

Editorial

This *Linnean* contains an article on “The Origin of Life” – somewhat surprisingly both Wallace and Darwin also addressed this subject.

Initially in 1862 Darwin wrote:

“It is mere rubbish thinking at present of the origin of life; one might as well think of the origin of matter”.

That same year Herbert Spencer, when questioned by Bates and Wallace on the great unsolved problem of the origin of life, replied that it was too fundamental to even think of solving at present – since not enough was known of the essential constituents of matter.

Just nine years later Darwin (aged 62) had changed his mind and imagined life to have originated:

“in some warm little pond”.

Wallace who had first asked the question of Herbert Spencer 40 years earlier answered it himself at the age of 80 in *Man's Place in the Universe* (1903).

Like Darwin he imagined that life started in an oxidizing rather than a reducing atmosphere – comprising oxygen, carbon dioxide, nitrogen and ammonia, with the latter being produced by electrical discharges through the atmosphere. Wallace further concluded that the universe consisted of the same kinds of matter throughout, thus there could only be one sort of life comprising these same chemical constituents that go to make up the living organisms on earth. He further concluded that only a planet such as earth, of optimum size and in a so-called optimum position (near what he imagined was the centre of the universe) was likely to provide those conditions favourable for life.

Three years later he followed this up with a further book *Is Mars Habitable?* (1907) as a rebuttal of Lowell's 1906 book – *Mars and its canals* – in which he, Wallace, argued that the canals were a natural phenomena (viz. not man made) such as seen in the cracking of basaltic rocks and that there was a scarcity of both atmospheric water vapour and surface water on Mars while average temperatures appeared to be very low:

“I am more than ever convinced that Mars is totally uninhabitable”.

Moreover he concluded

“no other form of life other than the type found on earth could have existed on Mars.”

Interestingly NASA supposes that some of the meteorites recently recovered from Antarctica came from Mars – furthermore they are said to contain signs of life in the form of fossil blue green algae/bacteria. If Wallace were alive today – would he have believed them?

This issue also includes two bicentenary articles. The first an extended account of Lyell's fossils (as promised) which really does conclude our celebration of his birth (1797); and the second marking the election of Edward Jenner as a Fellow of the Linnean Society in 1798.

Jenner's great contributions to natural history were his studies on the Cuckoo (*Phil. Trans. Roy. Soc.*, 1788) and on bird migration. At that time Linnaeus believed that swallows did not migrate

“but retire under water and live therein, all the winter”

whereas J.Th. Klein (Secretary to the City of Danzig) assumed that the sand martin retired

“into the holes in which that species bred up their young, and make their summer residence”.

It was, however, another FRS, Peter Collinson, who showed that both these views were incorrect (*Phil. Trans. Roy. Soc.*, 1760 : 459). Collinson noted that swallows immersed in water died, furthermore in September the swallows assembled in the reeds of the islands in the Thames but when the willows were later pollarded no swallows were to be found. He also got the Vicar of Byfleet to investigate the nests of sand martins – which proved to be empty during autumn, winter and spring.

As for the cuckoo, Linnaeus pointed out – that from the time of Aristotle it had been known that it laid its eggs in other birds nests¹. Then noting that the Italians (and Latins) called the bird *cucculo* or *cucco* while they applied the term *cuculus* to a husband who is unfaithful to his bed (hence cuckold) he named, the cuckoo *Cuculus*.

Society News

Dr. Norman Myers FLS has been made a CMG in the recent honours list.

Professor Gren Lucas OBE, Treasurer of the Society, has been appointed by the Deputy Prime Minister to the Council of English Nature, where he joins another Fellow, Professor David Hawksworth. The Earl of Cranbrook FLS has retired as Chairman and has been succeeded by Baroness Barbara Young from the RSPB.

Sadly, the Society has lost four of its stalwarts. **Dr. Colin Patterson FRS** died on 9th March; a memorial has been arranged in the Society on the afternoon of 17th July. On 7th April, **Dr. Ronald Keay CBE** died. He had been a Council member for four separate terms, a Vice-President three times and Treasurer from 1989–95. An obituary of Dr. Keay is to be found elsewhere in this issue. On 7th May, **Professor John Heslop-Harrison FRS** died. An appreciation of Professor Heslop-Harrison's life and work appeared in *The Linnean* in October 1996 (12, 4-5) in connection with the award of the Linnean Medal for Botany to him in May 1996. **Professor R.J.G. (“Bob”) Savage** died on 9th May; an appreciation of Professor Savage appeared in the *Zoological Journal of the Society* 112 3-12 (Sept/Oct 1994). Professor Savage was

¹ Linnaeus also explained that the ancients thought it a metamorphosed sparrow hawk; Gmelin pointed out “The opinion prevails among the vulgar of Suffolk that cuckoos are transformed into hawks in winter” (see Sheppard & Whitear - 1827 ‘A catalogue of the Norfolk & Suffolk Birds’. *Trans. Linn. Soc.* 15: 28). “In July I saw several cuckoos skimming over a large pond; they were feeding on dragonflies. Notwithstanding what Linnaeus says, I cannot be induced to believe that they are birds of prey”. (Gilbert White: *Natural History of Selbourne*).

also the Society's nominee on the Council of the National Trust.

New Foreign Members elected to the Society are **Professor Kåre Bremer FLS** from Sweden, **Professor Friedrich Ehrendorfer** from Austria and **Professor Niels Kristensen** from Denmark whose citations are given below.

Kåre Bremer is Professor of Systematic Botany and Dean of Biology at Uppsala University. Interests include phylogeny, evolution, biogeography, and classification of the Asteridae, a large group of flowering plants, and flowering plants in general. Other interests include cladistics, principles of phylogenetic reconstruction, cladistic analysis of morphological and molecular data, historical biogeography, and cladistic classification. He holds Linnaeus' chair and is an important member of both the Swedish Linnaeus Society and the Swedish Botanical Society.

Friedrich Ehrendorfer is Universitäts-Professor Emeritus at the University of Vienna, Austria, where his services to botany are being recognised by the award of FMLS. He is the author, co-author or editor of more than 230 publications in many journals, books and monographs. He is also Managing Editor of *Plant Systematics and Evolution*. His interests are wide ranging in the biology, biosystematics and evolution of higher plants, in phytogeography and in the ecology of terrestrial biota. He is perhaps best known for his seminal contributions on the Asteraceae family.

Niels Peder Kristensen is Professor of Systematic Entomology and Chairman of the Department of Entomology at the Zoological Museum of the University of Copenhagen. From 1986–89 he was Director of the Museum. He is a Fellow of the Royal Danish Academy of Sciences and Letters. Niels Kristensen is renowned for his outstanding work on the morphology and phylogenetic systematics of the primitive Lepidoptera. Our profound knowledge of the major lineages of this important order of insects is due largely to his illuminating research and leadership. He has made important contributions on the subjects of butterfly morphology and systematics and on the phylogeny of the insect orders. Professor Kristensen is a scientist of enormous scholarship and charm.

A single Fellow *Honoris causa* has also been elected. **David Frederick Attenborough FLS**, broadcaster and author has for many years been the leading interpreter of biology to the public through his many television programmes and books. Such series as *Life on Earth* in 1979, *The Living Planet* in 1984 and *The Private Life of Plants* in 1995 have brought natural history to the world. He has tirelessly promoted both science and conservation. He has received many awards and honours for his work such as the Command of the Golden Ark from the Netherlands, a Fellowship of the Royal Society and numerous honorary doctorates and medals. Many people have been inspired to become scientists, ecologists, naturalists and conservationists through his work. He is long overdue for recognition as a Fellow *Honoris causa* of the Linnean Society of London.

The Society was privileged to hear Colonel James Baker speaking on conservation on the Ministry of Defence Estate. Colonel Baker would be happy to talk with any member of the Society who may wish to carry out biological work on the Estate. He can be contacted at DEO (L) Conservation, Blandford House, Farnborough Road, Aldershot GU11 2HA.

Electronic Mail

Electronic mail has been in the Society for over two years. For *brief* messages to busy people it is a real boon. There have, of course been "improvements", such as attachments, where the layout of the original document is maintained during transfer, and faster speeds of transfer. These conspire to vitiate the original intention of e-mail, namely a cheap and cheerful way of sending and receiving short messages, which recipients can read at their leisure. We have a distributed system round the office, and a maximum information transfer speed of 28.8Kb/s. So a message of 1Mb (around the size of an issue of *The Linnean*), will take 35 secs to receive, *assuming that information transfer from our provider is at the maximum rate*. It seldom is; speeds of 20% or less of the maximum are more usual, so 2-3 minutes is the reception time. This costs money (telephone time). We have put a stop on our system so that messages larger than 1Mb are simply returned to the sender unread. Articles for *The Linnean* sent in by post in 14pt Times New Roman can be scanned here for inclusion in the copy.

Attachments sound wonderful and undoubtedly, when they work, they are. Mostly they do not because (i) they are encoded in some obscure cipher that we do not run, (ii) they have been put together on a platform which is incompatible with our own, e.g. *Word 7*, (iii) in order to ensure that the information is transmitted correctly, much additional information ("verification bits") needs to come with the original message, increasing its computer size by a factor of ten, and (iv) they are infested with viruses (see *The Linnean* *passim*), in which case our virus checker may warn us of this and the messages are deleted without any further ceremony.

You have been warned.

Picture Quiz

The January Quiz (13(4): 9) featured Miss Etheldred Benett (1776–1845).

"A lady of great talent and indefatigable research to whom I am under infinite obligations for many valuable communications on scientific subjects" wrote Gideon Mantell in the *Fossils of the South Downs* (1822:177), when he acknowledged the help of his correspondent in naming the sponge *Ventriculites Benettiae* after her. Although renowned and respected in her own time by geologists and other naturalists, until recently there was little biographical information available on Miss Benett. She was born in 1776, a daughter of Thomas Benett, a Wiltshire squire living at Pyt House, near Tisbury and spent most of her life at the family home of Norton House, Norton Bavant, near Warminster. Probably, her brother-in-law, the botanist and antiquary Aylmer Bourke Lambert (1761–1842), encouraged her to collect fossils. But, it is just as likely that the rich faunas around her home and also along the Dorset coast, where the family habitually spent a summer holiday, led to this interest. Unmarried, as a young woman she had both the time and resources to participate in the developing science of geology and adopt William Smith's stratigraphical principles when collecting. Most years she endeavoured to spend a month or so in London – *"as it is*



Clue – Said to have anticipated the Theory of Natural Selection.

the only pleasure I have in the year'; and during the Autumn stayed at Weymouth "where I cannot help collecting the fine fossils ... though I have had such quantities of them ...". Rejecting Mantell's suggestion to visit Portland (2nd Nov. 1835) she commented: "A lady going into the quarries is a signal for the men begging money for beer, and the few times I have been there I never got a specimen worth bringing home. All my Portland fossils have been purchased in Weymouth!" Later, she had far less time available for she wrote: "I am one of the working Bees in our family Hive" and for the last twenty years of her life was often incapacitated by illness, when "... I was not equal to the fatigue of searching for the [fossils] myself".

H.B. Woodward was the first to distinguish Miss Benett "as the first lady geologist, who devoted her time and talents to the **systematic study of the science**." He records that the silhouette had been presented to Samuel Woodward in May 1837, when she wrote: "... he has made me in bonnet, cap and velvet spencer;... I should say you have

me not, for I do not think it will give you the least idea of me. The dress I have never [worn] but in my pony carriage, and it makes me look at least ten years older than I am ! ... ” [As an aside, perhaps it is worth noting here that during research I have noted that this form of portraiture seems to have been highly favoured by the Woodward family].

Hugh Torrens (1983, 1989), Mary Creese (1994:26–27) & Sarah Nash (1990) have given an account of Miss Benett’s life and her contribution to palaeontology and the use of fossils through stratigraphy in establishing correlation of geological formations in southern England. Her knowledge of the formations occurring in Wiltshire was used by Greenough in compiling his *Geological Map of England & Wales* (1819) and subsequently published as an appendix to the third volume of *The History of Modern Wiltshire* by R.C. Hoare in 1831. The same year, after further revision it was published separately as *A Catalogue of the Organic Remains of the County of Wilts*, under her own name. The work was widely noted for it contained the description and illustrations of a number of new taxa, including Upper Greensand sponges. She first spoke of this task in a letter to Mantell on 23rd March 1818 : “*You will ... I fear think me bold indeed. ... It must depend on the kind assistance of my friends, but of that I feel secure, and my Wiltshire collection is pretty extensive already ...*”.

But family matters, her own poor health which frequently prevented progress for many years, as well as waiting fifteen years for three scientific gentlemen to assist with the descriptions of bryozoans and sponges delayed things until in the end, she decided to ‘do the best I could’ and finally achieved publication in 1831. In the Preface, she wrote that after such a length of time she had almost despaired of fulfilling her promise but was pleased to state: “*There are 34 new species of shells ... six of which I have figured, the new variety of Trigonia gibbosa being accidentally left out ...*”.

Because of its nomenclatorial significance, Spamer & Bogan (1989), have made some effort to locate and compare copies of the two versions of the Benett *Catalogues* (1831a & 1831b). Although copies were privately published, they have argued (p. 132) that as it ‘was freely distributed within the scientific community’ it has to be considered a valid publication. Their view has been accepted by the I.C.Z.N. [Op. 1609, 1990]. Most surviving copies have had a rather mobile history; 4 copies in the U.S.A. & others in the major British libraries have passed through several hands.

Torrens has pointed out (1983:12) that Miss Benett had been greatly influenced by the work of William Smith and adopted his practice of determining the relationship of geological sequences by their fossil content. Yet, in correspondence she was very disparaging about Smith’s publications even sending his *Strata* straight back when it was sent to her upon publication in 1817. A few years ago, involved in the symposium commemorating the Bicentenary of Gideon Mantell, I had the opportunity to examine the correspondence that she had had with both Mantell and two of the Sowerbys. This revealed their gradual realisation of the different sequences and relationship of the Cretaceous formations in various parts of southern Britain and attempts to correlate them e.g. (Benett to Mantell, 26th Nov. 1820): “*... I expect the Swindon Rock is the same as the Portland, or very near it*”. On another occasion she mentioned writing to Buckland (letter to Mantell, 15th May 1821): “*... I told him of Mr. Conybeare’s &*

Mr. Webster's objections to the term "Oak Tree Clay" and asked his opinion as to the propriety of your using the term "Weald Clay"; or "Blue Marl" for this bed ... he thinks you will be perfectly secure in adopting the term "Weald Clay". Amongst her manuscripts deposited at the Geological Society is a 'Section of Chicksgrrove Quarry, Wilts.', which to her annoyance Sowerby published in 1816 (Mineral Conchology 2, 1816: p.58).

Throughout the early 1800s she corresponded with Mantell in a joint effort to discover *'what analogy ... existed between the Sussex and Wiltshire Chalk'*, each of them providing lists of fossils, exchanging packages of available specimens, and accounts of the sequence and characteristics of local strata. The differences between the formations occurring in the two regions were soon realised but there was always the chance that a new exposure would provide a link, as well as more information. Her letters indicate that she tried to compare or distinguish her fossils and to some extent tried to interpret the significance of their presence, or absence. Understanding the importance of all fossil evidence, Miss Benett gathered fragments at every exposure she visited, knowing that their occurrence could assist correlation in some instances, and in others possibly prove to be a coveted 'new species'. Recognising her limitations Miss Benett declared: *"... I can only say that what I state as facts shall be found to be correct, and ... conjectural information ... must always be received with caution ..."*

The letters inevitably revealed a little of the lady herself. Although helpful she was generally fairly formal and precise, especially at the commencement of her collaboration with both correspondents, referring to herself as 'Miss Benett'. Her contact with James Sowerby was probably made through William Cunnington and a letter from Mantell (30th June 1813) written soon after Sowerby had described the first of her specimens, was sent on the recommendation of Aylmer Bourke Lambert. Even after corresponding with Mantell for thirty years, regarding him as *'my old friend'*, she was not above reprimanding him in a postscript (14th Nov. 1842): *"Pray allow me to remark that you have lately taken to spelling my Christian name 'Ethelred', whereas it is Etheldred as above"*.

It would seem that in her collecting Miss Benett was 'energetic': *'you may sometimes break fifty [nodules] before you find one in this state'*; 'cautious' – *"Mr. S. is inclined to think [this] is a new species, but I think we must wait for more specimens"*; and 'careful' – *"I must rest satisfied until more are found as I would not sacrifice specimens for examination unless I had 3 or 4"*. Other letters indicate that she provided Sowerby with specimens that enabled him to determine the nature of the 'great fibrous–'Pinna' then being found in Cretaceous deposits, that he eventually named *Inoceramus* (see *Trans. Linn. Soc.*, 13:453-8) – by taking them as 'travelling companions' on her journey up to London (letter 10th Feb. 1815). But, understandably, she was reluctant to lend, or send her scarce, more treasured, or fragile specimens, especially as during their transport by wagon they could get broken or lost. Somewhat piqued by one request she wrote: *"You will not wonder that I do not like to send single specimens, more especially as many of them have been in London already and were not made use of!"*

In a letter to Sowerby (21st Feb. 1814) she explained *"I have no pleasure in collecting for myself alone, therefore ... I endeavour'd to get enough for my Friends also"*. In June 1819, she purchased an extensive collection, of which many *'were not worth a pin'*, but she occupied herself in sorting through it and then sending many to the British Museum, *'who badly needed them'*! Other evidence of this altruistic attitude is given by her purchase of a large quantity of Crinoids from a quarry at Whatley, nr. Frome in 1837: *"... I have purchased enough for myself and friends, and the G.S. - B.M. and Oxford Museums and a few to spare"* and indicated to Mantell (12th June 1838) that *"... When fine specimens come my way, I cannot help buying them"*!.

From comments made in her correspondence, Miss Benett undoubtedly collected in the field herself, but in order to obtain good specimens whenever they became available, especially during the years that she was unable to travel, she *'gave liberal encouragement to the local collectors'* (Mantell, 1846:40). This had its disadvantages since unscrupulous collectors, endeavouring to provide something new constructed specimens from fragments. There are frequent references to asking *'my collector'* at Christchurch to send anything *'she'* may have found and as there cannot have been many women looking for fossils at that place, I am tempted to guess that this may well have been the Miss Beminster, who from 1821 sent many specimens to Sowerby. Similarly there is mention of my *'Warminster collector'*, *'of my Man at Weymouth'* and of a servant who on visiting the Isle of Wight returned with a mass of specimens. At different times, she became interested in Recent shells, notably in 1825, when discovering a quantity of fresh-water shells in the village and went to some lengths to determine them by consulting J. De Carle Sowerby. Dr. Mike Kerney has re-determined the shells she listed and suggests that several of these species would be new records for the present national mapping scheme.

Miss Benett suffered from many ailments during the latter years of her life and her letters to Mantell often comment on these frailties and the difficulties they caused when trying to follow her geological interests. Unable to walk easily, her journeys were restricted to those possible by her pony carriage and she declared that she could not summon sufficient courage to travel by the new rail-roads. Towards the end she wrote (9th Aug. 1843): *"My Fossil room is a perfect chaos, it is so very long since I have been able to do anything in it"*; quite often it was far too cold for her to venture into that room. She died on 11th January at Norton House and was buried at Boyton Church.

The subsequent history of Miss Benett's collection, its purchase for £185 by T.B. Wilson through his brother Edward Wilson of Tenby and the agency of Edward Charlesworth, then its donation to the Academy of Natural Sciences in Philadelphia has been described by Spamer, Bogan & Torrens, (1989:118 & 125). Elsewhere, I have listed many of the institutions that have Benett material (Cleevely, 1983:54). Inevitably, the Benett collection contains a number of important and unusual specimens. Among those of considerable interest are some unique specimens of *Laevitrigonia gibbosa* (J. Sowerby) collected by her from Tisbury, in which the body of the mollusc was completely preserved with *"the branchiae or gills being as clearly defined as when the animal was recent"* (Mantell, 1850:197; 1854:41). An unpublished plate figuring the specimens prepared for the *London Geological Journal*, that also

indicated they had been acquired by Wilson, has been in circulation since the 1840s, but until recently all attempts to locate these fossils have been fruitless. Through her link with Mantell, and his archive in the Turnbull Library, in Wellington, New Zealand that contains her letters, it has also been realised that her collection contained examples of *Iguanodon* teeth, some of which could be missing types. No doubt, Miss Benett, would be surprised, but highly delighted that her collection had fulfilled her first wish that it would be useful, still provided so much interest to palaeontologists and that in this circle her efforts will always be 'highly respected'.

R. J. CLEEVELY

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The above article by Ron Cleevely is a break with tradition – it is the first time that someone other than the Editor has compiled the Picture Quiz!

There were two correct answers: Michael Taylor and Richard Wilding.

The October Quiz (13(3):17) featured Charles Lyell (1797–1875) whose *Principles of Geology* (1830–1833) profoundly influenced both Darwin and Hooker.

Charles Lyell and the importance of fossils

The Lyells lived in their country seat at Kinnordy, 6 miles NW of Forfar where Charles was born on November 14, 1797 (the eldest of ten children). That same year his father took a 14 years lease (renewed for a second 14 years) on Bartley Lodge, Stony Cross, an estate of some 80 acres on the edge of the New Forest. The family moved to Stony Cross at the end of 1797 and remained there for the next 28 years but returning every summer to Kinnordy (a distance little short of 480 miles, no mean journey by post-horses).



Figure 1. Lyell's birth-place, Kinnordy House, near Kirriemuir.

Young Lyell initially attended the local school in Ringwood then went to Radcliffe's School in Salisbury and finally he was sent to Dr Bayley's school at Midhurst. In 1816 at the age of 17 he entered Exeter College, Oxford.

Lyell senior² (1767–1849) had moved to the New Forest mainly because of his interest in mosses but also to be nearer to his numerous botanical friends (Borrer, Dawson Turner, Brown and McLeay).

At the end of his first year at Oxford (1817) Lyell started the long vacation with geology as his first interest and the suggestion of his tutor that he attend Buckland's course of lectures next term. Nevertheless his interest in entomology remained. Thus when term finished he paid a visit to London specifically to see Francillion's insect collection. Then he went to stay with his father's old friend Dawson Turner in Yarmouth. On route he called in on James Smith at Norwich specially to be shown Linnaeus's insect collection as well as the British insect collections of Smith and Kirby.

² Lyell senior left his herbarium including 1,673 cryptogams and references to British hepatica to the Natural History Museum 1898.



Figure 2. Bartley Lodge in the New Forest. The wings have been added since Lyell's day.

Also staying with the Turner's in Yarmouth at that time was Dr Joseph Arnold (*Rafflesia arnoldi* see *The Linnean*, 12(3):16). Arnold was a competent geologist and took Lyell on collecting trips around the neighbouring chalk-pits. They collected numerous belemnites, echinoids and ammonites as well as Red Crag molluscs from Sutton while Lyell copied Arnold's list of local fossils to take back to Buckland, at Oxford, next term. From his letters to his father from Yarmouth it is clear that Charles Lyell was already well on the way to becoming a geologist. He is discussing with Arnold the local geological history and speculating with him on the opening of the Straits of Dover and at the same time demonstrating that he is conversant with the views of Werner, Humbolt and Buckland³. Finally he was already contemplating making a geological map of the Norfolk/Suffolk area.

Lyell was awarded his BA (2nd Class) in Classics in 1819. His particular interest in freshwater Tertiary deposits was soon in evidence and we find him examining both the Paris basin and the Isle of Wight (1823) and subsequently reading a paper on the Tertiary exposures of the Hampshire coast to the Geological Society in 1826. Incredibly by the end of the following year (1827) he had delivered his MS for *Principles of Geology* Vol. 1, to the publisher.

Lyell's Fossils

Throughout his career Lyell not only made extensive fossil collections himself, but was also constantly adding to them by purchase, exchange and gift. Some of the more important fossils he subsequently donated to the Natural History Museum. Thus in

3 It was probably this encounter with Arnold during their three weeks at the Dawson Turners that stimulated Lyell's life long interest in Quaternary and Tertiary affairs.

1827 he presented two specimens of a very rare giant beaver (*Trogontherium cuvieri*), one from the Norwich Crag of Thorpe and the other from the Forest Bed at Cromer (subsequently described by Owen in *British Fossil Mammals*).

In the spring of 1828 Lyell accompanied the Murchisons on a surveying tour of French and Italian localities from Auvergne to Padua – lasting some 8 months (Lyell took a servant with him who collected plants for William Hooker and insects for his sisters). Although they did not collect many fossils, Lyell was so impressed by the Auvergne Tertiaries that he returned to tour Auvergne again 16 years later in 1843 – when he examined over 300 squaloids and numerous extinct species of mammal collected by Abbé Jean Baptiste Croizet and Auguste Barnard.

On the conclusion of the tour with the Murchisons, Lyell spent a further 6 weeks in Sicily collecting Tertiary molluscs. It was here in Sicily that Lyell concluded that the proportion of extinct to living species could be used in dividing up his Tertiary classification into systems (see Vol. 3, *Principles of Geology*, 1833). In this task he was capably assisted by Gérard Deshayes who provided him with copious faunal lists of Paris Basin molluscs⁴. Lyell even purchased sets of French Tertiary molluscs from Deshayes which he later (1829) gave to Mantell – these eventually finished up in the Natural History Museum in 1838 (following the sale of the Mantell collection).

Lyell was married the following year (12 July 1831) and on his honeymoon journeyed through France, Germany and Switzerland. In Paris Lyell met Louis Jean Randolphe Agassiz (1807–73) who had ostensibly gone there from Neuchâtel in December 1831 to study the cholera epidemic. However, Agassiz was busily engaged in describing the numerous fossil fishes in Cuvier's collection rather than investigating cholera.

Agassiz had accepted Cuvier's view's with enthusiasm and concluded that there was no generic connection between species from different geological strata: species were immutable. Moreover he tried to explain to Lyell that fishes first appeared in the Silurian and from then onwards there was an increase in the number of fossil species and genera (and similarly with molluscs and echinoderms).

"Species do not pass insensibly one into another, but they appear and disappear unexpectedly, without direct relations with their precursors."

Lyell was impressed. Thus in Vol. 3 of *Principles* (1833: 327) he notes:

"I am informed by M. Agassiz, whose great work on fossil fishes is anxiously looked for by geologists, that after examining about 500 species of that class, in formations of all ages, he could discover no one common to the secondary and tertiary rocks, nay all the secondary species hitherto known to him, belong to genera distinct from those established for the classification of tertiary and recent fish."

Subsequently he and Agassiz became good friends and in 1834 Lyell took him to the British Association Meeting in Edinburgh and afterwards to Brighton to see the Mantell collection⁵.

4 Deshayes later prepared a catalogue of the fossil bivalve molluscs in the Natural History Museum, 1853-4.

Lyell quickly realised that Agassiz – by ensuring that the fossil fishes from different rock formations could be readily identified – was providing him with a tool for stratigraphy (and gave him the means of correlating the Secondary strata). Thus Lyell arranged for Agassiz to have a room set aside in the Geological Society (viz. in Somerset House) for his special use. Agassiz subsequently took and had sent there fossil fish specimens from the Egerton and Enniskillen Collections as well as from the Mantell and numerous smaller collections. Later Joseph Dinkel, Agassiz's artist, spent 7 years (1835–41) in that same room providing the illustrations for Agassiz's masterpiece : *Recherches sur les Poissons fossiles*, Neuchâtel, 1833–1844. By the end of 1844 Agassiz had analysed some 1,700 species and had put forward his notion of progressive development. Money for the project on Lyell's request was generously granted by the British Association for the Advancement of Science.

During 1835 Agassiz was a fairly frequent visitor to the Lyell household and at the Geological Society's Anniversary Meeting in February 1836, Lyell announced the award of the Wollaston Medal to Agassiz and the Wollaston Fund to Deshayes.

Lyell's preoccupation with Tertiary deposits (particularly the Pleistocene) caused him to visit first Sweden (1834) then Denmark (1837) and then Norway (1838). He identified Recent marine beds of molluscs near Stockholm and Uppsala as being of Baltic type while similar beds nearer the junction of the Skagerrak and Kategat he identified as containing molluscs which today live in more saline waters. Lyell was apparently helped in his identifications by the Linnean Collections and by Henrick Henricksen Beck (Prince Christian's Naturalist). Meanwhile he found time to continue his collections from the unconsolidated shell deposits in the Red Crag of Suffolk and Norfolk started with Arnold way back in 1817. Most of the Scandinavian fossils finished up in the Oxford University Museum having been presented by Sir Leonard Lyell in 1903, whereas the Red Crag specimens were given to Sowerby by Lyell himself (these are now in the Natural History Museum).

In 1843 Lyell visited Autun where he collected his first fossil fishes. Through his contact with Agassiz he decided they were palaeoniscids and probably as old as the coal measures; later, however, after Bunberry had identified the ferns and psarrolites which he had also collected, he decided they were Permian. These 6 specimens of *Amblypterus blainvillei* Agassiz collected by Lyell from Autun were eventually presented to the Natural History Museum in 1913 (see below) by Sir Leonard Lyell MP (Lyell's nephew).

In 1841–42 Lyell made his first trip to North America when together with Logan he collected from the St Lawrence region of Canada. The trip lasted 13 months and he and his wife returned to Liverpool on 27 August 1842 with 3 dozen boxes of fossils.

The Lyells returned to North America (Canada) again in 1845–6 but this time he was accompanied on his collecting trips by J.W. Dawson (who like Logan was of the Candian Geological Survey). Together they collected a large series of Carboniferous

5 Mantell's collection contained over 150 species of chalk fishes which had been patiently extracted from the Chalk of Sussex and subsequently prepared by Mantell himself. Agassiz was delighted and eventually figured many of them. They came to the Natural History Museum in 1838 with the rest of the Mantell Collection.

invertebrates – mainly gastropods and lamellibranchs. These were studied by Dr M. de Verneuil and inumerated with his memoranda in Lyell's *Travels in North America with Geological Observations on the United States, Canada and Nova Scotia*, John Murray, Albemarle Street, 1845 in 2 vols. Lyell presented the fossils to the Museum of Practical Geology in July 1855, but they were transferred to the Natural History Museum in 1880. The collection comprised two Carboniferous gastropods from Picton, Nova Scotia (*Pileopsis* and *Conularia*) and some 24 Carboniferous lamellibranchs including: *Cypricardia*, *Modiola*, *Avicula* and *Pecten*, mostly from Turo, Brookfield and Admiral's Rock, Nova Scotia as well as some fish scales from the Coal Measures of Hillsborough, New Brunswick (this donation also included agnathan remains from the Ludlow Bone bed in Wales). Ironically in 1846 Lyell met up again with his old friend Agassiz (who was on his way to Boston) when they discussed London Clay fishes.

The Lyells went to the United States again in the autumn of 1852 and for the last time in 1853 when they visited the New York Industrial Exhibition.

As a result of his 1845 trip Lyell had published a paper in 1847 'On the structure and Probable Age of the Coal-field of the James River near Richmond, Virginia' (Q.J.G.S. 1847, 3 : 261–280 pls VIII-IX). In this paper⁶ Lyell described the fossil fish *Dictyopyge macrura* (Redfield), from the Blackstone Mines, Virginia. Many years later Arthur Smith Woodward, Keeper of Geology at the Natural History Museum, wrote to Sir Leonard Lyell MP (Lyell's nephew and heir) asking him to look round Kinnordy to see if the type specimen of *D. macrura* (Woodwood mistakenly believed it was the

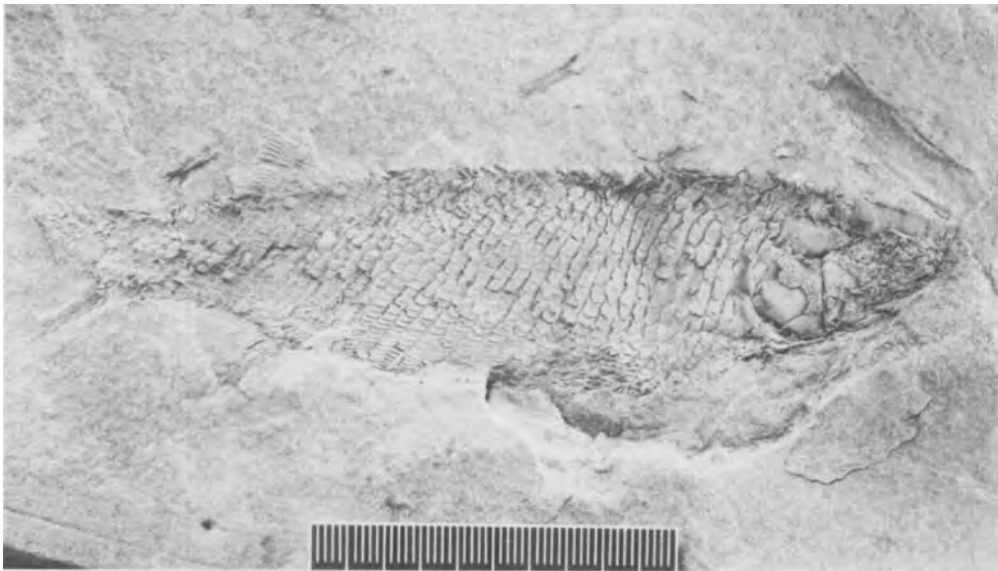


Figure 3. *Dictyopyge macrura* (Redfield) after cleaning.

⁶ Corrected for him by Agassiz and Egerton.

type) was still in the house. Eventually it was found – it had been black leaded and used as a doorstop in the kitchen for the past 66 odd years. The specimen is now in the Natural History Museum together with 2 specimens of the fossil fish *Catopterus gracilis* from Durham, 1 specimen of *Ischypterus* from Connecticut as well as the 6 specimens of *Amblypterus* from Autun (see above) all of which were found by Sir Leonard in his search for the type specimen of *Dictyopyge macura*. Lyell considered the Richmond fishes and coal plants to be Triassic – today they are said to be Lower Jurassic. All these 10 fossil fishes had clearly been particularly prized by Lyell and so had not been presented to any museum but had been retained for sentimental reasons.

He also had kept a fine specimen of a Tertiary shark spine (*Carcharodon augustidens* Ag.) from near Antwerp and this was finally presented to the Natural History Museum in 1980 by the present Lady Lyell.

Lyell and Dawson subsequently published the results of their collecting in Nova Scotia in a paper entitled “On a quadruped and land shells in the carboniferous rocks of Nova Scotia”. The gastropods and lamellibranchs are enumerated above – but the Natural History Museum also eventually received a walrus skull from the Tertiary of Martha’s Vineyard, and tail vertebrae of the whale *Hyperodon* and of a cetacean! Lyell had also been given an anterior tooth and an incomplete cheek tooth of the whale *Zeuglodon (Basilosaurus) cetoides* from the Eocene of Clarksville, Alabama as well as several *Mososaurus* vertebrae from Dallas City – these he presented to the Natural History Museum in 1868.

In the mean-time Lyell had presented to the Natural History Museum two very important specimens – firstly the eurypterid – *Pterygotus anglicus* (Agassiz) from the Lower Old Red Sandstone, Carmylie quarry near Arbroath, Angus in 1845 (thoracic plate and swimming foot subsequently described by Huxley and Salter, 1859) and secondly *Cephalaspis lyelli* Agassiz, the type species of *Cephalaspis* an almost complete animal (in counterpart) from the Lower Old Red Sandstone, Glamis, Angus in 1846. This latter specimen had been figured by Agassiz (1835) and is the genotype of *Cephalaspis* (He also donated Ox material from Ilford, Essex at the same time).

Lyell’s obsession with the Tertiary nevertheless continued, and in December 1853 he left London for a geological tour of Madeira with his wife Mary and her sister Frances and Frances’ husband – the botanist – Charles James Fox Bunbury. The party went on to visit the Canary Islands in February 1854 and returned to England in April.

Lyell apparently had two main reasons for visiting Madeira – firstly to see if volcanic cones were formed by upheaval and secondly and perhaps more importantly to investigate the distribution of fossil and recent land snails – following a report by the Rev. Richard Thomas Lowe (1833⁷) that the Madeiran group possessed some 44 species of land snail unique to themselves.

Soon after arrival Lady Lyell and her maid started a collection of living land snails to be used for comparative purposes. These were presented to the Natural History

7 Richard T. Lowe, “Primitiae Faunae et Florae Maderae et Portus Santi; sive Appecies quaedam novae vel hactenus minus rite cognitae Animalium et Plantarum in his Insulis degentium breviter descriptae,” *Cambridge Phil. Soc. Trans.*, 1833, 4, 1-70.

Museum by the present Lady Lyell in 1976. Meanwhile Lyell and Bunbury collected fossil snails on Madeira and later on Grand Canary. The collections made on Madeira (including some 50 Tertiary molluscs and several species of fern) were presented to the Natural History Museum in 1857. It includes 42 specimens of *Helix*, 6 *Haliotis* and 1 *Calyptrae* all from Barranco de las Palmitas, Madeira, and 4 specimens of *Ostrea* from Cueva de Baez, Madeira. The collection from Grand Canary was presented to the Natural History Museum in 1855 and 1860 by Lyell. It includes 2 drawers of Neogene fossils (some labelled post Pliocene) all from Las Palmas, 1 drawer of *Ostrea*, 2 specimen of *Spondylus* and 3 of *Pecten jacobaeus*.

During his visit to the Canary Islands, Lyell also made collections of fossil polyzoa (=Bryozoa)

“Of my 4 species of Bryozoa from the Grand Canary one is recent and three unknown so says the first rate authority Mr. Busk. One lunulite, one Retepora, one Eschora and one Flustra. I imagine the age may be Miocene or falunian but this is a mere guess as yet.”

These together with Bryozoan material he had collected from the Coraline Crag of Norfolk and from France he presented to the librarian of the Geological Society – William Lonsdale in 1856 who eventually passed them on to George Busk. They finally came to the Natural History Museum in 1899 with the rest of the Busk collection (including some 7,000 slides of invertebrates from HMS Rattlesnake, 181 polyzoa and hydroids from Kerguelen Island and several fragments of Linnean types!). Lyell also collected several Tertiary fishes including the sharks *Oxyrhina hastilis* Agassiz and *Galeocерdo* sp. from Las Palmas, and a tooth plate of the puffer fish *Diodon sigma* from Grand Canary, these he presented to the Natural History Museum in 1860.

Lyell’s visit to Madeira and the Canary Islands however, was to have a profound effect not only upon himself but more importantly on the course of events leading up to and including the publication of the Darwin – Wallace theory of evolution by means of natural selection in 1858.

On Madeira, near Santo Jorge, Lyell had found a bed of lignite containing fossil plants (including *Laurus* and *Myrica*) below a layer of basalt some 1,000 feet above sea level. He concluded that Madeira has existed as a land surface from at least the Miocene and that both it and the Canary Islands had been built up gradually by volcanic action. Nevertheless these islands not only had their own distinctive fossil and recent snails:

“Almost every land shell different from every one living in Porto S.^o, and the fossil helices of Madeira in like manner distinct from the Portosantan whether belong[ing] to the living or extinct shells.”
(to Bunbury, 19 Feb. 1856)

But also insects (information obtained from Wollaston⁸ who had spent a number of years on Madeira for the sake of his health) and plants:

“It seems to me that many species have been created, as it were expressly for each island since they were disconnected and isolated in the sea. But I can show that the origin of

8 T. Vernon Wollaston, *Insecta Maderensia; being an account of the Insects of the Islands of the Madeiran Group*, London, 1854, 634 pp.

the islands, which are of volcanic formation, dates back to a time when the surrounding sea was inhabited by a third or fourth only of the species now existing and all the rest (species of fossil shells, corals, etc.) have died out. But I must not run on as it would take me too long to point out how all these bear on one and the same theory – of the mode of the first coming in of species.” (to Frances Lyell, 17 Nov. 1855)

But why did the Madeiran islands have so many endemic species of mollusc, insect and plant – and no freshwater fishes other than the eel (*Ang. latirostris* Yarr.)?

“The Madeira’s are like the Galapagos, every island and rock inhabited by distinct species. What is a wonderful contrast with the British Isles (above a hundred in number) where the same fauna prevails everywhere, or if not strictly so, has at least in its distribution no respect to the barriers offered by channels of salt water”

(to L Horner, 28 March 1856)

In the autumn of 1855 Lyell had read Wallace’s paper “On the law which has regulated the introduction of new species” and this had influenced him greatly – so much so that he decided to start a note book on “the species question” (Wilson, 1970).

At this point Lyell decided to visit Charles Darwin. The Lyells arrived at Downe on the morning of Sunday April 13, 1856 and departed on Wednesday afternoon, April 16 (Emma Darwin’s Diary).

Initial discussion concerned the geology of Madeira (see letter to Georg Hartung 11, 15 April 1856) but later they examined the whole question of the formation and colonisation of oceanic islands and in essence – speciation itself including Wallace’s views as expressed in his 1855 paper. At this juncture (the morning of April 16) Darwin found it necessary to outline to Lyell the main features of his theory of natural selection in order to account for the large number of species endemic to the Madeira’s. Lyell was clearly very impressed and straightway urged Darwin to publish his theory on the formation of species by natural selection (in a short essay if necessary) in order to assure his priority⁹.

Lyell followed up his verbal blandishment with a note in his next letter (May 1–2, 1856):

“I wish you would publish some small fragment of your data *pigeons* if you please and so out with the theory and let it take date – and be cited – and understood..... The multiple creation of Agassiz will one day rank with spontaneous generation but Madeira seems to me to favour the single birth-place theory and I long to see your application of any modification of the Lamarckian species-making modification system”

Darwin replied on May 3, 1856:

“With respect to your suggestion of a sketch of my views; I hardly know what to think, but will reflect on it; but it goes against my prejudices. To give a fair sketch would be absolutely impossible, for every proposition requires such an array of facts. If I were to do anything it could only refer to the main agency of change, selection, – and perhaps

⁹ Contrary to *The Linnean* 11(1):19 - Darwin does not appear to have shown Lyell his abstract of 1844 - there is no evidence that he had communicated the substance of natural selection to Lyell before the morning of the 16th April 1856 (see Lyell’s notebook: Wilson 1970: 54).

point out a very few of the leading features which countenance such a view, and some few of the main difficulties. But I do not know what to think: I rather hate the idea of writing for priority, yet I certainly should be vexed if any one were to publish my doctrines before me, – Anyhow I thank you heartily for your sympathy.”

After a further conversation with Lyell on the morning of Thursday 8 May at Somerset House (prior to attending a council meeting of the Royal Society) Darwin wrote to Hooker:

“I had a good talk with Lyell about my species work, and he urges me strongly to publish something. I am fixed against any periodical or Journal, as I positively will *not* expose myself to an Editor or a Council allowing a publication for which they might be abused. If I publish anything it must be a *very thin* and little view giving a sketch of my views and difficulties; but it is really dreadfully unphilosophical to give a resumé, without exact references, of an unpublished work. But Lyell seems to think I might do this, at the suggestion of friends, and on the ground, which I might state, that I had been at work for eighteen years, and yet could not publish for several years, and especially as I could point out difficulties which seemed to me to require especial investigation. Now what think you?”
(9 May 1856)

After further debate with Hooker as to whether or not he should publish Darwin finally:

“Began by Lyell’s advice *writing* species sketch” (14 May 1856- Journal)

“on a scale three or four times as extensive as that which was afterwards followed in my *Origins of Species*: yet it was only an abstract of the material I had collected”

This work was steadily continued so that by the time Wallace’s manuscript arrived from Ternate in May/June 1858 overthrowing all his plans, Darwin had completed some ten chapters of his projected book. Darwin immediately forwarded Wallace’s manuscript to Lyell (18 June, 1858) who subsequently accepted Hooker’s compromise that they should not withhold their knowledge of Darwin’s priority – and so arranged for the simultaneous publication of the joint works (Wallace’s manuscript plus extracts from Darwin’s 1844 manuscript) through the auspices of The Linnean Society (1 July 1858)

Several other collections of Tertiary molluscs were given by Lyell to the Geological Society and subsequently presented to the Natural History Museum by that Society in 1911. These include a collection of post-Pliocene gastropods from Harwich (*Helix*, *Planorbis*, *Limnaea*, *Valvata* etc. – 8 specimens in all); a collection of Miocene molluscs from Touraine, France – comprising 10 specimens of *Fusus claratus*, 3 *Mitra tennistria*, 7 *Ancillaria* sp., 1 *Oliva* sp, 3 *Terebraplicaria* sp, 5 *Conus mercati*, 6 *C. acutangulus*, 5 *Pleurotoma obita*, 5 *P. terebra*, 1 *P. granaria*, 3 *Tornatella costellata*, 5 *Ringicula buccinea*; and 1 specimen of *Aequipecten* from the U. Tertiary, Cuba.

Finally Lyell gave a large collection of British fossil molluscs to Sowerby – many of which are figured in his *Mineral Conchology*. These came to the Natural History Museum on Sowerby’s death.

Apart from the collections mentioned above the IGS has a fair number of Lyell’s British fossils (mainly molluscs, echinoderms and belemnites) which had initially been

donated by Lyell to the Geological Society and then transferred to the Museum of Practical Geology in 1911.

According to Cleevely's 1983 *World Palaeontological Collections* BNHM, both Dundee University and the Geology Department of University College, London have a few fossil specimens said to have belonged to Lyell, while the York Museum has various fossils, minerals and rocks.

REFERENCE

WILSON, L.G. (Editor), 1970. *Sir Charles Lyell's Scientific Journals on the Species Question*. New Haven & London: Yale University Press. 1xi, 571pp.

B. G. GARDINER

The winner and only person to recognize Lyell was Dianne Edwards who will receive a special bicentenary mug. Ed.

Photo Quiz



Where was this made? Where was it found? (2nd time round)
(Note that the scale of 10 cm shown with this photograph the first time round was incorrect: it should have been 1 cm. The object is c5cm in diameter. Ed.)

*From the Archives***Some noteworthy early British floras and their diverse authors**

A flora is a guide to the native plants of a country or region. It gives both common and botanical (Latin binomial) names of each plant, its diagnostic characteristics and information on habit, flowering-time, location and uses, if any. Today, floras range in size from small paper-backs to large, glossy, 'coffee-table' volumes. All are highly illustrated with drawings and photographs and the text is, of course, in English. Such works originated with the first floras of the Swedish naturalist, Carl Linnaeus (1707–1778): *Flora Lapponica* (Lapland) 1737; *Flora Svecica* (Sweden), 1745; and *Flora Zeylanica* (Ceylon), 1747. His renowned *Systema Naturae*, 1735 [10th ed., 1758] and *Species Plantarum*, 1753, in which he propounded definitive classification and binomial systems for the plant kingdom, established botany as a science. For the next hundred years, floras would be based on these works.

The first British flora, in which these Linnean systems were used, was *Flora Anglica*, 1762 (in 8vo, price 10s.6d.) by William Hudson (1734–1793), an apothecary of Paton Street, London. He had been Assistant Librarian of the British Museum [1757–58] and later became Director and Demonstrator of the Chelsea Physic Garden [1765–71]. His *Flora* follows the pattern set by Linnaeus – the octavo size, no illustrations, and the text in Latin (Fig. 1), the usual language for medical and scientific works of that period. *Flora Anglica* became the essential manual for botanists in England. There was an augmented, second edition in 2 volumes, in 1778 and a reissue in 1798.

3. VIOLA acaulis, foliis reniformibus. *Sp. pl.* 1324. *palustris*.
 Fl. dan. 83. *Hall. hist.* 560.
 Viola palustris rotundifolia glabra. *Hist. Ox.* II. 475.
 s. 5. t. 35. f. 5. *Plot. Ox.* 144. *t. 9. f. 2.* *R. syn.*
 364.

Figure 1. *Flora Anglica* by William Hudson (1762) page 379
 – the entry for *Viola palustris*.

The *Flora* was, indeed, in such demand, that it became difficult to obtain and a replacement was required. This was finally provided by *Flora Britannica*, 1800, (2 volumes in 8vo; vol. 3, 1804) with its epitome, *Compendium Florae Britannicae*, 1800 (in 12mo), by James Edward Smith (1759–1828), President of the Linnean Society. Both floras are in Latin. An English translation of the *Compendium*, entitled: *A Synoptical Compend of British Botany, etc.*, by John Galpine, gent., owner of a nursery in Blandford Forum, Dorset, was published in 1806. It is a small pocket-book, in a novel tabular format, a sort of checklist of native plants. These works will be discussed in Part Two [*The Linnean*, October].

As the 18th century progressed the popularity of the study of botany markedly

increased, particularly amongst women, who learnt to identify plants scientifically and draw them, showing their specific features. Making botanising excursions into the surrounding woods and fields became a favourite pastime and a suitable flora was needed with which to name the flowers collected. Encouraged by his wife, just such a local flora, *Flora Bedfordiensis, comprising such Plants as grow in the County of Bedford, etc.*, 1798, was published by the Revd Charles Abbot (1761–1815) MA, DD, FLS 1793, FSA, Usher [Assistant Master] of Bedford School, also Vicar of Oakley Reynes and of Goldington (villages near Bedford).

In his bibliography, Abbot cites English and foreign authors, including Linnaeus and Hudson. Like their floras, Abbot's *Flora* is octavo in size and he also uses the modern Linnean classification and binomials. In two particulars, however, *Flora*

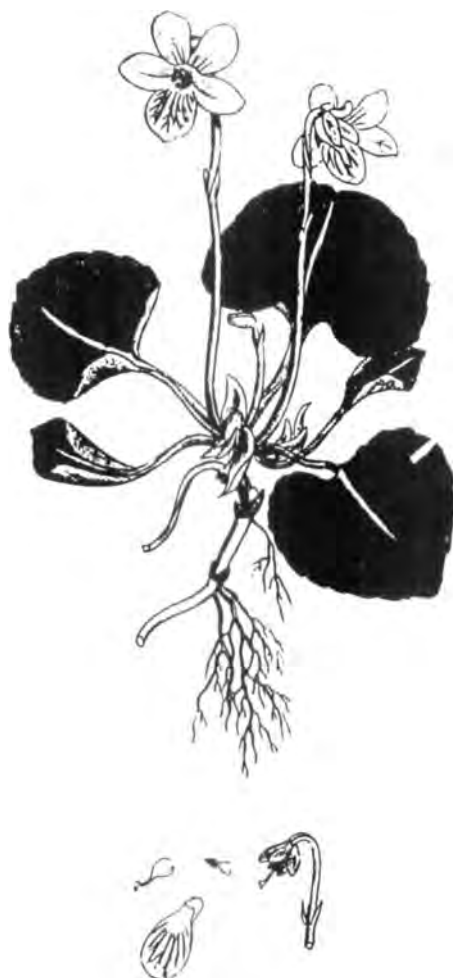


Figure 2. *Viola palustris* drawn by James Sowerby;
Plate 3 of *Flora Bedfordiensis* by Abbot, 1798.

Bedfordiensis is very different from them. Notwithstanding that the first two words of the title are in Latin, the text is in English. Abbot was very conscious that, apart from some rich and privileged ladies, the “fair sex” lacked a Classical education and so would be glad of a flora in English. His book, priced at 6s. 6d., would nevertheless have been an expensive luxury for most of them. The high price reflected the cost of the other unusual feature – six hand-coloured engravings. Plate 3 (Fig. 2) was by the distinguished botanical artist, James Sowerby (1757–1822). The other five plates may also have been his work. [See Part Two.] Abbot gives the English and Latin names of each species, a brief description, flowering-time and location (Fig. 3). *Flora Bedfordiensis* was the first flora of that county. It continued to be the standard work until the beginning of this century. It is a valuable record of the plants of the countryside, before it was irrevocably changed through the Enclosure Acts of the early 19th century and by the developments of the present day.

624. *Marsh Violet*. — *V. palustris*.

Stemless, leaves kidney-shaped. — *Flora Danica*. 73. Sowerby. 444. (*See our fig. No. 3.*)

Bogs, March, P. Rare.

Figure 3. *Flora Bedfordiensis* by Abbot, 1798, page 190
– the entry for *Viola palustris*.

In November 1787, Charles Abbot married Sarah Harris of Chocken Hall [Cherkenhill], a hamlet some 4 miles north-west of Great Malvern, Worcestershire. They may have met while he was on a botanising tour of that county, an area he often visited. She, subsequently, travelled about with him, when he went looking for rare plants and butterflies. At home, they made a garden of wild plants – a ‘*hortus Bedfordiensis*’ – tended by Mrs Abbot, who also prepared her husband’s herbarium. He praised her for its “superior beauty and excellence”, but, although it is still in a good state of preservation [now in Luton Museum], the want of named locations diminishes its usefulness today.

The origin of Abbot’s *Flora* may lie with this ‘*hortus*’. Most keen gardeners keep a record of the plants they grow. Abbot’s undated manuscript, *Catalogus Plantarum in comitate Bedfordia, sponte crescentium*, [sent to the Linnean Society in May 1795 and conserved in the library], listing 956 local plants, had perhaps begun with those in their wild garden, then later formed the basis of the *Flora*. That comprises 1225 species, including the Cryptogamia (ferns, mosses, algae etc.). [See Galpine, Part Two.]

In preparing his text, Abbot had been much helped by Anne Fitzpatrick, Countess of Upper Ossory, who accorded him the unusual and generous favour of allowing him to borrow books from her library. She lived at Ampthill Park, an estate adjacent to Woburn Park, the property of the Duke of Bedford, an arboriculturalist, whom Abbot knew and who may also have been useful to him. In common with many other authors

of the day, Abbot dedicated his *Flora* to Queen Charlotte, describing her as “the first female botanist”. She was well-known for her interest in the subject and, indeed, the science owes her a considerable debt for her patronage. [See also Part Two.]

Most importantly, Abbot had the support of his “amiable and interesting partner”, who encouraged him in his other main hobby, collecting butterflies. He had many unique specimens, one alleged to be worth as much as 10 guineas, though, at the sale after his death, his whole collection fetched only 4 guineas. Abbot frequently sent butterflies to Adrian Hardy Haworth (1767–1833) FLS 1798, an entomologist and botanist. In his *Prodromus Lepidopterum Britannicorum*, 1802, and *Lepidoptera Britannica*, 1803, Haworth often mentions his “amico meo” giving credit to Abbot for making the first capture, in England, of *Papilio paniscus* in 1798 [*Trans. Linn. Soc.* 5:276;1800], and, in Bedfordshire, of *P. Charlotta*, named for the Queen.

As well as botany and entomology, Abbot was interested in the antiquities of the county, a hobby he shared with the Revd Thomas Orlebar Marsh (1749–1831) MA, FLS 1793, Vicar of Stevington (a village close to Bedford), antiquary and amateur botanist. In 1813, Abbot became a Fellow of the Society of Antiquaries. When he had sought to become a Fellow of the Linnean Society in 1793, Marsh was one of his sponsors. His other sponsor was the nurseryman, James Dickson (1738–1822) FLS 1788, founder-member and Vice-President of the Society.

As a Fellow, Abbot was in constant correspondence with the President, Sir James Edward Smith, and with the other Fellows. He presented Smith with a copy of his *Flora* and sent him botanical specimens and information collected by himself and his wife. Abbot wrote to Smith in the friendly manner of a like-minded enthusiast. Smith makes numerous references to Abbot’s *Flora* in his own *Flora Britannica*, 1800, and, in his personal, interleaved, copy [in the Society’s library], he adds more of Abbot’s locations, including Chocken Hall (Mrs Abbot’s home) and Leigh Sinton, in the same parish. Smith ascribes to Abbot the discovery of the purple-leaved Helleborine, *Epipactis purpurata* (Fig. 4) at Leigh Sinton in 1807 [Smith, *The English Flora* Vol. 4, pp 41–42, 1828], but it may well have been Sarah Abbot, who actually found the plant. Abbot was very proud of his wife’s botanical knowledge and begged Smith to give her credit where appropriate. This, however, Smith declined to do, much to her husband’s chagrin. In contrast, in his own *Flora*, Abbot repeatedly cites the findings of his “fair associate”.

At Bedford School, Charles Abbot has the reputation of being a disagreeable, contentious, person, always finding fault. He was clearly not a dedicated teacher, nor was he happy in his job, a life-appointment. He allowed the number of his pupils to drop from about 16 to a mere 5 boys, so it was no surprise – except to Abbot himself – that, when the post of Master fell vacant in 1810, he was passed over. High taxation and increasing inflation due to the Napoleonic Wars had reduced his annual salary from £100 to £90, yet, when the new Master suggested that he could increase his income by taking in some boarders, he refused on the grounds that this would entail more school work.

With regard to his house, he, at least, had good reason to complain. The Usher’s House, in which he had lived since 1788, was new, built only in 1776, but defective drainage caused standing water in the cellar, spoiling and tainting the food, and,



Figure 4. Purple-leaved Helleborine, *Epipactis purpurata*;
Plate 2775 in *English Botany* by Sowerby, 1834. Suppl. ii.

whenever a fire was lit in the parlour, a cold, wet, miasma would fill the room. Could this be the reason for Mrs Abbot's premature death, and Abbot's, two years later, at the early age of 56? – from typhoid fever perhaps? Abbot had earlier suffered a complete nervous breakdown, the Master himself taking over his school duties. Charles and Sarah Abbot lie together in a plot, which he had already purchased for them, at the Priory Church, Great Malvern. *Flora Bedfordiensis* forms a fitting memorial to their joint endeavours.

ENID SLATTER

To be continued.

Correspondence

7.5.98

'High Croft'
Gunswell Lane,
SOUTH MOLTON, Devon

Dear Brian,

?? The First Female Palaeontologist

By now you will have had a considerable response to your Picture Quiz in the January issue of the *Linnean* identifying the 1837 silhouette of Miss Etheldred Benett.

Over the years whilst curating the collections at the B.M.(N.H.), I had occasion to learn a little about Miss Benett's fossil collecting activities, especially through her links with the Sowerbys and Gideon Mantell. But before providing these details I might add a little background to the question you raise as to whether Miss Benett was the first female palaeontologist, that may well have produced a few protests.

Of course, the emphasis is on palaeontology as a science rather than the mere acquisition of fossils. With regard to your article on Lyell's gradual change in attitude to women geologists and their admission to meetings and lecture rooms, perhaps, I can direct Fellows to the papers by Mary Creese. In her survey of British women involved in the geological sciences during the nineteenth century, she provided a list of twenty-eight, sixteen of whom were primarily involved in palaeontology. The basis for inclusion was that they had at least one publication in a scientific journal, or else had published a technical monograph. They broadly fall into two categories:

- i) those who were self- or privately taught
- and ii) those able to have university training (= 12).

Miss Benett falls into the first category & the more familiar names of later palaeontologists such as Gertrude Elles, Maria Ogilvie-Gordon, Catherine Raisin & Ethel Wood into the second. Jane Donald (Mrs. Longstaff), who specialized on Palaeozoic mollusca, is considered as overlapping these two traditions, for although having no formal training in geology, she became the acknowledged authority and was honoured by the award of the Balance of the Murchison Fund by the Geological Society in 1898 (Woodward, 1907:244; Cleevely, 1989:189).

Mary Creese (1994:33; 1996:73) writing on one of the outstanding British women field geologists, Maria Ogilvie-Gordon (1864–1939) mentioned the encouragement that she been given by several British geologists. However, when studying in Munich between 1891–95, she had to become a private student and on occasion was not allowed to attend lectures but listen from an adjacent room. Incidentally, Woodward discussed the various attempts to gain admittance of ladies to the Geological Society in the final chapter of his history of that society (1907:242–44).

Mary Creese (in footnote 6: p. 26) also mentioned the 'wife-assistants' of several notable nineteenth century geologists: Mary Buckland (1797–1857), Mary Ann Woodhouse Mantell (fl. 1820–30), Charlotte Hugonin Murchison (1789–1869) and Mary Horner Lyell (1808–1873) all of whom were directly involved with the work of their husbands. Lady Murchison is also acknowledged to have had considerable

influence on her husband's social life apart from his geological work. C.B. Rose and Samuel Woodward, the Norfolk geologists, tell of a British Association meeting at which they had collected some Palaeozoic fossils, but upon asking Sir Roderick for information were referred to Lady Murchison for precise identification since he claimed that she knew far more about such things.

The evidence of attributions given by the Sowerbys in seven volumes of their *Mineral Conchology* (1812–1846), which might be conceded as the earliest available work using material from a wide range of collectors, reveals that they obtained fossils from twenty-nine female collectors. Of these probably only ten provided a reasonable number of specimens for description, or illustration and the most significant of these were: Mrs Cobbold from Ipswich (48); Etheldred Benett (41); Miss Bemister from Christchurch (26) & Miss Hill of Pilton, Devon (23). Furthermore, three of them featured in the **earliest** parts of the **first** volume of the work published between August 1812 and April 1813. Elizabeth Cobbold & Etheldred Benett also corresponded with Mantell about their finds. The occurrence of Miss Benett in both surveys, in one as an author and the other as a major collector, would seem to support the assertion that she was the 'first'.

As promised, I also enclose an account of Miss Benett that uses material that I and others have come across in recent years. Hope it is of some use to you for *The Linnean*.

Yours as ever 'historically'

RON CLEEVELY

18.1.98

175, Whitton Road,
Twickenham, Middlesex

Dear Brian,

Your latest Picture Quiz in *The Linnean* was easy going for me. I recognised at once the silhouette of Miss Etheldred Benett (1776–1845), author of "A Catalogue of the Organic Remains of the County of Wilts". (1831). As the first lady palaeontologist, I would argue, as a Dorset man, that Mary Anning has priority. This silhouette, plus some biographical details appears in H.B. Woodward's "History of Geology" (pp 126–127), while the whereabouts of her fossil collections are contained in '*World Palaeontological Collections*' (BMNH) by R. Cleevely. The lady was excellently brought to life in 1990 at Lewes Town Hall, as part of the Gideon Mantell Bicentenary Symposium, when Ron Cleevely and friends dressed up in period costume to personate Mantell and others and to read some of their correspondence. Di Hawkes, Curator at Haslemere, became Etheldred Benett for the occasion. I enclose a copy of that evening's programme as a matter of interest.

Yours sincerely,

RICHARD WILDING

Voorstraat 41,
NL-1931 AH Egmond aan Zee
The Netherlands

Dear Editor

Re: Paper by Thireau *et al* in *The Linnean* 13(4), January 1998, p38–45.

When I read the paper by Thireau *et al* in *The Linnean* 13(4) 38–45 I noted several incorrect or incomplete details and wrong spellings. Throughout the spelling Stathouder (not Stadhouder) is used, probably because a French authority or worker used it. A stadhouder was a typically Dutch institution, something like a cross between a monarch and a president but with limited executive power. I wonder if a historian has studied the stadhouder status and powers. On p38 the stadhouder who bought much of the Seba collection is referred to as Dutch prince, which is in so far incorrect that a whole sequence of stadhouders were related (usually father to son) and were entitled to bear the hereditary title “Prince of Orange”. This is irrelevant in the present context. On p. 39 the “confiscation” (read stealing) of natural history objects the fossil *Mosasaurus* skull is mentioned, but the correct spelling is Maastricht. It has been suggested that negotiations with French authorities at a ministerial level may lead to a return of the specimen to its rightful owner. The story on the same page about a cavalry charge is in so far incorrect that there was (and is) no Texel river. The cavalry charge was on a single warship anchored and frozen in within the city limits of Amsterdam.

On p.38 it does not say what happened to the Seba holdings not acquired by the then Stadhouder. I also wonder what groups other than the herpetological ones were also acquired by the French Museum (I am thinking of insects and shells of Mollusca). Such specimens will be types as argued on pp. 38 and 39 of the paper discussed above and have the status of holotypes.

Sincerely

A.D.J. MEEUSE

I hope this is legible (I am in my 94th year of life and have no lab facilities).

10.3.98

5 Clifton Vale,
Bristol

Dear Brian,

Thank you so much for the offprints from the Society’s stores. Amongst them, I have been reading the paper on Joseph Banks, written by Daydon Jackson. He was the Linnean’s first representative on the National Trust Council and held the post for 32 years! I enclose the complete list in case it interests you. I am the ninth in 103 years. However the first two, Jackson and Gardiner, hogged 52 of those years. Three Fellows did eight years, three did less than that and three did more than that. In the early days Council met at very irregular intervals and the business was mostly formal. The members were not expected to spend a lot of time digesting long memoranda and arguing out policies. Indeed it is only in recent decades that it has come to play a much more active role. As far as I can discover, none of the previous holders of the

position ever took an active part in the Trust's affairs.

As ever,

BOB SAVAGE

LINNEAN SOCIETY REPRESENTATIVES ON NATIONAL TRUST COUNCIL

		Years
1895 – 1927	Dr. B. Daydon JACKSON	32
1927 – 1947	Professor J. Stanley GARDINER FRS	20
1947 – 1959	Professor Sir Gavin de BEER FRS	12
1959 – 1967	Professor A.R. CLAPHAM	8
1967 – 1975	Dr. John SMART	8
1975 – 1980	Dr. P.H. GREENWOOD FRS	5
1980 – 1984	Dr. McCLINTOCK	4
1984 – 1990	C.M. HUTT	6
1990 – 1998	Professor R.J.G. SAVAGE	8

23.11.97

Ridgebourne,
Kington,
Herefordshire

Dear Dr. Marsden,

As a Lyell collateral I was very interested in the October issue of *The Linnean*. I knew little of the Hooker Lyell connection – I can offer one small correction however. On page 17 it refers to the Lyell seat at Kinnordy being “not far from Glasgow” – Kinnordy is in fact near Kirriemuir in Angus on the opposite side of Scotland to Glasgow – quite a long way away today or a very long way in 1832!¹⁰

Yours sincerely,

LAWRENCE BANKS

Errata: Letter from Ralph Grandison (*The Linnean* **13** (4): 13). Otto Sander should read Otto Sonder; Ludwig Freres should read Ludwig Preiss.

¹⁰ Thank you for your correction - I was trying to make the point that although Kinnordy is some 60 miles from Glasgow - compared with the distance the Lyells travelled every summer from their home in the New Forest, when they reached the Hookers (whom they called on) they were relatively near their country seat. Ed.

Edward Jenner FLS

In 1798 Edward Jenner published his famous *Inquiry into the Causes and Effects of the Variolae Vaccinae....known by the Name of the Cow Pox*. In it he reported the results of his epidemiological observations and the clinical trial which showed that cowpox, a mild localized infection offered protection against smallpox (Baxby, 1981; 1996). A brief but informative and interesting account of Jenner's work on vaccination and of some of the opposition later mounted by individuals such as A.R. Wallace was published in *The Linnean* in 1995 (Gardiner, 1995). What might also be of interest to readers of *The Linnean* is that 1998 also marks the bicentenary of Jenner's election as Fellow of the Linnean Society.

The most obvious evidence of Jenner's association with the Society is his qualifications listed on the title pages of his second two monographs on vaccination, *Further Observations on the Variolae Vaccinae* of 1799 (Fig. 1) and *A Continuation of Facts and Observations on the Variolae Vaccinae* of 1800. These title pages were reproduced by Crookshank (1889) and by Lefanu in his authoritative biobibliography (1951); the latter also listed the full titles and qualifications in his second edition (1985). Jenner is briefly mentioned as a distinguished Fellow in the Society's early history (Gage, 1938) but only one of the many biographies consulted mentions it, and then only briefly (Fisk, 1959). It is not mentioned in Jenner's authorized biography (Baron, 1838) nor his obituaries or the 'old' *Dictionary of National Biography*; this last will be remedied in the new, revised edition.

At the time of his election and the publication of the *Inquiry* Jenner was 49 years old and vaccination was to dominate his life until his death in 1823. However, he did not spend quite so many of his early years investigating cowpox as some of his biographers suggest, and it was during these years that, to quote his election certificate, he showed himself 'as skilled in various parts of natural history and likely to become a useful member'.

Jenner had an early interest in natural history and collected eggs, nests, and fossils as a boy. These interests were developed during his medical training in London with John Hunter. Hunter was not just a surgeon, but was also a pioneer of comparative anatomy and physiology. He built up an extensive museum of almost 14,000 specimens arranged to illustrate his theories on the adaptation of structure and function throughout the animal kingdom. He published some observations but his museum has aptly been described as his unwritten book (Allen, 1974). Although damaged by bombing many specimens survive as the Hunterian Museum of the Royal College of Surgeons of England.

It would have been part of Jenner's duties to prepare specimens for the Museum, and apparently he was employed by Hunter to arrange and display some of the specimens brought back by Sir Joseph Banks from Captain Cook's first voyage in 1771. This was a particularly responsible task and indicates Hunter's trust in Jenner's capabilities. Apparently there were also plans for Jenner to accompany Banks on Cook's second voyage, although in the event neither went.

No documentary proof has been found to confirm these claims (Lefanu, 1985). However, they were made during the lifetime of Jenner and Banks and not contradicted

FURTHER
OBSERVATIONS
ON THE
VARIOLÆ VACCINÆ
OR
COW POX.

BY EDWARD JENNER, M.D., F.R.S., F.L.S., &c.

London :

PRINTED, FOR THE AUTHOR,

BY SAMPSON LOW, No. 7, BERWICK STREET, SOHO :

AND SOLD BY LAW, AVE MARIA LANE, AND MURRAY AND HIGLEY, FLEET STREET.

1799.

Figure 1.

or challenged by those who vigorously attacked Jenner's work on vaccination, and so we must assume their accuracy.

When Jenner returned to Berkeley he and Hunter maintained a lively correspondence until the latter's death in 1793. Many of Hunter's letters survive and in them he

bombarded Jenner with requests for information and specimens (Cornelius and Rains, 1976). Jenner provided specimens of marine mammals and carried out various experiments including some on the body temperature and digestion of hibernating hedgehogs. Many of these incidental findings were incorporated into Hunter's papers (Lefanu, 1985). Unfortunately however only one specimen known to be by Jenner has survived, but it is an interesting link to his most important non-medical observations (Fig.2).



Figure 2. Dissection of the lower half of a female cuckoo showing the left oviduct. Part of the lower duct wall has been removed to show the egg within it, and part of the shell has been removed to show the shell membrane. Prepared for John Hunter by Edward Jenner. Hunterian Museum, Specimen 3376A. Reproduced by permission of the Royal College of Surgeons of England.

Initially prompted by Hunter, Jenner began studying the nesting habits of the cuckoo, and it was Jenner who first reported that it was the newly-hatched cuckoo which ejected the eggs and nestlings of its foster-parents from the nest (Scott, 1974; Wyllie, 1981). Jenner validated his observations by placing newly-hatched cuckoos into a nest, and described the battle that ensued when he placed two newly-hatched cuckoos in the same nest. Each tried to eject the other until the eventual victor fell back into the nest exhausted. This was disbelieved by many at the time and much later, until confirmed by photography. The entry on Jenner in the old *Dictionary of National Biography* refers to the story as 'absurd'; again, this will be remedied. Jenner originally thought that the foster-parent was the culprit, but we can be sure that he later actually saw what really happened, rather than guessed, because he also described the transient anatomical modification to the cuckoo's back. This facilitates the ejection but disappears after about 12 days. Jenner also drew attention to similarities in weight and colour between the cuckoo's egg and those of the selected foster-parent. He also provided a plausible explanation for the female cuckoo's behaviour based on strong maternal instinct rather than cruel abandonment. He argued that the cuckoo spent too short a time in England to build a nest and rear young and so was impelled to ensure its young would be raised safely by other birds. For these observations Jenner was elected FRS in 1789.

At about the same time Jenner also conducted important studies on bird migration which were eventually published posthumously (Lefanu, 1985). At a time when some thought that birds hibernated he provided evidence that they migrated. More important, he provided evidence by dissection that the urge to migrate was due to changes in the reproductive organs and not due to food shortages or climate changes.

One curious fact is that Jenner's application for Fellowship was made as late as 1798, when he was in the midst of his vaccination studies. He was duly elected in July at about the time the *Inquiry* was being printed. Unfortunately his actual involvement with the Society is not known. The Society's records have a letter dated 25 July 1800 in which he offers financial support to the Society, whilst complaining that he had not received the papers and publications to which he believed his subscription entitled him. The title page of his fourth monograph *The Origin of the Vaccine Inoculation* of 1801 does not list his Fellowship. Perhaps his interest was beginning to wane, or perhaps he was becoming overwhelmed by his involvement with vaccination. In any event it is clear that his earlier interest in and contributions to natural history merited recognition by the Society. Further, that even if he had not become one of the major figures of medical history he would still have earned some lasting recognition for his pioneering ornithological studies.

DERRICK BAXBY

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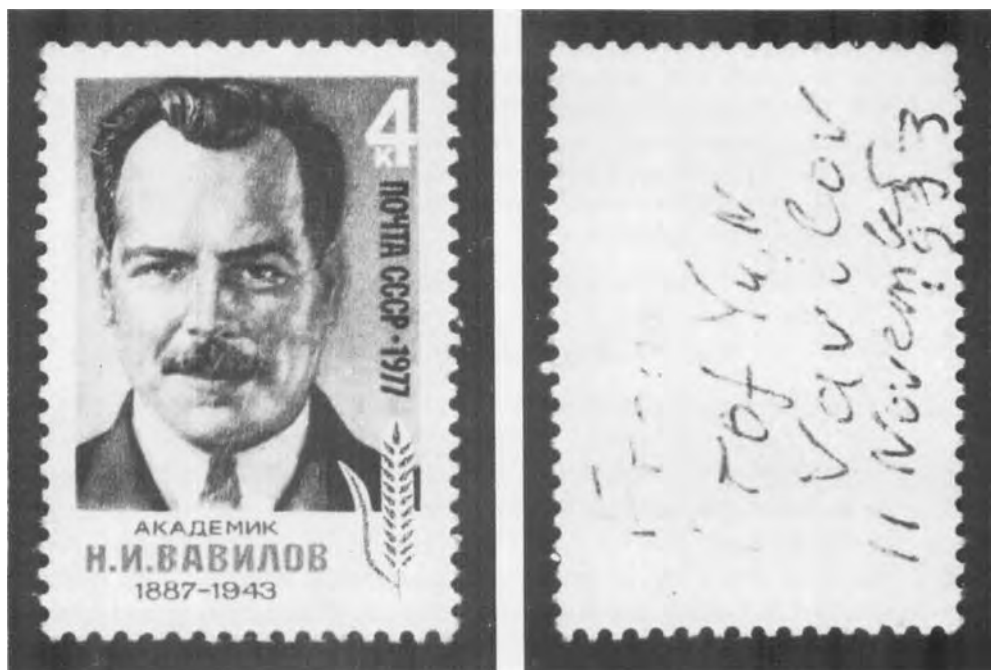
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St. Petersburg 1997

Professor Jack Hawkes, Past-President of the Linnean Society, accompanied by Dr. John Marsden, Executive Secretary, visited the N.I. Vavilov Research Institute of Plant Industry (VIR) in St. Petersburg from 12–21st August 1997 at the invitation of the Director, Professor Viktor Dragavtsev FLS. The visit was sponsored by the Society, and sought *inter alia* to examine the possibility of exchange visits between the Vavilov Institute (which is based on 14 separate sites stretching from St. Petersburg to Astrakhan) and UK institutions with an interest in plant breeding and genetic conservation.

Nicolay Ivanovich Vavilov (1887–1943), the father of the concept of genetic resources, journeyed indefatigably around the globe seeking the sources of settled agriculture and the wild relatives and primitive forms of our crop plants, whose genes he recognised as being so important in increasing disease resistance, improving yields of grain, tubers or fruit, enhancing drought and cold tolerance and survival. The visitors were presented with copies of the book he wrote in prison at the end of his life called *The Five Continents*, describing his experiences, first published in Russian in 1962, but thanks to IPGRI (International Plant Genetic Resources Institute) and VIR, now (1997) available in English. On 26th November 1987, the exact centenary of Vavilov's birth, the Linnean Society held a symposium jointly with the Institute of Archaeology at University College, London, whose proceedings, edited by Hawkes and Harris, were published in the Society's Biological Journal in 1990 [39 (1): 3–93]. Needless to say, there was no Russian participation at that time. In 1995 Vavilov's younger son, Yuri, visited the Society to obtain a copy of the Journal issue and left the Society with a signed copy of a Russian stamp commemorating Vavilov. Better late than never, but better never late. Vavilov is now something of a Russian scientific icon, and a small museum in the Institute commemorates his life and work. There is no commemoration of the infamous Trofim Lysenko, who succeeded him as Director of the Agricultural Research Institute which now bears Vavilov's name and who orchestrated Vavilov's death, probably that of his elder son Oleg in a mountaineering "accident" and countless others. It is to the shame of a number of notable UK scientists that their tardiness in the thirties in condemning Lysenko's phoney theory of inheritance (for which no reproducible evidence was ever adduced) may have led to the almost

wholesale destruction of Russian biology and biologists, blows from which the country has still not recovered.



The visitors were particularly well looked after, staying in the Institute Guest House near to the the southern end of the Nevski Prospect. Both visitors had been in St. Petersburg when it was known as Leningrad. Things are different now, as a market economy emerges from the wreckage, literally, of 70 years of misrule, which has seen virtually no attention paid to the proper maintenance of one of the world's most beautiful cities, an act of vandalism of which Ghengis Khan might well have been proud. Whilst the necessities of life are horrendously expensive for Russians, at least rationing and bread queues have disappeared and food of real quality is appearing in countless small privately owned shops and street markets dotted around the suburbs. The days of a Central Committee instructing all collective farms to plant spring wheat on a particular day of the year, no matter what the state of the weather, the soil or the availability of seed, have passed and we were told that more and better food was on the way.

Discussions with a number of Russian agricultural scientists highlighted problems in plant breeding, such as seed-banks, gene banks, climatic factors, disease and pest resistance, in Russia as well as adaptation to a wide range of soil types. Proposals for exchange visits are being worked out. It was also established that Russia and other countries of the former USSR certainly do have valuable seed banks, which should be used in a way which encourages their conservation and development for the whole world. One positive feature of the end of the Cold War has been the provision to the VIR of cold cabinets from the US armed forces as they progressively dismantle their

far-flung military bases. The cabinets are being used for long term seed storage under internationally agreed conditions.

In the course of these discussions it became apparent that, since the collapse of the Soviet Union, most of the resulting new countries, including Russia, had not ratified a number of international treaties, protocols and agreements, which seek to protect ownership of plant varieties. There is also currently no patent legislation in Russia, so we were told, and certainly Russia is not part of any wider patent group. Nor are they formally part of organisations like FAO or IPGRI. These omissions may have profound future implications for Russian and other agriculture. It is clearly in no-one's long-term interest that these new countries, with a total population in excess of 250M, should be open to exploitation by others of the genetic diversity of crop plants that they have to offer internationally. It is to be hoped that strong collaborative links in plant genetic resources and plant breeding institutes between the UK and Russia (as well as other Commonwealth of Independent States countries) will be formed to mutual benefit.

The President has communicated a version of the above report to the Department of International Development (formerly ODA) and the Minister has suggested that the Society follow this up through the "know how" fund which has been set up to support former Soviet block countries. This is being explored.

The Conservation Foundation, presided over by our newest Fellow *Honoris causa*, Dr. David Bellamy, has strong links with eastern Europe. Another Fellow, John Massey Stewart, is heavily involved in this work and has drawn the Society's attention to *Who Is Who in Biodiversity Sciences* in Russia and adjacent countries. This is edited by an old friend of the Society, Professor N.N.Vorontsov, whose personally autographed books on environmental matters are in the Society's Library.

Those wishing to consult this directory should contact John Massey Stewart at the Conservation Foundation at conservf@gn.apc.org or on 0171 591 3111.

Another visitor to the Society in October was Dr. Vladimir M. Zakharov, Head of the N.K. Koltsov Institute of Developmental Biology of the Russian Academy of Sciences. Much of his work has been concentrated on the after-effects of the Chernobyl nuclear explosion ten years ago. He has examined a variety of non-human species in and around the ill-fated reactor. Copies of his papers and books (in English) are in the Library. They do not make pleasant reading.

Origin of Life

I suppose that I first became interested in life's origins after reading an article by Manfred Eigen in *Scientific American*. It appeared in 1981 and was entitled *The Origin of Genetic Information*. In many ways this article typifies that era of thinking on these matters. Eigen had a Nobel Prize (for Chemistry) and thinking about life's origins has been a favourite pastime for laureates, e.g. Francis Crick. Secondly, it capitalised on our relatively recently acquired knowledge of the nature of our genes. Thirdly (of course) it offered a very plausible explanation of how small self-replicating nucleic

acids and associated proteins could develop into more complex structures.

Plausibility is what characterises articles on the origin of life. There are theories about the prebiotic soup, which many believe to have covered the Earth's surface in Hadean times (the prebiotic Earth; from the Greek, *hades* – hell) in which the synthesis of amino acids and nucleic acids, fats and cofactors took place. Here we are on stronger experimental ground. Experiments using (assumed) Hadean atmospheres and conditions – lots of electric discharges to simulate lightning, and heat – have demonstrated the possibility of deriving many of the components of living systems this way (but critically, not the components of the DNA of our genes), although

“....it comes as something of a shock to realize that there is absolutely no positive evidence for....(prebiotic soup's).... existence.”

Denton (1985)

The earliest speculations go back to considerations of how living cells might have evolved from non-living matter. This was the domain of Oparin, a Russian, and Haldane when there was only the crudest compositional information on living cells. All these speculations suffer from a significant hitch. Sound and sensible though their proponents may have been, none has succeeded in creating a living system in a test tube from sterile non-living ingredients these past seventy years.

So what is seen as life? On Earth it consists of a genetic apparatus, a system for passing information both within a generation and from generation to generation. Both are associated with DNA, but within the cell, the information transfer is mediated by RNA. Information transfer in living cells is now of very high fidelity, but yet allowing mutations to occur. Darwin and Wallace's great contribution to biology was to note that the selection of appropriate mutations is why you are able to read this today. And from the beginning, life has needed a continuous energy source to avoid violating the Laws of Thermodynamics, which stipulate that life's complexity can only be bought at the expense of energy. Finally all living organisms exist in one or more containers, which we call cells.

“....proteins, DNA and RNA are all essential for life as we know it....coming up with a plausible story for how DNA, RNA and proteins suddenly popped into existence simultaneously on a lifeless planet was....tricky”

Cohen (1996).

A satisfactory theory of life, then, will need to explain how, starting with a series of relatively simple chemicals, we can progress to a cellularly based self-sustaining organism capable of reproducing itself marginally imperfectly for billions of generations over the past 4 billion years.

It goes without saying that such a theory should be capable of experimental proof in every detail.

Darwin wrote in 1862: *“It is mere rubbish thinking at present of the origin of life; one might as well think of the origin of matter.”* Subsequently, in 1871, Darwin is quoted as saying that *“all the conditions for the first production of a living organism....(could be met).... in some warm little pond, with all sorts of ammonia and phosphoric salts, light, heat, electricity, etc. present”*, which he supposed led to a proteinaceous substance and on to more complex materials. This was quoted by the Russian, Alexander Oparin, in the first modern scientific paper on life's origin in

1924. Darwin further noted that such conditions could no longer be reproduced on the planet, since anything formed under such conditions **would be instantly devoured or absorbed, which would not have been the case before living creatures were formed.**

“The likelihood of the formation of life from inanimate matter is ten to a number with 40,000 noughts after it. It is big enough to bury Darwin and the whole theory of Evolution“

Hoyle (1981).

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According to my calculations from Hoyle's data, the good professor lost a factor of ten in his result. The figure should be $10^{400\,000}$. The quotation is from the www and is incorrect. What Hoyle actually said (at a meeting in California) was that the probability of intelligent life on earth was that. It is derived from a calculation of the probability of 2000 specific genes arising from the four bases which constitute the main building blocks of our DNA. There is little doubt that spice has been added to the debate on life's origins by the prospect, recently aired, that life might also have arisen on Mars and, within our own solar system, on the Jovian satellites Europa and Io.

“Ordinary folk.....may be forgiven for believing that the former existence of life on the Red Planet has been proved beyond reasonable doubt.”

Editorial comment in *Chemistry in Britain*.

Why is the prospect of life on Mars so headline-catching? Did life actually start on Earth? There are those – notably Hoyle again – who say that life originated in outer space. Hoyle has damaged his credibility by supposing that novel lifeforms are continually arriving at the Earth's surface from space and that these are responsible for pandemics such as AIDS and even food poisoning in Scotland. Yet Hoyle's extraterrestrial origin of life is not without rationality. In deep space, there are a variety of small molecules at high dilution (probably more dilute now than 4 billion years ago). Some of these, like formaldehyde, are reactive. Whilst the opportunity for reaction is limited because of the dilution, so is the chance of degradation.

Hoyle's colleague at Cambridge, Stephen Hawking (in a personal communication), has little time for these views. He believes that life may have originated in many places in the universe, because it seems that life arose on Earth at just about the earliest time that it was possible for any organism to have survived.

“At 3.5 billion years old, fossilised bacteria are the earliest evidence of life on Earth, and yet these relics....are identical to the sophisticated modern cyanobacteria....”

So when did it happen and what were those conditions? Geological evidence suggests that the Earth's surface began to solidify some 5BYbp (billion years before present) and that a crust existed some 800 million years later. Isotopic evidence (increasing $^{12}\text{C}/^{13}\text{C}$ ratios) indicates life was present only 300 million years after that, some 3.9BYbp. Fossil evidence presumes that the organisms were photosynthetic and, by analogy with their modern equivalents, nitrogen fixers. The Earth's surface was much warmer than now, so much more water would have vapourised, giving rise to clouds and thunderstorms. Lightning is a potent energy source, providing even today around

10^{-8} M HCN annually in the Earth's oceans, which is consumed mainly by marine organisms, so does not become a danger to other life forms. But, as Darwin noted, in the absence of such omnivorous bugs and in a billion years, the concentration of HCN might rise to high (M) levels. Before that could happen, given that HCN is a reactive molecule, it might polymerise to a multitude of other materials, which might be useful in a prebiotic world to generate life. In passing, it might be noted that adenine, a universal constituent of nucleic acids and with many other metabolic roles, is $(\text{HCN})_5$. Oparin also placed cyanide high on the list of possible chemical starters of life, but in his case cyan(ogen), C_2N_2 . Laboratory experiments also support the idea that lightning discharges in the presumed Hadean atmosphere lead to the synthesis of complex molecules, hence the idea of the prebiotic soup.

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"The emergence of life is a geological issue"

Russell & Hall (1997)

One point overlooked in our sales pitch for an chemical origin for life on earth is that those billions of years of cooking that prebiotic cyanide soup must have led to some fancy chemistry and some diverse products, **only a few** of which were likely to have been useful in the creation of life. How were these latter, and only these latter, to come together **without a lot of interfering molecules getting in the way?** How are messy chemical mixtures cleaned up? With techniques like chromatography, whereby compounds are specifically absorbed and desorbed on a suitable support. The traditional supports for chromatography have been alumina or siliceous minerals, like talc, of which there was plenty about on the prebiotic planetary surface. Even more significantly, such minerals catalyse the random synthesis of nucleic acids (specifically RNA) from the monomer units – adenosine, cytosine, guanosine and uridine, as has been demonstrated in the laboratory by Orgel and his colleagues. Polymers of up to 50 monomers can be made by this method. Not much of a gene, perhaps, but a start.

Random synthesis of polymers seems unlikely to lead to much useful, until another pebble is tossed into that pond of plausibility. That is the discovery of ribozymes. Ribozymes are sequences of RNA which are able to catalytically degrade and synthesise themselves or other RNA molecules. Such ribozymes are capable of selecting specific sequences for synthesis – another Darwinian experiment performed in the laboratory – and thereby enhance their chances of survival.

The two earliest scientific explorers of life's origins, Oparin (1924) and Haldane (1929) were concerned with the origin of cellular life. Haldane, who was unaware of Oparin's work until much later, and Oparin both felt that a cell was a prerequisite for life to develop, in that a cell provided protection from a potentially hostile external environment and a friendly milieu in which the appropriate reactions could occur. Oparin used the word gel to describe a suitable membranous material, Haldane the word film. Patronisingly, it might be said, good thinking, and, in the thirties, it added another topic to scientific debate in a world still reeling from what some saw as the excesses of the Darwinian and Mendelian revolutions in biology. The formation of spherical membranes we now know is not difficult to achieve, given a number of fairly simple constituents. Such suspensions of membranes – liposomes – are used for

cosmetics (probably ineffectively) and as drug delivery systems, where they have threatened much promise for a long time. Suspensions of polar lipids, hydrocarbon chains attached to charged groups such as phosphate or ammonium, when shaken can form liposomes easily.

“An energy-producing metabolic cycle, not RNA, triggered life on earth.”

C.R. Woese (1996)

And metabolism? Ignoring the Laws of Thermodynamics just won't do! Considerations of energy sources are as important as any other feature in the creation of early life. Currently this facet of the subject is enjoying a deal of attention and is the basis of a significant review by Russell and Hall in a recent issue of the *Journal of the Geological Society*.

Deep-sea exploration has revealed the existence in various parts of the globe of hydrothermal vents, wherein hot water escapes into the deep oceans from volcanic sources. Such water may be recycled from seawater which has penetrated the Earth's crust, but whose composition has been changed on its journey. It may have acquired mineral elements and gained or lost gaseous constituents, such as H_2S or CO_2 . It is at high pressure (perhaps 200 atmospheres). At present such subsea hot springs contain appreciable quantities of CO_2 which make the water acidic. It is assumed, however, with geological evidence, that some springs may also have run alkaline and, in Hadean times, a range of ca. 6 pH units might have been found amongst such thermal vents. The exposure of the ocean, much smaller in Hadean times, to CO_2 ensured that it was mildly acidic at all times.

If we look at the elements involved in metabolism today using Williams' table (Fig.1), we can see the deliberate mistake. Iron (Fe) is listed only as a trace element. In many organisms it is much more than that. Iron is an element involved in the carriage of oxygen in most organisms, in most oxidations and reductions in cellular metabolism, and in nitrogen fixation, an important process for life's continuing survival on the planet.

There is much iron in the Earth's crust, some of which would certainly have been dissolved in the acidic Hadean ocean. Life has seen to it that significant concentrations of iron have long since disappeared from our oceans. The prebiotic iron in the ocean may have been replenished from the acidic hot springs, but certainly not from the alkaline ones. Iron, like its close neighbours in the Periodic Table, cobalt and nickel, does not dissolve in alkaline solutions. In alkaline solution, iron simply precipitates, depending on what else is there, as complex gelatinous hydroxides or sulphides. So, where our alkaline vent leaked into the sea, iron would have been precipitated, along with cobalt and nickel. At this pH boundary, other things will precipitate, too – calcium phosphate, calcium or magnesium carbonate and various silicates, which, as has been mentioned, might catalyse polymerisation. Similar substances are precipitated today at these vents, though now mainly due to temperature and pressure changes, giving rise to chimneys, pots or “beehives” around the vent. The geological remains of such vents are occasionally found.

The deposition of mineral material eventually provided, when the vent was exhausted, a pot in which potential living systems could be confined reasonably

1																	18/VIII	
H	2												B	C	N	O	F	He
Li	Be												Al	Si	P	S	Cl	Ne
Na	Mg	3	4	5	6	7	8	9	10	11	12	Ga	Ge	As	Se	Br	Ar	
K	Ca	Sc	Ti	V	Cr	Mn	Fe	Co	Ni	Cu	Zn	In	Sn	Sb	Te	I	Kr	
Rb	Sr	Y	Zr	Nb	Mo	Tc	Ru	Rh	Pd	Ag	Cd	Tl	Pb	Bi	Po	At	Xe	
Cs	Ba	La-Lu	Hf	Ta	W	Re	Os	Ir	Pt	Au	Hg		Pb	Bi	Po	At	Rn	
Fr	Ra	Ac-Lr																

La	Ce	Pr	Nd	Pm	Sm	Eu	Gd	Tb	Dy	Ho	Er	Tm	Yb	Lu
Ac	Th	Pa	U	Np	Pu	Am	Cm	Bk	Cf	Es	Fm	Md	No	Lr

Essential major element

Essential trace element

Toxic element

Figure 1. The Periodic Table of the elements showing those of significance to life (by courtesy of *Chemistry in Britain*).



Figure 2. A deep sea thermal vent; the "pot" which has developed around the base of the vent is clearly visible (by courtesy of the Southampton Oceanography Centre).



Figure 3. A deep sea thermal vent, showing giant tube-worms (by courtesy of the Southampton Oceanographic Centre).

undisturbed, perhaps attached to the minerals constituting the pot. Some selectivity of attachment could limit the number of participants in the pot biochemistry. In the deep ocean, the effects of radiation of all kinds, vulcanism, meteorites and lightning are less than at the surface.

The deposition of a gel of iron compounds around alkaline vents could provide a potential source of early and renewable membrane material. Such a membrane would perforce have a chemical gradient across it. A pH difference of four or five units is roughly what we have in mitochondria today and which is used for energy production. Another feature of precipitation needs to be noted. The combination of iron and sulphur in the Hadean ocean could have led to the formation of iron-sulphur “cages” (Fig.5), which would have been capable of binding to amino acid material to provide an early

catalyst for oxidation and reduction not unlike the present day ferredoxins which, we must note, pervade all organisms. Iron-sulphur cages also incorporate other trace metals, notably vanadium and molybdenum, which are needed for nitrogen fixation, an important process as life was developing on Earth. Trace elements such as manganese, cobalt, copper and zinc share with iron a common chemical affinity for sulphur, a trait significantly not shared with aluminium, the commonest metal in the Earth's crust, and perhaps the reason for its exclusion from life. Sulphur has many other important rôles in metabolism, notably in the active sites of enzymes.

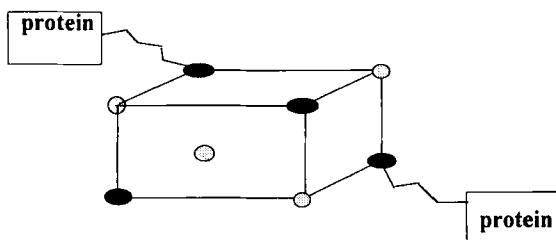


Figure 5. Iron-sulphur "cage" molecule. The smaller circles represent iron atoms, the larger, sulphur which makes attachments to proteins in e.g. ferredoxin.

All this seems a long way from the speculations concerning the origins and interrelationships of nucleic acids and proteins and on the evolution of the genetic code. In a recent Royal Institution lecture, John Maynard Smith has gone so far as to suggest that the chemistry involved in these processes might have been associated with other functions at the time that living systems were beginning their journey, nearly four billion years ago. The idea that life began on an inorganic (i.e. non-carbon containing) matrix merely takes Maynard Smith's suggestion rather further, whilst the idea that life might have evolved deep under the oceans is novel. The current preoccupation of some of those considering life's origins, that a confining membrane may be a key requirement for life to arise at all, is something of which Haldane and Oparin would have approved, although they might have had more difficulty in accepting that membrane might initially have been composed of iron compounds. That the formation of that first membrane might have involved the interaction between acidic and alkaline sub-sea vents is a product of our knowledge of the existence of such springs. Indeed, contemporary alkaline springs on the sea floor have only very recently been described by Buchardt *et al.* in a fjord in southwestern Greenland. The effluent has a pH of 10.4 and is composed of sodium bicarbonate, carbonate and chloride, with, it may be noted, considerable amounts of phosphate. The spring is surrounded by a chimney of hydrated calcium carbonate (ikaite) formed, presumably, by the interaction of the spring and calcium in the surrounding seawater. Although cold, this alkaline spring shares with the hot acidic deep ocean vents an abundance of surrounding

life (Fig. 3), which in the deep ocean depends for its existence on spring water minerals, prime amongst them sulphur compounds. Perhaps this time a sound and sensible theory will succeed in creating a living system in a test tube.

JOHN MARSDEN

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*This paper and a translation of Oparin's first paper published in a Russian journal in 1924 appear in full in Bernal J.D. 1967. *The Origin of Life*. London: Weidenfeld and Nicolson.

Library

The summer months will see a new team of student workers in the Library Reading Room busy cleaning and rearranging the books. This will be going on for the last two weeks in July and all of August. As it usually involves clearing entire sections of book shelves it would be helpful to know a day or so beforehand if you are likely to want any books which are NOT floristic or faunistic accounts or biographies, all of which have already been cleaned and reshelfed. We can then hunt through all the 'relocated' items which may be stacked on trolleys or temporary shelves and not in their usual place. This summer we hope to tidy up the cryptogamic botany section done last summer to fit in new accessions as well as a number of books overlooked or misplaced on the first attempt. The other focus will be on the books on evolutionary biology, a subject area which it would be helpful to have located together on the shelves rather than scattered around the Reading Room.

A provisional date has been fixed for the next book sale on 12th November, in conjunction with an evening meeting, please check your Meetings Card for details. The previous sale added nearly £400 to the Library Book fund as well as enabling us to fill many gaps in our holdings. Due to lack of time we have not been able to formally acknowledge these donations yet but they are gradually being dealt with. As before we are happy to receive donations of all kinds of books (not only biological books) and prefer to receive them well beforehand to give us time to sort them out.

We are most grateful to the wife of the late Theo S. Jones FLS for many useful donations to the Library from her husband's books which have not been listed separately here so as to save space.

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Reviews

Caucasian Dark Circle

Guns, Germs and Steel by Jared Diamond (Jonathan Cape, 1997, 480pp, £18.99; ISBN 0-224038095; p/b Vintage, 1998. £7.99 ISBN 0-099-30278-0) is the evolutionary history of *Homo sapiens* from the end of the last ice age, 13,000 years ago. Dr. Diamond, from UCLA, attempts to answer the question why Eurasians generally have so much by way of material possessions, whilst others of the World's human inhabitants have virtually nothing.

My first reaction to the opening pages of the book was that this was to be another breast-beating exercise by a guilt-wracked member of arguably the greediest society on earth. It is nothing of the kind. Dr. Diamond considers the special factors which, he believes, led to the rapid spread, from Ireland to Manchuria, of settled agriculture after it emerged some 6000 years ago in "the fertile crescent" centred on present day Anatolia. Settled agriculture also emerged independently in a number of other localities, notably in Ethiopia, Central and South America and in SW Asia, but in none of these places was it anything like as varied or developed as in Eurasia. Settled agriculture never arose in sub-Saharan Africa or in Australia, despite, in this latter case, trading contacts between native Australians and the settled societies of Chinese origin or influence in SE Asia.

The book is a mine of information which would otherwise be hard to come by. The treatment is scholarly, as one would expect of Dr. Diamond, but nowhere turgid. At the price it represents extraordinarily good value. One might challenge the sources cited in some cases, but these are minor matters. The book is a splendid read and has justifiably gained its author a Pulitzer Prize.

Why did some societies remain obstinately hunter-gatherers? Was it a case of if it works don't fix it? Or were some of our forebears so debilitated by parasitic disease so as to be unable to sustain the more disciplined life in the fields? Or innate conservatism? What was it that drove Eurasians to set up fortified hierarchies which depended for food on farmers? Anyone who has seen ancestral cereals growing in the wild may well ask how our predecessors came to recognise the virtues of such apparently inconsequential plants which have since become, give or take the odd mutation, our staple foods – wheat and barley. As crops, their seeds need harvesting and storing, with some being made ready for sowing the following year. Such serendipity allowed others to indulge in building anything from Ctesiphon to tessera, from Babylon to biremes, to formalise religion and language, to domesticate horses, cows, pigs, goats and sheep, to develop writing and, much later, literature. These settled societies discovered wheels, smelted metals and made weapons of horrifying destructiveness. More ominously, human population densities rose.

This cooperative spirit, even in extinguishing one's fellow creatures, is all the more remarkable when one considers that the instinctive reaction of *H. sapiens*, the hunter-gatherer, to an encounter with a stranger is to kill him, since he constitutes a threat to physical and genetic survival. Yet ancient Babylon was believed to have boasted a million inhabitants, not all of whom were related – an almost incredible number on

so small a site – until, along with Alexander the Great, most of them were carried off with cholera. These epidemic diseases were, perhaps, the gift of those same domesticated animals, in whose herds they could be maintained. And here is the seed of the dark side of this book. Stout Cortés and Pizarro achieved their conquests in the Americas, not with mere handfuls of human soldiers and artifice as some would have us believe, but with unseen armies of micro-organisms against which the Aztecs and Incas had no defence. Slavery sent cocktails of pestilence east and west to Asia and the Americas, which inflicted the coup de grace on native populations. Alcoholic cocktails did for many more. Surviving native populations in N. America and Tasmania were shot to extinction.

The fact, if you ever doubted it, is that *H. sapiens* is not an attractive animal, and no amount of pheromonally active deodorant can dispel this aura of thuggery. His social evolution has been rapid, at least in Eurasia, but in biological terms he remains an ape with a brain insufficient for his collective good. Dr. Diamond's treatment of the species, from the incessant and internecine tribal wars of New Guinea to the Western conquests of much of the globe, is even-handed. Man's inhumanity to man is everywhere, and much ingenuity has been expended in keeping it that way. 100 million landmines can't be wrong.

JOHN MARSDEN

BRITISH SALTMARSHES

edited by

B R Sherwood, B G Gardiner & T Harris

The saltmarshes of Great Britain total over 42,250ha and are among the most natural ecosystems remaining, especially where enclosure has not taken place.

Although fragmented they cover a larger area than the single largest saltmarsh (the Wadden Sea).

This wide distribution of sites support communities that span the northern and southern elements of the European range of variation. In addition they include important and, in some cases, specially-adapted invertebrates that include a number of rare species. They are also home for a wide variety of breeding and migratory birds.

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Thrips by William D.J. Kirk., Naturalist's Handbook No. 25., publ. The Richmond Publishing Co. Ltd., Slough UK., 1996. 70pp., line drawings, two colour plates. P/B ISBN 0-85546-307-4 £8.95., H/B ISBN 0-85546-308-2. £15.

A general field naturalist like myself, who has a specialist interest in one or two groups of living organisms, often observes phenomena in the field which involve other taxa for which long searches in specialist libraries are often necessary. An interesting observation may be lost simply because of the difficulty of knowing what this awkward member of the taxa is. Thrips are the sort of minute little creatures which a botanist with a hand lens cannot fail to have seen and admired, but will have considered that there is no hope of giving it a name. William Kirk's booklet, with its numerous line drawings and a few colour plates would appear to allow you to start to find out what your thrips is (yes apparently thrips is the singular for many thrips).

There are detailed anatomical descriptions of typical members of the three sub-orders of thrips that comprise our 160 species in the UK, which allow almost a naked eye separation of some of them. I found the descriptions of the mating, fighting and 'sneak mating' behaviour of the males fascinating. The existence of thrips feeding marks on leaves of cereal crops, circles on tomatoes, spots on peas and yellowing of cabbages all begin to make sense (and help to identify thrips). I shall look for pollen grains buckled by these creatures after they have sucked them dry. As soon as summer comes I shall be out looking for "rubus thrips" in our bindweed flowers, and "western flower thrips" in my greenhouse. I shall look forward to the spectacle of males fighting side by side with their abdomens, blasting pheromone to repel each other. The author almost casually suggests that this pheromone may be usable as a means of protecting crops! This is exactly what these little booklets are about, stimulating research by amateurs and who knows what will become of it?

Apart from the identification keys, line drawings and just two colour plates, there is a useful list of addresses as well as references, a good index. There are also very useful sections on collection, mounting these creatures. If I had any disappointment, it was the lack of more colour plates, but the booklet will hopefully inspire an artist to tackle this rewarding and useful task. A video camera may also unravel some useful tricks of sneak mating!

I commend this booklet to all field naturalists.

B.W. FOX

Obituary

Ronald Keay

Ronald Keay was a member of the Council of the Society for four terms beginning in 1963, Vice-President three times and Treasurer from 1989–95. He graduated in botany from Oxford in the early years of the war and was sent to Nigeria, where he remained for twenty years (six of which he spent on secondment at Kew) as Principal of the Forest School in Ibadan and finally as Director of the Federal Department of Forestry Research. There he published his *Outline of Nigerian Vegetation, Flora of West Tropical Africa* and *Trees of Nigeria* the royalties from the second edition of which he characteristically devoted to the Society's Dennis Stanfield Memorial Fund for tropical African botany (see below).

With a youngish family, he decided in the early sixties to try to obtain a post in the UK. Seeing that expertise in tropical forestry was unlikely to curry much favour in Britain, he sought a number of appointments where his administrative experience might be of value. One of these was the Deputy Executive Secretaryship of the Royal Society, on which he pinned rather few hopes, but his mentor, Sir George Taylor, then Director of the Royal Botanic Gardens, advised otherwise and Ronald was appointed on 17th May 1962, taking up the post in October. A prime task was to organise successfully the move of the Society from Burlington House to Carlton House Terrace in 1967, but he played a full rôle in the many activities of the Royal Society at the time.

In mid-December 1977, the Executive Secretary, Sir David Martin, died and within a month Ronald was offered the job by the then President, Lord Todd. This was a remarkable turn of speed by a body not noted for rapid locomotion. This was not, in hindsight, a good time for the Royal Society, coinciding as it did with Margaret Thatcher's assumption of power and culminating in the very public debate by the Fellowship as to her suitability as a Fellow herself, although all prime ministers of five years standing did, by tradition, become Fellows.

My acquaintance with Ronald goes back a mere dozen years, far less than many Society Members. Nevertheless those years marked a third age for him. In the late eighties Ronald served as President of the Institute of Biology after his retirement from the Royal Society and in 1989 he also became Treasurer of the Linnean Society. He was also very proud to have been an Honorary Visiting Professor at the University of Essex, and he served as a Trustee (and pensioner) of the Royal Society's pension fund. He continued his association with St. Martin-in-the-Fields Church, which he had established in his Royal Society days.

In all these unpaid tasks he was motivated by a strong sense of duty. He certainly took them all very seriously, visiting every Branch of the Institute of Biology (18 in the UK, and even getting to the one in Hong Kong) during his two-year term as President of the Institute. Ronald might well have decided against accepting the Treasurership of the Linnean Society, since his early experiences thirty years ago was of a Society so short of money that parts of its valuable collections – books, paintings and prints – were sold off. It was a policy to which he was opposed, and he was one

of those who joined together to prevent the sale of the unique biological collections of Linnaeus and Smith. As a reward, he was asked to organise a fund-raising drive for the Society, which raised the considerable sum needed for the first air-conditioned Archive Room, wherein the precious collections were stored and for other improvements.

It was this attitude – trying to solve problems, rather than sitting on his hands – which was characteristic of the man. Finding that he was responsible for the Society's Dennis Stanfield Fund, a memorial to one of his former colleagues from his days in Nigeria, he set about increasing the capital of the fund. He wrote letters to friends and acquaintances, and increased its capital by several thousand pounds. In this he was helped by an encyclopaedic knowledge of pretty well anyone whom he had ever met, or heard of, and it was a most valuable gift for the Linnean Society. There was nothing on which he could not find an appropriate authority. Unlike most of us he wrote all these things down. As a result he enjoyed a wide circle of friends, and in his last weeks it seemed that Birch Grove was a little like Clapham Junction, a situation he much appreciated, for he greatly enjoyed the company of others.

Yet the start of his term of office as Treasurer, which co-incided with my own appointment in 1989, started off badly. The recently appointed Finance Officer had been off sick for months and the Society's books had not been kept up, nor had creditors been paid. He had in me, moreover, an Executive Secretary who knew rather little about running a learned society and almost nothing about the Linnean Society itself.

Thanks to Ronald, we were out of the wood (and a Finance Officer) in a couple of months, without most being aware that there had been any sort of a crisis. It set the tone for the future. Ronald gained a detailed understanding of the Society's finances, which he successfully sought to make more intelligible to the wider membership of the Society. His rôle in the Society also helped him in his trusteeship of the Royal Society pension fund, since the Society's investments are extremely well managed. He moved the Society to producing an Annual Report, something he remembered from his Royal Society days, and which is now in its fifth year. In this he recognised earlier than most the increasing rôle of the Charity Commissioners in societies such as the Linnean.

Ronald enjoyed indifferent health for some years. Sadly this contributed to his giving up the Treasurership, but he left the Society's financial affairs in a great deal better shape than he had found them. He continued to come to the Society regularly and was a welcome visitor to all the staff. Those of us fortunate to be associated with Ronald's third age have a memory of a man of deep commitment, openness and decency. We are thankful that he was a part of us.

JOHN MARSDEN
