

# LYELL AND EVOLUTION: AN ACCOUNT OF LYELL'S RESPONSE TO THE PROSPECT OF AN EVOLUTIONARY ANCESTRY FOR MAN

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IN 1887 Huxley wrote:

. . . I see no reason to doubt that, if Sir Charles could have avoided the inevitable corollary of the pithecoïd origin of man—for which, to the end of his life, he entertained a profound antipathy—he would have advocated the efficiency of causes now in operation to bring about the condition of the organic world, as stoutly as he championed that doctrine in reference to inorganic nature.<sup>1</sup>

A number of recent historians have offered a fundamentally different explanation of Lyell's reluctance to give Darwin his unqualified support. Although these historians acknowledge both the enormous influence of Lyell on Darwin, and Lyell's dismay at the loss of human dignity implied by Darwin's theory, they have attempted to show that Lyell's view of earth-history was, at bottom, a profoundly anti-evolutionary view, which could not accommodate the history of life presented by Darwin and Wallace. Cannon, Hooykaas, Rudwick, and Wilson have argued<sup>2</sup> that

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<sup>1</sup> F. Darwin (ed.), *The life and letters of Charles Darwin* (2nd edn., 3 vols., London, 1887), ii, 193. Hereafter cited as *L.D.*

<sup>2</sup> W. F. Cannon, 'The uniformitarian-catastrophist debate', *Isis*, li (1960), 38–55; 'The bases of Darwin's achievement: a reevaluation', *Victorian studies*, v (1961), 109–34; 'The impact of uniformitarianism: two letters from John Herschel to Charles Lyell', *Proceedings of the American Philosophical Society*, cv (1961), 301–14; 'Charles Lyell is permitted to speak for himself: an abstract', in C. J. Schneer (ed.), *Toward a history of geology* (Cambridge, Mass., 1969), pp. 78–9. R. Hooykaas, 'The parallel between the history of the earth and the history of the animal world', *Archives internationales d'histoire des sciences*, x (1957), 3–18; *Natural law and divine miracle* (Leyden, 1959), 2nd impression issued as *The principle of uniformity* (Leyden, 1963); 'Geological uniformitarianism and evolution', *Archives internationales d'histoire des sciences*, xix (1966), 3–19.

M. J. S. Rudwick, 'A critique of uniformitarian geology: a letter from W. D. Conybeare to Charles Lyell', *Proceedings of the American Philosophical Society*, cxi (1967), 272–87; 'The strategy of Lyell's *Principles of geology*', *Isis*, lxi (1970), 4–33; 'Uniformity and progression; reflections on the structure of geological theory in the age of Lyell', in D. H. D. Roller (ed.), *Perspectives in the history of science and technology* (Norman, Oklahoma, 1971), pp. 209–27.

L. G. Wilson, 'The development of the concept of uniformitarianism in the mind of Charles Lyell', *Actes de X<sup>e</sup> Congrès International d'Histoire des Sciences; Ithaca, 1962* (Paris, 1964), ii, 993–6; 'The origins of Charles Lyell's uniformitarianism', *Geological Society of America. Special paper no. 89* (1967), 35–62; (ed.), *Sir Charles Lyell's scientific journals on the species question* (New Haven and London, 1970), hereafter cited as *Species journals*, using Wilson's pagination; 'Sir Charles Lyell and the species question', *American scientist*, lix (1971), 43–55; *Charles Lyell. The years to 1841: the revolution in geology* (New Haven and London, 1972).

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Lyell's own fundamental commitment was to a steady-state, anti-developmental conception of earth-history, and that Lyell understood this commitment to have been logically entailed by his conviction that the causes that have shaped the organic and inorganic worlds differed neither in type nor intensity from causes now acting. Therefore, these historians argue, the developmental, directional change proposed by Darwin's *Origin of Species* in 1859 was seen by Lyell as a threat to the system which he had been elaborating during the preceding thirty years and which he first propounded in his *Principles of geology*<sup>3</sup> in 1830. According to these historians, Lyell's original conception of the requirements of his new system forced him, in 1830, to reject the current special-creationist notion of the successive introduction, throughout the history of the earth, of increasingly complex plants and animals, and, in its stead, to substitute his own steady-state notion of a non-directional, non-progressive sequence of plant and animal forms. Lyell is held to have remained loyal to this steady-state vision of the history of life until the evidence for evolution became overwhelming. In Hooykaas's words: 'the idea of development is general, i.e. the whole concept of historic, irreversible and unique change went against his conception of uniformity.'<sup>4</sup>

Other historians,<sup>5</sup> more in line with the judgement of Huxley and other contemporaries of Lyell,<sup>6</sup> have argued that there was nothing in evolutionary theory that threatened Lyell's system. On the contrary, they argue that Darwin's and Wallace's theory of species origination was an extension—albeit a major extension—of Lyell's own work: the *Origin of species* brought to a triumphant conclusion the programme for a naturalistic explanation of earth history, organic and inorganic, that Lyell's *Principles* had launched. As Gillispie puts it, echoing Huxley: 'uniformitarianism in geology seems almost to cry out for evolutionism in biology.'<sup>7</sup> And Hodge and McKinney<sup>8</sup> have shown that Darwin and Wallace saw themselves, in Hodge's words, as pursuing 'unfinished Lyellian business'.

<sup>3</sup> C. Lyell, *Principles of geology* (3 vols., London, 1830–3). Hereafter cited as *PG*.

<sup>4</sup> Hooykaas 1966, op. cit. (2), 7.

<sup>5</sup> L. Eiseley, *Darwin's century* (London, 1959), pp. 97–115; 'Charles Lyell', *Scientific American*, cci (1959), 98–106. A. Ellegård, *Darwin and the general reader* (Göteborg, 1958), pp. 289–91. J. C. Greene, *The death of Adam* (Iowa, 1959), pp. 249–57; 'The Kuhnian paradigm and the Darwinian revolution', in Roller, op. cit. (2), pp. 1–23 (18–19). C. C. Gillispie, *Genesis and geology* (1951; New York: Harper Torchbooks, 1959), pp. 131–3. M. J. S. Hodge, 'On the origins of Darwinism in Lyellian historical geography', a paper read to the British Society for the History of Science, 10 July 1971. W. Irvine, *Apes, angels and Victorians* (Meridian paperback edn., New York, 1959), pp. 47–9, 74–5, 86–7. A. O. Lovejoy, 'The argument for organic evolution before the *Origin of species*', in B. Glass, O. Temkin, and W. Strauss (eds.), *Forerunners of Darwin* (Baltimore, 1959), pp. 356–414 (366–73). H. L. McKinney, 'A. R. Wallace and the discovery of natural selection', *Journal of the history of medicine*, xxi (1966), 333–57; *Wallace and natural selection* (New Haven and London, 1972). McKinney gives a full discussion of Lyell's thinking on evolution and, in particular, examines Lyell's response to Wallace's 1855 paper 'On the law which has regulated the introduction of new species'.

<sup>6</sup> For example, Wallace, Sedgwick, Hooker, Spencer, and Darwin himself. See notes 63–5.

<sup>7</sup> Gillispie 1959, op. cit. (5), p. 131. Cf. Huxley, in *LLD*, ii. 190.

<sup>8</sup> Hodge, op. cit. (5). McKinney 1966 and 1972, op. cit. (5).

According to this view, the theory of evolution by natural selection can now be seen, and was regarded by its founders and their contemporaries, as a 'conscious contribution to Lyellian historical geography'.<sup>9</sup> But this interpretation makes Lyell's reluctance to accept Darwin's and Wallace's gift appear rather odd. Why should Lyell have rejected the findings of two men who had been working in the research areas delineated by *Principles*, and who had produced what they regarded as a Lyellian conclusion to Lyell's unfinished enterprise? Two possible explanations have been put forward. First, it has been suggested<sup>10</sup> that Lyell had rejected, in 1830, any interpretation of the fossil record which implied that the history of life has been developmental, partly because he felt that the notion of organic progression was heavily contaminated by Christian ideas of crude miracles and associations with biblical accounts of earth-history. According to this view, Lyell believed that progressionist interpretations of the history of life had to be abandoned in favour of a pattern of non-progressive organic change, a pattern which could be supported by careful and subtle reference to the fossil record, and which was more philosophical, for although Lyell could not explain the exact mode of species' origination, he was at least freed from the task of explaining the suspiciously divinely-directed pattern of organic progression: he could dispense with mysterious and unexaminable 'laws of progression'. Thus, it is argued, Lyell, in working away at his self-appointed task of releasing geology from its load of redundant theological lumber in order to set the science on what he took to be its proper methodological feet,<sup>11</sup> threw out a valuable concept—a concept that he would not readmit until long after Darwin and Wallace had securely demonstrated that it was free from religious blight and that it involved no philosophical lapse. Secondly, and again more in line with Huxley's judgement, it has been suggested<sup>12</sup> that Lyell felt a strong aversion to the idea of man's being descended from the beasts, an aversion sufficiently strong to have made him procrastinate for ten years before grudgingly giving way to the theory of Darwin and Wallace. It is this second suggestion that I wish to develop in this article.

<sup>9</sup> Hodge, op. cit. (5), typescript p. 9.

<sup>10</sup> Eiseley, *Darwin's century*, pp. 108–15; 'Charles Lyell', op. cit. (5), 101–2. Gillispie, op. cit. (5), 131.

<sup>11</sup> As Cannon, Hooykaas, and Rudwick have made plain, we should be wary of accepting Lyell's own estimate of his scientific opponents. Lyell tended to make his 'paroxysmalist', 'convulsionist', or 'cosmogonist' opponents, as he scathingly called them, into straw men in order to score easy victories. But Cannon, Hooykaas, and Rudwick have demonstrated that Lyell's predecessors and contemporaries were substantial geologists in their own right, with a coherent and fruitful methodology at their service. They rarely deserved Lyell's scorn. See Cannon 1960, op. cit. (2); R. Hooykaas, *Catastrophism in geology* (Amsterdam, 1970); Rudwick 1971, op. cit. (2).

<sup>12</sup> Eiseley, *Darwin's century*, p. 105. W. Coleman, 'Lyell and the reality of species', *Isis*, liii (1962), 325–38 (326). Irvine, op. cit. (5), 139, 142–8, 210–11. Wilson, *Species journals*, op. cit. (2), pp. xxvi, l. M. J. S. Hodge, review of Wilson, *Species journals*, in *Isis*, lxii (1971), 119–20. McKinney 1966, op. cit. (5), 351; McKinney 1972, op. cit. (5), pp. 97–116.

I argue here that it was Lyell's fears for his ancestry that prevented him from ever totally accepting, heart and mind, Darwin's and Wallace's account of species origination. Furthermore, I argue that Lyell's original rejection of the idea of organic progression grew out of this same desire to preserve what he called, in 1832, 'the high genealogy of his species'.<sup>13</sup> In my view, Lyell's rejection of organic progression in 1830 can be separated, at one level, from the general trend of *Principles* and legitimately be considered as an anti-evolutionary strategem that works against the grain of the book. Similarly, Lyell's subsequent reservations about evolution may be considered apart from the main drift of his scientific work, a drift which flowed directly from the *Principles* to the *Origin*.

Until 1827 Lyell, in common with most other natural philosophers, accepted that the history of life had followed a broad pattern of increasing differentiation and complexification. Reptiles succeeded fish, mammals succeeded reptiles and so on. And within each class there had been a development from the earliest examples to the most recent: man, for example, was more complex than an early marsupial. Moreover, individual species, including man, bore unmistakable structural similarities, one with another. This progressive sequence—which, of course, was brought about by special creations, not transmutation—correlated closely with historical time: the rocks indicated, first, a time when no life existed on earth, and then recorded the existence of representatives of the great progressive sequence that culminated in man. In 1827 Lyell read Lamarck's *Philosophie zoologique*,<sup>14</sup> and although he possessed ample means of demolishing Lamarck's improbable mechanism for transmutation, he grasped, imaginatively and fearfully, the truth that *any* naturalistic account of species origination would use the widely-accepted notion of a progressive sequence of organic forms through time for its own revolutionary ends. And he realized that any transmutationist version of progressive development would be bound to link man and beast in common descent; some sort of evolution would provide the *only possible* naturalistic explanation of species origination, if there had indeed been a progressive sequence of animals and plants through time. And Lyell just could not accept the idea of man's having evolved in imperceptible stages from the apes. This blank refusal to contemplate evolution, coupled with his premonition that if progression were true, so too would evolution be true, made him reject progression—not as an act of crude

<sup>13</sup> *PG* (1st edn., 1832), ii. 21.

<sup>14</sup> Lyell to G. Mantell, 2 March 1827, in K. Lyell (ed.), *Life, letters and journals of Sir Charles Lyell* (2 vols., London, 1881), 168–9. Hereafter cited as *LLJ*. Dr Hodge has recently argued that Lamarck was not proposing a theory of 'common descent' at all. But for the purposes of my argument here, what Lyell thought Lamarck said is more important than what Lamarck actually said. It is clear that Lyell understood Lamarck to have been formulating a theory of species origination, and Lyell was not wrong in seeing that Lamarck's account allowed no special place for man. See M. J. S. Hodge, 'Lamarck's science of living bodies', *British journal for the history of science*, v (1971), 323–52.

duplicity, but because the implications of progression could not be squared with his deepest beliefs about man and God. So from 1827 Lyell attempted not only to discredit Lamarck's particular account of transmutation, but to overturn the prevailing notion of organic progression, as such. He was attempting to make any account of species origination that might depend upon the close temporal juxtaposition of structurally related organic forms impossible.<sup>15</sup> In 1832 he admitted that 'in proportion as the series of known animals grows more complete, none can doubt that there is a nearer approximation to a graduated scale of being',<sup>16</sup> but five years earlier he had set himself the anti-transmutationist task of defending the proposition that this scale of being bore no clear correlation with historical time. He realized that in loudly proclaiming, on one hand, the progressive advance of animal forms and, on the other, the mystery of God's beneficent creative activity, progressionist special-creationists like Sedgwick and Agassiz were merely digging their own graves, for their position depended absolutely on ignorance of the mechanism of species creation. Lyell realized that if progression were true, transmutationists would eventually solve the mystery of species creation, and progression, hitherto a valuable support for natural theology, would be given a new and, for the natural theologians, embarrassing significance. Lyell very perceptively realized that beliefs about man's special place in nature, which he shared with Sedgwick and Agassiz, could best be preserved by an abandonment of progression; in his view, to insist that man followed the apes in time, and that man was not very different from the apes in structure, was to invite ruin.<sup>17</sup>

What is peculiar to Lyell, and what makes him the inspired conservative, is that between 1826 and 1830 he detected the exposed nature

<sup>15</sup> It is significant that it was Wallace's 1855 paper 'On the law which has regulated the introduction of new species', *Annals and magazine of natural history*, 2nd ser., xvi (1855), 184–97, that prompted Lyell to open a notebook on the species question. Wallace's conclusion, which assumed the general truth of organic progression, was that '*Every species has come into existence coincident both in time and space with a pre-existing closely allied species*' (p. 196; Wallace's italics). Wallace's paper assumes that evolution has happened, although it does not provide a mechanism. In 1868 Lyell wrote to Wallace, outlining his own version of the history of evolutionary thought during the preceding thirty years. In this letter Lyell wrote: 'When I first read your paper declaring that each new species had come into the world co-incident in time & space with closely allied species, it struck me as true though not capable of geological demonstration, and it shook my confidence together with other arguments in the same paper in the independent creation theory more than anything I have read before'; copy of a letter dated 19 November 1868, in Lyell papers, University of Edinburgh Library; my italics. See *Species journals*, p. 3, and McKinney 1972, op. cit. (5).

<sup>16</sup> *PG* (1st edn., 1832), ii. 22.

<sup>17</sup> Cf. Cannon, 'The bases of Darwin's achievement . . .', op. cit. (2), 110. Cannon argues that the progressionist natural theologians were discomfited in 1859 because Darwin had 'stolen' their universe and fitted it out with a revolutionary mechanism. In one sense this may be true: the superficial similarity, yet underlying deep antagonism, between the Christian progressionists' account of the history of life and Darwin's goes a long way towards explaining the progressionists' wrath. But for reasons that this article aims to make clear, I believe that Darwin did not derive his 'framework' from the progressionist natural theologians, as against the framework of *Principles*. Lyell's anti-progressionism, for all his ingenuity, turned out to be a negligible obstruction to evolutionary thought; the line from *Principles* to the *Origin* is unimpeded.

of the progressionist position and henceforth determined to show that there had been no progression. In his *Principles* he reconstructed the history of life in terms that made evolution impossible. Yet, paradoxically, he presented this interpretation in a series of books whose whole outlook and methodology were bound to excite evolutionary speculation. Cannon concludes that 'Lyell feared evolutionary ideas in part because they seemed to him to support, or be derived from, Christian theology.'<sup>18</sup> I argue that exactly the opposite is the case: Lyell feared evolutionary ideas in part because they contradicted, and were not derived from, Christian theology. One crucially important question arises here: what precisely were Lyell's religious beliefs? Clearly it is not enough simply to assert that all Lyell's fears sprang from his allegiance to the dogmas of Christian theology. Here the problem is that we know far too little about Lyell's beliefs, so the following comments can be no more than tentative and provisional. But we can be quite certain that his beliefs did not merely form a vague background to his 'scientific' work; they entered into the finest texture of his thought. The case of Lyell firmly supports R. M. Young's contention that during this period 'the scientific debate directly involves theological and philosophical issues. These were constitutive, not contextual'.<sup>19</sup>

Rudwick closes his analysis of *Principles* with the tentative suggestion that the account of earth-history that Lyell presents in his book was a sort of objective correlative of his vision of 'a universe of perfect and wise design, a universe fully under the dominion of providential natural laws'.<sup>20</sup> This suggestion must surely form the basis of any reconstruction of Lyell's beliefs, for we can be certain that Lyell, far from believing the history of the world to have been a sequence of random, insignificant events, firmly believed that, in his own words, 'in whatever direction we pursue our researches, whether in time or space, we discover everywhere the clear proofs of a Creative Intelligence, and of His foresight, wisdom and power'.<sup>21</sup> And, more specifically, he praised the way in which Buckland, in his 'Bridgewater treatise' (1836), had shown that both the organic world and the inorganic world presented testimony 'of the adaptation of particular means and forces to the accomplishment of certain ends for which the habitable earth has been framed'. And Lyell mentioned Buckland's consideration not only of the admirable adaptation of plants and animals to their environments, but also of the convenient 'distribution of metallic and other minerals in the earth, and the position

<sup>18</sup> Cannon 1960, op. cit. (2), 39.

<sup>19</sup> R. M. Young, 'Darwin's metaphor: does Nature select?', *The monist*, lv (1971), 442-503 (444).

<sup>20</sup> Rudwick 1970, op. cit. (2), 33.

<sup>21</sup> *PG* (1st edn., 1833), iii. 384. This statement comes from the 'Concluding remarks' in the last volume of *Principles*, and Lyell retained it throughout all of the eleven editions that he personally supervised. See *PG* (11th edn., 1872), ii. 620.

of coal in stratified rocks'.<sup>22</sup> So we can be confident that Lyell subscribed to the specifically *natural* theological opinions of his contemporaries, even though *Principles* was seen by some of them to embody a denigration of those beliefs. Of his views on revealed religion, we know very little, although from the lack of references to the need for revelation in his published work, private journals, and letters, we may infer that it did not occupy a great deal of his thought. In this respect—his seeming lack of concern for the Christian Revelation—he differs markedly from his contemporaries, especially from ordained clergymen like Whewell, Sedgwick, and Buckland. But Lyell did not ignore or dismiss revelation; on the issues of the reconciliation of science and scripture, for example, and the accuracy of the Mosaic record, his position was generally orthodox, by the standards of the geologists of the 1830s. He certainly wished, as did most of his contemporaries, to prise apart geological explanation and overt scriptural reconciliation; he wished, as he put it, to 'free the science from Moses',<sup>23</sup> and he had little patience with what he called 'physico theologians'<sup>24</sup> or 'theological sophists'.<sup>25</sup> Yet he seems to have retained some vestige of belief in the credibility of the Mosaic record. In *Principles* he preserved an attenuated version of the Deluge,<sup>26</sup> and, in a letter concerning his appointment to the chair of geology at King's College, London, he wrote that he knew of 'no physical evidence tending in any degree to invalidate the opinion that the whole inhabited earth . . . may not have been deluged within the last 3 or 4,000 years',<sup>27</sup> thereby affirming not only a belief in the likelihood of a shrunken version of the Mosaic Deluge, but also a belief in the accuracy of the biblical chronology of the creation of man. Provisionally, then, Lyell's religious position seems to have been essentially deistic, with perhaps a certain respect for the Bible as a reliable account of God's dealings with man, if not with the world as a whole.

Other aspects of Lyell's religious views need further study. His attitudes towards Christian institutions—churches and universities, for example—are far from clear. But in this respect his brief period as professor at King's College is interesting. He claimed that 'the bishops cut short my career at King's College',<sup>28</sup> but it seems that this was because the governing body of the college had decided to exclude ladies from

<sup>22</sup> Lyell, 'Address to the Geological Society . . . 17 February, 1837', *Proceedings of the Geological Society of London*, ii (1837), 479–523 (517–21).

<sup>23</sup> *LLJ*, i. 268.

<sup>24</sup> *LLJ*, i. 271.

<sup>25</sup> *LLJ*, i. 310.

<sup>26</sup> *PG* (1st edn., 1833), iii. 271–4.

<sup>27</sup> Lyell to the Bishop of Llandaff, 28 March 1831, quoted in Wilson 1972, op. cit. (2), p. 310. In this letter Lyell also affirms that he believes that species originated by 'the direct intervention of the First Cause'. Professor Wilson comments: 'one could wish that Lyell had not written this letter' (p. 310), but the letter only corroborates what we can ascertain from *Principles*, the *Species journals*, and other sources. See, for example, the letter to Wallace quoted in note 15.

<sup>28</sup> Lyell to Dr Fleming, 1 May 1833, in *LLJ*, i. 397.

lectures. Consequently Lyell's lectures ceased to attract members of fashionable society and his class fell to fifteen, of whom only two were students of King's College, and, considering it not worth his while to carry on with the post, he resigned.<sup>29</sup> It is unlikely that the bishops, save perhaps the Bishop of Llandaff, were upset by the content of his lectures, especially as we know that he 'worked hard upon the subject of the connection of geology and natural theology'.<sup>30</sup> And we can be certain that Lyell never saw himself as a scourge of the churchmen; he seems, at the very least, to have wanted a quiet seemliness to prevail in scientists' relationships with the church. As late as 1866—the era of Tyndall and Huxley—Lyell was angry when Dr W. B. Carpenter, the physiologist, used the occasion of a popular Sunday lecture (one of a series in which Lyell had agreed to participate) to attack the clergy over their reluctance to embrace the antiquity of man. Lyell threatened to withdraw his support for the lectures and wrote to Huxley and Carpenter, complaining about what he regarded as a needless assault on popular feeling. He told Carpenter that there must have been many members of the audience at the lecture, 'whose feelings and old associations with what they have been taught to reverence as bible truth were rudely assailed & pained by the manner in which you spoke of certain passages of scripture'.<sup>31</sup>

But the 'religious' aspect of Lyell's thought which is most prominent and which looms through all his work, is his concern, even mania, for the status and dignity of man. He differed in this respect from men like Sedgwick and Wilberforce in that he does not seem to have regarded the preservation of man's unique status as a precondition for the maintenance of ethical and political standards. Unlike Sedgwick and Wilberforce, it did not occur to him that an evolutionary history for man might signal the collapse of Western civilization. Lyell's distaste for anthropoid origins is much more 'psychological', or perhaps aesthetic,<sup>32</sup> and does not stem merely from a specific theological dogma. The sources seem to be more personal, and are probably inaccessible to us. Yet Lyell's fear for man's dignity should not appear strange. It is arguably the young Darwin, Wallace, and Chambers, brought up on much the same Christian diet as Lyell, who are to be wondered at for parting with their 'high genealogy' with so little regret.

To recapitulate my thesis: we know that Lyell was slow to acknowledge Darwin's theory and that he never totally accepted evolution by

<sup>29</sup> See Wilson 1972, op. cit. (2), pp. 308–23, 353–60, 376; F. J. C. Hearnshaw, *The centenary history of King's College, London* (London, 1929), pp. 91, 107–9.

<sup>30</sup> *LLJ*, i, 382.

<sup>31</sup> Copy of a letter from Lyell to Carpenter, 22 January 1866, enclosed in a letter to T. H. Huxley, same date; Huxley papers, 6, 120, Imperial College of Science and Technology, London. See also W. B. Carpenter, *Nature and man. Essays scientific and philosophical* (London, 1888), pp. 86–8.

<sup>32</sup> The suggestion is Dr Hodge's.



natural selection as an adequate explanation of the history of life; we know that in his *Species journals* (1855–61), he was struggling to come to terms with the implications of human evolution; we know that in *Principles of geology* (1830–3) he presented the history of life in terms that would not admit its reconstruction as a series of interconnected evolutionary genealogies—that is, Lyell denied the evidence that evolutionists later built on; we know that his about-face on the question of organic progression occurred at precisely the same time (1827) as his first encounter with Lamarck's evolutionary theory. So it seems reasonable to suggest that Lyell's anti-progressionism was in some way a response to Lamarck, an anti-evolutionary gesture. Further, we can now see that throughout his life Lyell remained loyal to a set of beliefs about God, nature, and man, and that these beliefs were integrated into his 'scientific' work, although in the end they were incompatible with the sort of scientific explanation he was advocating. These beliefs slowly become explicit—a few years before his death he made some sad and frank admissions—but they are implicit, though sometimes difficult to establish, in all his early work. However, even though Lyell's ambitions and projects subserved a coherent, integrated personal philosophy, his published work took on an independent life of its own, and the conservative, rearguard aspect of his books was largely ignored, while the forward-looking, imaginative, and exciting aspects of his books were taken up by Chambers, Wallace, and Darwin for ends that their master could only lament. Lyell had launched a comprehensive programme for the naturalistic explanation of all phenomena in the organic and inorganic worlds, yet he personally demanded that somehow one particular phenomenon—the origination of man—had been the result of God's unique, personal, creative attention. Lyell initiated an explanatory enterprise, the conclusion of which he was psychologically incapable of accepting.

*Lyell's methodology and the foundation of his anti-progressionism*

In 1826 Lyell wrote an article for the *Quarterly review*, in which he surveyed the contemporary state of geology.<sup>33</sup> Two of the points he makes in this article have important implications for our understanding of his subsequent development. First, he says that although some recent excavations have produced fossils that were out of their anticipated progressive sequence, 'the general inference to be deduced from observed facts' is that 'in ascending from the lowest to the more recent strata, a gradual and progressive scale could be traced from the simplest forms of organisation to those more complicated, ending at length in the class of animals most related to man'.<sup>34</sup> Here Lyell is affirming that a 'pro-

<sup>33</sup> [Lyell], 'Transactions of the Geological Society of London', *Quarterly review*, xxxiv (1826), 507–40.

<sup>34</sup> *Ibid.*, 513.

gressive scale' in the organic world correlates with historical time. Secondly, he suggests that

in the present state of our knowledge, it appears premature to assume that existing agents could not, in the lapse of ages, produce such effects as fall principally under the examination of the geologist. It is an assumption, moreover, directly calculated to repress the ardour of inquiry, by destroying all hope of interpreting what is obscure in the past by an accurate investigation of the present phenomena of nature.<sup>35</sup>

This suggestion is clearly recognizable as a preliminary statement of what emerged three years later as Lyell's '*principle of reasoning* in the science'.<sup>36</sup> It is also clear that in 1826 Lyell saw no conflict between a belief in a 'progressive scale' ascending through time, and a conviction that 'existing agents' might well be capable of having produced all geological phenomena. This is important, as it has been argued that Lyell thought that his principle of reasoning specifically excluded the possibility of belief in organic progression.<sup>37</sup> In 1826 we can see the two running side by side. When Lyell formally enunciated his principle of reasoning three years later, in a letter to Murchison,<sup>38</sup> he had also come to reject the notion of organic progression. But he does not seem to have believed that the two developments were linked necessarily; and, logically, they were not. The two developments proceeded independently of each other.

First, Lyell's principle of reasoning. In April 1828, shortly before he left for the continental tour that was to vindicate for him what he believed to be a new approach to geological explanation, Lyell wrote to Constant Prévost, a French geologist who had worked under Cuvier and Lamarck, and who seems to have been converging simultaneously with Lyell on the same principle of reasoning. Lyell wrote:

I assure you that I could discuss for a month with you the grand subject which you allude to as to the correspondence between the former & present state of physical causes. But surely we are placed in a somewhat unnatural situation for instead of assuming as we ought to do the identity of all the causes in nature in the former & present state of the planet, just as we anticipate the correspondence of those causes in all future time, we start by imagining a discrepance & thus throw the onus probandi on those who assert what all ought to believe without proof until the contrary can be made clearly manifest.<sup>39</sup>

Lyell then set off on his continental tour, convinced that this assumption of 'the identity of all the causes in nature in the former & present state of the planet' would yield the best, indeed the only acceptable geological explanations. On his way home from a most successful journey he wrote

<sup>35</sup> *Ibid.*, 518.

<sup>36</sup> Lyell to R. I. Murchison, 15 January 1829, in *LLJ*, i. 234. Lyell's emphasis.

<sup>37</sup> See, for example, Rudwick 1970, *op. cit.* (2), 8; Hooykaas 1966, *op. cit.* (2), 7; Cannon 1960, *op. cit.* (2), 55.

<sup>38</sup> Lyell to Murchison, 15 January 1829, in *LLJ*, i. 234.

<sup>39</sup> Unpublished letter from Lyell to C. Prévost, 20 April 1828; copy in Lyell papers, Edinburgh University Library.

to Murchison, declaring his conviction that 'no causes whatever have from the earliest time to which we can look back, to the present, ever acted, but those *now acting*', and giving a further dimension to his conviction, he added that these causes have 'never acted with different degrees of energy from that which they now exert'.<sup>40</sup> So between 1826 and 1829 Lyell gained sufficient confidence in his tentative proposal for him to advance it as the methodology which would provide the basis of his forthcoming *Principles*.

What implications does Lyell's principle of reasoning have for the notion of organic progression, both in fact and in Lyell's opinion? Rudwick expounds Lyell's thinking on this matter as follows: 'If processes observable at present are representative in degree as well as in kind, of all those that have acted in the past, there cannot have been any overall directional trend in the history of the earth, which must therefore be in a "steady-state" condition.'<sup>41</sup> In a footnote Rudwick points out that 'there is an obvious logical flaw in this reasoning', but he maintains that 'the important point is that Lyell believed that a steady-state system was "necessarily" entailed by his principles'.<sup>42</sup> In my view, Lyell did not make the logical mistake contained in Rudwick's exposition. In other words, he did not believe that the proposition 'the causes that shaped the world have differed neither in type nor intensity from those now acting' necessarily entailed the conclusion 'there has been no directional change throughout the history of life'. Lyell's juxtaposition, in the 1826 *Quarterly review* article, of a belief in organic progression and a preliminary statement of his principle of reasoning, makes this fairly clear. And, formally, there is nothing in Lyell's principle of reasoning that precludes a developmental view of the history of life. The account given by Darwin and Wallace postulated no causes and no intensities different from those working in the modern world, and although Lyell certainly had strong reservations about their theory, he did not object on the ground that it offended against his principle of reasoning. The *Species journals*, which Lyell kept between 1855 and 1861 and which will be discussed in greater detail below, show that he regarded progression and, later, evolution as threats not to his scientific principles but to his religious beliefs. But, even without these later admissions, the same conclusion holds. Lyell's rejection of organic progression derived from sources other than his principle of reasoning.

The 1826 *Quarterly review* article perhaps points to a more likely source of Lyell's rejection of organic progression. In this article, the concluding pages of which are written in the exalted style of the progressionist natural theologians, Lyell stresses the close osteological

<sup>40</sup> *LLJ*, i. 234. Lyell's emphasis.

<sup>41</sup> Rudwick 1970, op. cit. (2), 8.

<sup>42</sup> *Ibid.*

similarity between man and the rest of the mammalia. Further, he emphasizes that the *quadrumana* 'approach much nearer [in their osteological character] to the human species than any others'.<sup>43</sup> He concludes the article by saying that palaeontological research has revealed that all parts of the organic world, past and present, are 'parts of one connected plan', and that man 'forms an inseparable part' of this scheme. He even uses the term 'links in a chain' to describe fossils. Then, once more emphasizing that 'There is a gradation of animated beings, from those of the simplest to those of the most complicated organization', he rounds off by quoting Bishop Butler: 'we are placed . . . in the middle of a scheme, not a fixed but a progressive one.'<sup>44</sup> In the passage that Lyell quotes<sup>45</sup> Butler is not, of course, discussing fossil progression. He is arguing for the principle of sufficient reason; that is, he argues that if we imperfectly comprehend all the details of God's scheme, and if perhaps some details of the scheme appear to us redundant, or even malignant, we can be sure, since we are confident that the scheme as a whole is essentially good, that each detail of the scheme, however perplexing to us, is an essential part of God's harmonious plan. It is in this sense that Butler uses the term 'progressive' scheme: as the scheme progressively unfolds, the significance of hitherto obscure aspects of its workings become clear. Lyell seems to have found in Butler's argument an apt way of saying that, geologically, all is for the best. As he put it, employing Butler's argument: 'sources of apparent derangement in the [geological] system appear, when their operation throughout a series of ages is brought into one view, to have produced a great preponderance of good.'<sup>46</sup>

So in 1826 Lyell was, first, predisposed to believe that 'existing agents' had produced all geological phenomena—and in his definition of geology, he included changes in the organic world;<sup>47</sup> secondly, he was in general agreement with the notion of the progressive development of life through time; thirdly, he was not afraid to emphasize that man 'forms an inseparable part' of this scheme; and, fourthly, he was confident that the study of geology revealed the workings of God's beneficent plan. In the following year Lyell read Lamarck.

It was Lamarck who changed Lyell from a confident progressionist into an ingenious anti-progressionist. The evidence is not conclusive if we look only at Lyell's statements during the year in which he read Lamarck, but his subsequent attitudes, and a number of plain statements later made by him, all point back to his reading of Lamarck as the source

<sup>43</sup> Lyell 1826, op. cit. (33), 513.

<sup>44</sup> *Ibid.*, 538–9. Lyell's italics.

<sup>45</sup> J. Butler, *The analogy of religion* (1736), part one, chapters VII and VIII. In the Oxford University Press 'World's Classics' edn. (London, 1907), this section is on pp. 144–65.

<sup>46</sup> Lyell 1826, op. cit. (33), 539.

<sup>47</sup> Cf. *PG* (1st edn., 1830), i. 1. The first sentence runs: 'Geology is the science which investigates the successive changes that have taken place in the organic and inorganic kingdoms of nature . . .'

of his about-face on the question of organic progression. Lyell read the *Philosophie zoologique* in 1827, and straight away he wrote to Mantell, giving his impressions. At once, Lyell says that Lamarck's theories 'delighted me' and that he feels 'none of the *odium theologicum* which some modern writers in this country have visited him with'.<sup>48</sup> Now my argument can accommodate this difficulty only by proposing three, overlapping, somewhat rickety interpretations. First, it is possible that Lyell, as a young man writing to a fellow-enthusiast, would be keen to show that disreputable French works do not upset him. If this is the case, Lyell was deceiving himself. Secondly, the rest of the letter indicates that Lyell had responded to Lamarck by recasting the history of life in anti-evolutionary terms. Thus Lyell could now present a palaeontological case that would be proof against Lamarck, who would then pose no threat and so could be treated lightly. Thirdly, it may well be that Lyell indeed does feel no '*odium theologicum*' in the strict sense, for, as I have argued, Lyell's objection to human evolution was not the result of a straightforward application of dogmatic theology. Lyell never, in the ensuing years, visited Darwin and Wallace with theological odium. Lyell's concern was not doctrinal—or ethical or political—but personal and perhaps entirely idiosyncratic. But, even so, it would be an abuse of the term to suggest that his objection to man's descent from the beasts was entirely non-theological, so Lyell's apparently jaunty reception of Lamarck remains a problem, though it would be a more serious problem if that paragraph in the letter to Mantell were our sole source of information on Lyell's reaction to the idea of evolution.

Whatever the source of Lyell's disclaimer, the rest of the letter gives some support to the proposal that Lyell had glimpsed the possibility of transmutation and had turned abruptly away from it. Lyell recognizes the 'mighty inferences' of Lamarck's argument:

if pushed as far as it must go, if worth anything, it would prove that men may have come from the Ourang-Outang. But after all, what changes species may really undergo! How impossible will it be to distinguish and lay down a line, beyond which some of the so-called extinct species have never passed into recent ones.<sup>49</sup>

Here Lyell recognizes that the enormous difficulties of establishing where one species stops and the next starts, potentially support Lamarck, and all would-be transmutationists; and it must be remembered that during the twenty years that had passed since Lamarck first published his book, knowledge of the variety and extent of modern floras and faunas, and of the fossil record, had increased significantly. So if, as Lyell formerly believed, man forms 'an inseparable part' of a progressive sequence of closely related organic forms, then, perhaps through his reading of

<sup>48</sup> Lyell to G. Mantell, 2 March 1827, in *LLJ*, i. 168–9.

<sup>49</sup> *Ibid.*, 168.

Lamarck, Lyell recognized too that man's precious dignity was in peril, even though Lamarck's particular version of transmutation could fairly easily be discredited.

The rest of the letter shows Lyell formulating a new and, in effect, anti-evolutionary interpretation of the fossil record. He outlines to Mantell a new theory of selective deposition. Lyell now argues that we find no complex organisms in early strata not because such organisms did not then exist, but because they never found their way into deposits. The simple forms that predominate in early strata are not characteristic, or representative, of the complete floras and faunas that flourished at the time of their deposition; they are merely a small selection of those organisms whose environment was conducive to their deposition and eventual fossilization. Lyell offers Mantell a modern analogy to illustrate his new theory. Water-birds of the present day have never been found embedded in the marl of the Scottish lakes they now inhabit. Accordingly, he argues that it is only to be expected that Mantell should have found no fossil birds in his Sussex excavations. Then Lyell moves to his conclusion: 'you see the drift of my argument—*ergo*, mammalia existed when the oolite and coal, &c were formed.'<sup>50</sup> It is here that Lyell takes a revealing false step. His use of modern analogy, and his insight into the extreme selectivity of fossil preservation were imaginative and reasonable, but to say that *because* certain present-day organisms do not readily become fossilized, '*ergo*' similar organisms *existed* in the earliest periods is quite unreasonable. It is Lyell's attempt to push a real insight to absurd lengths which suggests that his new conviction—that complex organisms did not succeed less complex, but have always existed contemporaneously with them—was dictated less by a careful appraisal of the rocks than by a compulsion to find some way of reconstructing the history of life in terms that were incompatible with evolution. Lyell had found a 'scientific' way of dividing man, who, axiomatically, was regarded as a recent creation, from the higher mammalia, which Lyell now pushed back as far down into the past as the earliest rocks, beyond the point from which transmutation could plausibly be suggested. In other words, Lyell was attempting to 'lay down a line, beyond which . . . extinct species have never passed into recent ones'; transmutationists could be blocked if it could be shown that, far from there having been a gradual complexification of organic forms through time, the most complex classes existed with all other classes, in the earliest times, alongside the most simple, and that when creative additions were made to floras and faunas, the addition represented no advance in complexity. Lyell had found a position from which the special creation of man might be defended—a far stronger position than Sedgwick's, for Sedgwick continued to proclaim the smooth temporal and anatomical transition between man and ape. Given that Sedgwick and

<sup>50</sup> *Ibid.*, 169. In 1827 little was known of the strata below the carboniferous series.

Lyell both believed, in 1830, in special creations, Lyell's position was the more secure, because his picture of the history of life *demande*d that creations be special; he denied the progressionist pattern that would eventually prove Sedgwick's undoing. The implication of Lyell's new position was that man's origination must have been special, as he was, according to Lyell, preceded by no orderly sequence of progressively complexifying mammal species, from which he could have been genealogically derived. As far as Lyell was concerned, the nearest anatomical relative of man might be an as yet undiscovered gorilla-type creature who wandered at large at the time of the deposition of the early Carboniferous 'Grauwacke and transition limestone', but who avoided dying in circumstances that would have ensured his eventual fossilization. Thus, Lyell laments to Mantell, 'I wish among your Groombridge fossils there had been a good cetacean, for theoretically it would be of more importance than the iguanodon.'<sup>51</sup> It would have pleased Lyell if a whale had been found below, or at least contemporaneous with, a reptile.

A letter Lyell wrote to Darwin in 1863, thirty-six years later, when Lyell was reluctantly giving way to Darwin's theory, supports the proposal that it was a reading of Lamarck that set Lyell to defend the high genealogy of his species. Even when we have taken into account the dangers of hindsight, Lyell's account is consistent with what we can infer from his earlier writings. He recalls:

I remember that it was the conclusion he came to about man that fortified me thirty years ago against the great impression which his arguments at first made on my mind . . . When I came to the conclusion that after all Lamarck was going to be shown to be right, that we must 'go the whole orang', I re-read his book, and remembering when it was written, I felt I had done him injustice.<sup>52</sup>

Incidentally, it is worth noting here that Lyell lumps together the theories of Darwin and Lamarck; he sees Darwin as having shown that Lamarck was right. When referring to evolutionary theory, Lyell was slow to distinguish between Lamarck's and Darwin's versions, a habit that Darwin, justifiably, found irritating.<sup>53</sup> But for Lyell, the two versions of the theory were indistinguishable in their most important aspect; they both postulated man's descent from the beasts.

In sum then, between 1826 and 1830, when he published the first volume of *Principles*, Lyell founded the two principles that characterize all his subsequent work. On one hand, he became thoroughly committed to naturalistic, gradualistic explanations of all geological phenomena,

<sup>51</sup> *Ibid.*

<sup>52</sup> Lyell to Darwin, 15 March 1863, in *LLJ*, ii. 365.

<sup>53</sup> E.g. *LLD*, iii. 14, 15. See also: T. H. Huxley to Lyell, 17 August 1862, in L. Huxley (ed.), *Life and letters of Thomas Henry Huxley* (2 vols., London, 1900), i. 200; McKinney 1972, op. cit. (5), pp. 115-16.

organic and inorganic, and was convinced that such explanations could be provided only on an assumption of the identity of past and present causes, both of type and intensity. But, haunted by his interpretation of Lamarck, Lyell also set himself the task of defending the high genealogy of his species, by putting forward the proposition that structural relationships in the organic world, past and present, bear no clear correlation with time. He set out to disrupt the notion that, through time, there had been a succession of closely related forms which culminated in the apes and man.

Lyell emerges from this interpretation as a brilliant conspirator who foolishly took up an untenable position in order to preserve a lost cause. Plainly, this will not do. There were, in the 1820s and 1830s, perfectly coherent, although not ultimately persuasive reasons for opposing organic progression, and in *Principles* Lyell presents a good deal of evidence to support his case. As far back as 1826 Lyell had drawn attention to the Stonesfield mammals,<sup>54</sup> pointing out that they showed that mammals existed in periods hitherto believed to have been characteristically, perhaps exclusively, reptilian.<sup>55</sup> Also, Lyell was right to emphasize the extreme selectivity of fossil deposition. And on the question of transmutation itself Lyell had a battery of cogent practical objections. Lastly, as Rudwick points out,<sup>56</sup> Lyell's plan to use fossil testacea as quantitative faunal chronometers for dating strata would have been severely jeopardized if species turned out to be mutable. Lyell's position was conservative but coherent. But was Lyell a conspirator? Was he consciously aware of what I take to be the source of his rejection of organic progression? No clear answer is possible. Obviously, if Lyell had been challenged on the issue in 1830, he would have retorted with purely 'scientific' reasons for his rejection of organic progression, claiming that it was the only valid conclusion to be drawn from an examination of the fossil record. But it is clear, from letters and his journals, that Lyell believed that progression, once its miraculous creative additions were explained naturalistically, would become evolution, and there can be no doubt that he was repelled by the implications of evolution. We shall never be able to unravel Lyell's thought processes, but it seems to me probable that the prospect of evolution affronted his beliefs about the way God had ordered the world, and that his faith in the providential pattern of earth history was so powerful that it regulated, or governed, his selection and adoption of 'scientific' theories. Consequently when, in 1827, he perceived that he could construct a coherent account of the history of life which squared with his providentialism, excluded the possibility of evolution, and

<sup>54</sup> In 1814 jaws of what Cuvier identified as marsupial mammals were found in the Secondary Oolite at Stonesfield, near Oxford. See Wilson's introduction to *Species journals*, pp. xxv–xxvi.

<sup>55</sup> Lyell 1826, op. cit. (33), 529–32.

<sup>56</sup> Rudwick 1970, op. cit. (2), 24–5.



explained the phenomena, that option was obviously the one he took up. And, once he was embarked on his anti-progressionist course, evidence in his favour began to pile up, until, in the 1830s and 1840s, Lyell had drawn up a sound anti-progressionist case. He did not have to say one thing and believe another. His confidence reached its peak in 1851, when he gave his anti-progressionist presidential 'Anniversary address' to the Geological Society,<sup>57</sup> then went into a rapid decline, culminating, in 1855, in the doubts of the *Species journals*. For twenty-five years Lyell found good reasons for believing in what most of his contemporaries regarded as a very dubious proposition. In December 1859, shortly after reading Darwin's *Origin*, Lyell wrote in his journal:

I have long had a suspicion, growing gradually to a conviction that if ever the development in time could be established with any approach to that completeness which its most strenuous advocates claim for it, the transmutation hypothesis would also prove true.<sup>58</sup>

Lyell's 'suspicion' is, I believe, traceable to his first reading of the 'transmutation hypothesis' of Lamarck.

#### Principles of geology<sup>59</sup>

I have suggested that Lyell's published work took on a life of its own. Few readers seem to have grasped the anti-evolutionary structure of Lyell's discussion of the fossil record (volume i, chapter IX). If reviewers or critics mentioned it at all, they were usually puzzled by Lyell's adoption of an interpretation that was so plainly at odds, as they saw it, with the phenomena.<sup>60</sup> But Whewell, while overlooking Lyell's anti-progressionist chapter, in his review of volume one, did grasp its significance a year later. Then, in his review of volume two of *Principles*, Whewell approves Lyell's refutation of Lamarck and goes on to attack the idea of transmutation in general. He wrote:

The transmutationist endeavours to account, by physiological laws, for the successive appearance and extinction of different races of animals,

<sup>57</sup> Lyell, 'Anniversary address of the President', *Quarterly journal of the Geological Society of London*, vii (1851), pp. xxv-lxvii.

<sup>58</sup> *Species journals*, op. cit. (2), p. 337. Cf. *PG* (10th edn., 2 vols., 1867-8); Lyell there speaks of 'a theoretical question of surpassing interest with which the palaeontologist has been busily engaged since the time of Lamarck, namely, whether it is conceivable that each fossil fauna and flora brought to light by the geologist may have been connected, by way of descent or generation, with that which immediately preceded it.' (i. 167). In 1827 Lyell had resolved to show that there was no such connexion.

<sup>59</sup> Dr Rudwick has rightly criticized historians who, in searching for Darwin's 'forerunners', have misleadingly isolated the section of Lyell's work that deals with the organic world from its context within the overall strategy of the *Principles*; see Rudwick 1970, op. cit. (2), 5. Nonetheless, having established the sense in which I think Lyell's attitudes towards species were integrated into his overall project, I believe I am justified in concentrating on just one or two aspects of *Principles*.

<sup>60</sup> E.g. [G. P. Scrope], '*Principles of geology* . . . vol. i. (1830)', *Quarterly review*, xliii (1830), 411-69 (467). A. Sedgwick, '*Presidential address to the Geological Society of London* . . .', *Proceedings of the Geological Society of London*, i (1831), 281-316 (305-6). W. D. Conybeare to Lyell, February 1841, in Rudwick 1967, op. cit. (2), 281-2.

of which the earth offers the record. It has appeared that this attempt is utterly futile, even if the zoological speculator were allowed to assume such a succession of animals as that to which his theory points. We need not, therefore, explain how entirely unlike such a succession is to the geological one, for Mr Lyell has shown, in his former volume (chap. IX), that the evidence of what has been called the successive development of organic life, as derived from the earth's strata, fails altogether . . . So far as we *can* trace the history of the new species and families which have inhabited the earth, they have made their appearance exactly *as if* they had been placed there, each by an express act of the Creator—each provided by its Author with such powers and habits, with such organs and constitutions as adapted it precisely to the condition of things in which it was to live.<sup>61</sup>

Lyell must have been gratified that the anti-evolutionary significance of his discussion had been appreciated. But Whewell's response was not typical. The future evolutionists read a quite different story from *Principles*. Herbert Spencer, for example, after reading what Wilson confidently calls Lyell's 'devastating criticism'<sup>62</sup> of Lamarck, decided that Lyell had expounded transmutation so plausibly that the exposition, rather than Lyell's subsequent rejection, commanded assent.<sup>63</sup> And we have the testimonies of Wallace, Hooker, Huxley, Asa Gray, and Darwin himself,<sup>64</sup> all saying that *Principles* stimulated evolutionary thought, and none of them ever mentions being deflected from his work by Lyell's anti-progressionism. The testimony of Sedgwick, who of course profoundly disagreed with Darwin, is also interesting. In 1865, when Lyell was well on the way to accepting the principle of evolution, Sedgwick wrote to a friend: 'Lyell has swallowed the whole theory, at which I am not surprised—for without it, the elements of geology, as he expounded them, were illogical.'<sup>65</sup> Even Whewell, in the review just quoted, was still slightly uneasy, although he said that Lyell's defence against transmutation was secure, for Whewell also said that merely by discussing transmutation, Lyell might encourage unwelcome speculation: as Whewell put it,

<sup>61</sup> [W. Whewell], 'Principles of Geology . . . vol. ii. (1832)', *Quarterly review*, xlvii (1832), 103–32 (117); Whewell's italics.

<sup>62</sup> Wilson 1971, op. cit. (2), 43.

<sup>63</sup> H. Spencer, *Autobiography* (2 vols., London, 1904), i. 176.

<sup>64</sup> For Wallace see: *Darwin-Wallace celebration* (London: Linnean Society, 1908), 118, and McKinney 1972, op. cit. (5), pp. 32–43, 49–50, 54–9. McKinney gives a detailed account of how Wallace used Lyell's work. For Hooker see J. D. Hooker, 'Presidential address to the 1868 Norwich British Association meeting', *Report of the 38th meeting of the British Association for the Advancement of Science* (London, 1869), pp. lix–lxxv (lxxi). For Huxley see *LLD*, ii. 190–4, and T. H. Huxley to C. Lyell, 25 June 1859, in Huxley, op. cit. (53), i. 173–4. Huxley urges Lyell to accept Darwin's forthcoming theory as 'it is the logical development of Uniformitarianism and that its adoption would harmonise the spirit of Palaeontology with that of Physical Geology'. For Gray see A. Gray, 'Natural selection not inconsistent with natural theology' (1860), reprinted in A. H. Dupree (ed.), *Darwiniana. By Asa Gray* (Cambridge, Mass., 1963), pp. 72–145 (84–5, 90). For Darwin see: Darwin to L. Horner, 29 August 1844, in F. Darwin and A. Seward (eds.), *More letters of Charles Darwin* (2 vols., London, 1903), ii. 117, hereafter cited as *MLD*; Darwin to Lyell, 6 March 1863, *LLD*, iii. 12. Darwin says: '... I respect you as my old honoured guide and master.'

<sup>65</sup> Sedgwick to Dr Livingstone, 16 March 1865, in J. W. Clarke and T. McK. Hughes (eds.), *The life and letters of the Reverend Adam Sedgwick* (2 vols., Cambridge, 1890), ii. 411–12.

transmutation 'is one of those conjectures easily suggested to the spirit of wide and venturous speculation which these studies almost irresistibly call into action'.<sup>66</sup> Whewell was unconsciously prophetic: the anti-evolutionary device built into *Principles* was not powerful enough to halt the speculation which the book's overall programme of naturalistic explanation set in motion.

Before considering in detail Lyell's discussion of organic progression and transmutation in *Principles*, brief consideration must be given to his treatment of climatic change and the theory of the cooling earth. According to the writers who see *Principles* as an exposition of a thorough-going 'steady-state' account of the history of the earth, Lyell was obliged to reject not only organic progression, but also inorganic progression, because a steady-state system would have been undermined if it could have been shown that earth history had followed the irreversible, directional path of gradual refrigeration.<sup>67</sup> I have argued that Lyell's rejection of organic progression did not derive from his conception of the obligations of his principle of reasoning. Is the same true of his rejection of the notion of overall refrigeration? And what was the precise nature of that rejection? Lyell's discussion of cooling-earth theories, throughout the twelve successive editions of *Principles* deserves a separate study, but a brief discussion may here be useful.

In 1830 Lyell's discussion of refrigeration seems, at first sight, to have been governed less by objections to directionality, as such, than by a desire to exclude the speculations of what he called 'cosmogonists'<sup>68</sup> from geology, for Lyell regarded theories about the origin of the planet as generally fanciful and inherently conjectural. Lyell complains that those who use the idea of a cooling earth as a device for supposedly 'explaining' climatic change, are thereby 'relieved from all necessity of enquiry into the present laws which regulate the diffusion of heat over the surface [of the earth]'<sup>69</sup> and, according to Lyell, this disregard for 'present laws' has been the great drag on advance in geology. Thirty years later, in *Antiquity of man*, he still insisted that 'it was not till geologists ceased to discuss the condition of the original nucleus of the planet . . . that they began to achieve their great triumphs'.<sup>70</sup> Lyell's own proposal, in 1830, was that, rather than indulging 'fancies in framing imaginary systems for the government of infant worlds', geologists should fix their thoughts 'steadily on the connection at present between climate and the

<sup>66</sup> Whewell 1832, op. cit. (61), 109.

<sup>67</sup> E.g. Rudwick 1970, op. cit. (2), 28–9. Hooykaas 1963, op. cit. (2), pp. 28–9, 36–7.

<sup>68</sup> *PG* (1st edn., 1830), i. 104.

<sup>69</sup> *PG* (1st edn., 1830), i. 105. Dr Rudwick has pointed out that Lyell often misunderstood his opponents' arguments, choosing to see them as far more crude, miracle-laden, and unscientific than they truly were. This tendency is at work here: Lyell attempts to discredit, as 'cosmogonists', all those who employed cooling-earth theories in their explanation of geological or climatic change; see Rudwick 1971, op. cit. (2).

<sup>70</sup> Lyell, *Geological evidences of the antiquity of man* (London, 1863), p. 470. Hereafter cited as *Antiquity of man*.

distribution of land and sea; and if we then consider what influence former fluctuations in the physical geography of the earth must have had on superficial temperature, we may perhaps approximate to a true theory'.<sup>71</sup> Note here that Lyell is proposing only to investigate 'superficial' temperature change. And his conclusion, after brilliantly combining Humboldt's work on isotherms with contemporary knowledge of Secondary palaeontology, is that past geographic change can account for the observed climatic change.<sup>72</sup> At the close of his discussion of climate, Lyell offers some remarks on the 'gradual diminution of the supposed central heat of the globe'. He gives a brief and somewhat perfunctory account of Fourier's and Cordier's work on earth temperature, and concludes by expressing the hope that 'experiments will continue to be made, to ascertain whether there be internal heat in the globe, and what laws may govern its distribution'. But

In the mean time we know that great changes in the external configuration of the earth's crust have at various times taken place, and we may affirm that they *must* have produced *some effect* on climate. The extent of their influence ought, therefore, to form a primary object of enquiry . . .<sup>73</sup>

This is an essentially modest claim: Lyell is merely appealing for parsimony of causes; he is suggesting that we do not need to introduce unknown agents into our calculations when known agents will suffice to explain the phenomena. Perhaps Lyell is claiming even less than that; perhaps he is merely saying that past geographical change should form a primary object of geologists' enquiries. Lyell's position may have been, as Cannon puts it, one of 'true philosophic caution'.<sup>74</sup> However, Lyell's treatment, in subsequent editions of *Principles*, of the question of the cooling earth indicates that the modest claims of 1830 were merely the outworks of an extremely tenacious defence of a belief in a steady-state history of the inorganic world: Lyell certainly did believe that the earth had always maintained, and would continue to maintain, an overall temperature stability. Nearly forty years later, in 1868, when developments in thermodynamics were making it increasingly difficult for him to sustain his belief, Lyell simply made a bold, arbitrary assumption of some as yet unknown providential power which preserves the stability of the earth's temperature. He asks, in the tenth edition of *Principles*: 'why should we despair of detecting proofs of such a regenerating and self-sustaining power in the works of the Divine Artificer?'<sup>75</sup> And here the strength of Lyell's commitment to his belief in the stability of the earth's temperature forces him to call in the unknown to explain away the by then persuasive evidence of fairly rapid refrigeration.

<sup>71</sup> *PG* (1st edn., 1830), i. 104.

<sup>72</sup> *Ibid.*, i. chapters VII–VIII.

<sup>73</sup> *Ibid.*, i. 141–3. Lyell's italics.

<sup>74</sup> Cannon 1960, op. cit. (2), 46.

<sup>75</sup> *PG* (10th edn., 1867–8), i. 143.

But Lyell did not object to the ideas of organic progression and, eventually, evolution on the grounds that such schemes could not be reconciled with his belief in an overall temperature stability in earth history. He saw no inconsistency, and there was none. Although at the highest level, Lyell's distaste both for human evolution and for a slowly freezing earth probably stemmed from what Rudwick has called his faith in 'a universe of perfect and wise design',<sup>76</sup> Lyell's expressed objection to Darwin and Wallace was not that their theory contradicted his own belief in inorganic non-direction, but that it linked man and beast in common descent.

Lyell's discussion of organic progression in the first edition of *Principles* (volume I, chapter IX) is complicated, but basically his argument falls into three sections. First, he sets out to show that there is no 'foundation in fact' for the proposition that 'in the successive groups of strata, from the oldest to the most recent, there is a progressive development of organic life, from the simplest to the most complicated forms'. Secondly, he asserts that man is a recent creation. Thirdly, he attempts to show that this unique event—the creation of man—neither supports the idea of progression, nor need undermine our conviction that 'the system of the world has been uniform from the beginning'.<sup>77</sup>

In elaborating his first point, Lyell relies on the selectivity of fossil preservation to help him through the major difficulty of explaining away problems like the much higher incidence of mammals in modern deposits than in earlier deposits. Then he seizes on fossils that had been found out of their anticipated progressive sequence, and magnifies their importance. For example, he says of the Stonesfield mammals: 'The occurrence of one individual of the higher classes of mammalia, whether marine or terrestrial, in these ancient strata [i.e. lower Oolite] is as fatal to the theory of successive development, as if several hundreds had been discovered.'<sup>78</sup> Here, as in the 1827 letter to Mantell, Lyell is pushing reasonable insights to unreasonable limits. Conybeare and Wallace, among others, challenged Lyell's conclusion. In 1841 Conybeare wrote to Lyell, giving his comments on the sixth edition of *Principles*. In this edition Lyell was able to call upon more examples to illustrate his contention that complex forms had existed in earlier periods than had hitherto been suspected, but Conybeare still affirms that the only reasonable interpretation of the fossil record is to accept that it demonstrates 'a converging series from the most to the least perfect of the Vertebrata'. Conybeare admits that this generalization has sometimes been extended too widely over the whole animal kingdom, but still he is surprised at Lyell's exaggeration of the significance of one or two exceptions to the

<sup>76</sup> Rudwick 1970, op. cit. (2), 33.

<sup>77</sup> *PG* (1st edn., 1830), i. 145.

<sup>78</sup> *Ibid.*, i. 150.

anticipated faunal succession: 'surely', he writes to Lyell, you 'cannot consider the wretched little marsupials of Stonesfield to counterbalance the general bearing of the whole evidence'.<sup>79</sup>

H. L. McKinney, in his detailed study of Wallace, has shown exactly what Wallace thought of Lyell's anti-progressionism. Like Conybeare, Wallace was working from a revised edition of *Principles*—the fourth (1835) edition—and he made comments on chapter IX in his own notebooks on species. He starts by quoting a sentence from Lyell:

'Some of the more ancient Saurians approximated more nearly in their organization to the types of living Mammalia than do any of the existing reptiles' . . . which? just what I want. Lyell says the Didelphys [i.e. Stonesfield mammal] of the Oolite is fatal to the theory of progressive development. Not so if low[ly] organized mammalia branched out of *low* reptiles or fishes. All that is required for the progression is that *some* reptiles should appear before Mammalia & birds or even that they should appear together. In the same manner reptiles should not appear *before* fishes, but it matters not how soon after them. As a general rule let Naturalists determine that one class of animals is higher organized than another, & all that the development theory requires is that *some* specimens of the lower organized group should appear earlier than any of the group of higher organization.<sup>80</sup>

It is plain that Lyell's basically anti-evolutionary interpretation of the fossil record proved to be an insignificant obstacle to Wallace's development, whereas most other aspects of *Principles* contributed directly to the establishment of Wallace's evolutionary theory.

Lyell's argument for the recent creation of man, the second section of his discussion, is extremely shaky, and Conybeare told him so.<sup>81</sup> In flat contradiction to everything he had previously argued concerning fossilization, Lyell almost gratuitously assumes that if man really were ancient, we should have found human relics. He makes only one feeble point in favour of this distinction between human remains and the remains of the rest of the organic world: the fact that man leaves behind him imperishable artefacts. Lyell adopts a double standard here: he says that the absence of human remains in early strata is evidence of man's non-existence, whereas the absence of remains of other complex creatures in early deposits is evidence only of the likelihood of these creatures' non-fossilization, not of their non-existence. This was not, however, quite as arbitrary as it might look. Lyell generally assumes that most species have a limited life-span. Therefore if a species is thriving now, it is almost certain not to have existed when the earliest strata were laid down. Conversely, species whose fossils are found in early strata are not likely to be alive now. Lyell could, then, argue with some plausibility that we

<sup>79</sup> Rudwick 1967, op. cit. (2), 281–2.

<sup>80</sup> Quoted in McKinney 1972, op. cit. (5), p. 41. Wallace's italics. Wallace changes Lyell's wording slightly. McKinney's whole discussion of Wallace's commentary on Lyell is of great interest.

<sup>81</sup> Rudwick 1967, op. cit. (2), 282.

should not expect to find human fossils in the very earliest strata. But, this granted, his distinction between the probabilities of finding human and non-human fossils was still more arbitrary than reasonable. While he could account for, and was pleased to accept at face value, the lack of human remains in the earliest deposits, his insistence that all classes of plants and animals have been represented at all periods of earth history left him with the major difficulty of explaining away the lack of remains, in the earliest strata, of species from the more complex classes like mammals and birds.<sup>82</sup>

In the third section of Lyell's discussion the argument becomes rather difficult to follow. His friend Scrope was surely understating the case when he wrote that Lyell's 'reasoning on this subject is somewhat too wiredrawn'.<sup>83</sup> But from among the convolutions of Lyell's argumentation one thread may perhaps be disentangled and expounded thus: even if we allow that the creation of physical man represented an advance from a sequence of progressively more complex simpler forms that preceded him (and we are not *obliged* to admit this, as this progressive sequence is illusory), we need not even then admit that man was the last step in a progressive series, because what essentially characterized the creation of man was not his more perfect animal form, but the addition of reason to that animal form; and since such an addition has no analogue in the past, the creation of man must be considered as a deviation from the previously established laws of nature. But then, if the creation of man was a deviation, what guarantee have we that there have been no other supernatural deviations in the past, or that there will be none in the future? The answer is, ultimately, 'none'; but the *absolute* uniformity of the order of nature has never been proposed. However, the impact on the rest of the natural world of this unique union of reason and animal form should not be exaggerated; man's impact on the physical world has not been great. As far as the previously established laws of nature are concerned, man's appearance was as small a deviation as the accomplishment of the spiritual objective would permit, and it need not undermine our confidence in the continuing uniformity of nature.<sup>84</sup>

This section of Lyell's argument illustrates his dilemma: he was committed to showing that the origination of all natural phenomena—whether a rock stratum or the creation and extinction of species—could be explained naturalistically. Yet one particular phenomenon, man, who was clearly in most respects as natural a phenomenon as a sparrow, had to be singled out and shown to be the direct result of God's intervention. There was nothing odd about wanting a special place for man: Lyell was entirely typical of his, and many subsequent generations. What

<sup>82</sup> *PG* (1st edn., 1830), i. 154–5. Cf. *PG*, ii (1832), 253–71.

<sup>83</sup> Scrope, *op. cit.* (60), 467.

<sup>84</sup> *PG* (1st edn., 1830), i. 155–65.

makes Lyell stand out is his struggle to preserve this distinction within an explanatory project whose whole impetus tended to obliterate the very distinction he wished to make. He made two defences in his 1830 presentation against the possibility of man's being regarded solely as a natural phenomenon: first, he hoped to show that since man was not preceded by an orderly sequence of related forms, his origination must have been the result of special, independent creative attention, and, secondly, he said that even if man did stand at the head of a sequence of increasingly complex forms, there are distinct qualities—reason, spirituality, and an ethical and moral capability—unique to human beings that still demonstrate some sort of special supernatural creative attention. When the first line of defence fell, when Lyell had to accept progression as an established fact, Lyell occupied the second, and never left it.

Volume two of *Principles* opens with a sustained discussion of Lamarck and transmutation.<sup>85</sup> Lyell subjects the more outlandish aspects of Lamarck's theory to a stern attack, but Lyell's presentation of the species problem, as it stood in 1832, is, in effect, if not intention, far less damning of the idea of transmutation than the rejection of Lamarck might imply. And much of the exposition of Lamarck is so beguilingly written that it seems that part of Lyell responded to the attractions of evolution. After all, as Lyell himself admits, transmutation provides naturalists with a system which dispenses with 'the repeated intervention of a First Cause',<sup>86</sup> and the main point of *Principles* was to do just that. Indeed, in the 1863 letter to Darwin already quoted (note 52), Lyell admits to having been impressed by Lamarck. One wonders what Lyell would have made of the idea of evolution in 1830 if he had been confident that a good case could still have been made out for the uniqueness of man. But, just as it stands, Lyell's preliminary exposition of the attractions of transmutation<sup>87</sup> reads like a scenario for the 1840s and 1850s, with Darwin or Wallace cast in the role of 'the student'.<sup>88</sup>

Lyell rejects Lamarck's particular account of transmutation principally on the ground of the manifest improbability of a creature's being able to produce an entirely new organ as a result of 'the efforts of internal sentiment'.<sup>89</sup> And in succeeding chapters of volume two Lyell piles up weighty objections against transmutation as such. He argues that hybrids are ultimately sterile, that the domestic breeding analogy is invalid, that the limits of variation within species are circumscribed, and that there are inflexible lines of demarcation between species. Also, he puts forward one curiously unlyellian objection: he says that during any

<sup>85</sup> An analysis of Lyell's discussion of species is given in W. Coleman, 'Lyell and the reality of species', *Isis*, liii (1962), 325–38.

<sup>86</sup> *PG* (1st edn., 1832), ii. 18.

<sup>87</sup> *Ibid.*, ii. 1–21.

<sup>88</sup> *Ibid.*, ii. 19.

<sup>89</sup> *Ibid.*, ii. 8–9.



change in a particular station in the environment, those organisms that are already marginally better adapted to the new circumstances will move in and displace the occupiers, long before the original residents could adapt themselves to the new environment.<sup>90</sup> This is an odd argument from a man who usually argued for the extreme slowness and gradualness of geological change. Wallace later picked up the inconsistency.<sup>91</sup>

But against these objections to transmutation we should set other equally important aspects of volume two. Lyell accepts that 'in proportion as the series of known animals grows more complete, none can doubt that there is a nearer approximation to a graduated scale of being', and he accepts that species demarcation within this scale is very difficult.<sup>92</sup> He explores variation within species (chapter III); he discusses distribution (chapters V, VI, VII); he notes the struggle for existence between species,<sup>93</sup> and he discusses the pressures of the environment on species and the causes of their eventual extinction (chapter IX). The whole volume is informed by a sensitive grasp of the complexities and subtleties of ecological balance. The fact that his conclusion was that species are immutable does not conflict with the proposal that in volume two of *Principles*, Lyell was first demolishing one improbable explanation of transmutation but secondly, and inadvertently, staking out research areas for Darwin and Wallace. Lyell's rejection of organic progression in volume one could not, and did not, stifle the trains of thought inevitably set in motion by volume two.

Before leaving *Principles*, one rather neglected aspect of Lyell's thought must be examined: the extent to which he used the argument from design in explaining the adaptation of organisms to their environments.<sup>94</sup> In using adaptation both as an explanatory device and as an illustration of God's providential wisdom, Lyell differed not at all from progressionist natural theologians like Buckland, Sedgwick, and Agassiz. Lyell says that the 'Author of Nature . . . ordained that fluctuations of the animate and inanimate creation should be in perfect harmony with each other.'<sup>95</sup> And purely within the animal world,

the various species of contemporary plants and animals have obviously their relative forces nicely balanced and their respective tastes, passions, and instincts, so contrived, that they are all in perfect harmony with each other.<sup>96</sup>

Swallows were not introduced until there were swarms of flies for them to feed on.<sup>97</sup> 'Providence' put causes into operation to keep the ravages of

<sup>90</sup> *Ibid.*, ii. 173–4. Cf. Coleman, *op. cit.* (85), 335.

<sup>91</sup> See McKinney 1966, *op. cit.* (5), 346–7; McKinney 1972, *op. cit.* (5), pp. 37–8.

<sup>92</sup> *PG* (1st edn., 1832), ii. 22–3.

<sup>93</sup> *Ibid.*, ii. 131.

<sup>94</sup> See Coleman, *op. cit.* (85), 333–4; Greene 1959, *op. cit.* (5), pp. 252–3, 313–14.

<sup>95</sup> *PG* (1st edn., 1832), ii. 159.

<sup>96</sup> *Ibid.*, ii. 42.

<sup>97</sup> *Ibid.*, ii. 125.

destructive caterpillars within 'due bounds',<sup>98</sup> and the dog's capacity to retrieve game can be explained only by conjecturing that 'such remarkable habits . . . were given with no other view than for the use of man and the preservation of the dog which thus obtains protection'.<sup>99</sup> There is an overt teleology here, and in other parts of Lyell's consideration of the organic world. Plainly Lyell subscribed to the providential interpretation of the adaptation of organism to environment that Wallace and Darwin were to destroy.

We can now perhaps summarize Lyell's views, as revealed in *Principles*, on the introduction of new species. All classes of organisms, including birds, mammals, reptiles, and fish, have been represented at all periods in earth-history. As individual species died out, due to the inexorable change of the inorganic environment, together with pressures exerted by other plants and animals, there were introduced, by means unknown, fresh examples of the established classes to fill the places of the extinguished species. The new species might or might not be more complex than its predecessor, and might, or might not, bear a close structural resemblance to its predecessor: a new reptile might be filled into an ecological niche created by the extinction of a mammal, for example. The only principle that seems to have characterized God's creative activity is the one which ensured that the 'fluctuations of the animate and inanimate creation should be in perfect harmony with each other'. We can surmise that Darwin had Lyell's views on the introduction of species in mind when he entered the following terse comment in his first notebook on transmutation in 1837:

Has the Creator since the Cambrian formation gone on creating animals with same general structure.—Miserable limited view.<sup>100</sup>

And Wallace privately challenged Lyell's view that the plant and animal worlds reveal balance and harmony:

Lyell talks of the 'balance of species being preserved by plants, insects, & mammalia & birds all adapted to the purpose'. The phrase is utterly without meaning. Some species are very rare, others very abundant. Where is the balance? Some species exclude all others in particular tracts. Where is the balance? When the locust devastates vast regions and causes the death of animals and man, what is the meaning of saying the balance is preserved. [Are] the Sugar Ants in the West Indies [as well as] the locusts which Mr Lyell says have destroyed 800,000 men an instance of the balance of species? To human apprehension there is no balance but a struggle in which one often exterminates another. When animals or plants become extinct, where is the balance?<sup>101</sup>

<sup>98</sup> *Ibid.*, ii. 136.

<sup>99</sup> *Ibid.*, ii. 41.

<sup>100</sup> G. de Beer 'Darwin's notebooks on transmutation of species', *Bulletin of the British Museum (Natural History)*. *Historical series*, ii (1959-63), 3-200 (67).

<sup>101</sup> Quoted in McKinney 1972, *op. cit.* (5), p. 38. See also p. 45, where Wallace makes similarly short work of demolishing the notion that small adaptive features in plants and animals prove God's wonderful design.

Although Lyell believed that the whole course of earth-history was providentially directed, in practice he makes a distinction between the ways events are brought about in the organic and inorganic worlds. In his presentation it is obvious that there is no divine superintendence of each earthquake, or the countless factors that together produce the erosion of a coastline, even though, in Lyell's view, processes in the inorganic world do tend toward providential ends. Indeed, *Principles* set out to show that inorganic geological processes, past, present and future, could be exhaustively explained without invoking supernatural interventions. But Lyell's presentation of the way in which he thought species have been fitted into the pattern of geographic change indicates a somewhat ambivalent conception of God's modes of operation in the organic and inorganic worlds. He elaborates his idea in the famous 1836 letter to J. F. W. Herschel. In this letter Lyell pictures species as being created in 'accommodation to the changes which must continue in the inanimate and habitable earth'. His picture is of a God who has complete foreknowledge of events in the inorganic world, but who seems to exercise no immediate personal control over those events. It is almost as if God is obliged to manufacture his new creations so as to be perfectly adjusted to the random change which forever unrolls in his own world. Lovejoy expresses the idea perfectly: 'while . . . miraculous interpositions were taking place in order to keep the organic kingdom in a going condition, the Creator was not for a moment allowed, by most . . . geologists (including . . . Lyell . . .) to interfere in a similar manner in their own particular province of the inorganic processes.'<sup>102</sup> Lyell says, in his letter to Herschel, that 'The Presiding Mind' shows amazing skill in fitting the original individual, or pair, of each species into the pattern of inexorable environmental change, by ensuring that their food supply is ready for them, and that they, and their descendants—which have a built-in capacity for limited adaptive variation in response to environmental change—are equipped to withstand all the pressures of the inorganic environment and the depredations of other species for as long as the species is destined to survive, and no longer. To illustrate the astonishing foresight and skill demonstrated in the creation of species, Lyell compares God's knowledge with the skill Babbage would have to possess were he able to forecast 'the place of every wheel in his new calculating machine at every movement'.<sup>103</sup> Lyell does not develop his discussion fully, but it is clear that while he was content to see all traces of the supernatural banished from explanations in the inorganic world, he wished to retain frankly teleological, very traditional modes of explanation for the organic world, even though he believed that both worlds were, in the long term, directed providentially.

<sup>102</sup> Lovejoy, *op. cit.* (5), p. 365.

<sup>103</sup> Lyell to Herschel, 1 June 1836, in *LLJ*, i. 464–9.

But here and there Lyell hints that species creations may be ‘carried on through the intervention of intermediate causes’.<sup>104</sup> Now unless transmutation of some sort was accepted, any belief in the production of new species by secondary causes which have acted undisturbed since life first began, was hollow. Considered as a vague statement of faith in the uniformity of nature, it has meaning; but as a move towards the establishment of a naturalistic mechanism for species origination, it has very little. Lyell claimed, in *Principles*, that each species need not be morphologically related to its immediate local predecessor or contemporaries, and by doing this, he cut himself off from the *only conceivable* mode of naturalistic species origination. Lyell may have blustered about the sufficiency of intermediate causation, but so long as his picture was of, say, a pair of elephants suddenly appearing in a world in which all species were utterly immutable and in which, in any case, there was no necessary resemblance between the elephants and the forms that preceded them in the same place on the globe, then Lyell, if he thought about it, must have been contemplating a miracle of the most supernatural kind:<sup>105</sup> there was no conceivable naturalistic means of non-evolutionary species origination that would not have made nonsense of all contemporary beliefs about the laws governing matter. Thus Huxley’s rather unkind jibe at Whewell for adopting a position that might force him into contemplating ‘the sudden concurrence of half-a-ton of inorganic molecules into a live rhinoceros,’<sup>106</sup> applied equally well to Lyell. Obviously, this was not a position that Whewell, or Lyell, or anybody else wished to advertise, but it was, nonetheless, a position that their theories—and especially their non-progressionism—obliged them to occupy. It is obvious that part of the calamity of 1859 was due to the poverty, or complete lack, of alternatives to the theory of Darwin and Wallace. Ignorance masquerading under faith in the adequacy of unspecified and, one suspects, unspecifiable intermediate causes was no real substitute for a sound naturalistic theory which depended on nothing that could not, at least in principle, be observed in nature. Of course Lyell too said that since creations have occurred in the past, they must be going on in the present and are thus, in principle, observable; but what could he possibly have believed that we might see if we were fortunate enough to be on the spot when a new species came, ‘naturalistically’ into being?

However, in practice, these occasional statements of faith in the sufficiency of uninterrupted secondary causation from the dawn of life onward meant no more than a confession of ignorance: Lyell just did

<sup>104</sup> *Ibid.*, i. 467.

<sup>105</sup> Lyell occasionally admitted that non-evolutionary species creations must be miraculous. See, for example, *Species journals*, p. 57; *Antiquity of man*, p. 421; letter to Wallace, *op. cit.* (15).

<sup>106</sup> *LLD*, ii. 194. For a discussion of Whewell and species origination, see: W. F. Cannon, ‘The problem of miracles in the 1830s’, *Victorian studies*, vi (1960–1), 5–32; Ellegård, *op. cit.* (5), pp. 12–17.

not know how species originated, and it is unlikely that he ever pursued the implications of his position to their absurd conclusion. Instead, like Whewell, he writes about species 'as if they had been placed there, each by an express act of the Creator',<sup>107</sup> and even in the letter to Herschel, where Lyell expands his views on the origins of species by 'the intervention of intermediate causes', he contradicts himself somewhat by presenting his argument in terms that imply an external agent at work in the creation of species.<sup>108</sup> Lyell also tells Herschel that 'German critics' have attacked him,

saying that by the impugning of the doctrine of spontaneous generation, and substituting nothing in its place, I have left them nothing but the direct and miraculous intervention of the First Cause, as often as a new species is introduced, and hence I have overthrown my own doctrine of revolutions, carried on by a regular system of secondary causes.<sup>109</sup>

The German critics were right.

What is the essential difference, then, between Lyell's proposals concerning species origination, and the views of the progressionists? In both cases an intimate bond between organism and environment was postulated: in both cases organic change closely paralleled inorganic change: in both cases the introduction of new species was, in the last resort, miraculous. Why then could Lyell not allow that classes of organisms had been introduced according to an ascending scale of complexity? His public objection was that an ascending scale through time implies a mysterious and gratuitously assumed law of progressive development. That is to say, according to a supposed law of progressive development, mammals were not introduced into Carboniferous environments, for instance, because in such primitive conditions of the globe only relatively simple forms like ferns could exist. And then, as conditions in the globe improved and quietened down, successively more complex creatures, culminating in man, could be introduced. In opposition to this, Lyell claimed that during the earliest periods of which we have geological records, conditions were no more 'primitive' than are certain areas of the globe today, but were capable of supporting the most complex forms of life. In other words, Lyell said that wherever the geological and palaeontological evidence enables us to reconstruct, imaginatively, a past environment capable of supporting mammals, there mammals may reasonably be expected to have lived.<sup>110</sup> Now this is a perfectly coherent proposal, and during the 1830s and 1840s, Lyell was

<sup>107</sup> Whewell 1832, op. cit. (61), 117.

<sup>108</sup> *LLJ*, ii. 468–9. The grammar of the passage indicates the presence of an agent who plans the details of species' structure, who 'foresees', 'makes', and 'confers' adaptive variations.

<sup>109</sup> *Ibid.*, ii. 467. I have not identified the German critics.

<sup>110</sup> Lyell's own experience of the peculiarities of island floras and faunas, gained during a trip to the Canary Islands in 1853–4, seems to have shaken his confidence in this view. There, he found islands which had existed at least since Miocene times yet which had no indigenous land mammals. See Wilson, in *Species journals*, pp. xxxvii–xli.

able to produce evidence of classes of organisms being found in earlier and earlier formations, and, moreover, he was able to assemble a good case to show that within classes there had not been an invariable development through time: some Silurian fish were as 'developed' as fish swimming in a modern sea. There can be no doubt that Lyell, once he had committed himself to non-progressionism, earnestly believed in his own reasoning, and this reasoning was coherent. But there are indications of his other motives for rejecting progression.

First, Lyell was faced with the problem of showing that all contemporary generalizations about the history of life, as revealed by the fossil record, were entirely false; and, as Conybeare pointed out, this was sheer obfuscation. Secondly, we know that Lyell associated the ideas of progression and transmutation very closely. In volume two of *Principles* he wrote: 'the theory of progressive development arose from an attempt to ingraft the doctrines of the transmutationists upon one of the most popular generalizations in geology.'<sup>111</sup> Lyell's later writings develop his insight into this perilous link. Thirdly, in his earliest exposition of progression, Lyell emphasized that any progressionist sequence places man, temporally and anatomically, in close juxtaposition to the apes.<sup>112</sup> And fourthly, when Darwin and Wallace presented him with a theory which dispensed with mysterious laws of progression, or assumptions about primitive conditions of the globe, and when they also offered him a theory which would account for phenomena like the persistence of simple types or the emergence of less complex forms within classes, he was still unmoved. Darwin and Wallace removed the scientific objections to progression, but they intensified Lyell's fears for man's dignity.

*The collapse of anti-progressionism, and Lyell's ambivalent acceptance of Darwin's theory*

The good reasons for objecting to organic progression and the real fears for man's dignity run side by side in Lyell's 'Anniversary address' to the Geological Society in 1851.<sup>113</sup> In the twenty years that had elapsed since the first volume of *Principles* was published, the work of Sedgwick and Murchison had extended the stratigraphical column beyond Lyell's 'Grauwacke and transition limestone', down through the Silurian and Cambrian Systems. Consequently Lyell had a greater problem than in 1830 in denying that mammals were absent from the earliest formations, and in his address he judiciously refrains from pushing his theory of selective deposition to its very limits: he never clearly says that mammals abounded during the Cambrian period. But his position is substantially the same as in 1830. He still maintains that selectivity of

<sup>111</sup> *PG* (1st edn., 1832), ii. 60.

<sup>112</sup> Lyell 1826, op. cit. (33), 513.

<sup>113</sup> Lyell 1851, op. cit. (57).

deposition will account for the absence of fossils of mammals, birds, insects, and flowering plants in the earlier strata, and he maintains that when examples of these classes are found, they are often as developed as their modern counterparts. He points out, for example, that sharks, 'which Prof. Owen places at the top of the highest of eleven orders of fishes, ranged in ascending order of organization', have been found in Upper Silurian deposits.<sup>114</sup> And, in a typical application of this theory of selective deposition, he suggests that the reason why no flowering plants have been found in Carboniferous deposits is that these deposits are deltaic and therefore not likely to include 'the contemporaneous plants of the mountainous or Alpine regions'.<sup>115</sup> Lyell sums up his attitude towards selective fossilization by declaring:

If we infer the poverty of the flora or fauna of any given period of the past, from the small number of fossils occurring in ancient rocks, we are bound to remember that it has evidently been no part of the plan of Nature to hand down to us a complete or systematic record of the former history of the animate world.<sup>116</sup>

But beside this reasonable, if extreme position runs a thread which leads to perhaps less scientific sources of objection. It is worth quoting a whole paragraph. Lyell here is enumerating those points of the progressionist case that he wishes to challenge in his address. He proposes to examine whether

in the animal kingdom the cephalopod, fish, reptile, bird and warm-blooded quadruped made their appearance upon the earth, one after the other,—the *Orthoceras* occurring in the oldest Silurian strata, the fish in the upper Silurian and Devonian, the reptile in the carboniferous, the bird in the triassic, the first quadrumanous mammal in the tertiary, and lastly, man in the post-tertiary era;—*a series, if established, which would seem almost irresistibly to lead us to the inference that a gradual advance towards a more perfect organization more and more resembling man, was intimately connected with geological chronology, the creation of the human species constituting the last term in a regular series of organic developments.*<sup>117</sup>

It is as if Lyell was giving the progressionists a muffled warning of the dangers of their position. In four separate places in the address Lyell emphasizes his own opinion that no such progressive sequence can be established;<sup>118</sup> he reasserts his anti-evolutionary picture of the history of life. But significantly, Lyell also says, in one of his denials of progression, that even if

the doctrine of successive development had been palaeontologically true, as I have endeavoured in this discourse to show that it is not . . . still I should have been wholly unable to recognize in his [man's] entrance upon the earth the last term of one and the same series of developments.

<sup>114</sup> *Ibid.*, p. lii.

<sup>115</sup> *Ibid.*, p. xlii.

<sup>116</sup> *Ibid.*, p. lxxvii.

<sup>117</sup> *Ibid.*, p. xxxix; my italics.

<sup>118</sup> *Ibid.*, pp. xxviii, lxx, lxxii, lxxiii.

Even then, the creation of man would rather seem to have been the beginning of some new and different order of things.<sup>119</sup>

Lyell is here preparing the second line of defence for man that he had already sketched out in *Principles*. If progression, and hence, in Lyell's prophetic view, inevitably evolution had to be admitted, man's dignity might still be preserved.<sup>120</sup>

Although Lyell publicly maintained his anti-progressionism throughout the 1850s, his private journals indicate that his conviction was weakening. These *Species journals*, which Lyell kept from 1855 to 1861, are a rich source of material, and no attempt is being made here to assess them exhaustively.<sup>121</sup> Just those points which bear on the theme of this article are drawn out from among the many pages of discussion on classification, distribution, variation and other topics. It should be pointed out, however, that the journals cannot be neatly separated into 'scientific' as against 'theological' divisions. Lyell recognized no such rigid distinction; he couples, often on the same page, examination of hard data with speculations on the philosophical and ethical problems raised by evolution.

First, it is plain that Lyell had indeed anticipated the fate of the progressionists. He says that the 'two theories of transmutation & progression approach far more near than the progressionists are willing to confess, or desirous of stating'.<sup>122</sup> And in November 1859, a few days after reading the proofs of the *Origin*, Lyell wrote:

They [the progressionists] approach what they desire to avoid, but this may show that the current of facts is too strong & that the wind which fills their sails, the gale of their wishes & preconceived desires to isolate man, is too feeble to enable them to keep clear of the rocks on which their old theology will be wrecked, tho' doubtless to create a new & improved one.<sup>123</sup>

Lyell's sails too were driven by the gale of a preconceived desire to isolate man, and his own voyage was, in the end, just as unsuccessful. The progressionists' claim rested on the perilous assumption that although there is a smooth gradation both in time and structure between man and the higher mammals, the transitions between species had been effected by separate exertions of divine creative power. Lyell, on the other hand, consistently denied any such smooth gradation, thereby setting naturalists the impossible task of seeking naturalistic origins for species, whose origins, since they had no necessary connexion with their predecessors,

<sup>119</sup> *Ibid.*, p. lxiii.

<sup>120</sup> Lyell's address called forth a sarcastic and highly critical review from Richard Owen. See [Owen], 'Lyell on life and its successive development', *Quarterly review*, lxxxix (1851), 412-51.

<sup>121</sup> McKinney 1972, *op. cit.* (5), gives an excellent discussion of aspects of Lyell's journals; see pp. 97-116.

<sup>122</sup> *Species journals*, p. 238.

<sup>123</sup> *Ibid.*, pp. 292-3. Cf. pp. 222-3.



could only have been supernatural. The *Species journals* reveal Lyell examining the work of naturalists who had ignored, swiftly rejected, or substantially altered his ingenious anti-progressionism.

Secondly, Lyell admits his real objection against transmutation, notwithstanding the cogent arguments of *Principles*. In June 1859, in an entry which reviews his own former treatment of man, he admits that 'the chief objection to the hypothesis of transmutation was naturally the inseparable connexion which it established between Man & the lower animals'.<sup>124</sup> But Lyell recognizes that there simply was no serious alternative to evolution by descent; in 1856 he wrote: 'the successive creation of species is a perpetual series of miraculous interferences',<sup>125</sup> and plainly his former belief in miraculous interferences could not be defended against a sound theory of naturalistic species origination.

Lyell's debate with Darwinism, as set out in the journals, is less an objective appraisal of the merits of Darwin's case than a record of the intractable spiritual problems raised by transmutation. Lyell at one point even questions the point of his own life's work:

November 1, 1858. If the geologist dwelling exclusively on one class of facts, which might be paralleled by the existing creation [arrives] at conclusions derogating from the elevated position previously assigned by him to Man, if he blends him inseparably with the inferior animals & considers him as belonging to the earth solely, & as doomed to pass away like them & have no farther any relation to the living world, he may feel dissatisfied with his labours & doubt whether he would not have been happier had he never entered upon them & whether he ought to impart the result to others.<sup>126</sup>

If we compare this sad, disillusioned statement with comments Darwin was making in his own notebooks on transmutation, the essential difference between the two men is highlighted. Darwin's reaction to an insight similar to Lyell's, as recorded in Darwin's 1838 notebook, reads:

Man in his arrogance thinks himself a great work worthy the interposition of a deity. more humble & I believe truer to consider him created from animals.<sup>127</sup>

It is this exactly opposite reaction to the idea of man's being an inseparable part of nature that hampered any real exchange of views between Darwin and Lyell. At the deepest level they failed to connect.

Thirdly, it is clear that even when Lyell began to accept the bulk of Darwin's theory, he did not see the theory as a means of expelling the last remnants of the miraculous from scientific explanation. Indeed, one of his frequently expressed reservations about Darwin's theory is that it

<sup>124</sup> *Ibid.*, p. 280.

<sup>125</sup> *Ibid.*, p. 57.

<sup>126</sup> *Ibid.*, p. 196.

<sup>127</sup> de Beer, *op. cit.* (100), 106. See also p. 69 for another statement of Darwin's ready acceptance of an evolutionary ancestry for himself.

'deifies matter'<sup>128</sup> and usurps God's personal activity. Lyell wanted to see natural selection, if it finally had to be accepted, as a description of the mode of God's immediate creativity, rather than as a denial of it—especially, of course, in the case of man. He wrote: 'we ought to consider development as a mode of explaining creation, not of getting rid of it.'<sup>129</sup> This attempt to subsume evolution and natural selection under the providential laws which he believed guided the world presented Lyell with enormous difficulties. His attempts to expound evolution as a mode of God's immanent, creative activity, typically fall into the following pattern.<sup>130</sup> If the tendency of species to vary is a manifestation of divine activity, then God is responsible for both adaptive and maladaptive variations. This means that God produces, using an example Lyell employs, both the idiot and the genius.<sup>131</sup> Alternatively, God produces the adaptive variations while the maladaptations must be explained by some sort of manichean device. In this sort of discussion Lyell reveals himself as an unsophisticated theologian, and consequently it is not surprising that he was unable to solve the immense and intractable problems into which his speculations led him. But what is important is that, unlike Darwin, who seems, in the main, to have been not particularly concerned about the theological implications of natural selection, Lyell was concerned at all times to integrate the truths that the natural world revealed, with his belief in a benevolent creator who had made a world which manifests order and demonstrates benevolence. In the pre-Darwinian world, such an integration was easy—even obvious. Lyell's journals record an early and powerful response to the threat to that integration posed by evolution by variation and natural selection.

In January 1860, two months after he had first studied the proofs of the *Origin*, Lyell made a brief entry into his journal:

It is not from enquiries into the physical world, present or past, that we gain an insight into the spiritual; we may arrive at conclusions unwelcome to our speculations.<sup>132</sup>

It is a revealing comment. Before 1859 natural philosophers in Britain had confidently believed precisely the opposite. They were certain that enquiries into the physical world were bound to elicit clear insights into 'the spiritual', insights so unambiguous that they could be used as a foundation for a defence of the Christian faith. In 1859 that foundation

<sup>128</sup> *Species journals*, pp. 427, 449, 445.

<sup>129</sup> *Ibid.*, p. 168.

<sup>130</sup> *Ibid.*, pp. 458–9.

<sup>131</sup> See, for example, *ibid.*, pp. 88, 355, 358, 427–9.

<sup>132</sup> *Ibid.*, p. 348. This admission came as a response to a passage in W. R. Greg's *The creed of Christendom: its foundation and superstructure* (London, 1851). In the passage that Lyell cites, Greg is not discussing the natural world at all; he is discussing the hypothetical case of a man who is trying to justify a particular belief. Greg writes: 'erroneously conceiving that it [i.e. the belief] must be a product of reason, he diligently looks about to discover the logical processes which have generated it; and clings to the shallowest crudities rather than surrender (as he conceives) the title-deeds of his faith' (pp. 300–1). Did Lyell recognize himself here?

turned to dust, and Lyell's brief statement can stand as an emblem of his recognition of that disaster. Lyell realized that the natural world perhaps no longer could sustain belief: perhaps knowledge of the workings of the natural world even challenged belief. Belief was still possible of course, and some men—Asa Gray and Kingsley, for example—attempted to assimilate evolution into their natural theology; but, in the main, belief had henceforth to be maintained in spite of, rather than because of, what the natural world manifested. Lyell now affirms his faith *despite* his science; he writes: 'if man c<sup>d</sup> be shown to come gradually out of the almost endless or beginningless past, it w<sup>d</sup> not destroy those inner convictions of the heart & soul.'<sup>133</sup>

Such spiritual problems rarely troubled Darwin, and in the exchange of letters that followed the publication of the *Origin* there was no real engagement over the issues that troubled Lyell. We are hampered here by the apparent loss of most of Lyell's letters, and have to infer his side of the exchange from Darwin's replies. But, without making unwarranted assumptions about Lyell's letters, the lack of any sustained debate on levels other than the exchange and discussion of data is marked.<sup>134</sup> On one side, Darwin was waiting for a simple ratification of his theory—a ratification that never came; while from Lyell's side came metaphysical questions that do not seem to have much interested Darwin. Lyell plainly states his reservations in the first letter he wrote to Darwin after reading proofs of the *Origin*; he says: 'It is this which has made me so long hesitate, always feeling that the case of Man and his Races, and of other animals, and that of plants, is one and the same.'<sup>135</sup> The tone of Darwin's replies is established in the letter he wrote on 11 October 1859, in reply to a longer (missing) letter in which Lyell had evidently made more extensive criticisms of the *Origin*. Darwin squares up to the sections of Lyell's letter in which scientific problems were raised, but when he comes to the section of Lyell's letter dealing with assumptions about creative power, Darwin is somewhat puzzled. Lyell has apparently asked him: 'Must you not assume a primeval power which does not act with uniformity, or how could man supervene?' This hope for a distinction between man and the rest of nature has a long heritage in Lyell's thought; but to Darwin it is obscure and unnecessary, and his reply to Lyell, after an affirmation of the capacity of natural selection to account for the emergence of 'intellectual powers' is that 'I would give absolutely nothing for the theory of Natural Selection, if it requires miraculous additions at any one stage of descent.'<sup>136</sup> For Darwin, it was all or nothing; he offered Lyell neither comfort nor understanding.

<sup>133</sup> *Species journals*, p. 233.

<sup>134</sup> See Young, *op. cit.* (19), 442–503, where Young gives an account of Darwin's response to various attempts, including Lyell's, to reconcile evolution with the tenets of natural theology.

<sup>135</sup> Lyell to Darwin, 3 October 1859, in *LLJ*, ii. 325.

<sup>136</sup> Darwin to Lyell, 11 October 1859, in *LLD*, ii. 210–11; cf. ii. 174, 176–7.

As the correspondence develops, Darwin grows blunter in his contributions to what he called 'our quasi-theological controversy',<sup>137</sup> and is very off-hand with Lyell's worries about man. In January 1860 he added this stinging postscript to a letter:

P.S. *Our* ancestor was an animal which breathed water, had a swim bladder, a great swimming tail, an imperfect skull, and undoubtedly was a hermaphrodite!

Here is a pleasant genealogy for mankind.<sup>138</sup>

And a few months later he wrote:

I am sorry to say I have no "consolatory view" on the dignity of man. I am content that man will probably advance, and care not much whether we are looked at as mere savages in a remotely distant future.<sup>139</sup>

Darwin's casual attitude stands in marked contrast to the knotty spiritual inner debate that Lyell was conducting in his journals at this period.<sup>140</sup>

### *The Antiquity of man and the tenth edition of Principles*

The speculations of the *Species journals* reached a sort of culmination in the *Antiquity of man*, which Lyell published in 1863. W. Irvine has written that in this book Lyell 'enunciated an elaborate ambiguity in five hundred pages'.<sup>141</sup> Irvine's judgement is amply justified. In the book that was expected to clarify Lyell's views on Darwinism, Lyell managed to leave himself uncommitted. The *Antiquity of man* is Lyell at his most elusive.

During his preparation of *Antiquity of man* Lyell corresponded with Huxley, who was preparing his own *Evidence as to man's place in nature* (1863). In one of his letters to Huxley, Lyell gives his comments on first proofs of Huxley's section 'on the relations of man to the lower animals'. Lyell criticizes Huxley for being unnecessarily severe on those who cannot readily accept that man has descended from the lower animals, saying that the offending passage 'is not in good taste'.<sup>142</sup> Then he goes on:

the compensation of future progress will be poor comfort to most of your readers blinded, I suppose, as I am "by traditional prejudices".—I forget the exact words of Popes line about the angels

"Who view a Newton as we view an ape"

but if an angelic descendent, when farther developed views us in this

<sup>137</sup> Darwin to Lyell, 15 April 1860, in *LLD*, iii. 303.

<sup>138</sup> Darwin to Lyell, 10 January 1860, in *LLD*, ii. 266; cf. *MLD*, i. 191–4.

<sup>139</sup> Darwin to Lyell, 4 May 1860, in *LLD*, ii. 262. Francis Darwin dates this letter 4 January 1860, but the American Philosophical Society Library dates it 4 May 1860. The latter date seems more likely.

<sup>140</sup> *Species journals*, pp. 378–83.

<sup>141</sup> Irvine, op. cit. (5), p. 142. Irvine's book contains a sensitive account of the relationship between Darwin and Lyell after 1859.

<sup>142</sup> Huxley seems to have responded to Lyell's criticism by modifying the passage concerned; see Huxley to Lyell, 17 August 1862, in *Life and letters of Thomas Henry Huxley*, op. cit. (53), i. 200. The passage in question closes Huxley's essay 'On the relations of man to the lower animals'. See Huxley's *Man's place in nature and other anthropological essays* (London, 1894), pp. 151–6.

light it will not comfort our wounded feelings in having lost the noble pedigree which we dreamt of.<sup>143</sup>

When Lyell eventually published his own *Antiquity of man*, it was evident that he had not yet managed to overcome his 'traditional prejudices'.

As a preliminary to his discussion of Darwin's theory, Lyell sketches in his own version of the history of what he calls 'the twin branches of Lamarck's development theory, namely, progression and transmutation',<sup>144</sup> adding, a few pages later, that perhaps 'the dangers apprehended from transmutation arise from the too intimate connection which it tends to establish between the human and merely animal natures', and that progressionists have never realized that facts would 'be discovered which would imply a material connection between the outgoing organisms and the incoming ones'. Lyell also emphasizes that progressionists have always insisted on a too simplistic pattern of progression, and he goes on to give examples of fossil finds that have vindicated his own more conservative position. But he concludes by accepting once more what he had himself believed before he read Lamarck; he admits that a theory of broad overall complexification of organic forms through time is now 'an indispensable hypothesis.'<sup>145</sup>

Then Lyell expounds Darwin's theory (chapter XXI), showing how its adoption would solve a number of outstanding problems, such as the significance of rudimentary organs, the peculiarities of insular floras and faunas, the striking resemblance between embryos of different species, and the similarities between extant and extinct floras and faunas in particular localities. Another of the attractions of Darwin's theory, in Lyell's view, is that 'it enables us to dispense with a law of progression as a necessary accompaniment of variation',<sup>146</sup> thereby remedying the great deficiency in Lamarck's account of transmutation. Darwin's theory will explain the complex, branching system of general complexification, occasional retrogression, and some persistence of unaltered forms. Lyell then considers alternatives to Darwin's theory. There are no serious contenders: 'what we term "independent creation", or the direct intervention of the Supreme Cause, must simply be considered as an avowal that we deem the question to lie beyond the domain of science.'<sup>147</sup> In the next chapter (chapter XXII), he raises possible practical objections to Darwin's theory. Where are the fossils of the intermediate forms? Why have not seals or bats become land animals on islands where there were no resident mammals to prevent their entry on to the land? How do simple forms persist unchanged? Lyell answers each of

<sup>143</sup> Lyell to Huxley, 9 August 1862, Huxley papers, 6.66, Imperial College of Science and Technology, London. The line from Pope, which Lyell only slightly misquotes, is from Pope, *Essay on man* (1733-4), epistle II, line 34.

<sup>144</sup> *Antiquity of man*, op cit. (70), p. 395.

<sup>145</sup> *Ibid.*, pp. 405-6.

<sup>146</sup> *Ibid.*, pp. 412.

<sup>147</sup> *Ibid.*, p. 421.

these objections, relying on the imperfection of the fossil record, his own observations of island floras and faunas, and on his own vast experience of fossil conchology.

These two chapters bring Lyell close to acceptance of Darwinian evolution; but he cannot accept the direction of his own reasoning, and so the concluding chapter becomes an elaborate, obscure, and protracted exercise in beating about the bush. The problem glimpsed in 1827 had now to be tackled. He states the problem: 'will not transmutation, if adopted, require us to include the human race in the same continuous series of developements, so that we must hold that Man himself has been derived by an unbroken line of descent from some one of the inferior animals?'<sup>148</sup> Any exposition of the argument here will fail to do justice to the vagueness and ambiguity of Lyell's actual presentation. Whereas formerly Lyell had always carefully weighed and judged arguments, here he shelters behind quotations from other writers, seldom making it clear whether or not the quotations represent his own views. For example, he outlines Huxley's contention that the human brain differs from the apes' only in size and not in structure. He follows this with a quotation from Rolleston, in which Rolleston argues that the difference in size between man's brain and the apes' is sufficiently great as to amount to 'a difference in kind'. Without developing this possible objection, except to introduce a suggestion that the presence of the 'soul' may be more important than the sheer size of man's brain, Lyell moves on, leaving the reader unsure of Lyell's precise position with regard to the physical relationship between man and the higher mammals.<sup>149</sup>

Lyell leaves the question of man's physical relationship with the rest of the higher mammals hanging in the air, and similarly he does not make an unequivocal personal statement about man's relationship with the beasts once man's moral, intellectual, and religious qualities are considered. However, we can be fairly confident that here Lyell stood firmly behind the writers he pushes forward to speak for him, since their views coincide with the views expressed by Lyell, both in his private journals, and in the first edition of *Principles*. But Lyell makes curious choices of spokesmen. One is Henry Hallam, and the other is J. B. Sumner. Neither Hallam's *Introduction to the literature of Europe*, nor Archbishop Sumner's *Records of Creation* is a work of science, yet Lyell seems to have found in these works expressions of his own opinions. In the extended passage from Hallam that Lyell prints, Hallam argues that 'moral and physical evil in mankind' may be traceable to the 'partial inconsistencies' and 'anomalies' which arise from the inevitable conflict between man's animal nature—a nature that man shares with the rest of the animal world—and man's 'intellectual endowments', which are

<sup>148</sup> *Ibid.*, pp. 472–3.

<sup>149</sup> *Ibid.*, pp. 491–3.

special gifts of God.<sup>150</sup> There is an echo here of the problems raised in the *Species journals*. The choice of Sumner is perhaps even more illuminating. *Records of Creation*, published in 1816,<sup>151</sup> seems to have been fairly influential in Lyell's development. In his chapter on 'the Wisdom of the Creator' (volume ii, chapter I) Sumner declares that 'the original mandate of eternal Wisdom provided, as far as we can learn from physical researches, for a world of which we cannot see the termination',<sup>152</sup> and Lyell seems to have followed, in his own discussion of man in *Principles*, Sumner's chapter 'On the design of the Creator in regard to the existence of mankind upon earth' (volume ii, chapter II). There Sumner had said that it is 'the power of progressive and improvable reason' which distinguishes man from beast,<sup>153</sup> and Lyell seems to have taken over both the concept and even the phraseology.<sup>154</sup> So it was perhaps no arbitrary choice when Lyell decided to push Sumner forward as a spokesman in 1863. Sumner's providential interpretation of the adaptation of organism to environment was by then obsolete,<sup>155</sup> but Sumner's delineation of man's relationship to the rest of creation was, in Lyell's view, still valid. He quotes this from the Archbishop:

Animals . . . are born what they are intended to remain. Nature has bestowed upon them a certain rank, and limited the extent of their capacity by an impassible decree. Man she has empowered and obliged to become the artificer of his own rank in the scale of beings by his peculiar gift of improvable reason.<sup>156</sup>

<sup>150</sup> *Ibid.*, pp. 500–1, quoted from H. Hallam, *An introduction to the literature of Europe* (4 vols., London, 1837–9), iv. 162–3. What Lyell called Hallam's 'profound reflections on "the thoughts of Pascal"' (*Antiquity of man*, p. 500) are worth looking at, especially as they are the source of the phrase 'the archangel ruined' which Lyell uses to denote his old beliefs about man's place in creation (*LLJ*, ii. 362, 376; *PG* [10th edn., 1868], ii. 493). Hallam says, of Pascal's conception of fallen man: 'it is not the sordid grovelling, degraded Caliban of [the vulgar Calvinist] school, but the ruined archangel that he delights to paint' (Hallam, *op. cit.*, iv. 158).

<sup>151</sup> J. B. Sumner, *A treatise on the records of creation and on the moral attributes of the Creator* (2 vols., London, 1816). Sumner's book was second prize-winner in a competition that had invited treatises on 'the Evidence that there is a Being all-powerful, wise, and good, by whom every Thing exists; and particularly to obviate Difficulties regarding the Wisdom and the Goodness of the Deity; and this, in the first place, from Considerations independent of written Revelation; and in the second place, from the Revelation of the Lord Jesus: and from the whole, to point out the inferences most necessary for, and useful to Mankind' (*op. cit.*, i. p.v.). Sumner follows this specification exactly, emphasizing, in his section on natural theology, the reliability of the 'Mosaic History' and its lack of conflict with geological discovery, but going on to declare that 'where Reason . . . leaves us, Revelation takes us up' (volume i, p. xii).

<sup>152</sup> *Ibid.*, ii. 10.

<sup>153</sup> *Ibid.*, ii. 19.

<sup>154</sup> See, for example, Lyell, 'Memoir on the geology of Central France . . . by G. P. Scrope', *Quarterly review*, xxxvi (1827), 437–83 (475), where Lyell speaks of man's 'Capability of progressive improvement'. See also *PG* (1st edn., 1830), i. 156, where Lyell says that what especially marked the creation of man was 'the union, for the first time, of moral and intellectual faculties capable of indefinite improvement, with the animal nature'. Perhaps the concept of 'improvable reason' was a commonplace, but the similarity between Lyell's and Sumner's presentation, and Lyell's decision to quote Sumner extensively, over thirty years later, in *Antiquity of man*, indicates a close connexion. Lyell knew Sumner personally; see *LLJ*, ii. 154–5.

<sup>155</sup> For example: 'if the ant has peculiar sagacity, it is but a compensation for its weakness; if the bee is remarkable for its foresight, that foresight is rendered necessary by the short duration of its harvest' (Sumner, *op. cit.* [151], ii. 17).

<sup>156</sup> Quoted in *Antiquity of man*, p. 497.

That is to say, reason, *improvable* reason, which gives man his capacity to become civilized and to advance intellectually—to evolve culturally, to use an anachronistic term—did not emerge from the lower animals as a product of the same processes that may have formed man physically, but were bestowed on man alone, as a unique gift.

Darwin demanded the abandonment of this sharp distinction, and, for Lyell, it was a wrench. Lyell never made himself at home in Darwin's and Huxley's world; in spirit, he never left the providential world of Sumner. As he admitted in a letter to Hooker, complete acceptance of Darwin's theory would mean the abandonment of 'old and long cherished ideas, which constituted the charm to me of the theoretical part of the science in my earlier days, when I believed with Pascal in the theory, as Hallam terms it, of "the archangel ruined"'.<sup>157</sup> So we should not be surprised that, on the penultimate page of *Antiquity of man*, Lyell suggests that in the transition from the lower animals to man there was 'one bound' which 'cleared . . . the space which separated the highest stage of the unprogressive intelligence of the inferior animals from the first and lowest form of improvable reason manifested by Man. To say that such leaps constitute no intervention to the ordinary course of nature, is more than we are warranted in affirming.'<sup>158</sup> Nor should it surprise us that Darwin, for whom 'bounds' and 'leaps' were anathema, told Lyell that this statement 'makes me groan'.<sup>159</sup>

But not everyone groaned: Herschel wrote to Lyell saying how pleased he was with the idea of 'jumps', or 'discontinuous deviations' as expounded in *Antiquity of man*. 'In point of principle', he wrote, these jumps amount 'to the introduction into the process, visibly and perceptibly of *mind, plan, design*, to the plain & obvious exclusion of the haphazard view of the subject & casual concourse of atoms'.<sup>160</sup> Herschel, who called Darwin's *Origin* an exposition of 'the law of higgledy-piggledy',<sup>161</sup> was plainly much more impressed by what he took to be Lyell's retention of providential direction of the process of evolution.

The exchange of letters between Lyell and Darwin after the publication of *Antiquity of man* again shows Lyell attempting to explain that 'my feelings, more than any thought about policy or expediency, prevent me from dogmatising as to the descent of man from the brutes',<sup>162</sup> while Darwin grumbles on to Hooker, Asa Gray, and Lyell himself, saying how 'greatly disappointed'<sup>163</sup> he felt at Lyell's 'timidity'<sup>164</sup> in not speaking

<sup>157</sup> Lyell to J. D. Hooker, 9 March 1863, in *LLJ*, ii. 362. See also note 150.

<sup>158</sup> *Antiquity of man*, p. 505.

<sup>159</sup> Darwin to Lyell, 6 March 1863, in *LLD*, iii. 12.

<sup>160</sup> Herschel to Lyell, 13–14 April 1863; Herschel's emphasis. Copy in Herschel papers, Royal Society of London, whose permission to quote extracts is acknowledged.

<sup>161</sup> Darwin to Lyell, 12 December 1859, in *LLD*, ii. 241.

<sup>162</sup> Lyell to Darwin, 11 March 1863, in *LLJ*, ii. 363.

<sup>163</sup> Darwin to Lyell, 6 March 1863, in *LLD*, iii. 11.

<sup>164</sup> Darwin to Hooker, 24 February 1863, in *LLD*, iii. 9; Darwin to Gray, 23 February 1863, in *LLD*, iii. 10.



out on man. It is obvious that Darwin—and Wallace<sup>165</sup>—did not share Lyell's fears for man's high genealogy, but the correspondence shows that Darwin never perceived that for Lyell there might have been a problem involved. His own smooth, apparently untroubled acceptance of an evolutionary ancestry for man seems to have blinded him to the perfectly understandable and clearly articulated fears of his old teacher. It does not seem to have occurred to Darwin that Lyell might have been surrendering anything of value. Darwin wrote to Hooker: 'the best of the joke is that he [i.e. Lyell] thinks he has acted with the courage of a martyr of old.'<sup>166</sup> Plainly, Darwin saw nothing courageous in giving up one's high genealogy. But perhaps the most biting insensitive comment Darwin made was when he returned to Lyell some proofs of the tenth edition of *Principles*. In chapter nine of volume one Lyell struggles with his everlasting problem of differentiating between man and beast. His struggles call forth this comment from Darwin:

You will think me rather impudent, but the discussion at the end of Chapter IX on man, who thinks so much of his fine self, seems to me too long, or rather superfluous, and too orthodox, except for the benefited clergy.<sup>167</sup>

With such disastrous failures of sympathy for each other's predicaments, it is perhaps a wonder that the friendship of the former master and pupil survived at all.

Lyell published the tenth, extensively revised edition of *Principles* in 1867–8,<sup>168</sup> and in volume two he came as near as he ever came to accepting Darwin's theory. It was not near enough for Darwin. When Lyell was preparing volume two, Darwin had optimistically written to him: 'I rejoice from my heart that you are going to speak out plainly about species.'<sup>169</sup> But when Darwin read the volume, he felt obliged to tell Lyell: 'I was dreadfully disappointed about Man.'<sup>170</sup> Lyell's discussion of man was as complicated as ever, with attempts to make qualifications, vague objections, obscure distinctions, anything that might help him to establish some sort of case which admitted the principle of evolution but which circumscribed its application. In the passage that Darwin criticized

<sup>165</sup> See McKinney 1972, op. cit. (5), pp. 95–6, 150.

<sup>166</sup> Darwin to Hooker, 24 February 1863, in *LLD*, iii. 9.

<sup>167</sup> Darwin to Lyell, 9 October 1866, in *MLD*, i. 272.

<sup>168</sup> Wallace's response to Lyell's tenth edition was important. In his review for the *Quarterly review* Wallace first announced his new conviction that unaided natural selection could not exhaustively account for the emergence of man, though Wallace's reservations were different from Lyell's. See [Wallace], 'Sir Charles Lyell on geological climates . . .', *Quarterly review*, cxxvi (1869), 359–94. Wallace explained his new views to Lyell in a letter dated 28 April 1869. The original letter is in the American Philosophical Society's Darwin-Lyell papers, but Lyell quotes extensively from it in a letter of his own to Darwin dated 5 May 1869, in *LLJ*, ii. 442–3. For a discussion of Wallace's change of opinion concerning the evolution of man, see R. Smith, 'Alfred Russel Wallace: philosophy of nature and man', *The British journal for the history of science*, vi (1972–3), 177–99.

<sup>169</sup> Darwin to Lyell, 18 July 1867, in *LLD*, iii. 72.

<sup>170</sup> Darwin to Lyell, 4 May 1868, in *LLD*, iii. 117.

so insensitively, for example,<sup>171</sup> Lyell's determination to make a qualitative distinction between the origination of man and the origination of the lower animals leads him, as in the first edition, into jeopardizing his case for the undeviating uniformity of nature. Such a problem would not have arisen if Lyell had been able to accept the full implications of Darwin's theory, but, in the last resort, Lyell did not want the course of nature to be uniform.

In the section that deals with the applicability of Darwin's theory to man (volume II, chapter XLIII), Lyell argues that Darwin has not explained *creation* at all: he writes that 'even if we could discover geological evidence that every modification between a mere power of sensation like that of a sponge and the intelligence of an elephant had been represented by every intermediate degree of instinct and capacity', and even if these intermediate stages followed chronologically and had strong analogies in embryological development,

*still the mystery of creation would be as great, and as much beyond the domain of science, as ever.* It is when there is a change from an inferior being to one of superior grade, from a humbler organism to one endowed with new and more exalted attributes, that we are made to feel that no modification of a progenitor, no principle of inheritance, can explain the phenomenon. [my italics]

Plainly Lyell is limiting the extent of Darwin's achievement rather drastically. He is saying that Darwin's enterprise has not been to explain 'the creation of species', but only to investigate 'whether species have been introduced into the world one after the other, in the form of new varieties of antecedent organisms and in the way of ordinary generation'. Then, in a way that subtly demeans Darwin's achievement even further, by making his work look like an elaborate footnote to Lamarck's, Lyell makes his final 'commitment' to evolution:

Was Lamarck right, assuming progressive development to be true, in supposing that the changes of the organic world may have been effected by the gradual and insensible modification of older pre-existing forms? Mr Darwin, without absolutely proving this, has made it appear in the highest degree probable . . .

And Lyell rounds off his discussion by declaring, in a somewhat facile way, considering the serious objections raised in the *Species journals*, that such an evolutionary system represents a 'preconceived plan' which demonstrates even more 'power, wisdom, design or forethought' on the part of the 'Supreme Creative Intelligence' than a series of 'separate, special, and miraculous acts of creation'. Lyell's commitment to evolution, then, was hardly likely to warm Darwin's heart. His acceptance was grudgingly given and limited in extent. Lyell said first, that evolution is only 'in the highest degree probable'; secondly, that it does not explain 'creation';

<sup>171</sup> *PG* (10th edn., 1867–8), i. 167–73.

thirdly, that it is purposive in a way that Darwin had always denied. Lyell accepted evolution only after he had cobbled together an interpretation of it that preserved the providential view of the world that formed the foundation of his thought.

Lastly, Lyell raises—but does not effectively lay to rest—the hoary old problem. He says:

A more serious cause of disquiet and alarm arises out of the supposed bearing of this same doctrine on the origin of man and his place in nature. . . . Such a near bond of connection between man and the rest of the animate creation is regarded by many as derogatory to our dignity.

Then in a rather sad passage which closes the chapter Lyell ponders whether ‘science’ will ever be able to welcome new truths, ‘instead of resisting the new discovery, long after the evidence in its favour is conclusive’. For the word ‘science’, we should read ‘Sir Charles Lyell’. In the closing sentence of the chapter, he finally admits that man’s hope for an ‘ideal parentage’ is illusory.<sup>172</sup>

A few years earlier, Lyell had written: ‘it cost me a struggle to renounce my old creed’.<sup>173</sup> The ‘creed’ that Lyell renounced, held, as its first article of faith, that man is distinguished from the rest of the natural world by clear marks of God’s unique, creative attention. In this article, I have attempted to trace Lyell’s struggles to come to terms with evidence that suggested that no such distinction could be maintained.

<sup>172</sup> Ibid, ii. 491–4. Lyell’s position here is much the same as Asa Gray’s. See Dupree, *op. cit.* (64), especially pp. 48, 106.

<sup>173</sup> Lyell to T. S. Spedding, 19 May 1863, in *LLJ*, ii. 376.