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Richard Owen's Reaction to Transmutation in the 1830's

Adrian Desmond*

Introduction

FOLLOWING Michael Bartholomew's study of 'Lyell and Evolution' in 1973,¹ scholars have become increasingly interested in the response of gentlemen geologists² to Lamarckism during the reign of William IV (1830–7). Bartholomew contended that Charles Lyell was 'alone in scenting the danger' for man of using transmutation to explain fossil progression,³ and that he reacted to the threat of bestialisation by restructuring palaeontology along safe non-progressionist lines. Like his Anglican contemporaries, Lyell was concerned to prove that man was no transformed ape, and that morals were not the better part of brute instinct. Dov Ospovat has subsequently suggested that Lyell's theory of climate was equally an attempt to thwart the transformists and 'preserve man's unique status in creation'.⁴ In other words, Lyell's biology and geology were inextricably related in *Principles of Geology* and his ideology affected his science as a whole. Finally, Pietro Corsi has identified the Continental materialists who most probably alerted Lyell to the danger, intimating that a conservative British response became imperative when Lyell 'saw signs of the diffusion of transformism in England itself, where it could even form an unholy alliance with prevailing progressionist and directionalist interpretations of the history of life on earth'.⁵

Despite this retooling in the Lyell industry, little has been done to identify the British Lamarckians or the extent of their social and scientific

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¹ Michael Bartholomew, 'Lyell and evolution: an account of Lyell's response to the prospect of an evolutionary ancestry for man', *British Journal for the History of Science*, 1973, 6, 261–303.

² Studies on the social status of these cultivators of science include Jack Morrell and Arnold Thackray, *Gentlemen of science: early years of the British Association for the Advancement of Science*, Oxford, 1981; Martin J. S. Rudwick, 'Charles Darwin in London: the integration of public and private science', *Isis*, 1982, 73, 186–206; Roy Porter, 'Gentlemen and geology: the emergence of a scientific career, 1660–1920', *Historical Journal*, 1978, 21, 809–36; S. F. Cannon, *Science in Culture: The Early Victorian Period*, New York, 1978.

³ Michael Bartholomew, 'The singularity of Lyell', *History of Science*, 1979, 17, 276–93 (281). Study of Owen tends to suggest that Lyell was not in fact alone.

⁴ Dov Ospovat, 'Lyell's theory of climate', *Journal of the History of Biology*, 1977, 10, 317–39 (318).

⁵ Pietro Corsi, 'The importance of French transformist ideas for the second volume of Lyell's *Principles of Geology*', *British Journal for the History of Science*, 1978, 11, 221–44 (241).

threat in the radical thirties. So in this paper I want to extend the scope of the enquiry, first by taking up the point dropped by Corsi: that is, to discuss the dissemination of transformist ideas by Lamarckians and Geoffroyans in Britain. And second, I hope to anchor the debate more firmly in the social context by analysing the radical connections of some leading transformists. This will allow me to treat the Anglican geological response as part of the wider reaction to the threat of reform, and perhaps revolution, in the 1830s.⁶

To expedite matters, I will shift the focus onto the socially prominent Anglican morphologist and palaeontologist Richard Owen (1804–1892). My reasons for singling out Owen are straightforward: we can identify the transformists with whom he was acquainted, and have sufficient manuscript material to follow his changing opinions of them. There were important differences between Owen and Lyell which work to our advantage. Owen was not wealthy and had to earn a living, holding various teaching posts besides his curatorship and (from 1836) Chair at the College of Surgeons. This left him more ‘institutionalised’ in a sense—he was subject to certain conventions, and his response to Lamarckism had to be compatible with ‘official’ College thinking. Since the College itself was under attack from medical reformers, some with radical and transformist connections, we have an institutional framework for interpreting Owen’s response. Also, it is true that Lamarckism was as relevant to London zoologists as geologists (or more so), and since the embryo Zoological Society housed both anti-transformists and transformists, we can plot the social consequences of Owen’s anti-Lamarckian gambit inside the Society, and follow the decline of Lamarckism in terms of its shrinking power base.

Owen is an attractive subject for a final reason. His anti-Lamarckian ideology had as strong a heuristic function as Lyell’s. It led not only to his innovative approach towards monotremes, apes, and saurians, but I hope to show that it was inextricably related to the development of his archetypal morphology and von Baerian paleontology in the 1840s. Thus the present study is relevant to the recent research of Ospovat and Peter Bowler on Owen’s model of fossil divergence, which Bowler has called ‘revolutionary’.⁷ By studying Owen’s attitude to Lamarckism, we can help

⁶ On the widespread fear of revolution see E. P. Thompson, *The making of the English working class*, 3rd edn, London, 1980, pp. 887–915; J. F. C. Harrison, *Early Victorian Britain 1832–51*, London, 1979, Ch. I. The use of revolutionary rhetoric by the radicals is discussed in J. Hamburger, *James Mill and the art of revolution*, New Haven, 1963. On the reform movement generally see E. Halévy, *The triumph of reform 1830–1841*, 2nd (revised) edn. (tr. E. I. Watkin), London, 1950; G. B. A. M. Finlayson, *England in the eighteen thirties*, London, 1969; and W. Thomas, *The philosophic radicals*, Oxford, 1979.

⁷ Dov Ospovat, ‘The influence of Karl Ernst von Baer’s embryology, 1828–1859: a reappraisal in light of Richard Owen’s and William B. Carpenter’s “Palaeontological application of Von Baer’s Law”’, *Journal of the History of Biology*, 1976, 9, 1–28; Ospovat, *The development of Darwin’s theory: natural history, natural theology, and natural selection, 1830–1859*, Cambridge, 1981, esp. ch. V; Peter J. Bowler, *Fossils and progress: paleontology and the idea of progressive evolution in the nineteenth century*, New York, 1976, ch. V, esp. p. 110; Adrian Desmond, *Archetypes and ancestors: palaeontology in Victorian London 1850–1875*, London, 1982, chs. I and II. J. H. Brooke has discussed Owen’s hatred of the *Vestiges*’ transmutation in ‘Richard Owen, William Whewell, and the *Vestiges*’, *British Journal for the History of Science*, 1977, 10, 132–45.

to explain *why* he pioneered these new approaches, and since his ideology stemmed from fears about Lamarckism's socially-disruptive consequences we have a means of tackling the social basis of the new science.

How did Owen make contact with Lamarckism?

To suggest that Owen saw Lamarckism as a 'threat' presupposes that this doctrine of species change was presented in a form which imperilled some aspect of his political, religious, or social beliefs. A 'threat' also has an aura of immediacy about it—it implies that he encountered Lamarckism as a *living* issue. Lamarck himself might have died a blind pauper in 1829, and been ceremoniously interred by Cuvier in a disparaging *éloge*,⁸ but transformism was no straw man in the 1830s. In this section I suggest that Owen was made forcefully aware of its strengths in London and Paris at this time; that he was personally familiar with its advocates, whom he—for a time at least—counted as his friends; but that he soon realised its appalling implications for man.

In July 1831 Owen, then Assistant Conservator of the Hunterian Museum, made his first trip to Paris. He had been invited by Georges Cuvier (1769–1832), founder of the 'Macedonian Empire'⁹ of comparative anatomy and the most influential professor at the Muséum d'Histoire Naturelle. (The invitation had been made the previous year: during the July Revolution Cuvier had opportunely slipped into England, and had met Owen at the College of Surgeons.) The Paris Muséum was enjoying considerable prestige at this time and played host to savants from all over Europe. It boasted professors of world renown, two of whom, Étienne Geoffroy Saint-Hilaire (1772–1844) and Henri Ducrotay de Blainville (1777–1850), are important from our perspective, for it is Owen's reaction to their British supporters that will concern us here. Little is known about Owen's trip, which lasted from late July until early September. His grandson did publish one letter, written from Paris to William Clift, Conservator of the Hunterian Museum, in which Owen mentions meeting Cuvier and listening to lectures at the Institute. But mostly it relates his enjoyment of the Royalist fireworks and the arrival of Louis-Phillipe.¹⁰ Unfortunately, nothing is said of his views concerning the riots or the rampant anti-clericism which had fuelled the July Revolution, of which he must have been aware (he could hardly have missed the burnt-out shell of the Archbishop's palace next to Notre Dame). A respectable, aspiring Anglican, already the recipient (as his mother proudly notes¹¹) of Cuvier's patronage, could not have been blind to the republican mood.

⁸ G. Cuvier, 'Éloge de M. de Lamarck', *Mémoires de l'Académie Royale des Sciences*, 1835, 13, 1–31; on Lamarck see Richard W. Burkhardt, Jr., *The spirit of system: Lamarck and evolutionary biology*, Cambridge, Mass., 1977.

⁹ Camille Limoges, 'The development of the Muséum d'Histoire Naturelle of Paris, c. 1800–1914' in R. Fox and G. Weisz (eds.), *The organization of science and technology in France 1808–1914*, Cambridge, 1980, 211–40 (222).

¹⁰ Rev. R. Owen (ed.), *The life of Richard Owen*, 2 vols., London, 1894, i, 51–8.

¹¹ *Ibid.*, pp. 58–9.

We do however possess one document which is crucially important for understanding Owen's experience in Paris, since it shows how he might have been introduced to the contentious issues in French comparative anatomy. He kept a pocket book, in which he noted social engagements and scientific observations. From this we learn that he stayed at the Hôtel du Jardin du Roi, and that another guest was the Scottish invertebrate zoologist Robert E. Grant (1793–1874)—a Geoffroyan transcendental anatomist, Lamarckian transformist, and Professor of Comparative Anatomy at the new London University. The notebook reveals Owen becoming increasingly familiar with Grant. On innumerable occasions he breakfasted and dined with him (seeking out new restaurants for the purpose); they attended lectures together, and engaged in deep discussions, and Grant regaled Owen with stories of his travels in Germany, Austria, Bohemia, and Italy.¹² Grant was a frequent summer visitor to the Muséum and had collaborated with Geoffroy the previous year on the question of the platypus's oviparity, a subject closely connected with transformism, and one which Owen himself was shortly to take up. Given Grant's familiarity with the Muséum and friendship with the inexperienced Owen, it is not hard to imagine him showing Owen the ropes. Grant had an irrepressible nature and an unrestrained enthusiasm for Lamarck (as Darwin found on talking to him at Edinburgh five years earlier¹³). And seeing that the famous debates between Geoffroy and Cuvier had only taken place the previous summer in the Academy¹⁴, and that Geoffroy was currently working out the specific sequence of crocodile transformations, it is eminently likely that Grant canvassed for his friend Geoffroy in Owen's presence. Owen recorded that he 'Bought *Philos. Zoologique*'¹⁵; and although it is uncertain whether this was Lamarck's *Philosophie Zoologique*, which was reissued in 1830, or Geoffroy's *Principes de Philosophie Zoologique* (1830), it is clear that he was now intimately aware of the transformists' case. Another entry, written after attending one of Cuvier's Saturday *soirées*, confirms that he had heard at first hand of Cuvier's grievances with Geoffroy, and we also see the ape problem mentioned here for the first time:

Saturday [20 August 1831] . . . In the Evening at Cuvier's. Mad. Cuv & Mad^{lle} Duvaucel to both gave Mr & Mrs Clifts regards on which they returned kind regards stayed till 11. Cuvier shook hands at going away—Had a long convers. about Orang with him [Owen himself had only recently dissected an orang utan at the Zoological Society]. He said he had never dissected a Chimpanzee, was going to write upon [Sternum?] contra

¹² Richard Owen MS. Notebook 5, *passim*, British Museum (Natural History)—hereafter cited as BM(NH).

¹³ Nora Barlow (ed.), *The autobiography of Charles Darwin*, New York, 1958, p. 49.

¹⁴ Geoffroy Saint-Hilaire published extracts and commentaries on this debate in *Principes de philosophie zoologique*, Paris, 1830. Toby Appel has discussed the politicking in 'The Cuvier-Geoffroy debate and the structure of nineteenth-century French zoology', Princeton University Ph.D. thesis, 1975.

¹⁵ Owen, *op. cit.* (12), entry for Wednesday 17 August 1831.

Geoffroy. Dr Grant introduced me to Fred. Cuvier, who enquired what had been done with our dead Giraffe.¹⁶

Taken as a whole, this notebook evidence is suggestive. Grant's London lectures were conceived within a framework of materialistic deism,¹⁷ and he had shown himself a strong supporter of Geoffroy and Blainville (Blainville was no transformist himself although, as Toby Appel has indicated, he championed aspects of Lamarckism for anti-Cuvierian ends¹⁸). In Paris, Owen must have been made forcefully aware of the support Geoffroy received, and that naturalistic models (like Grant's) of serial development using a cooling-earth 'engine' could only serve to reinforce transformist speculations.

The reception of Lamarckism in London

There is evidence that Owen was familiar with the outlines of Lamarckism before he left for Paris. This is partly circumstantial: for example, at Edinburgh in 1824–5 he had studied under the extra-mural anatomist John Barclay (1758–1826), who in that year had delegated the invertebrate part of his course to Grant. In March 1825 the Geoffroyan transcendentalist Robert Knox (1793–1862) was actually to go into lecturing partnership with Barclay.¹⁹ Knox of course became famous for his radical anti-church stance, but such a position was by no means unusual at this time. A materialist philosophy of mind was hotly debated in one of the student societies in Edinburgh in 1825–7, the Plinian Natural History Society, in which Grant and Knox were active. One student member, the phrenologist W. A. F. Browne (1805–1885), insisted categorically on the material basis of mind, while another, W. R. Grey, failed to see any difference in kind between human and animal intelligence.²⁰ The overt materialism of this group was probably a product of the intersection of the naturalistic anti-Kirk ideology of younger members attracted to phrenology²¹—Browne being a disciple of the leading Edinburgh phrenologist and social reformer George Combe (1788–1858)—and the philosophical deism

¹⁶ *Ibid.*, entry for Saturday 20 August 1831.

¹⁷ I have discussed Grant's debts and the structure of his lectures in 'Robert E. Grant: the social predicament of a pre-Darwinian transmutationist', *Journal of the History of Biology*, 1984, 17, 189–223.

¹⁸ Toby A. Appel, 'Henri de Blainville and the animal series: a nineteenth-century chain of being', *Journal of the History of Biology*, 1980, 13, 291–319.

¹⁹ R. Knox, *Great artists and great anatomists*, London, 1852, pp. 73, 211–2; for Knox's radicalism and stay in Paris see H. Lonsdale, *A sketch of the life and writings of Robert Knox the anatomist*, London, 1870, chs. II and XX; and Isobel Rae, *Knox the anatomist*, Edinburgh, 1964, pp. 25–7. Grant's studies in Paris are detailed in 'Biographical sketch of Robert Edmond Grant', *The Lancet*, 1850, 2, 686–95.

²⁰ Desmond, *op. cit.* (17); Plinian Minutes MSS, Vol. 1, 1826–28, Edinburgh University Library, Dc.2.53, ff. 51, 56, 57; on which see also Howard E. Gruber, *Darwin on Man: a psychological study of scientific creativity*, New York, 1974, p. 39.

²¹ Steven Shapin, 'The politics of observation: cerebral anatomy and social interests in the Edinburgh phrenology disputes', in Roy Wallis (ed.), *On the margins of science: the social construction of rejected knowledge*, Sociological Review Monograph 27, 1979, pp. 139–78. Browne's role in the phrenological movement is considered in R. J. Cooter, 'Phrenology and British alienists, c. 1825–1845. Part 1: Converts to a doctrine', *Medical History*, 1976, 20, 1–21.

imported from France by older MDs like Knox and Grant. (Grant by 1826 was already mating transformist speculations with contemporary ideas on directional earth history.²²) If Owen did not encounter transformism at this time, he was certainly aware of it shortly after; one of his early reading lists, watermarked 1828, includes Lamarck's *Histoire Naturelle des Animaux sans Vertèbres* and papers by Lamarck and Grant²³—and since Owen was shortly transcribing passages from Grant's papers on cephalopods in the *Edinburgh New Philosophical Journal*,²⁴ he had probably seen Grant's anonymous papers on transformism in earlier numbers. Overall it is hard to imagine that such an astute observer as Owen was blind to the heterodox fringe at Edinburgh, or to the reports of Geoffroy's transformism that continued to be published in Jameson's *Journal*.²⁵

At this stage it is important to establish the kind of preconceptions Owen entertained before personally engaging the philosophic deists in Paris. The evidence suggests that in London he had already been encouraged to view Lamarckian materialism from an antagonistic romantic perspective. He had moved to the capital in 1825 to continue his apprenticeship under the surgeon John Abernethy (1764–1831) at St. Bartholomew's Hospital. Abernethy, as President of the College of Surgeons, went on to help Owen become assistant to Clift in the Hunterian Museum. Abernethy was a vitalist whose attack on William Lawrence's mechanist transgressions had brought him to the notice of the influential 'Germaniser' and transcendentalist S. T. Coleridge (1772–1834). Coleridge of course had long denounced the mechanisms and atheisms of Anglo-Gallic corpuscular philosophy which had led to the 'Lawrencian' aberration, telling his confidante and literary executor, the surgeon Joseph Henry Green (1791–1863), that no system of materialism in which organisation stood supreme could possibly incorporate the Soul.²⁶ There is no denying that the College of Surgeons had a strong Coleridgean lobby. Knowing Coleridge's liking for Lincoln's Inn, his interest in medical philosophy, and sympathy for Abernethy and Green, one can imagine that these gentlemen-surgeons, as much as chemists like Davy, provided his models for the ruling clerisy of the National Church in *On the Constitution of the Church and State* (1830).²⁷ Abernethy attended Coleridge's philosophy

²² R. E. Grant, 'On the structure and nature of the *Spongilla friabilis*', *Edinburgh Philosophical Journal*, 1826, 14, 270–84 (283); [Grant], 'Observations on the nature and importance of geology', *Edinburgh New Philosophical Journal*, 1826, 1, 293–302; [Grant?], 'Of the changes which life has experienced on the globe', *ibid.*, 1827, 3, 298–301.

²³ Richard Owen, 'Books referred to for Natural History', Royal College of Surgeons MS. 275h.3.5.

²⁴ Sir Richard Owen Scientific Notes, c. 1828–1832, British Library Add. MS. 34,406, f. 38—hereafter cited as BL.

²⁵ For example the anonymous 'Of the continuity of the animal kingdom by means of generation, from the first ages of the world to the present times', *Edinburgh New Philosophical Journal*, 1829, 7, 152–5.

²⁶ E. L. Griggs (ed.), *Collected letters of Samuel Taylor Coleridge*, Oxford, 1971, v, p. 47; June Goodfield-Toulmin discusses the Lawrence affair in 'Some aspects of English physiology: 1780–1840', *Journal of the History of Biology*, 1969, 2, 283–320.

²⁷ S. T. Coleridge, *On the constitution of the church and state according to the idea of each* (ed. J. Barrell), London, 1972.

lectures at the Crown and Anchor; and Coleridge, disliking aspects of Abernethy's vitalism, tried to instil in him more philosophic means of repulsing 'the attacks of Lawrence, and the Materialists'.²⁸ Partly it was this background of *medical* disputes, and in particular the 'hot controversy between Abernethy & Lawrence',²⁹ which gave Coleridge's lectures their topicality. For his part, Abernethy in his 1819 Hunterian Oration drew from Coleridge's lectures the moral that patriotism and sound philosophy were synonymous³⁰—with the implication, so common in this period of post-Napoleonic reflection, that French Godless materialism had been at root of the Terror, and that therefore Lawrence's crime was a political one. But the real Coleridgean driving force inside the College was J. H. Green, and it was Green's profoundly Coleridgean romanticism that most concerns us in connection to Owen. Green had studied philosophy in Berlin, and had met Coleridge by 1817. He attended Coleridge's Thursday class and became his leading medical disciple. As Professor of Anatomy at the College, Green was in the process of delivering an extended four year course on zoology (1824–7) when Owen first arrived; and this course, Trevor Levere speculates, was developed by Green with one eye possibly on Coleridge's unpublished manuscript *Theory of Life*.³¹ The lectures were conceived within a framework of *Naturphilosophie*, using Carus as a textbook. Long afterwards Owen recalled that

the first characteristic . . . of this course—extended over 4 years—is that it embraced the entire range of the Science. For the first time in England the comparative Anatomy of the whole Animal Kingdom was described, and illustrated by such a series of enlarged and coloured diagrams as had never before been seen. The vast array of facts was linked by reference to the underlying Unity, as it had been advocated and illustrated by Oken and Carus.³²

Owen attended the lectures for three years, and even provided dissections to accompany it. He also left notes covering the last year (1827) from which it is clear that Green had considered 'nature as a series of evolutions' (i.e. a progressive unfolding) while denying that in any Lamarckian sense 'the lower [forms] can assume the characters of the higher'.³³ Hence on his arrival in London Owen had been put in touch with conservative Coleridgean and anti-materialist romantic thought. The anti-Lamarckian theme he went on to develop in the thirties, and the more idealised aspects of romanticism were eventually to predominate in his work on archetypes and homologies in the early 1840s.

²⁸ Griggs. op. cit. (26) iv, p. 809.

²⁹ *Ibid.*, p. 928.

³⁰ John Abernethy, 'The Hunterian Oration, for the year 1819', in *Physiological lectures addressed to the College of Surgeons*, London, 1825, esp. pp. 60–8 (68).

³¹ Trevor H. Levere, *Poetry realized in nature: Samuel Taylor Coleridge and early nineteenth-century science*, Cambridge, 1981, pp. 44–5.

³² Owen's letter quoted in J. H. Green, *Spiritual philosophy: founded on the teaching of the late Samuel Taylor Coleridge* (ed. John Simon), 2 vols., London, 1865, i, p. xiv.

³³ Richard Owen, 'Notes and annotations', Royal College of Surgeons MS. 275.b.21, f. 131.

Study of the methodological presuppositions of zoologists in the metropolis also indicates that the environment in London in the late 1820s and early 1830s was potentially hostile to a materialistic transformism. The mid-1820s was a time of rapid differentiation, with the establishment of the Zoological Club of the Linnean Society (1822–9), the *Zoological Journal* (1824–34), and the Zoological Society (f. 1826), whose *Proceedings* first appeared in 1830. A limited prosopographical study³⁴ of the core group of zoologists involved in all three undertakings reveals that most came from affluent families, were educated at the London hospitals or Oxbridge, and were employed in law, medicine, or government service (the greatest number³⁵). Significantly, none of the frontrunners was educated at Edinburgh, where the wealthy sent their sons to receive a liberal education. This respectable group of avocationists-cum-careerists envisaged its role in providing exact classification and detailed description.³⁶ When members did champion a higher-level generalisation—as in the case of MacLeay’s circular system, advocated to varying degrees by William Kirby, Thomas Horsfield, William Swainson, Joshua Brookes, and N. A. Vigors³⁷—it was as an aid to classification. Any metaphysical sentiment that was expressed was antithetical to the materialism of the Plinian group; thus the *Zoological Journal*, despite its declared empirical aims, opened with one of a series of articles by John Oliver French on brute instinct, in which he distinguished the moral actions of men from conscious behaviour in animals.³⁸ Otherwise papers deliberately shunned theoretical matters, to the extent that the Poor Law reformer J. E. Bicheno in an address to the Zoological Club in 1826 actually criticised the obsessive attention paid to detail and called for the ‘subdivision of labour’ and cultivation of a more philosophical ‘class’ of generalisers.³⁹ But even his generalisers would have born little resemblance

³⁴ On the use of this technique see Steven Shapin and Arnold Thackray, ‘Prosopography as a research tool in history of science: the British scientific community 1700–1900’, *History of Science*, 1974, 12, 1–28. The ‘core group’ comprises officers and committee of the Zoological Club, most frequent contributors to the *Zoological Journal*, and scientifically-active members of the Zoological Society. As a bare minimum this would include: T. Bell (1792–1880), E. T. Bennett (1797–1836), J. E. Bicheno (1785–1851), J. Brookes (1761–1833), J. G. Children (1777–1852), J. E. Gray (1800–1875), T. Horsfield (1773–1859), W. Kirby (1759–1850), W. S. MacLeay (1792–1865), J. Sabine (1770–1837), J. F. Stephens (1792–1852), G. B. Sowerby (1788–1854), J. de C. Sowerby (1787–1871), N. A. Vigors (1785–1840), and W. Yarrell (1784–1856).

³⁵ Children was Keeper of the Natural History Collections at the British Museum, and Gray was his assistant; MacLeay was attaché to the embassy in Paris; Sabine was Inspector-General of Taxes; and Stephens worked in the Admiralty Office. For a recent study of zoologists holding public office see A. E. Gunther, *The founders of science at the British Museum 1753–1900*, Halesworth, 1980, chs. V–VII.

³⁶ These goals were spelt out in the ‘Introduction’ to the *Zoological Journal*, 1824, 1, pp. iii–vii, and in the yearly addresses; see also Sandra Herbert, ‘The place of man in the development of Darwin’s theory of transmutation. Part II’, *Journal of the History of Biology*, 1977, 10, 155–227 (170–6).

³⁷ E.g., W. Kirby, ‘Introductory address, explanatory of the views of the Zoological Club’, *Zoological Journal*, 1825, 2, 5–6; Joshua Brookes, *An address delivered at the anniversary meeting of the Zoological Club . . .*, London, 1828, p. 5; N. A. Vigors, ‘Observations on the natural affinities that connect the orders and families of birds’, *Transactions of the Linnean Society*, 1825, 14, 395–517.

³⁸ J. O. French, ‘An Inquiry respecting the term Nature of Instinct, and of the mental distinction between brute animals and man’, *Zoological Journal*, 1824, 1, 1–32, 153–73, 346–67.

³⁹ J. E. Bicheno, *An address delivered at the anniversary meeting of the Zoological Club . . .*, London, 1826, pp. 23–30.

to the Parisian system-builders. Lamarck might have been 'venerable',⁴⁰ but only because of his conchological classification, which this group was eminently suited to appreciate. It was typical of the *Journal's* descriptive bent that it dismissed Geoffroy's 'peculiar views' on crocodile descent as something of a curiosity.⁴¹

What Sandra Herbert claims for Darwin was equally applicable to Grant: that he could have found no audience for transmutation within the London scientific societies.⁴² However good the reception of Grant's and Knox's Parisian views by Plinian materialists, they were guaranteed no such welcome in the capital, except from the small doctrinaire radical faction—even Grant's philosophical anatomy and vertebral theory of the skull, though it profoundly influenced some students, drew sarcastic comments from disaffected fellow professors.⁴³ Yet if we consider the 'Scotch' Whig and Utilitarian ideology of the founders of the university it is obvious that on many counts Grant was suited to Gower Street. The institution was largely modelled on Edinburgh University, but there were significant differences. In Edinburgh two-thirds of the Chairs were actually under the patronage of the Town Council, which also played a supervisory role,⁴⁴ and this ensured strong links between town and university. But the Gower Street school was a joint-stock company; it owed financial allegiance to its merchant stockholders, not civic authority.⁴⁵ Hence the zoological community had little say in the choice of the first Professor of Zoology in 1827; and despite three of the four candidates being part of, or known to, the London zoological community (the most influential applicant was the founder of the Blenheim Street museum Joshua Brookes⁴⁶), the future Whig Chancellor Henry Brougham, himself

⁴⁰ *Zoological Journal*, 1825, 2, 428. Lamarck's conchological classification was well known from such works as Charles Dubois, *An Epitome of Lamarck's arrangement of Testacea: being a free translation of that part of his work, De l'histoire naturelle des animaux sans vertèbres*, London, 1824; and Edmund A. Crouch, *An illustrated introduction to Lamarck's conchology contained in his Histoire naturelle des animaux sans vertèbres*, London, 1827.

⁴¹ *Zoological Journal*, 1825, 2, 424–5.

⁴² Herbert, op. cit. (36), 159–64; Rudwick, op. cit. (1), 203–4.

⁴³ When Grant was asked to investigate student claims of G. S. Pattison's incompetence in 1831, Pattison snapped back that a man devoted 'to the idle and unprofitable speculations' of German anatomists and who 'spent nearly the whole session in an attempt to prove an absurdity, viz. that all the bones of the skull are vertebrae' was in no position to judge! *The Lancet*, 1831–2, 1, 86.

⁴⁴ J. B. Morrell, 'Science and Scottish university reform: Edinburgh in 1826', *British Journal for the History of Science*, 1972, 6, 39–56 (40–1).

⁴⁵ H. Hale Bellot, *University College London 1826–1926*, London, 1929, ch. II; Morrell's statement that the professors received small salaries plus student fees was only true up to 1831, when the 'guarantee money' was withdrawn, leaving professors like Grant barely able to maintain a respectable lifestyle. J. B. Morrell, 'Individualism and the structure of British science in 1830', *Historical Studies in the Physical Sciences*, 1971, 3, 183–204 (198).

⁴⁶ Brookes was a successful teacher; he published in the *Zoological Journal* and was President of the Zoological Club in 1828. Large portions of his museum, which he began disposing of in 1826, ended up in Grant's departmental museum: on which see J. Brookes to G. Birkbeck, 11 March 1826, College Correspondence MS. 1826: 53, University College London—hereafter cited as UCL. Others in the London community, notably Vigors and Gray, donated specimens to the university museum.

an Edinburgh man, settled on the 'outsider' Grant, who came with impeccable Scottish credentials.⁴⁷

But Grant's 'outsider' Edinburgh status is not enough to explain his eventual estrangement from the London community; after all, his clubbable colleagues—the chemist Edward Turner (1798–1837) and physiologist William Sharpey (1802–1880)—quickly ensconced themselves in London scientific society. Perhaps it is better if we look to Grant's naturalistic science and radical connections, which in the reform years would have alarmed conservative Oxbridge zoologists and their London allies like Owen. This social approach provides one of the most fruitful ways of tackling the growing antagonism between Grant and Owen (who came to dominate the metropolitan zoological community in the later 1830s). It also provides a major explanation for the failure of Lamarckism to transfer successfully to the capital.

The social background: Lamarckism and reform

Owen's and Grant's contrasting social self-perceptions and political aspirations are apparent from their respective social circles; circles which were effectively separated by a gulf in these years embracing the Reform Bill crises of 1831–2 and the first stirrings of Chartism. In the late 1820s Owen had begun developing strong ties with a number of prominent lawyers while he was still practising in Cook's Court in Lincoln's Inn Fields, among them David Pollock (1780–1847), who was appointed King's Counsel in 1833. (Owen actually carried a letter of introduction from Pollock with him to Paris, suggesting that he was put in touch with conservative elements in the French capital.⁴⁸) Another lifelong friend acquired at this time was the Oxford-educated W. J. Broderip (1789–1859), best known for his conchological cabinet, discovery of the Stonesfield 'opossum', and natural history vignettes. What we tend to forget is that throughout this period Broderip was by profession a magistrate at the Thames police court.⁴⁹

By contrast, Grant was a medical reformer who held naturalistic and secular views. In a sense he stood on the opposite side of the law, defending the attacks made by Thomas Wakley (1795–1862) on the abuses of the medical profession—attacks which had brought Wakley a stream of injunctions and libel suits (surely Sir Anthony Carlisle was referring to Wakley's actions when, in a letter to Owen, he talked of the 'active malevolence of ignorant savages'?⁵⁰). In its first ten years that scandalous

⁴⁷ His recommendations came from Brewster, Jameson, Barclay, and Fleming. On the 'anti-Edinburgh spirit' of the London quinquarians see J. Fleming, *The Lithology of Edinburgh*, Edinburgh, 1859, p. 73.

⁴⁸ David Pollock to unknown correspondent, 26 July 1831, Royal College of Surgeons MS. Cab. VIII (1)a75; see also Owen's *Life*, op. cit. (10), i, 42–3.

⁴⁹ *D.N.B.* Justice Broderip was a frequent visitor to the Owen household. He contributed to the *Zoological Journal* and was a founding member of the Zoological Society.

⁵⁰ Owen, op. cit. (10), i, 86. Carlisle was referring to attacks on his Hunterian Oration, which Wakley had mocked in *The Lancet*, 1826, 9, 689–93.

organ of medical reform *The Lancet* (founded by Wakley in 1823 with Lawrence and working-class champion William Cobbett) kept up an almost constant tirade against the 'self-perpetuating, tyrannical council of the college of surgeons' engaged in its 'sordid vocation',⁵¹ and he campaigned vigorously for institutional reforms to curtail its monopolistic practices. Wakley enjoyed fair support judging by rising circulation figures and the public subscriptions taken out to pay his court damages. Nonetheless his libellous attacks in the name of reform and a 'free medical press' brought him into headlong conflict with Owen's superiors at the College, particularly Sir Astley Cooper (1768–1841), Carlisle (1768–1840), and Abernethy, who sought an injunction to prevent Wakley pirating and publishing his College lectures (this was denied by the Lord Chancellor, in what was taken as a victory for the radical press⁵²).

Owen was employed by the College of Surgeons; he owed his position to Abernethy, and was encouraged and supported by Astley Cooper and Carlisle (Presidents of the College in 1827 and 1828 respectively, and surgeons to George IV). Prudence if nothing else would have dictated his condemnation of *The Lancet's* scurrilous attacks; anything less would have been incompatible with his position in the College and his friendship with the gentlemen of the Inns of Court. Now, Wakley printed Grant's sixty lectures on comparative anatomy, which 'brilliant course' he considered the best of its kind in England.⁵³ Grant in turn praised *The Lancet's* editor in class as 'a castigatōr of evil-doers'.⁵⁴ For this he was roundly attacked in the conservative *London Medical Gazette* (founded in 1827 to counter *The Lancet's* pernicious radicalism and unite 'those members of our profession who have its respectability at heart'⁵⁵). This condemnation stemmed partly from Grant's recommending a radical print to his *pupils*. The *Gazette* critic equated 'radical' with 'criminal' and asked what portion of Wakley's career, 'from his first attracting attention, in consequence of the destruction of his house by a still undetected incendiary, to his *fourth* conviction for

⁵¹ 'Address', *The Lancet*, 1830–1, 1, 1–5 (4).

⁵² See *The Lancet* 1823–33 *passim* for the constant mud-slinging. Wakley often took stock in the addresses prefixed to the journal: see esp. *The Lancet*, 1828–9, 1, 1–7; 1829–30, 1, 1–5. In 1835 Wakley entered the House of Commons as Radical M.P. for Finsbury; though a 'representative of labour' and in favour of the ballot (he was one of those present at the drawing up of the Charter) he was not a Chartist himself, but believed that changes should be brought about by parliamentary means: S. Squire Sprigge, *The life and times of Thomas Wakley*, London, 1899, 253–61, 312–5. Charles Brook, *Battling surgeon*, Glasgow, 1945, deals extensively with Wakley's radicalism.

⁵³ *The Lancet*, 1835–6, 1, 586.

⁵⁴ *The Lancet*, 1833–4, 1, 279. For Grant's own democratic, socially-levelling attack on the nepotistic, elitist Council of the College of Surgeons, during which he slated Owen's Hunterian Museum as an 'impediment' to the progress of liberal opinion, see R. E. Grant, *On the present state of the medical profession in England*, London, 1841, esp. pp. 50–1.

⁵⁵ The object and aims of the *London Medical Gazette* were spelt out in its first number: 1828, 1, 1–3. The *Gazette* similarly denounced the Professor of Midwifery, D. D. Davis, for shamelessly inviting Wakley to a London University *soirée* (where, to the *Gazette's* unutterable disgust, he was warmly received), and for supporting Wakley's call for a non-monopolistic College of Medicine to rival the College of Physicians and College of Surgeons: *London Medical Gazette*, 1830–1, 7, 372–3, 792–3; 1831, 8, 21–3, 218.

libel in a court of justice', Grant set as a standard for his students.⁵⁶ Grant was approving a journal which dealt in slander, libel, and blasphemy. The *Gazette* conceded that Grant was a 'good comparative anatomist and a learned naturalist' but warned him that he could only injure himself by siding with the violators of public decency.⁵⁷ However Grant and Wakley were well matched ideologically—both were vehement anti-monopolists, and both had a cruel wit when it came to Christianity. While Wakley made sarcastic comments on Jonah's residence in the whale or the miracle of loaves and fishes, Grant at the Godless College teased his students with 'satirical references to Providence'.⁵⁸

On any one of these counts, Owen's continuing friendship with Grant was impossible. Moreover, Grant's transformism (which Owen pointed to in the *Lancet* lectures⁵⁹) was a function of his mechanistic and reductionist philosophy. In the 1830s he was committed to a physicochemical explanation for all vital phenomena, whatever the practical difficulty caused by the complexity of the animal machine.⁶⁰ He sought no further than unity of composition and the laws of animal development for organic explanation, failed to place this in a natural theological context, and refused to consider man as anything but the highest animal—unlike Owen, failing to treat him as the seat of a responsible soul.

Given this secular streak in the British reform movement, Owen's sensitivity can be imagined. He was never as latitudinarian as, say, Baden Powell, and was somewhere to the right of Charles Babbage. In the 1830s he used comparative anatomy to elucidate man's relationship to the Deity,⁶¹ and only in the 1840s did he make a liberal appeal to law. Even then he was careful to interpret secondary causes as Divine 'ministers', while utterly repudiating the Puseyites' charges of pantheism.⁶² As early as 1832, six months after returning from Paris, he was musing on the use of the term 'Nature' to signify God's Works, and the self-delusion in imagining that this subsumed the moral dimension, or that from 'Nature' alone we could advance to the First Cause.⁶³

⁵⁶ *London Medical Gazette*, 1833–4, 13, 292–3; and Grant's response in *The Lancet*, 1833–4, 1, 644–5.

⁵⁷ *London Medical Gazette*, 1834, 13, 675–7; the *Gazette* acknowledged Grant's scientific abilities on more than one occasion: reviewing the first part of his *Outlines of comparative anatomy* (issued in 1835), it thought he was 'perhaps the most competent person in England to write a manual on the subject': *London Medical Gazette*, 1834–5, 15, 809.

⁵⁸ R. J. Godlee, 'Thomas Wharton Jones', *British Journal of Ophthalmology*, 1921, 93, 145–181; on Wakley's 'blasphemy', *The Lancet*, 1824, 1, 305, and 1826, 9, 692–3. University College was early nicknamed the 'Godless College', see for example E. Forbes to R. Owen, 2 November 1846, BM(NH) MS., Owen Corres. vol 12, f. 308.

⁵⁹ R. Owen, 'Report on British fossil reptiles, Part II', *Report of the British Association for the Advancement of Science*, Plymouth, 1841, 60–204 (197).

⁶⁰ Desmond, op. cit. (17); see, for example, Grant's comments in *The Lancet*, 1833–4, 1, 198.

⁶¹ R. Owen, Hunterian lectures, 1837, in 'Manuscript Notes, and Synopses of Lectures', BM(NH) MS., OC. 38. f. 81. On Owen's theology see Desmond, op. cit. (7), ch. 2, and J. H. Brooke, 'The natural theology of the geologists: some theological strata', in L. J. Jordanova and R. S. Porter (eds.) *Images of the earth: essays in the history of the environmental sciences*, Chalfont St. Giles, 1979, pp. 39–64.

⁶² Brooke, op. cit. (7); Desmond, op. cit. (7), pp. 44–7.

⁶³ R. Owen, MS. Notebook 7 (Jan.–May 1832), f. 64, BM(NH).

Owen's awareness of Grant as a professional rival must have been heightened by the limited arrangements for the teaching of comparative anatomy in the 1830s. Neither was independently wealthy, nor earned a salary commensurate with the needs of a gentleman,⁶⁴ and they were forced to vie for additional Chairs, including Charles Bell's Chair of Physiology at the university in 1831 and the Fullerian Chair of Physiology at the Royal Institution in 1837.⁶⁵ But their lives intertwined at a more personal level at the Zoological Society. Here both were active in the early 1830s, especially in an administrative and research capacity. They served on the Council and various committees together, and both published in the *Proceedings*; and Grant delivered courses to the Fellows in 1833 and 1834.⁶⁶ Yet there were widespread policy disagreements over the running of the Society which split the Fellows in 1835. Grant was first removed from the Publications Committee in April of that year. Then in a blaze of publicity (which made the front page of *The Times*), he was balloted off the council itself in May. Some observers had misgivings about the procedures used to achieve this and talk of election rigging was not uncommon. Wakley in particular was furious with Owen for voting with the cabal to oust Grant.⁶⁷ After this, relations between Grant and Owen understandably cooled and had degenerated into open hostility by the early 1840s.⁶⁸

Clearly their estrangement was symptomatic of a fundamental ideological divide, and Owen was well aware of the social threat. Geoffroyan science had been appropriated by the radical movement, and Grant supported reformers like Wakley, who stood accused of blasphemy, libel, and social abandon. Owen's academic position and his role as an aspiring Anglican anatomist, canvassing for patrons among the medical and legal elite of Lincoln's Inn, made any concession to the extremists impossible. This professional rivalry and his awareness of the strengths of a bestialising Lamarckism made a damning refutation all the more urgent.

⁶⁴ Grant earned only £117 p.a. on average from student fees throughout the 1830s (calculated from Professors' Fees Books MSS., UCL) after the withdrawal of his 'guarantee money' in 1831. Owen fared rather better; his salary was raised from £150 p.a. plus £50 remuneration (Royal College of Surgeons MS. 275 [18]h7) to £300 in 1833. But he still considered this inadequate to marry Caroline Clift (they had been engaged since 1827), and they only married in 1835 after Owen had been provided with premises above the Hunterian Museum.

⁶⁵ Desmond, *op. cit.* (17).

⁶⁶ Zoological Society Minutes of Council MS., vols 2 and 3, *passim*.

⁶⁷ *The Lancet*, 1836–7, 1, 766; for the President's defence of this action, see the *Statement by the President and certain members of the Council of the Zoological Society, in reply to observations and charges made by Colonel Sykes and others, at the General Meeting of the Society, on the 29th of April last, and at the monthly meeting on the 2nd of the same month*, London, 1835. The rumpus was reported in *The Times*, 29 May 1835, p. 1.

⁶⁸ It did not help that a box of fossils from Tasmania destined for Grant in 1841 somehow became relabelled and ended up with Owen at Lincoln's Inn. Despite protestations from Owen, Grant satisfactorily proved that a switch had occurred, see his letters to C. C. Atkinson on this throughout April and May 1841, UCL College Correspondence. As others have noted, angry exchanges took place between Grant and Owen at the Geological Society at this time over the interpretation of Koch's mastodon: Patsy A. Gerstner, 'Vertebrate paleontology: an early nineteenth-century transatlantic science', *Journal of the History of Biology*, 1970, 3, 137–48 (140–1); E. Cecil Curwen (ed.), *The journal of Gideon Mantell*, London, 1940, p. 159.

The question now is to determine the extent of Owen's scientific response and its success in countering the radical threat.

Owen's scientific strategem

Among contemporaries Geoffroy commonly evoked a mixture of deference and despair. When, at the height of his 'Paper War' with Owen in 1833,⁶⁹ Geoffroy wrote to Clift asking for information on *Ornithorhynchus*, the Irishman Joseph Pentland (1797–1873)—who actually had to work alongside Geoffroy at the Muséum—put in a covering note calling him a 'terrible *wrong head*', while conceding that 'his position as Professor at the Garden & President of the Royal Academy of Science for the present year, merit some considerations'.⁷⁰ Owen had seen Geoffroy at the Institute; Grant, as the obvious intermediary, might even have introduced them in 1831. But I have seen no evidence to suggest that their acquaintance was anything more than passing. Hence Owen could be more detached and critical in print. Actually he did accept a restricted 'unity of composition'—he even believed that in attempting to correct Geoffroy's abuses of this principle Cuvier had been led to underestimate the value of analogies (i.e. homologies).⁷¹ Nonetheless he was insistent that Geoffroy and his British followers had pushed the principle too far, and above all he was desperate to shake off its transformist connotations. In consequence, he adopted a number of anti-transformist ploys in the early to mid 1830s. His notebooks and published papers show that he focused on three areas, each important for the transformists' case: 1) the mammalian nature of monotremes (Geoffroy's word), and the vexed question of oviparity, 2) the ontogenetic development of the chimpanzee and its relationship to man, and 3) the fossil evidence for progressive development by transmutation.

On the first issue, travellers and anatomists were agreed that the platypus *Ornithorhynchus* was in some sense transitional. First described in 1799, the odd duck-billed animal was commonly assumed to be an edentate, although its reptilian features had been noted (particularly by Blainville) and from the first there were speculations on its oviparity. Others mooted an affinity with birds—Sir Everard Home (1756–1832) concluded in 1819 that ovarian development in the kangaroo, opossum and ovoviviparous platypus formed a 'beautiful series', and that the latter approached 'so nearly to the bird, as to complete the links of gradation between the human species and the feathered race'.⁷² But it was noticeably the transformists who actually separated monotremes into a distinct class. Admittedly Lamarck's belief, that this new class was stationed midway

⁶⁹ John Marshall mentioned the 'Paper War' in a letter to Owen, 7 April 1833, BM(NH) Ms., Owen Corres. vol. 19, f. 11.

⁷⁰ J. Pentland to W. Clift, 10 May 1833, BM(NH) MS., Owen Corres. vol. 21, f. 219.

⁷¹ R. Owen, Hunterian Lectures 1837, Royal College of Surgeons MS. 42.d.4, f. 96.

⁷² E. Home, 'On the ova of the different tribes of opossum and *Ornithorhynchus*', *Philosophical Transactions of the Royal Society*, 1819, 234–40 (234).

between mammals and birds, was no longer tenable by the 1820s, a number of anatomists, including Knox, having denied any deep-seated similarities between the bird and platypus.⁷³ But Lamarck also believed that the platypus was oviparous, and in this he was supported by Geoffroy, who placed monotremes between reptiles and mammals in a fifth vertebrate class. (He apparently told Grant in 1829 that *Ornithorhynchus* was 'a true *oviparous reptile*',⁷⁴ although in print that year he affirmed that it should be placed in a separate class.) He justified this on the grounds of a common cloacal opening, oviparity, and the lack of mammary glands. But by 1830 the last two points had become highly contentious. For direct evidence of oviparity, he had placed great stress on the description supplied by Grant of two eggs, which were said to have come from a platypus nest. However the eggs were suspiciously large, and even Grant was cautious, remarking on their resemblance to reptile eggs⁷⁵ (which is what they turned out to be).

On the other hand, field reports did tend to support Geoffroy. In particular, the Zoological Society of London heard from Lieutenant Lauderdale Maule of the New South Wales garrison, who had actually dug out nests and sorted pieces of egg-shell from the debris, and who had shot several females containing eggs the size of musket-balls.⁷⁶ But Owen consistently dismissed such evidence of egg-laying or incubation, and he continued to advance powerful anatomical and physiological arguments in favour of ovoviviparity.⁷⁷ Nor did he see the situation as changed when he detected an egg-tooth in the smaller of two nestlings shipped back by Maule.⁷⁸

This leaves an unfair impression of the strength of Owen's position between 1832–4, which really rested on his demonstration of the existence of mammary glands in monotremes. The tiny, simple-structured glands that Meckel had detected and interpreted as mammary, Geoffroy thought were analogous to the mucus-secreting odoriferous glands in shrews, and

⁷³ J.-B.-P.-A. Lamarck, *Philosophie zoologique*, 2 vols., Paris, 1809, i, 145–6, 342; R. Knox, 'On the osseous, muscular, and nervous systems of the *Ornithorhynchus paradoxus*', *Memoirs of the Wernerian Natural History Society*, 1824, 5, 161–74 (172).

⁷⁴ See Grant's letter quoted in 'On the egg of *Ornithorhynchus*', *Edinburgh New Philosophical Journal*, 1830, 8, 149–51. Cf. Geoffroy's retention of the fifth class in 'Considérations sur des oeufs d'*Ornithorinque*, formant de nouveaux documens pour la question de la classification des Monotrèmes', *Annales des Sciences Naturelles*, 1829, 18, 157–64.

⁷⁵ Compare Grant's letter reproduced in Geoffroy, 'Considérations', *ibid.*, with his more cautious statement 'On the egg of *Ornithorhynchus*' in the note above.

⁷⁶ Maule's letter was read on 11 Sept., 1832: *Proceedings of the Committee of Science and Correspondence of the Zoological Society*, 1832, Pt. 2, 145–6; Owen dismissed Maule's evidence in the appendix to his 'On the mammary glands of the *Ornithorhynchus paradoxus*', *Philosophical Transactions of the Royal Society*, 1832, 517–38 (533–4).

⁷⁷ These were (1) lack of detectible shell-secreting membranes, (2) lack of sufficient yolk to enable the embryo to survive in the egg, (3), the narrowness of the pelvis, preventing a large egg from being laid, and (4) the presence of mammary glands, suggesting that milk substituted for the yolk: Owen, 'On the ova of the *Ornithorhynchus paradoxus*', *Philosophical Transactions of the Royal Society*, 1834, 555–66 (563–4).

⁷⁸ R. Owen, 'On the young of the *Ornithorhynchus paradoxus*, Blum.', *Transactions of the Zoological Society*, 1835, 1, 221–8 (224).

he designated them 'Monotrematous glands'.⁷⁹ What Owen did was to make a comparative study of five females in different stages of the sexual cycle and relate the size of the gland to ovarian function. Because the gland was largest after gestation, he argued that it was most likely a mammary gland.⁸⁰ From a functional point of view, the presence of this gland rendered a yolk-filled egg unnecessary, and this, so far as Owen was concerned, settled the question of the platypus's affinities. (Throughout he hinted that Geoffroy's new class must stand or fall with the outcome of studies on this gland.⁸¹)

Owen was awarded an F.R.S. partly for this work, and his success testified to the superiority and exclusivity of his material, which in turn reflected continuing colonial expansion in New South Wales. By now, collectors like George Bennett (1804–1893) were actually sailing to the colony with equipment supplied by the College of Surgeons in order to ship Owen the necessary specimens. (Bennett's crates began arriving back at the College in July 1833.⁸²) Geoffroy was therefore forced increasingly to request information from the English, going through Grant, Clift, or the Secretary of the Zoological Society.⁸³ His requests were always courteously complied with, and he was supplied drawings and transcripts of colonists' letters; but, with the exception of one of Maule's nestlings being given to him, the material for dissection generally remained the property of the College or Zoological Society. Hence Geoffroy's difficulty in answering Owen with anything like enough detail, and his resorting to special pleading.⁸⁴

No one raised the issue of transformism during the debate, although Owen was dealing with known transformists, and Geoffroy's new class had a direct bearing on the taxonomic gradualism which was so important for transformist theories. Only when faced with the spectre of the transmuted human did he bring the subject into the open. Like Corsi, I believe that the threat in this area was far more real than is generally imagined. It might

⁷⁹ Geoffroy Saint-Hilaire, 'Sur un appareil glanduleux récemment découvert en Allemagne dans l'Ornithorhynque, situé sur les flancs de la région abdominale et faussement considéré comme une glande mammaire', *Annales des Sciences Naturelles*, 1826, 9, 457–60. Owen's parries and Geoffroy's ripostes are reported in *Proceedings of the Zoological Society*, 1833, Pt. 1, 28–31, 91–6.

⁸⁰ Owen, op. cit. (76), 530–1.

⁸¹ R. Owen ['preparation of the mammary gland of *Echidna Hystrix*, Cuv.'], *Proceedings of the Committee of Science and Correspondence of the Zoological Society*, 1832, Pt. 2, 179–81; Owen, op. cit. (76), 517; op. cit. (77), 555.

⁸² Bennett's letters beginning February 1833 from Sydney, as well as a list of his specimens with comments by Owen, are located in Royal College of Surgeons MS. Cab. VIII (1)b.L. See also the various letters from Bennett to Owen at BM(NH) MS., Owen Corres. vol. 3, ff. 252–371, vol. 4, ff. 1–54. The College of Surgeons awarded Bennett an honorary Gold Medal in recognition of the value of his shipments.

⁸³ Grant's letter to Geoffroy, op. cit. (74); *Proceedings of the Zoological Society* 1833, Pt. 1, 15–6; Geoffroy to W. Clift, 9 May 1833, BM(NH) MS., Owen Corres. vol. 23, f. 42.

⁸⁴ *Proceedings of the Zoological Society*, 1833, Pt. 1, 15–6, 29, 91–2. Perhaps this explains why those with interests similar to Geoffroy's failed to back him; e.g. Grant in 1834 considered the platypus a mammal, while Blainville (whose animal chain might have benefitted from an intermediate class) came out on Owen's side.

have been a quarter of a century since Lamarck speculated on the transition of a quadrumanous animal into man,⁸⁵ but as recently as 1827 the materialist Bory de Saint-Vincent (1780–1846) had strengthened the transformists' position. Lamarck had believed that the 'Angolan orang' was considerably inferior to man in intelligence; Bory now pointed out the similarity of ape to human brains and the orang's cultural adaptability, and he questioned Frédéric Cuvier's wisdom in denying apes reason. He also speculated that they only failed to speak because they lacked proper vocal apparatus—and given that, they might even appear superior to a Hottentot. And while Lamarck only mooted that new habits could transform ape into man, Bory actually instanced the case of resin-gathering peasants from Marenin who habitually climbed trees and as a result had acquired remarkably dextrous toes, to the extent that they could write with them.⁸⁶ As he commented, a toe which functioned as a hand was the main anatomical feature supposed to differentiate ape from man. Bory concluded that vanity drove us to place oranges with the 'stupid brutes' while elevating ourselves to a dignified position—an argument hardly appreciated by Lyell and Owen who were expressly defending the 'nobility' of man.

More evidence to support the transformists' case was found in the facial angle. Geoffroy in 1812 had established three genera of 'orangs': *Troglodytes* (chimpanzee), *Pithecus* (orang and gibbon), and *Pongo* ('Wurmb's Ape'). The latter was named after the first adult orang, killed in Borneo about 1780 and described by Baron von Wurmb of the Dutch East India Company. It seemed a more 'brutal' ape with protruding muzzle and smaller brain; thus it was classified separately from immature oranges, with human-like physiognomy. In Geoffroy's sequence the ape increasingly resembled man: Wurmb's ape had a facial angle of 30°, whereas *Troglodytes* had a facial angle of 50°, i.e. it was an immature chimpanzee with a flatter face.⁸⁷ Although by the later 1820s both Bory and Georges Cuvier had established that *Pithecus* and Wurmb's ape were merely age variants, still the graduated series *Pithecus-Troglodytes-Homo* was maintained because the chimpanzees were only known from young specimens. Hence Pierre-André Latreille in *Familles Naturelles du Règne Animal* (1825) listed the apes in terms of decreasing facial angle, while Bory in 1827 pointed out that the angle was only marginally greater in Hottentots than chimpanzees.⁸⁸

⁸⁵ Lamarck, op. cit. (73), i, 249–57; but remember that Lamarck's book was reissued in 1830.

⁸⁶ Jean Baptiste Bory de Saint-Vincent, 'Orang', *Dictionnaire Classique d'Histoire Naturelle*, Paris, 1827, xii, 261–285 (264–7). Corsi, op. cit. (5), 228–9, discusses Bory.

⁸⁷ Geoffroy Saint-Hilaire, 'Tableau des Quadrumanes', *Annales du Muséum d'Histoire Naturelle*, 1812, 19, 85–122 (87–9).

⁸⁸ Bory, op. cit. (86), 268; P.-A. Latreille, *Familles naturelles du règne animal*, Paris, 1825, pp. 43–4. Lyell was aware of the problem. In reference to Lamarck's theory, he discussed Camper's facial angle and its gradation from dog to man. But Lyell simply dismissed any parallel 'graduated scale of intelligence' as 'visionary speculation', particularly since ape intelligence had been exaggerated 'at the expense of the dog'. C. Lyell, *Principles of geology*, London, 1832, ii, 60–1.

It was this muzzle shortening from ape to man that Lamarck had sought to explain naturally in *Philosophie Zoologique* by means of changing dietary habits. But Owen claimed that the facial angle had lent undue support to ‘theories of animal development’,⁸⁹ and he now attempted to knock the middle rung out of the sequence; in short, to show that the *mature* chimpanzee also had a bestial physiognomy and was as distant from man as was Wurmb’s ape. He began by devising strategies in his notebook, working out how best to present his evidence. The egghead appearance of the human infant, he jotted, ‘would afford a very erroneous notion of the endowments of Man . . .’. He worked up this theme:

If a Statuary or Phrenologist were presented with a human skull having the proportions of that of a child before the shedding of the ^{milk} temporary teeth; the one would recognize in it the exaggerated ^{front} proportion & facial angle of a demigod ^{an antique} and the other predict from its undue cerebral development the intellectual powers of an Aristotle or a Bacon.⁹⁰

Chimpanzee endowments had been exaggerated for the same reason; knowledge of only the big-brained young had misled classifiers. Owen in print now described the osteology of the first adult specimen, supporting his anti-Lamarckian case by emphasising its bestial physiognomy—the prognathous skull with large canines, and broad expanses of crests to accommodate powerful chewing muscles. For purposes of classification, he now demanded the total irrelevance of immature ape anatomy and a revision of contemporary ideas about the gradualness of ‘transition of the *Monkey* to the *Man*’.⁹¹ He rounded off with a vigorous refutation of Lamarck and Geoffroy, and repudiated Bory’s notions by demonstrating that the ape’s toe musculature was designed for grasping, which precluded its ‘transmutation into a higher race of beings’.

But whether there were ‘impassable generic distinctions between *Man* and the *Ape*’, as Owen claimed, really depended on how one chose to interpret the evidence. The following year, for instance, Geoffroy conceded that the orang’s cranial development did result in a ‘bestialité révoltante’. But as a transformist he failed to share Owen’s sense of loss of human dignity at the prospect of a close relationship. Since indeed he interpreted these allometric changes as a case of ‘successive development in a single

⁸⁹ R. Owen, ‘On the osteology of the Chimpanzee and Orang Utan’, *Transactions of the Zoological Society*, 1835, 1, 343–79 (343).

⁹⁰ Owen, MS. notebook 11 (1834–6), f.87, BM(NH).

⁹¹ Owen, op. cit. (89), 343–4, 354–5, 370–2. Later, he actually likened the change to a ‘metamorphosis’. Owen, ‘Osteological contributions to the natural history of the Chimpanzee (Troglodytes, Geoffroy), including the description of the skull of a large species (Troglodytes Gorilla, Savage) discovered by Thomas S. Savage, M.D., in the Gaboon country, West Africa’, *Transactions of the Zoological Society*, 1849, 3, 381–422 (415).

species' he might actually have drawn pro-transformist deductions from the same facts.⁹²

Owen's response to this moral threat was appreciated by the scientific clerisy, in particular the influential men of the Cambridge network now dominating the British Association (founded 1831)—and it was after being encouraged by the Association managers that Owen presented his palaeontological strategy. Jack Morrell and Arnold Thackray have discussed the Oxbridge Anglican takeover of the Association and how, in the face of continued agitation for reform and 'fierce democracy',⁹³ the Oxbridge men had turned it to advantage as an organ of social stability. Owen closely identified with the gentlemen of the Association, and they in turn generously funded his projects—awarding him £200 in 1838 to draw up a Report on British Fossil Reptiles, and adding a further £250 on completion in 1841 for publication of the illustrations.⁹⁴ This massive vote of confidence made it politically expedient for Owen to present his most damning evidence against Lamarck at the forum provided by the Association.

What prompted Owen's move into palaeontology can again be gauged by a study of manuscript sources. I do not think that he was passively led to it by, say, Hunter's fossils, or that he was necessarily following a Paris–Edinburgh tradition, where comparative anatomy was closely linked to fossil zoology. Nor do I think that fear of foreign competition was wholly the cause, though there is evidence that it played a part. Despite the internationalist pretensions of the early Association, this body is better understood, in T. S. Traill's constitutional metaphor, as the 'Parliament of Science'.⁹⁵ As was the case at Westminster, so the Association was concerned with sovereignty—with British priority in the scientific field. Hence when the fossil fish collecting Tory MP for Chester Sir Philip Egerton (1806–1881) returned from a Continental tour worried by the omnivorous tendencies of German palaeontologists like Hermann von Meyer (1801–1869) and Count Münster (1776–1844), he advised Owen at the 1837 meeting to 'harvest' the British saurians before they were 'gathered by a Foreigner'.⁹⁶ But again the notebooks suggest that the

⁹² Geoffroy Saint-Hilaire, 'Considérations sur les singes les plus voisins de l'homme', *Comptes rendus de l'Académie des Sciences*, 1836, 2, 92–5 (94); and Geoffroy, 'Études sur l'Orang-Outang de la Ménagerie', *ibid.*, 1–8 (7). Incidentally, mention of 'human dignity' occurs frequently in Geoffroy's papers on apes, and his attempt to allay fears shows that the problem was not confined to the British alone.

⁹³ Morrell and Thackray, *op. cit.* (1), 302, also ch. I, and 245–56; on Owen's identification with the Association elite and ensuing patronage, pp. 217, 346, 492, 500. He acknowledged this aid in 'Report on British fossil reptiles', *Report of the British Association for the Advancement of Science*, Birmingham, 1839, 43–126 (43). On the Cambridge network see S. F. Cannon, *op. cit.* (2), 29–71.

⁹⁴ *Report BAAS*, Newcastle, 1838, xxviii; *ibid.*, Plymouth, 1841, xxii; the records show that the Geological Committee received £118 2s 9d in 1839 to further the Report: *ibid.*, Birmingham, 1839, xv.

⁹⁵ T. S. Traill, 'Address', *Report BAAS*, Liverpool, 1837, xxv–xlii (xlii); see also Roy MacLeod and Peter Collins (eds.), *The parliament of science: the British Association for the Advancement of Science*, Northwood, 1981.

⁹⁶ P. Egerton to Owen, 26 October 1840, BM(NH) MS., Owen Corres. vol. 11, f. 17. Of course, the national context and Britain's prestige were brought very much to the fore by the 1830s Declinist

primary instigating factor may have been less directly patriotic—or rather that Owen's higher nationalistic feelings were being mediated through anti-Lamarckian channels. It is difficult to escape the conclusion that his first note on the subject more precisely defined his real fears. During or shortly after November 1834 he jotted:

fact wholly at variance with every theory that would derive the race of Crocodiles from Ichthyosauri & Plesiosauri by any process of gradual transmutation or development.⁹⁷

He does not say what the 'fact' is; nor is Geoffroy named, but there can be no doubt that Owen has targeted Geoffroy's *Mémoires*, in which crocodiles are derived from teleosaurs, and these from ichthyosaurs. Geoffroy had placed his fossil sequence in an explicitly transformist context, and in his first paper on the Caen *Teleosaurus* in 1825 had praised Lamarck's two laws and recommended his *Philosophie Zoologique*. Between October 1830 and August 1831 Geoffroy read five memoirs to the Academy on the relationships of the new saurian, the fourth dealing with the environmental influence which could have affected the teleosaur's transformation. The fifth was actually delivered on 29 August 1831 when Owen and Grant were together in Paris.⁹⁸ Geoffroy did not lose interest in the subject. In 1836 he came to London, both to see Grant and search for teleosaur limbs in Oxford.⁹⁹ Nor was Geoffroy any quieter on the issue of transformism, for the following year, during a heated debate with Blainville over the Indian *Sivatherium*, he made his famous claim that the age of Cuvier was closing and belief in immutability was on the wane.¹⁰⁰

Owen's notebook and the conclusions to his 'Report on British Fossil Reptiles' suggest that he was reacting to Geoffroy's *Mémoires* of 1833 and Grant's *Lancet* lectures which advocated species 'metamorphosis'.¹⁰¹ Since Owen's ideological position was established before he began his tour of the major fossil collections around the country, he was consequently able to marshal evidence specifically to refute Geoffroy's ichthyosaur-teleosaur sequence. In fact the fabulous enaliosaur fossils of the Glastonbury Scripturalist Thomas Hawkins (1810–1889), some of the best fossils at

debate. That imperial geologist Roderick Murchison did extract a certain nationalistic capital from Owen's Report, see note 106 below. Murchison's imperialism is treated by James A. Secord, 'King of Siluria: Roderick Murchison and the imperial theme in nineteenth-century British geology', *Victorian Studies*, 1982, 25, 413–42.

⁹⁷ Owen, MS. notebook 11 (1834–6), f. 1, BM(NH).

⁹⁸ Geoffroy Saint-Hilaire, 'Divers mémoires sur de grands sauriens . . . Téléosaurus et Sténéosaurus', *Mémoires de l'Académie Royale des Sciences*, 1833, 12, 1–138; he praised Lamarck's laws in 'Recherches sur l'organisation des gavials', *Mémoires du Muséum d'Histoire Naturelle*, 1825, 12, 97–155 (150–1).

⁹⁹ That he came to see Grant is evident from his letter quoted in the 'biographical sketch', op. cit. (19), 691–2; although Bourdier notes that his destination was Oxford: F. Bourdier, 'Geoffroy Saint-Hilaire versus Cuvier: the campaign for paleontological evolution (1825–1838)', in C. J. Schneer (ed.), *Toward a history of geology*, Cambridge, Mass., 1969, 36–61 (55).

¹⁰⁰ Geoffroy Saint-Hilaire, 'Du Sivatherium de l'Himalaya', *Comptes Rendus de l'Académie des Sciences*, 1837, 4, 77–82 (77).

¹⁰¹ Owen, op. cit. (59), 197; and Grant in *The Lancet*, 1833–4, 2, 1001.

Owen's disposal, were probably unavailable to him until 1835, when the collection was unpacked and displayed at the British Museum.¹⁰² Owen began methodically collecting data late in 1837, and the size of his funding was only exceeded by the length of his 'Report', the two parts of which took a total of five hours to read to the Association. At Birmingham in 1839 he described eighteen new enaliosaurs, which put him in a commanding position to tackle the transformists on stratigraphic grounds. Thus, concluding at Plymouth in 1841, he reported that ichthyosaurs could be traced 'generation after generation, through the whole of the immense series of strata' without the slightest sign of mutation. A species 'quits the stage of existence [in the chalk] as suddenly as it entered it in the lias, and with every appreciable osteological character unchanged'. The 'metamorphosis' of ichthyosaur into teleosaur was anyway chronologically impossible because 'the Teleosaur ceases with the oolite, while the Ichthyosaur and Plesiosaur continue to co-exist to the deposition of the chalk'.¹⁰³ And as a *coup de grâce* he designed a quadrupedal dinosaur with a constitutive anti-Lamarckian component.¹⁰⁴

Owen was ideally placed to denounce radical-sympathising Lamarckian materialists. The Oxbridge dons and their London allies controlling the Association shunned radicals and utilitarians, and discouraged delegates from Mechanics' Institutes and progressives further left than Brougham. (Grant, not unexpectedly, preferred to spend his summers in Paris, refusing to attend Association meetings which, he told Mantell, did not 'conduce to the advancement of science'.¹⁰⁵) The privileged sector supported Owen. Sir Philip Egerton thought the reptile Report 'glorious', and Roderick Murchison was proud to see English masonry used to complete Cuvier's temple of nature. For denying nature any 'self-developing energies' Owen was also applauded. Lord Francis Egerton remarked that Owen's fossil animals were 'pregnant with the proofs of wisdom and omnipotence in their common Creator'.¹⁰⁶ So Owen gained immense respect at the Association—at Birmingham he actually received a standing ovation. And such was the success of this project that the managers in 1841 voted another £200 to enable him to start work on British fossil mammals.¹⁰⁷

¹⁰² The collection was bought for £1250 and lodged in the basement of the British Museum in November 1834; on 12 February 1835 Charles König reported that it had been unpacked: British Museum MS. 'Officers Reports', 1834, vol. 16, f. 3737; 1835, vol. 17, f. 3819. Owen mentioned the collections that he had visited in op. cit. (93), p. 44.

¹⁰³ Owen, op. cit. (59), 198–200.

¹⁰⁴ A. J. Desmond, 'Designing the dinosaur: Richard Owen's response to Robert Edmond Grant', *Isis*, 1979, 70, 224–34.

¹⁰⁵ R. E. Grant to Gideon Mantell, 16 July 1850, Mantell MS. papers 83, folder 44, Alexander Turnbull Library, Wellington, New Zealand.

¹⁰⁶ F. Egerton, 'Address', *Report BAAS*, Manchester, 1842, xxxi–xxxvi (xxxv); R. I. Murchison and Major E. Sabine, 'Address', *ibid.*, Glasgow, 1840, xxxv–xlvi (xl); Sir P. Egerton to R. Owen, 26 October 1840, BM(NH) MS., Owen Corres. vol. 11, f. 17.

¹⁰⁷ *Report BAAS*, Plymouth, 1841, xxii; Rev R. Owen, op. cit. (10), i, 185. Owen mentioned the audience being urged by the Chairman to rise to its feet to show its appreciation in a letter to Caroline Owen, n.d., BL Add. MS. 39955, f. 244b.

Anti-transmutation and new directions in comparative anatomy

Owen's repudiation of Lamarckian-radical thought no doubt endeared him to Peelite conservatives and to stauncher Tories hardened against Chartist agitation. But it would be unfortunate if we now acquiesced to a century old tradition and pictured his actions as obstructionist and unrewarding; or if we accepted current positivist historiography and accused Owen along with Herschel and Lyell of 'evading' what with hindsight turns out to have been the 'true' problem (that is, evolution).¹⁰⁸ On the contrary, the timing of events suggests that Owen's anti-transformism actually had a *positive* heuristic function, and that it acted as a catalyst to the development of newer, more sophisticated approaches to comparative anatomy in the 1840s.

A crucial aspect of Owen's science in the late 1830s and early 1840s was his advocacy of von Baerian embryology and denial of recapitulation. According to Ospovat, Owen was stimulated by Martin Barry's articles.¹⁰⁹ This may be true, but a study of Owen's Hunterian lectures of 1837 establishes that transcendental anatomy, embryology, and anti-transmutation were inextricably linked in his mind. More importantly, these lectures allow us to take the further step and ask *why* Owen was so receptive to von Baer. We have to remember that Grant was inspired by Geoffroy and Blainville, and rested his transformist case on three interrelated factors: a lineal progression of life from monad to man, a 'unity of composition' holding throughout the *entire* series, and a confirmatory recapitulation of the sequence during foetal development. Grant also echoed Geoffroy and Blainville on specifics: hence following Geoffroy's celebrated clash with Cuvier at the Academy over the 'analogical' relations of cephalopods and fishes, Grant in the *Lancet* lectures likewise bridged the gap between molluscs and lowly fishes, thus joining the two allegedly disparate classes.¹¹⁰ But then Grant was intent on finding universal taxonomic criteria (which could apply to the entire animal kingdom), suggesting that serial development and unity of plan were more important for him than Cuvier's *embranchements*.¹¹¹ This serial development was also reflected at an embryonic level. While Geoffroy experimented with monsters frozen at some recapitulated stage, Grant 'amused his audiences'¹¹² by informing them that their brains had once been those of

¹⁰⁸ Cf. Neal C. Gillespie, *Charles Darwin and the problem of creation*, Chicago, 1979, p. 31.

¹⁰⁹ Ospovat, *Development of Darwin's theory*, op. cit. (7), 130–2. Here Ospovat clearly elucidates the relationship that Owen conceived between embryology and unity of composition. He was evidently inspired by Martin Barry, 'On the unity of structure in the animal kingdom', *Edinburgh New Philosophical Journal*, 1836–7, 22, 116–41, 245–64. That other incipient von Baerian, W. B. Carpenter, was certainly influenced by this article. See W. B. Carpenter, 'On unity of function in organized beings', *ibid.*, 1837, 23, 92–114.

¹¹⁰ Geoffroy, op. cit. (14), *passim*. R. E. Grant, 'On the organs of support of the cephalopodous mollusca', *The Lancet*, 1833–4, 1, 505–14; 'On the development of the skeleton of the vertebrated classes, and on the osteology of fishes', *ibid.*, 537–46.

¹¹¹ [R. E. Grant], 'Baron Cuvier', *Foreign Review and Continental Miscellany*, 1830, 5, 342–80 (368).

¹¹² *London Medical Gazette*, 1833–4, 13, 927.

tadpoles, fish, and crocodiles; and he insisted that the same simple laws regulated foetal and fossil development.

The consequence of this foetal/fossil relationship must further have convinced Owen that the entire Grantian edifice was unsound. Grant's and Blainville's serial succession manifested not only in recapitulation and the animal chain, but, applied to the geological record, it generated a model of *linear* fossil development. Thus neither could accommodate 'anachronistic' forms, like the famous Stonesfield 'opossum'. It simply fell out of sequence; according to them, the Secondary strata should house nothing higher than reptiles, with mammals making a first appearance only in the Tertiary. They considered *Amphitherium*, as Blainville christened the fossil jaws, wrongly diagnosed and probably reptilian. The issue was forced to a head between December 1838 and January 1839 when Owen and the Oxford geologist William Buckland (1784–1856) contrived to out-manoeuvre Grant at the Geological Society and get the 'Botheratiotherian' fossils officially promoted to the rank of mammals, i.e. 'get the promotion gazetted in the Report of the Geol. Society'.¹¹³ In a series of letters they hatched plans to persuade a member of Grant's camp to recant publicly and swing the vote—targeting Grant's guest at the Society, the visiting Philadelphian palaeontologist Richard Harlan (1796–1843), who had brought over his own controversial fossil *Basilosaurus*. Buckland told Owen that, because of Grant's views, it was 'desirable for the sake of everybody both in London & Paris to put the Marsupial Character of the Stonesfield beasts beyond all doubt as speedily as possible',¹¹⁴ and when Owen did so at the Society Buckland invited Lord Brougham along to witness the result. In print Owen vindicated Buckland and Broderip and convincingly demonstrated that *Amphitherium* was a Secondary mammal, with the implication again that the record was not one of inexorable ascent.

While Owen attacked these apparent palaeontological consequences, it was clear that to demolish transmutation itself he would have to tackle its morphological and embryological underpinnings. We have seen that the case for transmutation largely rested on a taxonomic continuum, in fossil and foetal development as well as in the animal chain, and proof of this continuum lay in Grant's finding universal criteria to allow the 'unity of

¹¹³ W. Buckland to R. Owen, 4 January 1839, Royal College of Surgeons MS. (1)a/19. H. de Blainville, 'Doutes sur le prétendu didelphe fossile de Stonefield', *Comptes Rendus de l'Académie des Sciences*, 1838, 7, 402–18. R. E. Grant, *General view of the characters and the distribution of extinct animals*, London, 1839, pp. 7, 42–4, 54. On the politics of Blainville's position see Appel, op. cit. (18), and on his animal series J. Lessertisseur and F. K. Jouffroy, 'L'idée de série chez Blainville', *Revue d'Histoire des Sciences*, 1979, 22, 25–42. I have discussed the Stonesfield 'opossum' in two papers: 'Robert E. Grant's later views on organic development: the Swiney lectures on "Palaeontology", 1853–1857', *Archives of Natural History*, 1984, 11, 395–413; and 'Interpreting the origin of mammals: new approaches to the history of palaeontology', *Zoological Journal of the Linnean Society*, 1984, 82, 7–16.

¹¹⁴ W. Buckland to R. Owen, 11 December 1838, Royal College of Surgeons MS, (1)a/6; Buckland to Lord Brougham, 14 December 1838, UCL Brougham MSS. 1957. R. Owen, 'Observations on the fossils representing the *Thylacotherium Prevostii*, Valenciennes, with reference to the doubts of its mammalian and marsupial nature recently promulgated; and on the *Phascalotherium Bucklandi*', *Transactions of the Geological Society*, 1841 6, 47–65. On Harlan's role, see Gerstner op. cit. (68), 145–7.

composition' principle to stretch from monad to man. This is the point Owen chose to contend. In the 1837 lectures he slated transcendentalists for their excesses, accusing Geoffroy and his disciples of abusing an otherwise sound principle—insisting that only the 'truly philosophic inquiry' of von Baer and the German embryologists could provide the lasting foundation of 'organic affinities'.¹¹⁵ Now, von Baerian embryology helped in two ways: as Ospovat suggests, it was useful to Owen in setting strict limits to Geoffroy's 'unity'. Rather than recapitulate the entire series of inferior forms, each embryo in von Baer's scheme developed from the germ towards the characteristic organisation of its *embranchement*. Embryonic divergence therefore resulted in a characteristic archetype for that *embranchement*.¹¹⁶ Because unity of plan among molluscs and fishes was restricted to the primary or germ stage of foetal development—after which there was a fundamental divergence—it was impossible to conceive of a squid transmuting into a fish. So Owen had destroyed the transformists' continuum. But divergence also (and more directly) broke the recapitulatory crutch of transmutation—and in 1837 this was one reason why he found it so attractive. At this time he despaired of teachers who advanced such 'baseless speculations' as

that the Human Embryo *repeats in its development* the structure of any part of another animal; or that it *passes through the forms* of the lower classes;— . . . that a Fish is an overgrown Tadpole. Such propositions . . . imply that there exists in the Animal Sphere a scale of Structure differing *in degree* alone:—nay, they imply the possibility of an individual, at certain periods of its development, laying down its individuality, and assuming that of another Animal;—which would, in fact abolish its existence as a determinate concrete reality.¹¹⁷

Characterised in this way, recapitulation demanded a literal transmutation during foetal development, which repeated in microcosm the alleged 'metamorphoses' of fossil forms. Both obliterated individual existence, thus raising immense doctrinal problems; they also left morphological differences—even between man and beast—a mere matter of degree. The twin manifestations of transformism were indissolubly linked in Owen's mind. He carried on:

The doctrine of Transmutation of forms during the Embryonal phases is closely allied to that still more objectionable one, the transmutation of Species. Both propositions are crushed in an instant when disrobed of the figurative expressions in which they are often enveloped; and examined by the light of a severe logic.¹¹⁸

Owen's abhorrence of transmutation in its brutalising aspect therefore

¹¹⁵ R. Owen, Hunterian Lectures 3 and 4, May 6 and 9, 1837, Royal College of Surgeons MS 42.d.4, ff. 95–8. See also Lectures 1 and 2, May 2 and 4, 1837, BM(NH) MS. ff. 66–7.

¹¹⁶ Ospovat, *Development of Darwin's theory*, op. cit. (7), 130–2; Owen, *Lectures on the comparative anatomy and physiology of the invertebrate animals*, London, 1843, pp. 367–71.

¹¹⁷ Owen, Hunterian Lectures 3 and 4, op. cit. (115), ff. 97–8.

¹¹⁸ *Ibid.*

made von Baer's embryology very useful to him. With it he could limit Geoffroy's 'unity' principle and destroy recapitulation *in toto*, thus breaking Grant's and Geoffroy's morphological and foetal continuum and with it the backbone of the transmutationists' case.

So Owen's adoption of von Baerian embryology was inextricably related to his anti-Lamarckian campaign. And having accepted the divergence model, he found that he could apply it to the fossil record to generate an image of progressive specialisation away from the generalised archetype. In this way he could explain successive development without invoking any necessary fossil ascent. This subject has been fairly extensively studied recently,¹¹⁹ and rightly so, since Owen's morphological science was central to his sophisticated palaeontology of the early 1850s. And it was onto this new palaeontological model of divergence that Darwinism was to be mapped.¹²⁰

Conclusion

In this essay I have investigated some of the reasons why gentlemen devotees of science were so profoundly antipathetic to transmutation in the 1830s. It is not enough simply to identify the transformists, we have to comprehend the nature of the threat in moral, social, and political terms to appreciate the vehemence of the Anglican reaction. In line with this objective, I have attempted not only to follow the spread of transformism at home, but to consider it in relation to the radical movement. The degree to which social interests can shape esoteric scientific knowledge is still debated.¹²¹ Nevertheless one can make a good case for their operation in this instance. Given that Owen's anti-radicalism was fostered in a college under siege, and that his professional rival was a Lamarckian who had the support of reformers, we can better appreciate the meaning of Owen's morphological studies. His revamped monotremes, apes, and dinosaurs were not simply the product of disinterested application. Each was rebuilt to anti-Lamarckian specifications expressly to meet urgent social needs. His response to a dehumanising transformism was therefore part of the broader campaign against radical and materialist doctrines imported from Paris and spread at the 'Godless College' or through the pages of *The Lancet*; as such, it was loudly hailed by the scientific clerisy, alarmed at the threat to its power from democratic forces following the Reform Bill.

The patronage Owen received testified to the degree of establishment backing. It was no coincidence that his social elevation followed so closely on the success of his anti-transformist campaign. He was able to exploit his *entrée* into privileged Oxbridge society provided by friends like Buckland

¹¹⁹ See sources cited in op. cit. (7).

¹²⁰ Carpenter himself realised this: W. B. Carpenter, *Nature and man: essays scientific and philosophical*, London, 1888, p. 107. For a discussion see Desmond, op. cit. (7), *passim*.

¹²¹ A valuable discussion and useful bibliography is provided by Steven Shapin, 'History of science and its sociological reconstructions', *History of Science*, 1982, 20, 157–211.

and the Master of Trinity, William Whewell (both of whom were active in the Geological Society and British Association), using them to canvass for greater support and recognition. At Whewell's over Christmas 1841 Owen met the Chancellor of the Exchequer and Lord Brougham and 'represented to them my present anomalous position, holding a Cuvierian rank without the means of doing it justice.'¹²² A fortnight later he presented Buckland with plans for a definitive series of works on comparative anatomy which, he complained, would have to be shelved for want of funds. He portrayed this as the country's loss ('I am unwilling that England should lose the credit of producing that Work on Comparative Anatomy, which France & Germany have, as yet, failed in achieving'¹²³). And it was Buckland's subsequent representations to Sir Robert Peel—emphasising Owen's European reputation, Association backing, and 'sound & temperate' religious views¹²⁴—which resulted in Owen's appropriating the lion's share of the Civil List funds put aside for scientists in 1842, taking £200 of the available £300.¹²⁵ Within two years Owen was himself a guest at Drayton Manor, arranging for his portrait by Pickersgill to be hung alongside Cuvier's in Sir Robert's gallery.¹²⁶ At the same time, the Hunterian Professor was becoming powerful enough to dispense his own patronage, particularly if it promised a beneficial scientific return.¹²⁷

Finally, I might stress the advantage of viewing Owen's science in political terms. Like a Peelite conservative at the time of the *Tamworth Manifesto*, he sought to safeguard establishment interests by maintaining Established Church standards. Yet he too promoted a cautious and conserving type of reform; thus this same anti-Lamarckian ideology was crucial to the generation of his archetypal morphology and von Baerian palaeontology in the 1840s, and historians are agreed that these were among the most significant developments in pre-Darwinian comparative anatomy.

¹²² As Owen reported to Caroline Owen, 27 December [1841], BL Add. MS. 45927, f. 38.

¹²³ Owen to W. Buckland, 11 January 1842, BL Add. MS. 40499, f. 252.

¹²⁴ W. Buckland to Sir Robert Peel, 12 January 1842, BL Add. MS. 40499, f. 250.

¹²⁵ Sir Robert Peel to Owen, 1 November 1842, BL Add. MS. 40518, f. 24; Owen to Peel, 1 November 1842, BL Add. MS. 40518, f. 26; see also W. Whewell to Owen, 9 November 1842, BM(NH) MS., Owen Corres. vol. 26, f. 283. Roy MacLeod has discussed pensions in 'Science and the Civil List 1824–1914', *Technology & Society*, 1970, 6, 47–55; and I have mentioned Grant's failure to obtain a pension from the government even in 1854 in op. cit. (113).

¹²⁶ Owen to Buckland, 26 December [1844], BL Add. MS. 40556, f. 294; Buckland to Peel, 27 December 1844, BL Add. MS. 40556, f. 292.

¹²⁷ For example, he moved to obtain an F.R.S. for W. Brodie before the latter returned to New Zealand, telling Buckland that it 'may add to the great determination which he manifests to collect and transmit specimens & information from that colony'. Since Brodie had already armed Owen with a seventh *Dinornis* species, Owen was well aware of the advantages that could accrue from such an award: Owen to Buckland, 17 January 1845, BL Add. MS. 38091, f. 207; and 13 November 1844, BL Add. MS. 38091, f. 205.