar theory". In a further letter (30 May 1843) Waterton complained of the disparaging remarks published in Lardner's *Cabinet Cyclopedia*; *Fishes* 2:111 where Swainson ridiculed his riding upon the caiman in Guinea ("a constant propensity to dress truth in the garb of fiction") and then cast doubt upon the method of preserving specimens with corrosive sublimate. Other letters include such choice comments as:

"and now let me request you must earnestly write a critique on Audubon's work. That ornithological imposter ought to be exposed. – So much for Audubon's drawings and Swainson's critique, these men ought to be whipped!" (3 July 1835)

"Swainson, and Jameson, and Macgillivray, and all his other supporters shall have their ignorance brought home to them. I will prove their consummate ignorance" (1 December 1840).

And finally a quote I could not leave out:

"Cuvier, though a great philosopher, and a most honest gentleman, knew no more about the real habits of most birds than I did about his grandmother." (30 January 1846)

The final word we leave to Edmund Selous:

"For me the most entertaining feature of the great Waterton-Swainson controversy is the equally assured and erroneous assertions – especially negative assertions – which each of the frowning champions makes in regard to the habits, powers and possibilities of the animal or family of animals, around which it raged and particularly the curious and interesting manner in which some of these *ex cathedra* cocksurednesses can be checked and their falsity exposed."

B.G. GARDINER

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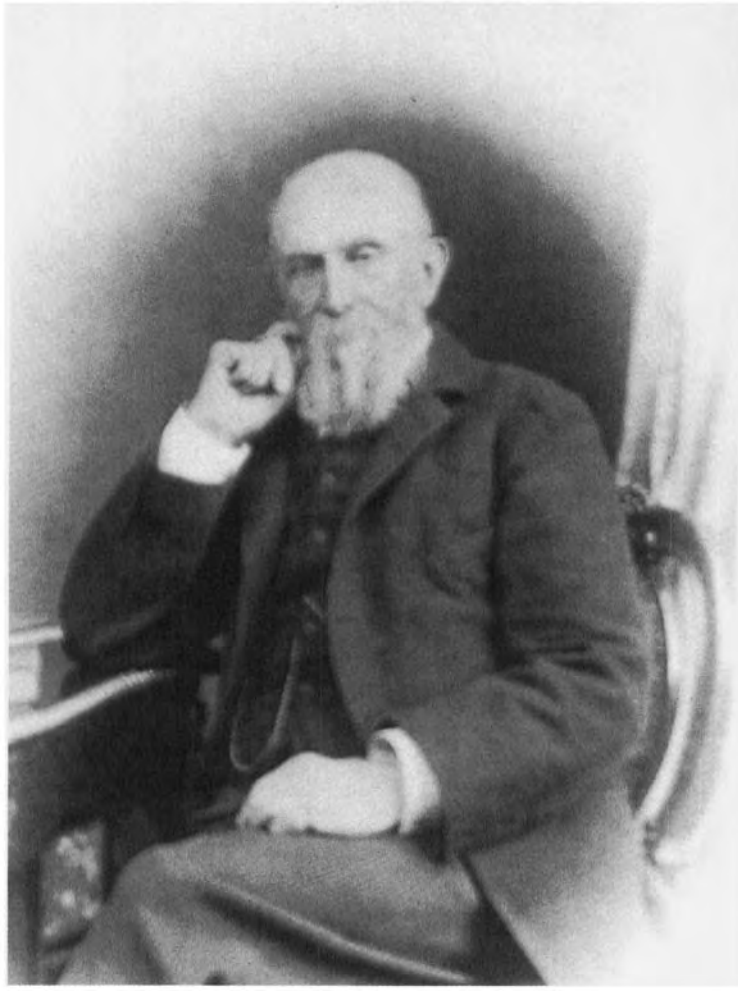
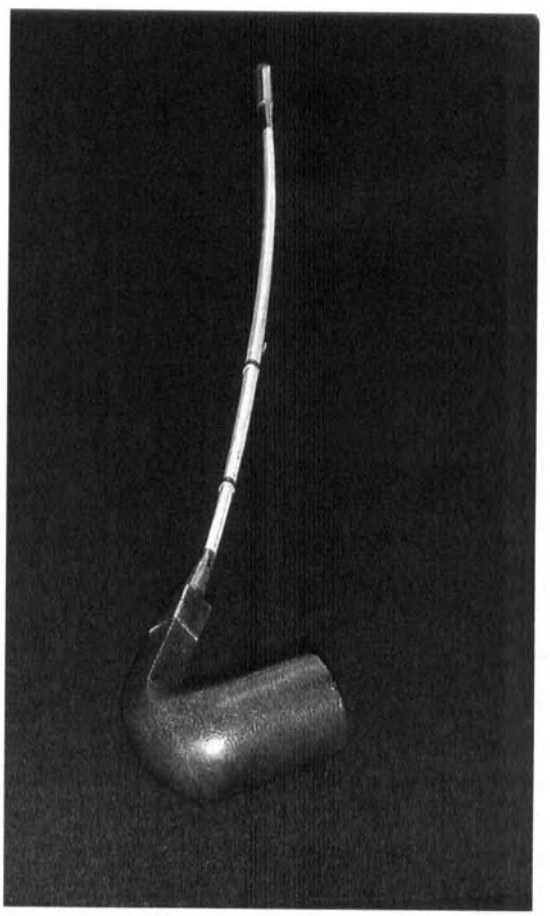


Clue: Present at the reading of the Darwin/Wallace paper: a supporter of Agassiz.

From the Archives

Hanging about at the foot of the Linn's staircase, as one does, I began to inspect the treasures displayed there. These include an elongated object labelled "Darwin's Pipe". It has an amber mouthpiece and mahogany bowl. The stem is joined to the bowl by an elegant silver tube. The stem itself has a longitudinal groove along its length and is said to be made from the tibia of an albatross obtained in the Galapagos. The pipe was exhibited at the Darwin/Wallace centenary reception in 1958 by Colonel Richard Meinertzhagen (one time President of the British Ornithologists' Club), and presented by him to the Linnean Society after the meeting. It was said to have been given originally to his sister by Dr Guillemard of Cambridge.

What seems to have escaped notice, both during the original meeting and by the many distinguished zoologists hanging about waiting for their spouse to reappear from the Linnean's cloak-rooms over the past 42 years, is that the pipe is about 30cm long and less



than 1cm in diameter. This is an improbable ratio for a tibia. It also implies an albatross that once stood about 2 metres high (on rather spindly legs!). This has prompted closer inspection by BG who reports that the silver hallmarks on the stem of the bowl indicate manufacture in Birmingham in 1928.

Thus it appears that Darwin's pipe can be added to the rapidly lengthening list of bogus specimens associated with the former doyen of British ornithology and Master Spy (see references below). It seems incredible that Meinertzhagen's enormous assortment of fakes and frauds should have been flaunted so blatantly before his peers, and that they should have escaped notice for so long. He must be laughing in his grave.

PAT MORRIS

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Correspondence

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16.10.2000

Dear Brian

Alfred Russel Wallace and the grass roots of natural history and social justice

In view of the evident interest in that remarkable man, Alfred Russel Wallace, as witnessed by numerous issues of *The Linnean*, it occurs to me that Linnean Society members might like to be informed of his activities in connection with the local natural history society here in Croydon (then called the Croydon Microscopical and Natural History Club) whilst he was living in the town between 1878 and 1881.

At the 9th Annual Soiree, held at the Public Halls on 6th November 1878, Wallace exhibited "Butterflies from the Malay archipelago, among which were specimens of the Leaf butterfly (*Kallima paralekta*), of Sumatra". He was "balloted for and elected" a member on 26th February 1879, and on 19th March that year he joined in a discussion concerning, *inter alia,* "a species of Pig, the Babirusa, or Pig Deer (*Babirusa alfurus*), an animal with which he was well acquainted". On 19th November he proffered comment on the colouring matter found in the feathers of the Helmet Birds of West Africa. At the following Annual Soiree he exhibited "a large number of skins of tropical birds".

It was at the Annual General Meeting on 21st January 1880 that Wallace provided additional evidence for his 'advanced' views (as if Darwinism wasn't bad enough!), by announcing his intention to propose that ladies be allowed to attend the Club's meetings.

He gave notice that at the meeting on February 18th, he would move the following addition to the rules:

> "That the reader of a paper be allowed the privilege of having lady visitors introduced on the occasion when his paper is read, on announcing his wish at the previous meeting."

On the due date Wallace declaimed his proposal "in a speech of considerable length", and it was supported by Dr. Alfred Carpenter [1825–92] (an eminent physician of his day, and President of the Society for 1877 –78.)

> "The matter having been debated at considerable length, Mr. Wallace replied, and the motion was then put to the meeting, and negatived by a very large majority, only nine members voting in favour of the motion."

This was hardly the Club's finest hour! His thwarted attempts to introduce ladies to meetings, if not to actual membership, notwithstanding, Wallace read a paper (subsequently published) at a meeting on 17th March 1880 "On the peculiar species of the British fauna and flora". At the same meeting (without his wife, one assumes!) he exhibited a "case of lepidoptera from the Isle of Man, showing in some species a remarkable diminution in the size of the wings as compared with the members of the same species in England." Wallace also attended the meeting held on 17th November 1880, at which he commented on a paper by Alfred Tylor, F.G.S. "Colourization in animals." He was clearly, despite the procedural setback, a keen member (we have his

signatures in our attendance book for at least seven meetings), and attended the Annual Soiree, on 24th November 1880, exhibiting "a number of interesting objects of Malay manufacture" and a microscope.

In May 1881 he moved to Godalming, and he is therefore not included in our printed membership list of that year.

I have traced only two other instances of Wallace having any connection with local natural history societies, the Rugby Natural History Society (in 1881) and the Essex Field Club (in 1886) (*My Life*).

Ladies were not admitted to membership of the Club (later the Croydon Natural History and Scientific Society) until 1897, and no lady was a member of its Council until the year of Wallace's death, 1913. Our first lady President was elected as recently as 1955.

Yours sincerely
PAUL W. SOWAN
President/Librarian

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15 September 2000

Dear Sir,

In response to your query in *The Linnean* **16**(3) about Robert Williams Wood: the 1917 ("last") edition of the 1909 book you have was reprinted in 1959 by Dover Publications. The back cover of this reprint edition the following information:

> "The late Robert Williams Wood was one of America's foremost physicists, the winner of the Rumford Premium, and many other scientific distinctions. Like Lewis Carroll and Bertrand Russell, however, he had a second side, perhaps even a second personality. His intimate friends knew him as a humorist almost without equal.
>
> *How To Tell the Birds From the Flowers* and its sequel *Animal Anatomies* were the only collections of Wood's humor that were ever printed in book form. They were immediately recognized as classics, and within fifteen years of their original publication, went through some 28 different editions. They started a style in humor, a new type of nonsense verse that was widely imitated (though not excelled), and have had repercussions even to this day in advertising art."

The 1959 edition includes both of the books mentioned in the quote above, as well as additional poems and sketches and an introduction by Professor Wood's wife. As for the *Nature Series* of which the 1909 edition formed No. 23, I have no information.

Yours sincerely,

PROF. DARYL P. DOMNING, FLS

[The Editor thanks other Fellows for similar letters]

Robert Williams Wood (1868–1955) and Flornithology

Robert Williams Wood (1868–1955) was a noted American physicist (see Weber, 1982) and the author of three books on physics, including *Physical optics* (1905, 1911, 1934, a 1967 reissue, and a 1913–14 French translation). Wood is now perhaps best known for his flornithological *How to tell the birds from the flowers,* a delightful work with Wood's witty verses and charming illustrations. This went through several versions that are listed in full in the bibliography below.

In 1907 there appeared *How to tell the birds from the flowers: A manual of flornithology for beginners* as part of *Nature series,* no. 23. In 1908 *Animal analogues* appeared in *Denatured series,* no. 24. These were amalgamated in 1917 as *How to tell the birds from the flowers, and other wood-cuts: A revised manual of flornithology for beginners.* This definitive edition was reissued by Dover in paperback in 1959 under the same title and with some textual additions and a new one-page introduction by Margaret Wood White.

Professor Wood specialized in a combination of misleading drawings and ridiculous verse. As his contemporaries recognized, Wood punned as ingeniously as Lewis Carroll, and drew as delightfully as Edward Lear, with the same mixture of casual deliberate crudeness and subtlety.

Some commentators claim that his humor, which continues to delight readers, works on a deeper level, in that he demonstrates very ingeniously that surface similarities are not really similarities in nature. The title itself has now passed into popular folklore.

The 1959 reissue is the largest collection of Wood's drawings and verses ever printed. It contains the complete text of the latest edition of *How to Tell the Birds from the Flowers* (including *Animal Analogues*) together with three complete illustrated poems and two sketches that appeared in earlier editions and had been omitted [from the 1917 edition]. Unfortunately, the new introduction is quite uninformative.

The 1907 book (there are reissues or reimpressions – "editions" – that have later dates, e.g. 1909) appeared as no. 23 in *Nature series,* whereas the 1908 book appeared as no. 24 in *Denatured series.* I know of no other works in these series issued by the (small and obscure) publisher Paul Elder and Co., San Francisco, and suspect that this is one of Wood's sly jokes. His *Nature series* also seems a play on the long-running but unnumbered *Nature series* of Macmillan and Co., London and New York, which contained such famous works as *Flowers, fruits, and leaves* (1886) or *On British wild*

flowers considered in relation to insects (1875, 1893) by the prolific author Sir John Lubbock, 1st Baron of Avebury (1834–1913) [*Elected a Fellow in 1858, his portrait is on the left of the Presidential chair in the Society's Meeting Room – Ed.*].

Because *The Linnean* **16**(3):4 (July 2000) cited "The Parrot and the Carrot," perhaps the best of the bird-plant analogies in the flornithology book, I will give a plant-other-animal example from *Animal analogues,* namely, "The Pansy and the Chim-pansy":

> Observe how Nature's necromancies
> Have clearly painted on the Pansies,
> These almost human counten-ances,
> In yellow, blue and black nu-ances.
> The face however seems to me
> To be that of the Chim-pan-zee:
> A fact that makes the gentle Pansy.
> Appeal no longer to my fancy.

I have two copies of the 1959 Dover reissue. I will donate the extra copy to the Library of the Linnean Society in the hope that some cladist will use the information therein to develop a new cladogram for animals and plants.

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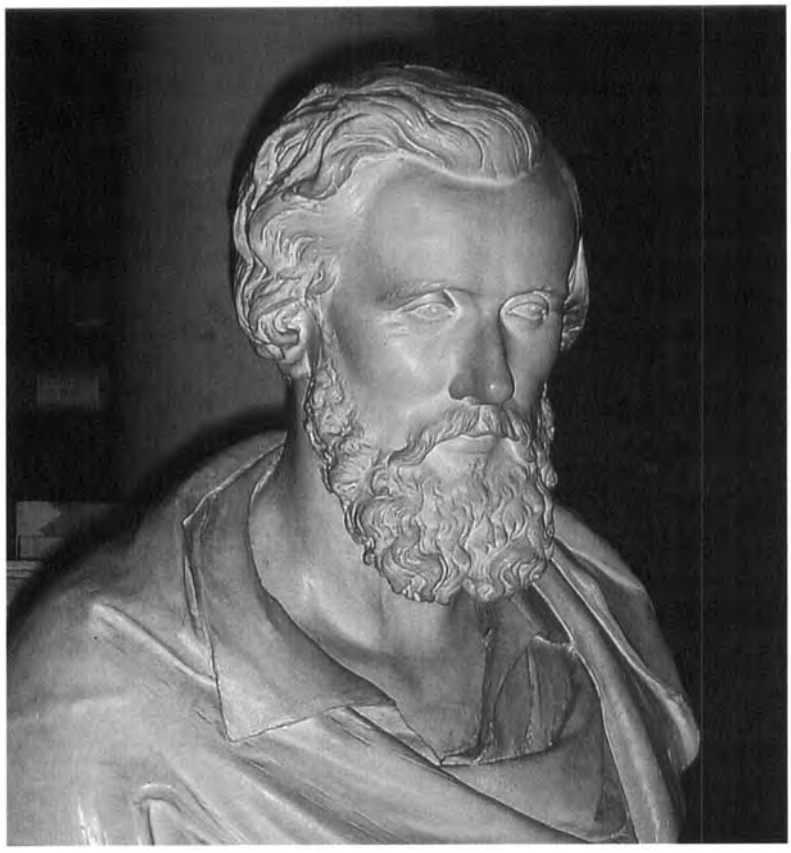
[The Library has received the extra copy and The Society expresses its gratitude for this gift – Ed.]

James Murie, the aggressive librarian (or the case of giving a dog a bad name so that you can kick him for the next century)

Aspects of the career of James Murie (1832–1925) were addressed in an account in *The Linnean* **13**(3). The positions he occupied as a member of the Linnean Society's salaried staff included Sub-editor, Assistant-Secretary and Librarian. As Librarian he was the Principal Executive Officer of the Society (Gage, 1938). The previous account cited above and written by Margot Walker presents a critical assessment. The present paper has been written in the hope that a more balanced account may go some way to providing a corrective view.



Bust of James Murie on the upper floor of the Linnean Society library.

Among the criticisms levelled at him were that he altered manuscripts submitted for publication in a high-handed fashion and made incorrect alterations to papers. Another Fellow "complained that Fellow's letters were addressed 'Esq' and Associates 'Mr', which was condemned as "an abominable piece of snobbery". We are left to assume

that this was something Murie had initiated whereas it was a perfectly ordinary form of distinction in a class-conscious period, as witness the cricket scores in the quality newspapers of the period (and much later) which made the distinction between amateur cricketers by giving initials and surname (and in some cases the use of Mr., Dr., or other title as appropriate) and professional players (surname followed by initials). These distinctions were reinforced by the annual cricket match between the "Gentlemen" and the "Players".

Murie had held many positions as a medical man. Prior to joining the Linnean Society he was Prosector at the Zoological Society of London (*The Linnean* account uses the term Praesector, although Sherren (1895) refers to the post as the Prosector, as did Murie in several reports he published of his dissections of animals). His duties at the Zoological Society were to dissect animals which had died in the menagerie and in reporting the causes of death, contribute to the advance of zoological knowledge as well as to lessen the mortality rate. His direct supervisor was Richard Owen, the most distinguished anatomist in Britain at the time who had dissected many of the Zoo's dead animals in the past. Walker reports that Murie made "*post mortem* examinations of over 4,000 creatures in the zoo". If true, this was an astonishing work rate as he only held the post for five years (1865 – March 1870) suggesting that he dissected about three dead animals a day every day other than Sundays. The Murie reports I have examined in the Zoological Society's publications were often extremely detailed and could not have been churned out at such a rate.

Murie has been accused of being aggressive, failing in his official duties, of writing "savage pathological reports", and of criticising the Zoo's management. Despite these failings he was elected as Librarian at the Linnean Society after a contested election. However, Murie proved to be a difficult person to have running the Linnean Society and he attracted a lot of criticism – yet when he resigned as Librarian in 1887, surprisingly, there were Fellows who supported him. The Society gave him a pension of £100 per annum, which seems relatively generous as his service totalled only twelve years (Gage, 1938). Murie retired to a cottage at Leigh-on-Sea, Essex, where he died in 1925.[1]

From the critical accounts of his work and personality one might imagine Murie's remaining years were spent filled with bitterness and complaints. In fact, in retirement he entered a very productive period of his life, which has been ignored by those who have written about his career. He became a member of the Kent and Essex Sea Fisheries Committee (I have seen him referred to as the Committee's Honorary Biologist, although this may not have been a formal appointment). In 1903 the Committee published his *Report on the Sea Fisheries and Fishing Industries of the Thames Estuary*. The first part was all that was published but it was a substantial account running to 250 pages in octavo. The Sea Fisheries Committees were set up by the Sea Fisheries Act of 1888 and their function was to regulate the inshore fisheries by means of Committees appointed partly by the appropriate maritime County Councils and the Board of Trade. The composition changed with time but the County Councils are still

1 After his death it was found that he had not drawn or spent one penny of his Linnean pension, the accumulated value of which was almost £4,000. The Society attempted to get the money returned but it failed! (Ed.)

the major constituent, together with representatives of the fishing industry.

Leigh-on-Sea was an important fishing area in Murie's time as it had been for several centuries, along with Gravesend and Barking (which in the 1850s was the largest fishing port in England) (Wheeler, 1979). Murie's *Report* includes chapters on the hydrography of the Thames Estuary, the history of the Thames-side communities, the fisheries of the Thames Estuary, marine mammals, fishes which were of commercial importance and their biology and the invertebrates which were commercially important such as shrimps, oysters, cockles and mussels. It contained much original biological information and Murie was up to date with information on fishes elsewhere in the British coast and the North Sea in general. In short, the *Report* was an important document because of the period in which it was written. In his time the Estuary still contained much of its native fauna, which would shortly be virtually wiped out by pollution and over-fishing.

The second volume was written but was never published although several gatherings were set in type and exist as 32 proofed pages in the Zoology Library of The Natural History Museum. The remainder of the text set in galley proof and as hand-written copy with a large number of illustrations was found in Murie's coal cellar after his death, much of it stained and soiled and almost obliterated in places, particularly where it had got damp. These papers were rescued by Mr W. Pollitt, then Borough Librarian at Southend on Sea; the text was copied in longhand and the original artwork saved where possible and remounted. The copies are still preserved in the Reference Library at Southend and although photocopies have been made of the fair copy it has never been published (indeed, so much, particularly of the artwork, has been lost, it is probably no longer worth attempting to resurrect the text now, particularly because the original text is no longer available).

Although I have referred to the fair copy in the Reference Library, I have never attempted to establish why Murie's major work was abandoned with only half of it published. The whole was an important historical and social document which was lost to fisheries workers and historians and certainly deprived Murie of credit for his work. It is possible, even probable, that his irascible temperament caused him to fall out with the members of the Sea Fisheries Committee so that plans to publish the remainder of the *Report* were abandoned, but this is no more than a suggestion.

ALWYNE WHEELER

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Linneaus's species concept and his views on evolution

No name is more intimately associated with the species concept than that of Linnaeus. What is less generally recognised is that he adopted the theory of transformism in order to explain the origin of varieties and species by hybridisation.

Linnaeus came from a Lutheran background and there is little doubt that he was initially both devout and orthodox, attending church regularly throughout his life. It is in the light of this background that we must examine his theories. Moreover, it is important to note that early in his career he decided that man had been ordered by God to study nature and thus the myth of creation acted as his inspiration. Despite this his opinions and ideas did not remain fixed throughout his career and he eventually viewed himself as an illuminant or Magnus (Cain, 1993b) interpreting nature's laws on God's behalf.

Linnaeus's initial ideas on genera and species were set out briefly in *Systema Naturae* (1735) in which he says:

> "every genus is natural, created as such at the beginning – hence not to be rashly split up or stuck together by whim or according to anyone's theory."

These ideas were more formally set out in his *Fundamenta Botanica* of 1736 in a series of 365 aphorisms (viz. 157: "we count as many species as there were different forms created") which were explained and expanded in *Critica Botanica* published in 1737.

> "All species reckon the origin of their stock in the first instance from the veritable hand of the Almighty Creator: for the Author of Nature, when he created species, imposed on his creations an eternal law of reproduction and multiplication within the limits of their proper kinds. He did indeed in many instances allow them the power of sporting in their outward appearance, but never that of passing from one species to another. Hence today there are two kinds of difference between plants: one a true difference, the diversity produced by the all-wise hand of the Almighty, but the other, variation in the outside shell, the work of Nature in a sportive mood. Let a garden be sown with a thousand different seeds, let to these be given the incessant care of the Gardener in producing abnormal forms and in a few years it will contain six thousand varieties, which the common herd of Botanists calls species. And so I distinguish the species of the Almighty Creator, which are true from the abnormal varieties of the Gardener: the former I reckon of the highest importance because of their author, the latter I reject because of their authors. The former persist and have persisted from the beginning of the world, the latter, being monstrosities, can boast of but a brief life." (Translation by Ramsbottom, 1938)

However, it soon became clear to Linnaeus that his initial idea on the fixity of species:

> "we have to go back to the day of creation to decide which forms constitute species"

was clearly an unworkable definition. Therefore by the time he wrote *Genera Plantarum* in 1737 (that is in the same year as *Critica Botanica*) he had adapted his definition of species to include all those forms of structure which occur in nature and whose appearance is not due to habitat or other chance factors. Furthermore in many of his works of this period, he reiterated what he had written in *Critica Botanica*:

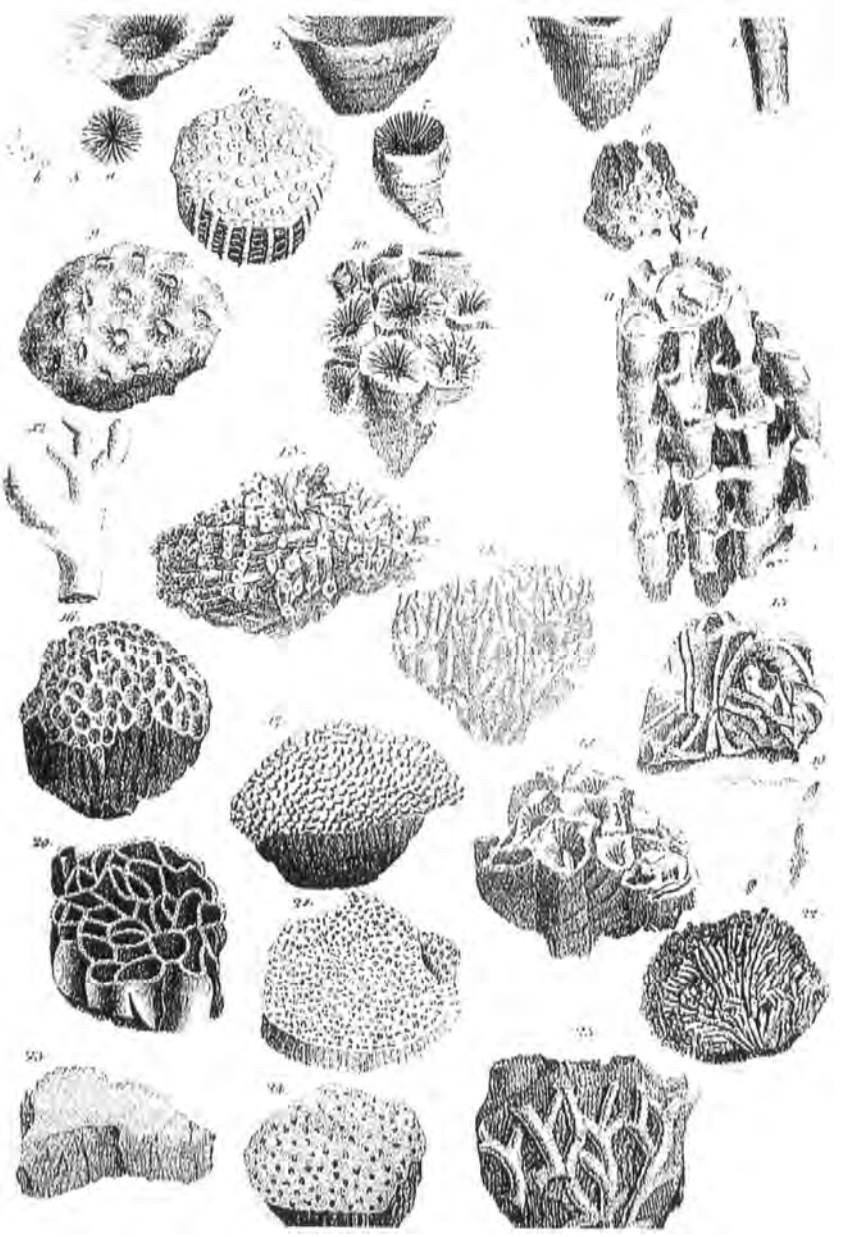

Plate from *Corallia Baltica*, 1745.

"the species of Botanists come from the All-wise hand of the Almighty, the varieties of Florists have proceeded from the sport of nature, especially under the auspices of the Gardeners."

To Linnaeus at this point in time the momentary creation was the fundamental guarantee of order, which ensured species remained constant. Nevertheless by 1743, as a result of his travels in Öland and Gotland in 1741, and in particular along the limestone coast road to Korpeklint where he noted that the banks of stones:

> "showed the yearly increase in the land was so obvious, we could hardly see a better example anywhere,"

Linnaeus concluded that the Earth was increasing in size. This idea of the accretion of the land, first put forward in his famous conferment address: *Oratio de Telluris habitabilis incremento* (*Growth of the habitable earth*, [1744])[1] he apparently adapted from Newton (or Hiärne, Swedenborg and Celcius (Frängsmyr, 1983)). In it he states that the earth was much smaller than today and that this paradise island had a high mountain at its centre, giving a variety of climates within a limited area. Then as the waters receded it gradually grew and grew and large continents emerged. He also recognised numerous beds of fossils which he believed were the remnants of an ancient world attesting to a continuous creation of the earth and to the fact that nature was in a state of continuous change:

> "the innumerable *petrifactions* of foreign animals, and of animals never seen by any mortal in our days, which often lie hid among stones under the most lofty mountains, are the only remaining fragments of the ancient world, and reach far beyond the memory of any history whatever. So large a quantity of these and other stones covers the globe, that no man has hitherto been able to break through them and penetrate to the originally created earth." (Translation by Smith, 1785).

Elsewhere in the essay he introduces the concept of the geometrical rate of increase in nature citing examples of plants producing numerous seeds and pointing out that if only 2 seeds were to germinate each year this would give a population of 1,048,576 in just 20 years. Then, as a rider, he notes:

> "and we also have to take into account various methods of vegetative reproduction."

Linnaeus then describes how many plants are protected against animals by spines, prickles, thorns, and other means. He gives examples of seed dispersal, of the mimicry of fruits and seeds preventing them from being eaten and in a concluding paragraph deals with the balance between herbivorous and carnivorous animals, birds, fishes, insects – and the animal and vegetable kingdoms. According to Ramsbottom's (1938) interpretation: Linnaeus "seems to be approaching the idea of a struggle for existence."

Despite his belief in the accretion of the land, Linnaeus continued with his proposal that there was only one pair of every living thing created in the beginning. At the same time he dismissed the diluvial theory by pointing out:

> "it is not credible that the Creator should fill the world with animals only to destroy them all shortly afterwards through the deluge – with the exception of a single pair of each species preserved in the ark."

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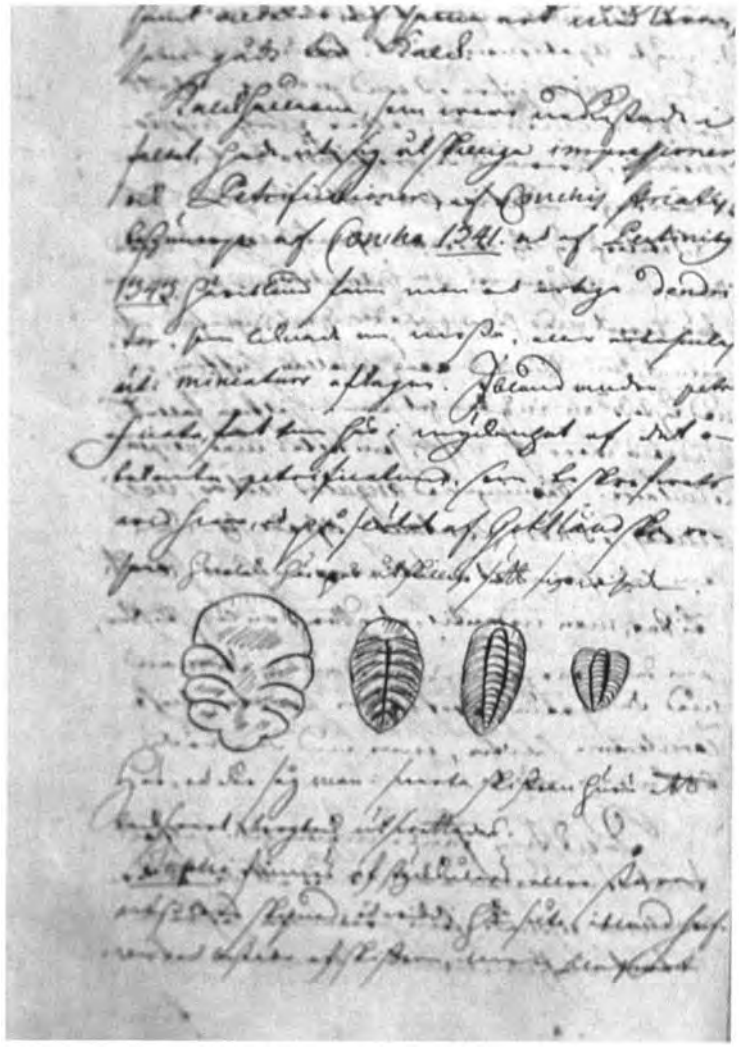
1 Linnaeus's obsession with the growth of land can be traced back to the Lapland journey of 1732 when he concluded from the presence, in a lens of clay near Hudiksvald, of two small bivalves (one small, white and smooth = *Telina baltica*; one larger and brown = *Mytilus edulis*) that the area had formerly been under the water.



Part of a page from the Västergötland diary, 1746.

Previously Linnaeus had travelled through Dalecarlia (1734) where he noted that the Vala mountain immediately above Lake Grafel (791m above sea level) was grooved with many horizontal ridges – these, he concluded, had been caused by the rising water immediately after the deluge!

Linnaeus continued his practical geological observations on his visits to Västergötland in 1746 and then to Skåne in 1749. By this time he was convinced that the earth was even older than the Chinese estimate of 6,000 years and that his accretion of the land or diminishing waters theory was supported by raised beaches, shell banks, and terraced gravels, while the presence of fossil beds and petrified molluscs attested to a continuous creation.

> "In former times, as now, nature built up the land, tore it away and built it up again" (*Skånska Resa* 1751: translated by Frängsmyr, 1983)

As a result of these visits, Linnaeus proposed not only how the various rocks were formed, but also how the sedimentation process might have taken place. He suggested that when seawater is still, slime, mud and silt settle on the original seabed entombing both animals and plants. The silt then crystallises into fine gravel which eventually forms sand – except in the case of sea snails and mussels which form their calcareous shells from seawater. He then put forward his own original theory of the importance of seaweed in this process (see Wästgöte-Resa, 1747: 86, 114, translated by Frängsmyr, 1983), imagining that wrack such as the Sargasso weed would have had a calming effect on the surface waters, thereby allowing or assisting in the crystallization or sedimentation process. He further postulated that the wrack eventually decomposed to slime and that this formed the slate.

Meanwhile, in 1744, the year of the publication of *Oratio de Telluris habitabilis incremento*, one of Linnaeus's students, Daniel Rudberg, was deputed to investigate the problem of species-crossing in plants following the depiction by Stehelin of a toadflax plant bearing a perfectly regular flower. Since *Linaria vulgaris* is characterised by bilaterally symmetrical flowers, Linnaeus concluded that this aberrant regular flower was a hybrid with an unknown father, and since there was so great a difference in floral structure, created a new genus – *Peloria* for its reception. The subsequent thesis was published in 1744:

> "we do not know by which other flower *Linaria* has been impregnated when it produces the *Peloria*...besides it must be noted that in the flowers of *Linaria*, the neck is almost closed so that it would be hard for it to admit the pollen of some other flower to fertilise its pistil unless the lips of the corolla had been torn and eaten away beforehand by insects... If it can be decided with certainty that the *Peloria* must have arisen as a hybrid species from the *Linaria* and some other plant, a new truth would come to light in the vegetable kingdom and that process (hybridisation) in the case of plants would be further advanced than in animals – since animal hybrids such as the mule and others lack the ability to propagate themselves... Although *Peloria* is generated from *Linaria* it is not due to crossing." (Daniel Rudberg)

Today we recognise that *Peloria* is no more than an epigenetic mutation which has changed the symmetry of the flower from bilateral to radial.[1]

Linnaeus subsequently wrote to Johann Gmelin telling him that he believed *Peloria* was the consequence of hybridization. To this Gmelin replied in a letter dated 17 May 1745, that he had evidence of hybridization in *Delphinium* as well as other genera (Eriksson, 1983). A later dissertation *Sponsalia Plantarum* (Johannes Gustavus Wahlbom, 1746) contains descriptions of experimental hybridization in tulips as well as accounts of *Brassica* hybrids, while *Plantea Hybridae* (Johannes J. Haartman, 1751) lists 100 plants which Linnaeus believed to be hybrids on strictly taxonomic grounds including *Trifolium hybridium:* "a hybrid between *T. repens* and *T. pratense.*"

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1 See Cubas, Vincent and Coen (1999), who concluded that such mutations may play a more significant role in evolution than has hitherto been suspected. They also argued that epimutation arising in a plant meristem could be transmitted by vegetative propagation as well as sexual means (*Nature* **401**, 6747).

Peloric *Linnaria*.

In *Somnus Plantarum* (Petrus Bremer, 1755) Linnaeus finally considered that permanent varieties had originated from "genetic" circumstances and not from the nature of the soil – in other words through hybridization.

Thus Linnaeus concluded that genera and species were produced by natural hybridization, that is sexual reproduction. His explanation of how this occurred is outlined in several publications including *Metamorphoses Plantarum* (1755), whereas the most compendious expression of his hybridization theory occurs at the end of the 6th edition of *Genera Plantarum* (1764).

According to Eriksson (1983), Linnaeus's ideas stemmed from Andrea Cesalpino (1524–1603) who proposed that all plants have two distinctive anatomical structures – the cortex and medulla. Linnaeus imagined that hybridization involved the combination of an outer cortex (which in plants includes the stamens of the flowers) and an inner medulla (which makes up the pistils). He further believed that the male cortex formed the roots and the external appearance or form of the plant and was therefore responsible for its nourishment, while the female medulla provided for its inner life. Eriksson (1983) made the point that:

> "Linnaeus goes so far here that he is inclined to attribute some sort of will, an inclination to form to the medulla. This is as near as he ever came to a vitalistic point of view."

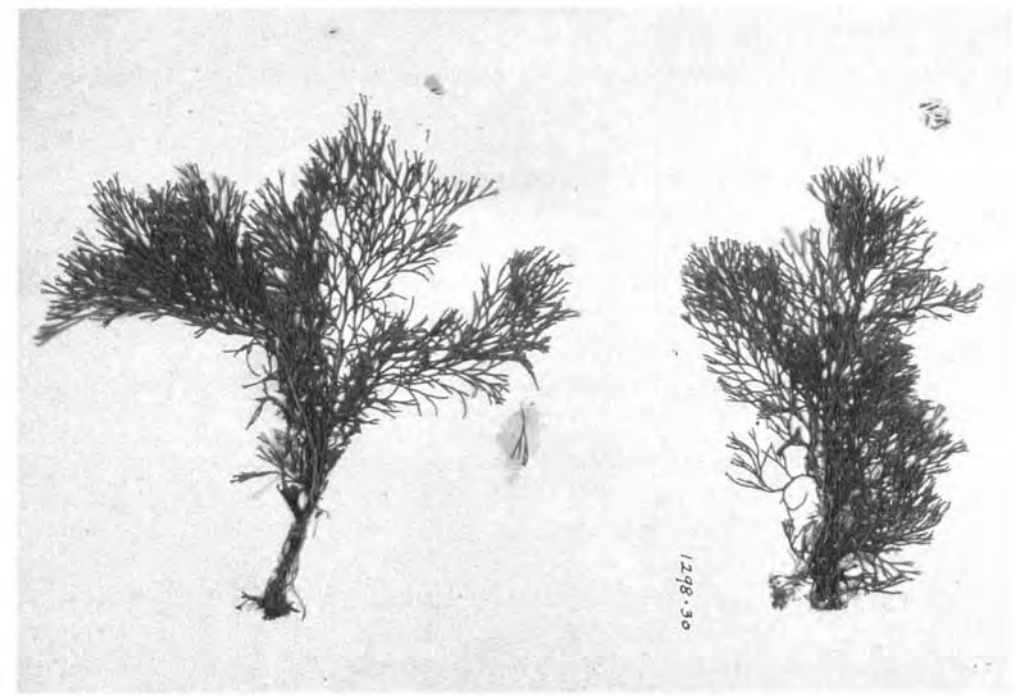
Cain (1993b), on the other hand, found that Linnaeus's library contained a number of occult or semi-occult treatises. These, he reasoned, help explain why Linnaeus viewed himself as an illuminant and why he developed the theory of the cortex and medulla as the means of evolution of plants by hybridization and the key to all medicine.

Linnaeus believed that every species was not created by God but by nature's laws. These laws (as Broberg, 1979 remarked) "were however, designed by God who seems to have withdrawn from direct interference with his Creation – so leaving only natural orders as the original act of Creation." Linnaeus completed his ideas on the derivation of species by suggesting that permanent varieties arose as a consequence of hybridisation between species from the same genus.

In his dissertation submitted to the Imperial Academy of St Petersburg: *Disquisitio de ..... Sexum Plantarum* (1760), Linnaeus endeavoured to demonstrate that sexuality was the ultimate force in nature through the integration of his theory of hybridisation and the sex of plants into the "chain of being".

> "That the subject may be properly understood, it is in the first place necessary that we should accurately understand the nature of vegetable bodies. In order to do which, we must pursue the great chain of nature till we arrive at its origin; we should begin to contemplate her operations in the human frame, and from thence continue our researches through the various tribes of quadrupeds, birds, reptiles, fishes, insects and worms, till we arrive at the vegetable creation. We perceive the human body to consist of a double principle, the nervous and the vascular; or, what is the same thing, of a medullary and a cortical substance. By the former, I mean the spinal marrow, arising from the organised brain, and sending off the nerves; by the latter, the vessels, with the heart attached to them, by which the medullary part is nourished." (Translation by J.E. Smith, 1786.)

He then imagines that the medullary substance is latent in the egg of the mother whereas the cortical substance is derived from the sperm of the father – thus sexuality is



Sertularia parasitica 1298.34.

the ultimate force in nature. He then points out that the Zoophyta (*Sertularia*, hydroids, etc), which he considered formed a link between the animal and plant Kingdoms, have a rooted base and produce flowers:

> "in short more like plants than animals excepting this circumstance only, in which they come nearest to the latter, that by means of a nervous system, they are endued with voluntary motion... Plants as well as animals consist of two different substances, the medullary and the cortical. The cortical nourishes the plant not only by its root but by its whole surface. To illustrate the generation of plants we must take our first lights from the animal kingdom and pursue the chain of nature till it leads to vegetables."
>
> (Translation by J.E. Smith, 1786.)

Elsewhere in the dissertation, Linnaeus develops the idea of the universatility of metamorphosis (a theme taken up and extended in *Mundus invisibilem* [1767], Broberg,1975). He first considers that insects undergo metamorphosis, which when completed enables the adult to become sexually active. He then draws the analogy that the butterfly is no more like the caterpillar than the flowers to the herb that bears them:

> "The evolution of flowers is exactly similar to the exit of insect from their caterpillar."
>
> (Translation by J.E. Smith, 1786.)

From the opening paragraphs of this dissertation (*Disquisitio de ... Sexum Plantarum* 1760, see above) it is abundantly clear that Linnaeus considered man to be at the summit of the great chain of nature. That man was on the same ladder as the rest of creation, Linnaeus had recognised in his first edition of *Systema Naturae* (1735) when

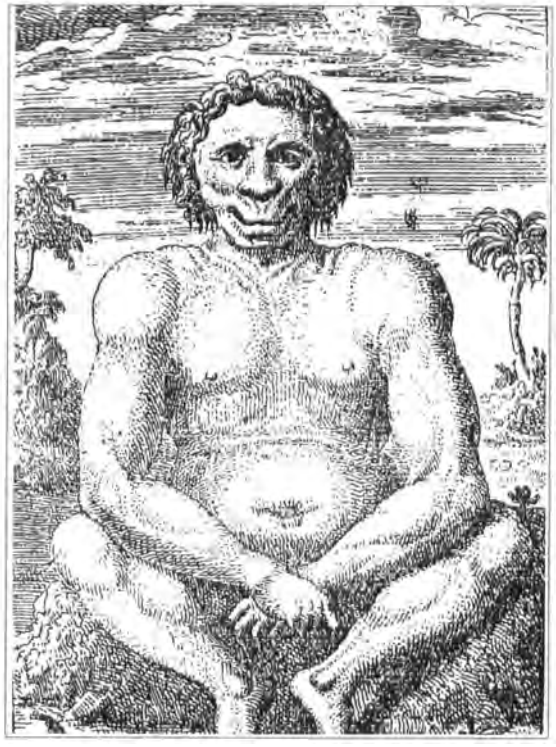
he placed man together with *Simia, Satyrus, Papio* and *Bradypus* as genera of Anthropomorphia in the Quadrupedia as *Homo* with the entreaty *"nosce te ipsum"*.

However, we have to recognise that the methodology used in this first edition of *Systema Naturae* owed as much to Peter Artedi as to Linnaeus himself. Linnaeus and Artedi were fellow students who first became acquainted in 1729. They soon found that they shared similar aspirations and together they decided to classify and arrange the natural world. Artedi, being more interested in zoology, took the fishes, amphibians and reptiles – and the Umbelliferae. Linnaeus, who was already working on his sexual system for plants ("the singular structure and remarkable office of the stamens and pistil enticed my mind to enquire what Nature had concealed in them. They commended themselves by the function they perform") took all the remaining vegetable kingdom, the insects and birds. Both decided to work on mineralogy and mammals.

It now seems clear that the decision to classify man with apes was taken jointly by Artedi and Linnaeus. That this is indeed the case can be deduced from Broberg (1983) who has summarised an unpublished MS of Artedi (written in the early 1730s) entitled *Idea Institutionum Trichozoologiae* in which he included man with the apes close behind. Sadly Artedi was drowned in an Amsterdam canal in the early hours of September 28th 1735, following a convivial evening, drinking with his employer Albertus Seba. Previously, Linnaeus and Artedi had made a pact – if one should die, then "the other would regard it as a sacred duty to give to the world what observations might be left behind by him who was gone." Accordingly, in 1738, Linnaeus published Artedi's posthumous work *Icthyologia.* In so doing, Linnaeus has shown us what an important part Artedi played in the birth of systematics and the methodology he himself used in the *Systema.*

Following criticism from Wallerius (1741), Klein (1743), Gmelin (1747) and many others who objected that man could not be called "like himself," nor could he be called four footed, since he was clearly bipedal, Linnaeus changed the terms Quadrupedia to Mammalia and Anthropomorphia to Primates in the tenth edition of *Systema Naturae* (1758). He also gave man the binomial name *Homo sapiens*. In the same edition there is a sensation – Linnaeus introduced a completely new species of man: *Homo troglodyte* meaning night man or cave dweller. Moreover, in a footnote on the same page, he introduced a third species: *Homo caudatus hirsutus.* Hence, having placed man in his classification, Linnaeus saw no reason why he should not include more than one species. Sadly, he had to rely on published accounts, many of which were either wildly inaccurate or totally unreliable. *Homo troglodyte* is probably an orang-utan (Beckman, 1714), while behind *Homo caudatus* stands a human with an atavistic extension of the caudal vertebrae (Broberg, 1983) or perhaps an hamadryas baboon (Broberg, 1975). Linnaeus also included two apes in the tenth edition: *Simia satyrus* and *S. sylvanus* as well as the baboon (*S. sphinx = Papio).*

In his *Philosophia Botanica* of 1751, Linnaeus had propounded the aphorism (77) "Natura non Facit Saltus" ("there are no leaps in nature") His hybridization theory additionally proposed that "any gaps are because we do not as yet know all of the animals and plants in the world." In this fashion Linnaeus strove to find intermediate links between the three human species and the apes. This resulted in the dissertation



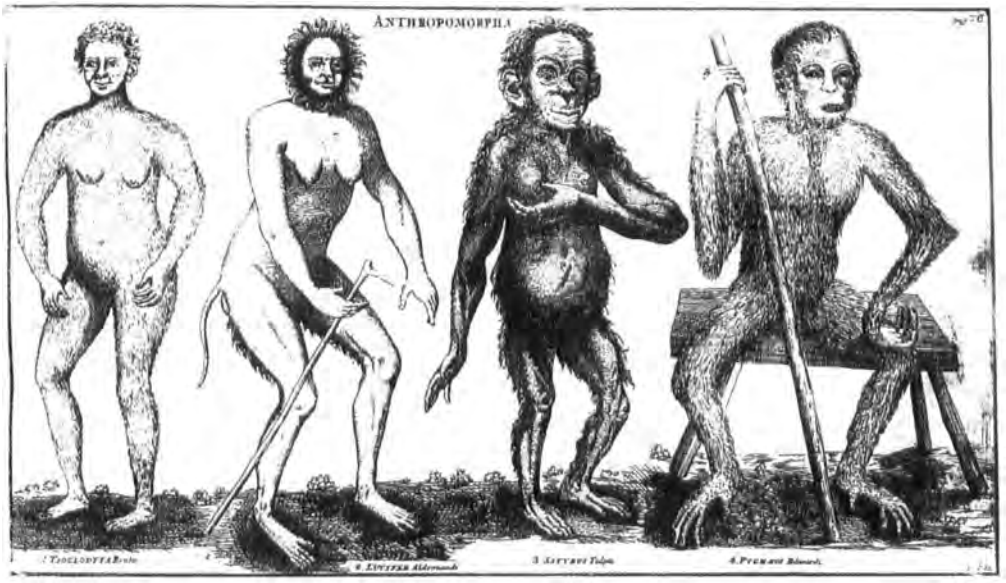
Possible model for Linnaeus' *Homo troglodyte* (from Beckman's *A voyage to and from the Island of Borneo*, 1714).

Anthropomorphia (C.E. Hoppius, 1760) where Linnaeus described two new species of man – *Homo troglodyta* Bontu and *Lucifer Aldrovandi (= Homo caudatus)* – and two intermediate forms between man and apes, *Satyrus tulpii* and *Pygmaeus edwardi*.

Pygmaeus edwardi, Broberg's (1975) preferred candidate for the orang-utan, Linnaeus considered to be closest to the apes and to be more a distant relative than *Satyrus tupii* (pygmy chimpanzee, Broberg, 1975). He emphasised that although the forehead was hairless and the head round, the hindquarters of the *Pygmaeus* were far more ape-like than human. Then he pointed out that *Satyrus* differed from *Pygmaeus* in having its face, abdomen and arms virtually hairless, but despite walking upright, it had hands for feet and therefore could not be very closely related to man.

In the last edition of the *Systema Naturae* (1766) Linnaeus considered *Homo sapiens* to have originally been a native of the tropics and a vegetarian (dates being his original food) pointing out that man now supports his existence by means of agriculture – adding animal food to that intended for him by nature. Linnaeus then enumerated five varieties: *H. americanus, H. europaeus, H. asiaticus, H. afer* and *H. monstrosus*. The latter included several other varieties occasioned by peculiarity of climate and/or artificial management. One such group – Monorchides – Hottentots having one testicle extirpated – is struck through in Linnaeus's own interleaved copy which contains his corrections, signifying that he no longer regarded it as a variety!

After dealing with *H. sapiens,* Linnaeus then concludes with *H. troglodyte.* In a



Anthropomorpha (C. E. Hopkins, 1760)

footnote he adds that *H. caudatus* "ranks as a fairly close cousin of ours" and comes from Java and Nicobar. Again, in his interleaved copy of *Systema Naturae* (1766) as with the Monorchides, Linnaeus has double scored lines through *H. troglodyte* and *H. caudatus*, with the implication that he no longer regards them as members of the genus *Homo*.

Nevertheless, despite the realization that these two species of man were invalid, Linnaeus continued his search for further human species. Thus in the 1771 supplement to *Systema Naturae (Mautissa Plantarum altera .... Regni Animali* appendix), Linnaeus introduced a fourth species: *Homo lars*, said to come from the Moluccas and the forests of Bengal. His description closely matches that of a gibbon. Whatever Linnaeus's and Artedi's motives concerning the classification of man, there can be no doubt that Linnaeus's inclusion of man as part of the chain of being in the first edition of *Systema Naturae* (1735) dispelled the mystery surrounding the designation "Man" (Broberg, 1983). His subsequent inclusion of *H. troglodyte, H. caudatus and H. lars* within the genus *Homo* showed both his belief in intermediate organisms and in his hybridization theory (which led ultimately to permanent varieties).

Finally, in his capacity as an illuminatus or magnus (Cain, 1993b) in the third and final part of *Systema Naturae (*1768), in that part dealing with minerals, Linnaeus attempted to integrate his hybridization theory into the origin of life. Here, citing Thales and Moses, he argues that water was the origin of all life. He then continues, emphasising that the creator proceeded from the simple to the more complex, suggesting that:

> "the minerals came into being from primordial "marriages" between the "fatherly" salts and the "motherly" sands in continuous crossings in a manner similar to the crossings between the higher taxonomic groups among plants and animals." (Broberg, 1979: 39).

CONCLUSION

Linnaeus's views on hybridization and on evolution did not alter his avowed intent of revealing, in a systematic way, the divinely ordered works of Creation. In so doing his most useful contribution to biology was his introduction of a consistent binomial-specific nomenclature for animals and plants. He also recognised that organisms could be classified into major groups by two main methods – one using natural characteristics, the other based on superficial and obvious characteristics arranged in an arbitrary manner.

Linnaeus's major divisions of the animal kingdom are somewhat less artificial than those of the plant kingdom (Stearn, 1971).

As Alessandro Minelli (1999) has just written in the preface of the new edition of the *International Code of Zoological Nomenclature:*

> "The Linnaean hierarchy will not be able to survive alone: it will have to coexist with the ideas and terminology of phylogenetic systematics. It is too prescriptive in that it forces all taxa (and their names) into the arbitrary ranks of the hierarchy: too permissive in that it may be equally applied to paraphyletic as to monophyletic groups.
>
> The Linnaean tradition will be supplemented not replaced by new semantic and lexical tools."

Linnaeus's major groups for plants, on the other hand, being based primarily on the number of floral parts, are artificial, whereas the arrangement of the genera within those artificial groups is often quite natural (Stearn, 1971). As far as the *International Code of Botanical Nomenclature* is concerned, Crane & Kendrick (1997) have pointed out that whereas recent cladistic analyses of green plants recognise an extensive hierarchical series of relatively well-supported groups, the *Code* places emphasis on a mandatory hierarchy in which different ranks have different formal rank-based endings. They suggest that the *ICBN* should de-emphasize the importance of ranks and relax the constraints on how they are treated, concluding that modifications of the Botanical Code are urgently needed to permit integration of systematic knowledge and botanical nomenclature.

ACKNOWLEDGEMENTS

The above paper formed the basis for the Grant Memorial Lecture delivered at University College London on Wednesday 17 November 1999. I extend my sincere thanks to Professor J.W.T. Moody FLS whose initial summary of Linneaus' ideas on evolution prompted me to give this lecture.

B.G. GARDINER

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Library

Plumbing work in September has, we hope, solved the long-term problem of water leaking from above. The damaged pipes mentioned in previous issues have been replaced by new plastic pipe-work and, by raising the input level, the flow now has more of a "head" to help prevent future problems. Meanwhile work on installation of new water mains for Burlington House still has not been completed and persistent drips in the East Basement stores remain to be rectified. Luckily this is well away from any of our journal stores but does not help in keeping humidity levels down. We look forward to resolving that problem before the end of the year and already have plans for cleaning and renewing the lighting in this badly-lit and very grimy area. This will be done as soon as we are reasonably confident that there is no fear of further flooding.

The book sale has so far brought in round £700 to the Library purchasing funds as well as adding some "gap-fillers" to our collections. A massive influx of material from Mr Stuart Baldwin helped increase the range of material on offer as did the duplicate material received from the estate of the late B.E. Smythies. Some leftover material will go to charity shops and other good homes, including educational institutions here and overseas as well as to BookNet, which helps makes them available to a wider public. Although we have a certain amount of unsold "classics" which will go into the next sale we are always happy to take your unwanted books for the next occasion but prefer to be warned in advance if you plan to deliver any substantial volume of material.

Library Donations: September & October 2000 with selected other accessions this year.

We are grateful for all the following gifts to the Library this autumn. Some were received earlier but omitted from the last listing as we were advised by the Editor (wrongly as it appeared) that space would be restricted. This time I have tried to make sure that all significant donations, received by the time of going to press, have been listed but I may have missed some that came in to the Office and have not yet reached the library or which appeared in the Library without any indication of the donor. I have also added a selected list of other purchases with special emphasis on floras, faunas and evolutionary biology as there has not been space for a fuller accession for last year.

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Book Reviews

A beginner's guide to secondhand bookdealing, by Baldwin, S.A., 1999.
Witham: Stuart Baldwin, x+214pp. ISBN 0 9508063 5 8, Hardback £24.

Frankly, I approached this book with a biased and jaundiced view. Many is the time I have seen Stuart Baldwin manning his book stall at meetings, stocked with reprints and books of departed colleagues and even being forced to dig into my pocket and buy back reprints of my own authorship because I had long lost my only copy through student attrition. And my apprehension was sharpened when I read p.2 "The major aims of this book are to make money for the author-publisher (so he can buy more books for his library)...". Not an auspicious start! But I was reconciled by the message of authority

and practicality that this book offers, both for those who want to set up a second-hand book dealing business and those of us who may not realise what we have accumulated through simply being bibliophiles.

Stuart Baldwin's credentials have been gained through the school of hard knocks. Starting as a computer salesman, graduating through the rites of a lecturer in setting up small businesses, to translating his hobby (secondhand bookselling) into a business guarantees that the reader gets a straightforward and practical account of what this trade entails. It's an autobiography with suggestions, thrown out at low cost, to like-minded aspirants.

Visiting Stuart Baldwin's 'Fossil Hall' den is like delving into Pandora's Box. The books are passionately cared for, categorised for easy recognition and encompass such a diversity that I doubt that any palaeontologist would come away without buying one exemplar. He is certainly on top of his trade, knowledgeable in all aspects of palaeontological and geological publishing. For those of us who are entertaining the idea of setting up a secondhand book business this book contains a great deal of sound practical advice on how to make connections, how to prepare a business plan, how to convince your bank manager for the loan you need, how to price the books, how to use the internet and produce catalogues, and what brand of leather polish might be used etc.

But what is there in this book for the vast majority of us who are customers? Surprisingly there is a wealth of information – for here are the poacher and gamekeeper exposed together. Baldwin cites lots of references to major catalogue series which quote prices, auction or catalogue, such that it should be possible to check out whether your 'Anon (Chambers) – *Vestiges of Creation*' should really be let go or bought for that price. And, money aside, many of the references direct you to sources of genuine academic interest such as year of publication and how many impressions were made (I've tried a few). This information is given alongside interesting gems such as the meaning of abbreviations used commonly by booksellers for the quality of a book or the real meaning of folio and octavo etc. References to trade associations are also given but although many are listed the annotations really only deal with those operating within the UK.

Stuart Baldwin's entrepreneurialism comes through in the four chapters dealing with setting up the business. The remarks here may be as easily applicable to selling garden produce as books. As an aside, I note that my opening remark of taking a jaundiced view is echoed here by Baldwin's similar reaction to bank managers ...(p.137) "You should bear in mind .....their job is to make as much money for the bank as they can."

The last section of the book is devoted to e-mail and the internet as a vehicle for communicating with bibliophiles, finding and selling. Of course, he is right. This is already the major medium for selling and buying. Many URLs are listed, although some will prove to be ephemeral.

For me this book was a candid revelation of the real world of secondhand book buying. There is no pretence that all is done for the love of books. There is a harsh business reality nestling between the leather that we need to be reminded of now and again.

PETER FOREY
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Time, Love, Memory: A Great Biologist and His Search for the Origins of Behavior, by Jonathan Weiner, New York, Vintage Books, 2000. 300pp., Published in UK by Faber, Price Hbk. £18.99, Pbk. £8.99. ISBN 067976390–2.

The shortest read I recall is Kant's *Critique of Pure Reason*. Try as I may, I have not succeeded in turning over the first page. I can say much the same for *Bradshaw* which, for younger Members, was an annual UK railway guide which became extinct with the demise of the original private railways way back in the last century. Yet the Oxford University Railway Society spent most of its time, as far as I could see, poring over the tattered volumes in my tutor's rooms to establish the cheapest or quickest way of travelling from Portishead to Great Yarmouth on Sunday in 1876. Tastes do vary.

Like many another scientist, at Christmas kind people give me volumes on dinosaurs, cetaceans, orchids, cloning, foods – all life is here. Such books are generally written by scientists in aid of what is known as The Public Understanding of Science. In each book, the drift usually becomes evident after a couple of chapters and should I persevere to the very end, I will find, as Gilbert wrote, "merely corroborative detail, intended to give artistic verisimilitude to an otherwise bald and unconvincing narrative". Bald and unconvincing? Well, there is certainly a spate of such works at present, yet the public position on biological matters remains one of suspicion and woeful ignorance. Many of these volumes must suffer the fate of Kant's lugubrious *Critique*.

Need it always be so? Probably not. There is a ready sale of literature, which people read in trains, aeroplanes, in bed or on holiday. Most hardly represent an intellectual challenge, but they have a beginning, a middle and an end which urge the reader to press on. Is science like that? Given the truly awesome increase in our knowledge of life's processes, it surely ought to be. Why cannot books be written which grip the reader, any reader, in putting across modern biology? I have good news. There are books of this sort. Jonathan Weiner FLS has written *The Beak of the Finch* showing evolution in action on the Galapagos Islands. His latest work, *Time, Love, Memory,* is a geneticist's view of the origin of behaviour. Seymour Benzer started life as a physicist, served his apprenticeship to become a sorcerer in microbial genetics (as was the fashion in the 50s and 60s) and then in the late 60s turned to thinking about the biological basis of behaviour. This is dangerous ground, as E.O. Wilson discovered after he published *Sociobiology*. One of Benzer's family helpfully suggested that, before he did any research on fly brains, he should have his own examined. James Watson damned his ideas out of hand. Watson also damned E.O. Wilson at Harvard at about the same time, this time for being an ecologist. In both cases, Watson lived to rue his comments. But Watson's approval counts for a lot in US science.

Yet Benzer persevered. His chosen organism was the fruit fly *Drosophila melanogaster*, known to countless generations of budding biologists as Dros. With the invaluable benefit of hindsight, it turned out to be an inspired choice. Dros breeds rapidly and easily. Benzer has been able to bring all his wizardry as a physicist, chemist and microbial geneticist to his Dros laboratory, or 'Fly Room'. Mutants are generated by a chemical mutagen widely used in molecular biology. Initially, mutations were mapped using the classical crossing-over technique of T.H. Morgan; later, identification has been by DNA sequencing. All that has been needed are some reliable, reproducible and quantitative tests for Dros's sense of time, its quest for sexual satisfaction and its ability to

remember things. Benzer and his colleagues have brought great ingenuity to bear in designing the tests, segregating mutants and analysing the results. The results show that these muscarious mental abilities are, to a significant extent, inherited.

As with Wilson's extrapolation of ant social behaviour to a wider range of species, these results have not met with universal recognition, but Benzer has kept a much lower profile than Wilson on such issues. Indeed, prior to reading *Time, Love, Memory*, my main knowledge of Benzer was through a black mongrel dog of the same name owned by one of the population genetics group at York University in the 60s. As the departmental safety officer, I found the dog Benzer a considerable nuisance. The real Benzer provides a unique and fascinating thread in Weiner's book, interwoven with the scientific one outlined above. The book is not simply a record of recondite but ultimately mind-bending experimentation in America's prime universities and institutes. It also relates the social life of Benzer and his colleagues, the academic and personal lineages of the people involved, their routines (Benzer works at night), the battles they fought to preserve their research against encroaching molecular biologists and others, the things outside their laboratories that made them tick (or not – both Benzer's first wife and his great mentor Max Delbrück died of cancer in 1978).

The result is a book which is brimming with both scientific and personal incident. A book which does, indeed, have a beginning, a middle but given the nature of the subject no end – a great read. Where will it all end? In the April 2000 issue of *Scientific American*, Dr. Joe Tsien elaborates on the *Doogie* (smart) mouse, an animal which has been genetically engineered to have a better memory than its less well endowed fellows. Muscarious or murine, it seems that we can empower species with additional capabilities which would have seemed like science fiction only a few years ago. But we need to keep a sense of proportion. As Dr. Tsien says, we are unlikely to genetically engineer mice to play the piano. Or fruit flies to read Kant. *Unlikely* is the *mot juste*.

JOHN MARSDEN