Owen and Huxley: unfinished business

C.U.M. Smith

Thomas Henry Huxley’s famous put-down of Bishop Samuel Wilberforce at the 1860 Oxford meeting of the British Association for the Advancement of Science signalled a sea-change in British biology. The Darwinian interpretation as championed by Huxley overwhelmed the Coleridgean–Owenite view argued by Wilberforce. In this paper some of the loose ends left over from that encounter are considered. Was the Darwinian victory quite as complete as it has often seemed? Does the Owenite view still have something to commend it?

‘Then HUXLEY and OWEN
With rivalry glowing
With pen and ink rush to the scratch
‘Tis Brain versus Brain
‘Til one of them’s slain
By Jove! it will make a good match’

‘Monkeyana’, verse 8, Punch, XL, p. 206 (18 May 1861)

The 1860 meeting of the British Association for the Advancement of Science at Oxford has long been seen as a climactic point in the history of biology. It has spawned a rich mythology. The common perception is that it marked the final defeat of the Coleridgean–Owenite forces and the triumph of the Darwinist–Huxleyan ‘New Reformation’[1]. Biology has ever since proceeded, albeit with many a detour and diversion, along the road signposted by Charles Darwin and Thomas Henry Huxley. In an earlier paper[2] I discussed the complex of personal, professional and generational jealousies and conflicts out of which this decisive engagement was fashioned. I concluded that the episode provides an almost paradigmatic instance of the sensitivity of scientific thought to the general climate of ideas, which in turn reflects changes in social conditions. It does not, however, suggest that scientific ideas are fully determined by social forces and to that extent it supports ‘internalist’ readings. Rather, it suggests that social forces can block or canalise the flow of scientific ‘advance’, especially the interpretation of scientific observation. Schutz writes of ‘Realfaktoren’ (by which he means a complex of social factors) ‘which open and close, so to speak, the sluice gates, through which the stream of Idealfaktoren have to pass’[3]. In the first part of the century social conditions in the metropolis favoured the Germano-Coleridgean interpretation and allowed the Owenite analysis; in the second part Benthamite views gained the ascendancy and the Darwin–Huxley analysis won through.

The controversy in mid-century between Huxley and Owen was no dry-as-dust academic dispute over the details of neuro-anatomy. It was proxy for a far deeper and more dangerous engagement. The stand-off reflected two very different readings of the nature of things. John Stuart Mill once declared that there were only two coherent intellectual positions in the first half of the nineteenth century, that occupied by Coleridge and that occupied by Jeremy Bentham: ‘Coleridge used to say that every-one is born either a Platonist or an Aristotelian: it may be similarly affirmed, that every Englishman of the present day is by implication either a Benthamite or a Coleridgean’[4]. These are evidently very deep seated positions. Attempts have been made to relate them to social context. Farrington[5] argued that the Socratic–Platonic endeavour of Greek antiquity was a response to the social turmoil of the time, an endeavour to show that common ideas of the ‘good’, the ‘true’, etc. existed in all Athenians, would they but stop and think, and hence social cohesion was possible. Similarly, Coleridge, looking at the social and political upheavals of his time, traced them all to a deep root in Lockian philosophy: ‘the barren and worse thah been fig-tree of the mechanic philosophy’[6], and similarly turned to an otherworldly (in his case theological) remedy. He saw the incipient atomism of the new chemistry and the self-regarding individualism of early capitalism as two sides of the same coin: both denials of the larger vision, both assertions of what he sardonically called ‘psilosophy’ (which we might translate as reductionism) rather than the encompassing vision of philosophy. In both cases the underlying impulse can plausibly be seen as conservative: an attempt to give intellectual support to the old order.

The defeat of the Coleridgean–Owenite view was not final. It has ever since lived a somewhat subterranean existence, and still obtrudes at intervals nearly a century and half later. Although it would take up far too much space to trace this underground history in any detail I want in what follows to look at a small number of salient issues.

How different are humans from other animals?

Richard Owen was lauded by Alexander von Humboldt as ‘the greatest anatomist of his century’. His early task of putting the Hunterian collection in order gave him an unparalleled experience of both invertebrate and vertebrate. His output of books and papers on comparative anatomy was prodigious. The place of Man in Nature was as crucial to him as it was to Darwin and Huxley. By the middle of the 19th century he had developed a classificatory scheme, based on the brain, which placed humans apart in a separate mammalian subclass. The human brain, he wrote in a paper he contributed to Proceedings of the Linnean Society in 1857, ‘presents an ascensive step in development, higher and more strongly marked than that by which the preceding subclass was distinguished from the one below it … I am led to regard the genus Homo, as not merely representative of a distinct order, but of a distinct subclass of Mammalia, for which I propose the name ARCHENCEPHALA’[7]. This rhetoric, though no doubt pleasing (and appeasing) the conservative establishment, attracted the attention of the 32-year old Huxley: a generation younger than Owen, and casting around for an issue on which to make his reputation. The story is well known. Having been appointed to the Royal School of Mines at South Kensington to teach biological science he felt that one of the most pressing considerations was, precisely, to fit human beings into the great scheme of zoological classification. Hence when Owen’s paper was published in 1857 he read it avidly and immediately ‘set to work to re-investigate the subject’[8].

Huxley published his detailed rebuttal of Owen’s neuroanatomy in a paper he

C.U.M. Smith

Is currently Honorary Visiting Fellow in the Department of Vision Sciences, Aston University, Birmingham, UK B4 7ET and President of the International Society for the History of the Neurosciences. He is the author of many books and papers in neuroscience and in the history and philosophy of biology, especially neurobiology. He is currently working on a text on the biology of sensory systems and journal publications on Descartes, Huxley, Coleridge and J.Z. Young.
published in the first volume of the Natural History Review in 1861. The human brain was (he said) not, as Owen asserted, distinctively different from those of the higher quadrumana; the differences were (according to Huxley) "of no more than generic value". This provided the ground on which Huxley attacked Owen at the 1860 meeting. Owen, however, was not impressed. He repeated his neuroanatomical classification of the mammals almost word for word in the 1866 second volume of Anatomy of the Vertebrates. In volume 3 he included a large and fully illustrated section comparing primate brains and makes the point that the human brain, even when that of a foetus, is distinctively different. Nevertheless, he goes on, his opponents (he does not give Huxley the satisfaction of naming him) are quite wrong to accuse him of believing in 'special creation'. He is quite prepared to accept that primordia of the features by which he characterises the Archecephala can be seen in the other quadrumana. In particular he notes that beginning traces of the posthippocampal fissure can be seen in the infrahuman primates. It was merely the increased depth of this fissure which, pushing inwards into the posterior horn of the lateral ventricles, produced the so-called (but according to Owen definitive) 'hippocampus minor' in humans.

Owen pointed out, moreover, with some ascerbity, that he was never a supporter of special creation: 'Seven years before Darwin he writes 'I have expressed my opinion in favour of the theory of the progressive development of the species ...'". His argument with the Darwin–Huxley camp had to do not with the fact of evolution but the forces which brought it about. He did not believe in a single unique creation event but rather in the continuous creation of the most primitive living forms. He did not accept that Pasteur had disproved 'generatio spontana'. He thought that Pasteur's disinfectant procedures merely killed the living primordia which were floating with minute dust particles in the atmosphere. If there had been just one instance of 'creation' of 'a few forms or ... one', as Darwin puts it at the end of the Origin, why, he asks, have they not evolved into 'more perfect forms'? "Owen had a very dynamic view of the living world. Primordial organised forms were being continually formed at the base of the living world (so to speak) and evolving ineluctably to more complex, more perfect forms.

Owen had other arguments against the Darwinian theory. He believed that it 'personified' nature 'attributing (to natural selection) something which can only be predicated of intelligence' (Anatomy of the Vertebrates, vol. 3, p. 794). He took as his example the evolution of the modern horse's hoof from that of the three-toed Palaeotherium. He refused to believe that environments were different in the past than they are today. He argued that there was just as much marhsy, soft, ground then as now. Is it not, he asked, a 'personification of nature' to say that it selected the 'mid-hoof and rejected the others'? "One's surprise" he goes on 'is that "tropes" and "personified acts" should not have died out, as explanatory devices, with the "archaeus faber", the "nissus formativus" and other self-deceiving, world-beguiling, simulacra of science, with the last century". His model is always that of epigenesis. Like Teilhard de Chardin he sees a complexity increasing force at work throughout the animate world. In his more inspired moments he writes of a "predetermining Will", producing them in reference to a final purpose.

One of Owen's most telling examples of his evolutionary epigenesis makes use of that most Darwinian image, the coral atoll. 'What spectacle' writes Owen towards the end of the third volume of his Anatomy of the Vertebrates (pp. 804–807) 'can be more beautiful, striking and suggestive than the corals move from simple to complex forms'. Lamark's 'apetency' fails. Corals are too low in the scale to 'want to do something'. Darwin's mechanism also fails: there has been no change in the environment. There is only one other possibility: '. ...an innate tendency to deviate from the parental type, operating through periods of adequate duration ...'

Reading Owen's huge corpus of published work together with his unpublished notebooks and lecture notes brings home the immense quantity of work he got through in the 1830s, 1840s and 1850s. Year after year he presented detailed sequences of lectures on comparative anatomy to critical audiences at the Royal College of Surgeons. He fully justified Humboldt's encomium mentioned above. His Anatomy of the Vertebrates, it has authoritatively been said, was 'the result of more personal research than any similar work since Cuvier's Leçons d'anatomie comparée'. Hence he must have felt some what piqued at the huge interest engendered by The Origin of Species. His bitterness at being upstaged comes through in his review in the Edinburgh Review (April, 1860). In his unpublished notes he is less scornful, saying in some places that he sees evidence for the smooth sequence of small steps which Darwin believed accounted for evolution whilst in other places he argues for a more salutary account. Returning to the evolution of the horse's hoof [and here he must have felt himself an expert as he had closely studied equine palaeontology and had, in fact, introduced the still used division of the ungulates into the perissodactyls (odd toed) and artiodactyals (even toed)] he points out that 'a modern horse occasionally comes into the world with supplementary ancestral hooves'. "The mother of a "monstrous" tridactyl colt' he continues 'might repeat the anomaly and bring forth a tridactyl filly'. He instances a San Salvador family containing a dwarf, mentally retarded, brother and sister who were fasted to be exhibited as 'Arctees'. Might not something similar happen with tridactyl equines and bring back 'the race of the hipporions'? Might this not be a better model for the appearance of the single toed hoof of the modern horse? Species might originate, he writes, '. ...independently of external influence; change of structure precede that of use and habit; appetancy, impulse, ambient medium, fortuitous fitness, personified "selecting Nature", would have no share in the transmutative act'. We are here in the vicinity of Goldschmidt's 'hopeful monsters'. Salutary evolution of this type has never seemed very likely. Nevertheless, the recent great accession of interest in homoeotic mutations, albeit in fruitflies, suggests that alterations to genes controlling developmental processes are capable of inducing huge changes in the adult. Perhaps we might say that the jury is still out here also. Owen, of course, wanted to make use of this salutary evolution to account for the origin of Homo sapiens. It is no accident that the third volume of his immense and immensely erudite Anatomy of the Vertebrates ends abruptly and without comment with a figure showing the teeth and limbs of 'Paleotherium, Hippopan, Horse'. Thus, although the neuroanatomical dispute at the 1860 meeting and after was arcane, and the difference in interpretation seemingly insignificant, large issues turned upon it. By arguing that Owen's neuroanatomy was faulty Huxley found a way to undermine a major obstacle to acceptance of the Darwinian world-view. He was able to argue that Owen's immense reputation as an anatomist, a reputation which could be used to belittle the views of the more 'amateur' natural historian of Down House, was not unchallengeable. Owen was not able to recover. Indeed his papers suggest he was half convinced. Darwin's theory of external forces, abiotic and biotic, the struggle for existence, carried all before it. Yet an epigenetic model, shorn of its theistic baggage and teleological implications, is still an option favoured by some theoreticians. It remains as unfinished business. Salutary evolution, if not in the form of Goldschmidt's 'hopeful monsters', then as the now popular concept of punctuated equilibrium remains a possibility.

Mind in Nature

Whereas Darwin, after early struggles recorded in his notebooks, turned away from the implications of his theory for the problem of mind,excusing himself by saying that he lacked 'a metaphysical head', Huxley was sufficiently interested in philosophical issues (he had, after all, published essays on Descartes and Berkeley and a full-scale study of David Hume) to take up the challenge. For, like his ecclesiastical adversary, Bishop Wilberforce, he saw the
moral issue raised by Darwinism very clearly. If infrahuman animals were automata, as Descartes implied (and Huxley was a convinced Cartesian18 and had written a famous essay entitled ‘On Animal Automatism’), and if humanity had evolved without step-function from the animals, as Darwin maintained, then what became of what is classically described as ‘freedom of the will’ without which moral action is impossible? Huxley returned to this issue again and again, as have his like minded successors in British neurophysiology: C.S. Sherrington and J.C. Eccles. Sherrington, it will be recalled, was sufficiently exercised by the problem of free-will in a Newtonian Universe to write in his major philosophical work, Man on his Nature, that he was disposed to believe that ‘the theoretically impossible happens’19 and Eccles, Sherrington’s pupil, devoted the latter part of his life to what many see as a futile attempt to provide a space for free will by applying the indeterminism built into quan tum theory to the operation of the synapses20.

Huxley’s solution to the dilemma, and the charge of materialism, was to develop a form of absolute idealism. He writes in his essay ‘On the Physical Basis of Life’ (p. 160) that both ‘matter’ and ‘spirit’ are ‘unknown and hypothetical causes, or conditions, for states of consciousness’ and he says much the same in other places, for example in his essay on Descartes’ Discourse on Method (p. 193) he writes ‘matter’ and ‘force’ are, as far as we can know, mere names for certain forms of consciousness ... legitimate materialism is neither more nor less than a sort of shorthand Idealism’. He concludes (p. 245) that he is ‘utterly incapable of conceiving the existence of matter if there is no mind in which to picture its existence’21. This position is very close, if not identical, to that which Herbert Spencer had developed in his pioneering evolutionary psychology (first edition 1855, but more importantly, second edition 1870, 1872)22.

That Huxley’s philosophical position was closely similar to that of Spencer is not surprising. Both were members of the highly exclusive X club of London intellectuals which held some 240 meetings over its thirty years of existence. Huxley read the proofs of Spencer’s First Principles (the first volume of the vast Synthetic Philosophy) and his papers at Imperial College contain over eighty letters written to the philosopher. Huxley writes (and in more than one place) that ‘if we were obliged to choose between absolute materialism and absolute idealism, I should feel compelled to accept the latter alternative’ and Spencer writes, similarly, though more ponderously, that ‘Were we compelled to choose between the alternative of translating mental phenomena into physical phenomena, or of translating physical phenomena into mental phenomena the latter would seem the more acceptable of the two’23.

Owen, too, recognized the problem which the presence of mind posed for the evolutionist. The first of his 1842 Hunterian Lectures extends over 62 pages of handwritten manuscript and gives a thorough treatment of the issue. He wrestles with the same (perennial) problems that troubled Darwin and later Huxley. ‘We all know’ he writes ‘what in ourselves it is to feel’. But in others we can only infer feelings from behavioural signs. He asks the same question that Darwin asked in his notebooks: how far ‘down’ the scale of nature can we trace these phenomena of matter, the mammals? the reptiles? the fish? the insects? the worms? the sponges? and then across into the vegetable world, the mimosa? He ends, after sixty pages of judicious analysis, by taking a position ‘somewhere between the Linnaean definition of all animal life as sentient and the Cartesian that all infra-human life is mechanism’ but where to draw the line is, as he says, ‘extremely difficult, perhaps impossible’.

Like Darwin and Huxley he cannot easily escape the materialistic implications of his work. In the third volume of Anatomy of the Vertebrates he has much to say about the size of the brain determining power of thought. In a handwritten annotation to p. 144 he states that ‘the average weight of educated European brains is 61 oz and in the text he has Hottentot brains at 50 oz and Cuvier’s at 64 oz. His whole classificatory scheme for mammals, as mentioned above, is based on what he sees as a gradual increase in size and complexification of the brain. But, he concludes, ‘how the brain works in producing thought or soul is as much a mystery in Man as Brutes’ (p. 824).

But he is sensitive, like Huxley and Darwin, to the charge of materialism. And, like them, he responds by remarking ‘what “materialistic” means in the mind of the objector I nowhere find intelligibly laid down’ (p. 821). In another handwritten annotation to his own copy of Anatomy of Vertebrates he writes that ‘If force lines from the source of magnetic lines on earth, as indicated by the degree of correspondence between size of sunspots and degree of magnetic activity, may not lines of thought-force act from greater distance on terrestrial centres of such ...’

This, of course, is fanciful and it is to be emphasised that he did not publish this speculation. But the fact that he also sticks into his interleaved copy of Anatomy of the Vertebrates an account of Faraday’s iron-filing demonstration of magnetic lines of force opposite a passage in which he writes of the ‘same CAUSE which has endowed this world with power convertible into magnetic, electric, thermotic and other forms or modes of force, has also added the condition of conversion into the vital mode ...’ indicates his line of thought; and this implication is supported by a passage on p. 820 where he states quite baldly that ‘thought relates to the brain of man as does electricity to the nervous battery of the torpedo: both are forms of force, and the results of action of their respective organs’. Owen has an advantage on Huxley. Whereas, as we noted Huxley was a Cartesian, believing that nature was ultimately activated by inanimate forces; Owen took a far more hylozoic position. In the first set of Hunterian lectures which he gave as a young man of 32 in 1837 he writes that ‘we are irresistibly led (like Newton) to acknowledge the existence of an infinite spirit’ (p. 208) whilst he ends his sixth and final lecture of the sequence with the commonplace view (in his time) that the organic world constitutes a ‘grand series of Instruments ... by which the great end is at last obtained, of making Matter subservient to the manifestation of Mind’25. Although his views altered as his career developed we may nevertheless feel that he never genuinely sloughed off this pansyphic interpretation.

Conclusion
Thus, in conclusion we can see that the conflict of 1860 left behind a number of unsettled issues. Is it possible to construct a thoroughgoing Cartesian biology as Huxley wanted? Does it not leave the mind–body problem definitively insoluble: not only ignoramus but also ignorantibus? Does this not perhaps point to the submerged strength of the Coleridge–Owenite position? The Cartesian mind–body problem does not exist for the hylozoist. Stripped of its outdated theological overtones (which there seems little doubt that Owen, himself, fully believed) is there not still a possibility that, as some complexity theorists believe, matter has a thermodynamic tendency to increase in complexity? Is it possible that contemporary advances in understanding the genetics of developmental and regulatory genes will lead to a more open verdict on the possibility of sudden evolutionary change? These are some of the questions which remain submerged and largely disregarded in the onrush of modern biology. Perhaps in the next century they will receive fresh and serious attention.

Notes and references
1 Huxley, T.H. (1898) ‘Autobiography’ in Method and Results, p. 17, Macmillan
4 Mill, J.S. (1859) Dissertations and Discussions, Longmans, Green, p. 397, Reader and Dyer
5 Farrington, B. (1961) Greek Science Penguin