New perspectives on Edinburgh Lamarckians and other transformist thinkers

Evolutionary debates in the Athens of the North, 1790–1844

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Abstract

Recent scholarship has suggested that transformist ideas had a wider currency in Edinburgh in the first half of nineteenth century than had previously been acknowledged. The first objective of this study is to delve deeper into the reception of transformist theories there in the years 1790 to 1844. The main figures whose theories on the transmutation of species were discussed in contemporary sources are Carl Linnaeus (1707–1778), George-Louis Leclerc, Conte de Buffon (1707–1788), Erasmus Darwin (1731–1802), Jean-Baptiste Lamarck (1744–1829) and Étienne Geoffroy Saint-Hilaire (1772–1844); this study therefore concentrates on the reception of their work. The principle Edinburgh contexts in which the reception of their theories is explored are the University of Edinburgh, the extra-mural medical schools and the city's various learned societies and scientific journals, although the opinions of all those in Edinburgh known to have discussed transformism in this period are considered. The sources examined reveal that transformist theories were largely received with interest. Discussion of them was generally confined to scientific, or naturalistic, arguments, except in the cases of some Evangelical natural historians, who rejected them outright on theological grounds. This thesis also explores how some thinkers in Edinburgh went beyond discussing received ideas about transformism and developed their own theories, synthesising the work of earlier thinkers. The most important of these were Robert Edmond Grant (1793–1874), Robert Jameson (1774–1854), Robert Knox (1791–1862) and Henry H. Cheek (1807–33). This thesis also explores the genesis of the later transformist theory of Robert Chambers (1802–71), the anonymous author of *Vestiges of the Natural History of Creation* (1844), to establish to what extent he may have been influenced by the earlier transformists of the 1820s and 30s. Events in Edinburgh in the 1820s also had a wider resonance for the history of evolutionary ideas in Britain, as Charles Darwin (1809–1882) was a student at the University of Edinburgh between 1825 and 1827. It has long been suspected that his experiences in Edinburgh had a larger part to play in the development of his theory of evolution than he later cared to admit. Careful to avoid associating himself with the more speculative theories of earlier transformist thinkers, Darwin made little mention of them in his published writings. We already know, however, that Darwin had a close relationship with Grant during his time in Edinburgh and must have been familiar with his transformist ideas. This thesis aims to show to what extent the intellectual environment that Darwin found himself in was suffused with the idea of the transmutation of species. In broad outline, it can be concluded that transformism was much less controversial in Edinburgh in the first half of the nineteenth century than might be supposed from the prevailing historiography and had a significant number of sympathisers and adherents.

Declaration

I, William Hugh Wright Jenkins, declare that I composed this thesis, that all the work is my own and that the work has not been submitted for any other degree or professional qualification.

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Chapter 1: Introduction

Much has been written about Charles Darwin's brief time as a medical student at the University of Edinburgh. Some scholars, including James Secord and Jonathan Hodge, have gone so far as to suggest that, despite Darwin's own assertions to the contrary, his experiences at Edinburgh played a greater role in the development of his evolutionary theories than has generally been recognised.[1] Open any scholarly biography of Darwin and you will learn that he was a member of the Plinian Natural History Society, a student society where ideas such as spontaneous generation, materialistic theories of the mind and the inheritance of acquired characteristics were openly discussed. You will also learn of his friendship with Robert Edmond Grant (1793–1874), probably the most important promoter of evolutionary ideas in Great Britain in the early decades of the nineteenth century. Based on these facts and hints from other sources it has been suggested by James Secord that there existed a shadowy group of 'Edinburgh Lamarckians' around the better-known figure of Grant, which may even have included Robert Jameson (1774–1854), the professor of natural history at Edinburgh from 1804 to 1854. The paper in which this suggestion was made, 'Edinburgh Lamarckians: Robert Jameson and Robert E. Grant' (1991), was the starting point for this study.[2] In that paper it was suggested that an anonymous article entitled 'Observations on the nature and importance of geology', published in the *Edinburgh New Philosophical Review* in 1826, was not written by Grant, as had previously been widely believed, but by Jameson. In this paper the anonymous author expressed admiration for Jean-Baptiste Lamarck (1744–1829) and his transformist theories, which he discussed at some length.[3] Secord has gone on the suggest that, in the light of his reassessment of this

[1] See, for example, James A. Secord, 'Edinburgh Lamarckians: Robert Jameson and Robert E. Grant', *Journal of the History of Biology* 24: 1 (1991), 1–18 and M.J.S. Hodge, 'On Darwin's science and its contexts', *Endeavour*, http:/dx.doi.org/10.1016/j.endeavour.2014.10.003 (published online 6 November 2014)
[2] Secord, 'Edinburgh Lamarckians'
[3] Anon, 'Observations on the nature and importance of geology', *Edinburgh New Philosophical Journal* 1 (1826), pp.298–302

article, 'our current picture of the acceptance of evolution needs to be overhauled.'[4] Just such an overhaul is the primary objective of this study.

To preclude any anachronistic reading of events in the light of later developments, I will attempt to avoid viewing the Edinburgh transformists of the first half of the nineteenth century as in any sense 'precursors' of Darwin, but rather to place them in their own unique historical and geographical context. While Darwin was present in Edinburgh for a short time during the period under study, and it would be wrong to ignore the evidence of the recollections he later wrote of his time in the city, I will not be privileging his perspective. He will be very much a bit player in this story, reflecting his contemporary role and status as an undistinguished medical student rather than the much grander part he was later to play in the broader history of evolutionary thought. As we will see, however, his testimony regarding his time in Edinburgh, in conjunction with the evidence from other sources, does raise some intriguing questions regarding the role of the Edinburgh transformists in the development of evolutionary thought in Britain later in the nineteenth century.

While a significant amount is known about transformism in Edinburgh in the period 1825 to 1827 because it has been well researched by Darwin scholars, less is known about the years before and after these dates. [5] My principal objectives here will be, firstly, to endeavour to deepen our understanding of the nature and extent of Edinburgh transformist circles during the late 1820s and early 1830s and, secondly, to attempt to fit the picture that emerges into the wider context of the reception and development of transformist ideas in the city in the first half of the

[4] Secord, 'Edinburgh Lamarckians', p.18

[5] In this study I will generally use the terms 'transformism' or 'transmutation of species' rather than 'evolution'. I will do this firstly to avoid anachronism, as these were the terms used at the time and the theories they were used to describe were radically different from Darwinian evolution by means of natural selection and secondly because the term 'evolution' was used at the time, but in the very different context of foetal development, often in connection with preformationist theories of generation. Therefore to avoid possible confusion I will generally favour the terms that were at use in the early decades of the nineteenth century.

nineteenth century. The broader scope of this study will be the period between the last decade of the eighteenth century and 1844. This specific end date was chosen because it marks a significant turning point in the reception of transformism in Great Britain. In that year *Vestiges of the Natural History of Creation* was published, a book which generated a storm of controversy and introduced transformism to a wider middle-class audience for the first time, as Secord has chronicled in his magisterial *Victorian Sensation*.[6] As the publication of this book radically changed the terms of the debate on transformism, it seemed a good place to finish a study of the reception of transmutationist theories in the earlier decades of the century. The anonymous author of *Vestiges* was Robert Chambers, a prominent Edinburgh journalist and publisher. In this study I will also be asking to what extent the Edinburgh background of the author of *Vestiges* was a coincidence, and if it was not, what connections it is reasonable to infer between Chambers and the earlier Edinburgh transformists of the 1820s and 30s.

Two important recent biographies of Darwin cast considerable light on his time in Edinburgh and the context which formed a background to his studies there. The first volume of Janet Browne's exemplary Darwin biography, *Charles Darwin: Voyaging* (1995), devotes two chapters to Edinburgh.[7] Browne presents a comprehensive picture of the activities of the Plinian Natural History Society, of which Darwin was a member, Darwin's relationships with some of his fellow students and his short-lived friendship with Robert Grant (1793–1874). It is clear from her account that materialist and transformist ideas about the natural world were very much in the air in Edinburgh in the late 1820s. In their biography *Darwin* (1991) Adrian Desmond and James Moore also devote two chapters to their subject's time in Edinburgh.[8] Like Browne, they give much valuable analysis of the activities of the Plinian Society and Darwin's relationship with Grant, although they have

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[6] James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation* (Chicago, IL, 2003)
[7] Janet Browne, *Charles Darwin: Voyaging* (London, 1995)
[8] Adrian Desmond and James Moore, *Darwin* (London, 1991)

allowed themselves to take a rather more speculative approach than Browne, including conjectures regarding Grant's sexuality, which, while plausible, might seem to go rather beyond what the surviving evidence would justify. As in Desmond's other writings on the subject, a strong association is made between radical politics and transformism that not all other scholars have found entirely justified. It will be one of the objectives of the current study to test the validity of this association between transformist theory and radical political and social ideas. Another book by Desmond which gives a valuable insight into transformism in Edinburgh in the 1820s is his *The Politics of Evolution: Morphology, Medicine, and Reform in Radical London* (1989).[9] As the title would suggest, this work examines transformism in Edinburgh only as a prelude to a study of London radical medical circles. It does, however, contain much valuable research on the Edinburgh careers of Grant and his fellow extra-mural anatomy school lecturer Robert Knox (1791–1862) before they both moved to London. Desmond has also written an important earlier paper on 'Robert E Grant: The social predicament of a pre-Darwinian transmutationist' (1984), which also paints a lucid picture of Grant's career and opinions.[10]

As noted above, James Secord has made a major contribution to our understanding of Edinburgh in the 1820s in his paper on the 'Edinburgh Lamarckians', which has not only raised important questions about the prevalence of transformist opinions in Great Britain in the early decades of the nineteenth century, but served as the principal inspiration for this study. By suggesting that the 1826 paper on 'Observations on the nature and importance of geology' may have been by Robert Jameson, the professor of natural history at the University of Edinburgh, he raised an important question regarding the prevalence of transformist ideas at the period, and, as Jameson was solidly a figure of the establishment, threw into doubt the

[9] Adrian Desmond, *The Politics of Evolution: Morphology, Medicine, and Reform in Radical London* (Chicago, 1989)
[10] Adrian Desmond, 'Robert E Grant: The social predicament of a pre-Darwinian transmutationist', *Journal of the History of Biology* 17: 2 (1984), 189–223

association made by some scholars between radical politics and transformism. Pietro Corsi has also done much to show that transformist ideas were much more widespread, not just in Edinburgh, but on the broader European stage, in the first half of the nineteenth century than has often been supposed by scholars who have focussed narrowly on Darwin and his 'precursors'. In a number of recent papers, including ' Before Darwin: Transformist concepts in European natural history' he has provided an excellent overview of the widespread currency transformist ideas in fact enjoyed.[11] In an earlier paper on 'The importance of French transformist ideas for the second volume of Lyell's *Principles of Geology*' (1978), he has also demonstrated that surprisingly positive appraisals of Lamarck's theories were prevalent even among those who did not necessarily agree with his conclusions, notably the Scottish naturalist and Evangelical Church of Scotland minister, John Fleming (1785–1857), of whom more will be said in a later chapter.[12]

Less has been written about the decades after both Darwin and Grant left Edinburgh in 1827 than about the period when Darwin was a student there. Several scholars have, however, explored the Edinburgh background to Chambers' *Vestiges*, including Milton Millhauser, M.J.S. Hodge and Richard Yeo.[13] Probably the most important and comprehensive contribution to the subject in the last few decades has been Secord's 'Beyond the veil: Robert Chambers and *Vestiges*' (1989).[14] In this essay, Secord traces most of the elements of Chambers' 'development hypothesis' to the omnivorous reading that Chambers engaged in in his role as publisher of improving non-fiction literature, and especially as writer and editor of the *Chambers' Edinburgh*

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[11] Pietro Corsi, 'Before Darwin: Transformist concepts in European natural history', *Journal of the History of Biology* 38 (2005), 67–83

[12] Pietro Corsi, 'The importance of French transformist ideas for the second volume of Lyell's *Principles of Geology*', *The British Journal for the History of Science*, 11: 3 (1978), 221–44

[13] See Milton Millhauser, *Just before Darwin: Robert Chambers and Vestiges* (Middletown, CT, 1959);M.J.S. Hodge, 'The Universal Gestation of Nature: Chambers' *Vestiges* and *Explanations*', *Journal of the History of Biology* 5: 1 (1972), pp.127–51; and Richard Yeo, 'Science and intellectual authority in mid-nineteenth-century Britain: Robert Chambers and *Vestiges of the Natural History of Creation*', *Victorian Studies* 28: 1 (1984), pp.5–31

[14] James A. Secord, 'Beyond the veil: Robert Chambers and *Vestiges*', in James R. Moore (ed.), *History, Humanity and Evolution: Essays for John C. Greene* (Cambridge, 1989), pp.165–94

Journal. Below I will argue that the key elements of Chambers' transformism could in fact have been derived principally from a relatively narrow range of sources, although his wide reading allowed him to fill the pages of *Vestiges* with a vast array of disparate pieces of evidence and copious supplementary material.

In writing this study, it is my purpose not only to fill in some of the gaps in these previous studies, but also to attempt to question or confirm their conclusions as the evidence seems to dictate. In order to do this, I have identified six questions that I will attempt to address. My first objective is to determine to what extent earlier and contemporary transformist theories were known in Edinburgh in the period 1790 to 1844. Following from this, I will try to establish how these theories were interpreted and what different reactions they elicited in the city, both positive and negative, and to determine to what extent and by whom transformist ideas were accepted as valid. In counterpoint to this, I will also try to show on what grounds these theories were sometimes criticised and rejected. This will then allow me to try to establish whether attitudes towards transformism changed between 1790 and 1844. I will also be aiming to determine to what extent attitudes towards transformism were shaped by political, religious, cultural and social factors. In particular, I will examine two important questions; firstly, whether transformism was indeed largely the preserve of political or social radicals, or whether support for these ideas was more broadly based; and secondly, whether the rising tide of evangelicalism in Scotland in the early decades of the nineteenth century conditioned attitudes towards transformism. Having established to what extent earlier transformist ideas from outside Edinburgh were received, I will then be able to investigate how the Edinburgh transformists used these ideas to produce their own syntheses of the ideas they had encountered and develop their own original theories about the nature, origin and development of species.

Having outlined the objectives of this study, I will now give a brief summary of the structure and content of the chapters that are to follow. Firstly, before engaging directly with the evidence for the reception and development of transformist theories in Edinburgh, it will be necessary to provide an exposition of the theories themselves and the careers and influence of their originators. In order to do this chapter 2 will examine the work of the five main figures whose ideas on the origins and transmutation of species it can be positively demonstrated were known in Edinburgh in the period under study: Carl Linnaeus (1707–78), Georges-Louis Leclerc, Comte de Buffon (1707–1788), Erasmus Darwin (1731–1802), Jean-Baptiste Lamarck and Étienne Geoffroy Saint-Hilaire (1772–1844). It is doubtful if Buffon was indeed a transformist in the same way as the other figures on the list, but his ideas on the history of life and the nature and mutability of species were so important and so widely known, that he has to be included. There may well have been other transformists whose theories were known in Edinburgh, but I have confined myself to those whose names or theories emerge unambiguously from the sources.

After outlining the theories of the most significant transformist thinkers of the period, in chapter 3 I will turn to the institutional and intellectual context of Edinburgh in the first half of the nineteenth century. Much of the evidence for the reception of transformist ideas comes from the University of Edinburgh, and in particular from the classes of Robert Jameson and his predecessor in the chair of natural history John Walker (1731–1803). A second important context is that of the extra-mural medical schools. Three important figures who wrote on transformism were to be found teaching in these institutions: Robert Grant, Robert Knox and John Fletcher (1792–1836).[15] A significant number of scientific and natural history societies existed in Edinburgh at the time, and it is to these I will turn next. These included the Plinian Natural History Society, which has already been mentioned above as providing a forum for students to discuss a wide range of unorthodox opinions and ideas. A second important society was the Wernerian Natural History Society, which counted almost all Scottish natural historians of any importance among its members, as well as a considerable number of notable individuals from

[15] Fletcher was not himself a transformist, but did write on the subject.

further afield. The Royal Medical Society and the Royal Physical Society, both of which drew their membership principally from among Edinburgh's medical students, also provided congenial surroundings in which the latest theories about the natural world could be presented and discussed, including those relating to the transmutation of species. Edinburgh was also home to a number of scientific journals which published papers on transformism in this period. The *Edinburgh New Philosophical Journal*, edited by Robert Jameson, proved the most prolific source of transformist articles. This is where Grant published the bulk of his transformist papers, but there were also a significant number of other articles on the subject, mostly published anonymously. As we will see in subsequent chapters, there is significant scope for speculation regarding their authors. The *Edinburgh Journal of Science*, edited by the Evangelical natural philosopher David Brewster, also published at least one of Grant's transformist articles. Perhaps most interesting of all, the *Edinburgh Journal of Natural and Geographical Science*, edited by two medical students at the University, published a string of transformist articles during its short existence from 1829 to 1831.

Having established a context for the reception of theories of the transmutation of species in Edinburgh, chapter 4 will look at the variety of reactions elicited by the work of the five key transformist figures that I have identified. This will involve identifying and analysing references to the theories of Linnaeus, Erasmus Darwin, Buffon, Lamarck and Geoffroy in reviews, articles, books, society records and other unpublished sources from the period. I will look first at the relatively scanty references to the principally eighteenth-century figures of Linnaeus, Darwin and Buffon, before turning to the rather fuller record of reactions to Lamarck. As critics of Lamarck can be clearly divided into the more secular, or at least religiously moderate, figures associated with the university and extramural medical schools on the one hand, and natural historians who approached his work from an evangelical perspective on the other, I will treat these two groups separately. As we will see, theological doctrines regarding the possibility of progressive change in a fallen world and the power of God to intervene miraculously in the history of the earth give a very distinctive tenor to evangelical critiques of transformism which fully justifies examining these independently. I will devote a separate section to the reception of Geoffroy, to whose theories there are relatively fewer references in the sources and who was largely neglected by evangelical critics.

Not only did naturalists in Edinburgh react to the works of earlier transformist thinkers; some of them also had their own ideas on the transmutation of species, and it is to these I will turn next in chapter 5. The context for these developments was the progressive model of the history of life that came to dominate in geological circles in the early decades of the nineteenth century. In contrast to James Hutton (1726–97) and his immediate followers, who favoured a uniformitarian model of the history of the globe, Jameson and many of his circle favoured a progressive model of earth history.[16] In Jameson's case this was rooted in the theories of Abraham Gottlob Werner (1849–1817), with whom he had studied in Freiberg, and whose disciple he remained on his return to Edinburgh. For many transformists directional change in the physical environment of the earth proved a compelling mechanism for progressive change in the history of life. I will therefore be exploring the extent to which progressive theories of the history of the earth made plausible a progressive model of the history of life. After exploring these connections, I will examine how transformist thinkers in Edinburgh imagined the history of life, and what patterns they claimed to discern in the fossil record and to deduce from the forms of life alive at the present time. It will then be possible to investigate in more detail how they envisaged the relationships between genera, species and varieties and through what processes they conjectured that new species came into being. To round of this chapter, I will look at the work of two figures who considered the forms of living things to be mutable, but did not see such change as progressive: Robert Knox and

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[16] Radical uniformitarianism in the first half of the nineteenth century, as represented by Charles Lyell, had very few supporters; while Lyell's ideas were extremely influential, few other geologists subscribed to his steady state model of earth history. See Peter Bowler, *The Invention of Progress: The Victorians and the Past* (London, 1989), p.147

Hewett Cottrell Watson (1804–81). Both developed original and eccentric ideas on the transmutation of species while scorning any suggestion that any progress or purpose could be discerned in the history of life.

In 1844 the publication of *Vestiges of the Natural History of Creation* completely changed the terms of the debate on transformism. In chapter 6, building on the valuable work of James Secord, I will attempt to establish to what extent Robert Chambers' Edinburgh background may have influenced or inspired his transformist conjectures and whether any connection can be established between these and the transformist ideas which were current in Edinburgh natural history circles in the preceding decades. Chambers came from a very different background from most of the figures I will be discussing and moved in different circles. There is therefore little evidence for personal connections between him and any of the earlier Edinburgh transformists. With this in mind, I will be attempting to establish whether the fact that transformism burst into the public sphere in 1844 through the writings of an Edinburgh journalist and publisher was merely a coincidence or was in some way connected with the prevalence of transmutationist ideas in medical and natural history circles in the city in earlier decades.

Chapter 2: The context of European transformist thought, 1740–1830

Introduction

Transformism was by no means a new idea at the beginning of the nineteenth century. The purpose of this chapter will be to review the earlier thinkers whose ideas were influential in the Edinburgh of the early decades of the nineteenth century. A large number of natural historians, philosophers and physicians in the eighteenth and early nineteenth centuries had written about the transmutation of species, some of them at considerable length. Among the most significant were Pierre Louis Maupertuis (1698–1759), Denis Diderot (1713–84), Jean André Deluc (1727–1817), Jean-Baptiste Robinet (1735–1820) and Jean-Claude Delamétherie (1743–1817). Most of these thinkers were French, but in Germany Immanuel Kant (1724–1804) and Johann Wolfgang von Goethe (1749–1832), among others, had written on the subject. Much scholarship by historians of science in the English-speaking world, however, has concentrated on Jean-Baptiste Lamarck to the exclusion of other important pre-Darwinian thinkers, possibly due to his greater influence in Britain in large part owing to Charles Lyell's famous critique of his theories in the second volume of his *Principles of Geology* (1832). Pietro Corsi in particular has attempted to redress the balance, noting that 'contrary to long cherished views, Lamarck's was only one voice among many.'[1] It is, however, beyond the scope of this study to explore the great variety of theories that were current in Europe in the late eighteenth and early nineteenth centuries. In this chapter I will therefore only be examining the ideas of those for whom there is concrete evidence that their theories were widely known and discussed in Edinburgh in the period 1790 to 1844. In consequence my discussion will be limited principally to the work of Carl Linnaeus, Erasmus Darwin, George-Louis Leclerc, Comte de Buffon, Jean-Baptiste Lamarck and Étienne Geoffroy Saint-Hilaire. As we will see in later chapters, there is solid evidence for the influence of all of these

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[1] Corsi, 'Before Darwin', p.68

important figures in early nineteenth-century Edinburgh. There are, nonetheless, other transformist thinkers who are only referred to occasionally in the sources, and then generally in contexts unrelated to transformism. John Barclay (1758–1826), the Edinburgh extra-mural anatomy lecturer, for example, wrote a defence of vitalism entitled *Life and Organization* (1822), in which he mentioned a considerable number of transformists, including Maupertuis and Robinet, but without touching on their contributions to the debate on the transmutation of species.[2] Although their theories may have played a significant role in the evolution of transformist ideas in a broader context, if there is no evidence that their theories were discussed in the context of transformism in Edinburgh in the period under study, I have not included a consideration of their ideas here.[3]

Carl Linnaeus (1707–78)

Linnaeus, the great Swedish natural historian, best known for his influential system of classification, was professor of medicine and botany at the University of Uppsala from 1741. He became rector in 1750, a post he held until he had to resign it due to ill health in 1772. Starting in 1742, he developed a limited but very influential theory of the transmutation of species. We can date the beginning of his interest in the subject so precisely because we know that he was first stimulated to give thought to it as a result of being sent an unusual specimen of *Linaria*, or toadflax, by a student named Magnus Ziöberg.[4] The plant was indistinguishable from *Linaria vulgaris* apart from its flowers, which were of a completely different form. Linnaeus named it *Peloria* and came to the conclusion it must be a hybrid. But unlike most hybrids, *Peloria* appeared to breed true. Prior to this discovery, Linnaeus had been convinced that while new, temporary varieties could arise as a result of environmental

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[2] John Barclay, *An Enquiry in to the Opinions, Ancient and Modern, Concerning, Life and Organization* (Edinburgh, 1822)

[3] For a more general overview of the major pre-Darwinian transformists, the interested reader should consult Bentley Glass, Owsei Temkin and William L. Straus, Jr, *Forerunners of Darwin, 1745–1859* (Baltimore, MD, 1968)

[4] Lester G. Crocker, 'Heredity and variation in the eighteenth century concept of the species', in Glass, Temkin and Straus, *Forerunners of Darwin*, p.146

conditions, species were immutable. As he wrote in his *Critica Botanica*, first published in 1737:

> All the species recognized by the botanists came forth from the Almighty Creator's hand, and the number of these is now and always will be exactly the same, while every day the new and different florists' species arise from the true species so-called by the botanists, and when they have arisen they finally revert to the original forms. Accordingly to the former have been assigned by nature fixed limits, beyond which they cannot go: while the latter display without end the infinite sport of nature.[5]

In the succeeding years, Linnaeus found several more apparent examples of hybridisation giving rise to new species of plants. From the evidence of these hybrids, Linnaeus concluded that God had created only a limited number of original plant species, representing one species each from the existing orders of plants. All other genera and species were then derived from hybridisation among these different forms. As he expressed it in his *Praelectiones in ordines naturales plantarum*, based on lectures originally give in 1744, 'in the beginning the Creator created of each natural order only one plant with reproductive power', then 'by their various mixing different plants have arisen which belong to the mother's natural order as they are similar to the mother with regard to their fructifications, and are, as it were, species of the order, i.e. genera', and finally through hybridisation between the genera 'there will arise species that should be referred to the mother's genus as her daughters', thus accounting for all existing species.[6] Linnaeus' model of the origin of new species was not widely accepted by contemporary natural historians.[7] However, one thinker who did take these ideas seriously was Erasmus Darwin in England.

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[5] Quoted in James L. Larson, *Reason and Experience: The Representation of Natural Order in the Work of Carl von Linné* (Berkeley, Ca, 1971), p.97

[6] Quoted in Larson, *Reason and Experience*, p.69

[7] Peter J. Bowler, *Evolution: The History of an Idea* (Berkeley, CA, 2009), p.70

Erasmus Darwin (1731–1802)

Erasmus Darwin, the grandfather of Charles Darwin, was a highly successful physician in Lichfield, Staffordshire, as well as a poet and amateur natural historian. He was also a member of the famous Lunar Society of Birmingham, composed principally of local industrialists and intellectuals. In addition to Darwin, the society counted among its members such distinguished figures as Josiah Wedgwood, Joseph Priestley and James Watt. Darwin had studied medicine at the University of Edinburgh between 1754 and 1756, and Roy Porter has suggested that Darwin's transformism had its roots in his Edinburgh medical training, noting that 'Darwin's biomedical outlook – the product of his Edinburgh training – was informed by the evidence of change, both of degree and in kind, running ubiquitously through Nature.'[8] Be that as it may, Darwin's work, both in prose and poetry, is riddled with references to his ideas on the transmutation of species.

As noted above, his earliest references to transformism relate to Linnaeus' theory of the generation of new species by hybridisation, which may have been the starting point for his speculations on the subject. In his *The Loves of the Plants*, first published in 1789, Darwin noted that:

> The illustrious author of the Sexual System of Botany, in his preface to his account of the Natural Orders, ingeniously imagines, that one plant of each Natural Order was created in the beginning; and that the intermarriages of these produced one plant of every Genus, or Family: and that the intermarriages of these Generic, or Family plants, produced all the species: and lastly, that the intermarriages of the individuals of the Species produced the Varieties. [9]

He also seems to have been familiar at this time with the directional model of earth history embodied in Buffon's *Époques de la Nature* (1778),although Darwin put a

[8] Roy Porter, 'Erasmus Darwin: Doctor of evolution?', in James Richard Moore, *History, Humanity and Evolution: Essays for John C. Greene* (Cambridge, 1989), p.44
[9] Erasmus Darwin, *The Loves of the Plants* (London, 1806), p.x

more progressive spin on it,[10] for in the same work he speculated: 'Perhaps all the productions of nature are in their progress to greater perfection? an idea countenanced by the modern discoveries and deductions concerning the progressive formation of the solid parts of the terraqueous globe, and consonant to the dignity of the Creator of all things.'[11] He went on to express similar themes of the universal progress of nature in his poem *The Economy of Vegetation*, published in *The Botanic Garden* in 1791, the footnotes to which contained many transformist speculations.[12] However, it was in his longest work, the *Zoonomia* (1794–6), which Darwin himself described as a treatise on 'medical theory',[13] that his transformist ideas saw their greatest development.

While Darwin's theories were significantly elaborated and developed in *Zoonomia*, he still acknowledged the theory of the origin of new species through hybridisation; 'as Linnaeus has conjectured in respect to the vegetable world, it is not impossible, but the great variety of species of animals, which now tenant the earth, have had their origin from the mixture of a few natural orders.'[14] However, he then proceeded to leave Linnaeus' relatively modest vision of the origin of new species far behind. In section 39 of the *Zoonomia*, on 'Generation', Darwin listed six arguments for the transmutation of species that brought together the germs of ideas that were going to be of fundamental importance for the later development of transformist theories: 1) the analogy between the development of the individual animal, for example the metamorphosis of the caterpillar into the butterfly, and the evolution of the species; 2) the changes brought about by the domestication of animals and plants; 3) changes brought about by accidental environmental causes before birth, such as the production of 'monsters'; 4) the obvious unity of body plan among vertebrate

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[10] As will be noted later in this chapter, Buffon's theory of the earth was more one of degeneration than of progress.
[11] Darwin, *Loves of the Plants*, pp.9–10
[12] See, for example, Erasmus Darwin, *The Botanic Garden, a Poem in Two Parts; Containing The Economy of Vegetation and the Loves of the Plants with Philosophical Notes* (London, 1825), p.11
[13] Porter, 'Erasmus Darwin', p.41
[14] Erasmus Darwin, *Zoonomia*, vol 2 (London, 1801), pp.230–1

animals; 5) the transmission to their offspring of changes in the bodies of animals produced during their lifetime 'by their own exertions in consequence of their desires and aversions'; 6) the existence of intermediate forms between major taxonomic groups, such as frogs, which unite the attributes of fish and land animals.[15] Later in the book he also looked to the fossil record for evidence of transformism, arguing that the differences between fossil and modern plants and animals provided powerful evidence for transformism. He argued that the observations of naturalists show

> that most of the inhabitants of the sea and earth of very remote time are now extinct; as they scarcely admit, that a single fossil shell bears a strict similitude to any recent ones, and that the vegetable impressions or petrifactions found in iron-ores, clay, or sandstone, of which there are many of the fern kind, are not similar to any plant of this country, nor accurately correspond with those of other climates, which is an argument countenancing the changes in the forms, both of animals and vegetables, during the progressive structure of the globe, which we inhabit.[16]

He had already hinted at this in one of the footnotes to his poem *The Economy of Vegetation* in *The Botanic Garden* a few years previously, where he speculated about the fate of the fossil ammonites, asking: 'Were all the ammoniae destroyed when the continents were raised? Or do some genera of animals perish by the increasing power of their enemies? Or do they still reside at inaccessible depths in the sea? Or do some forms of animals change their forms gradually and become new genera?'[17] In the same footnote to this poem he had also thrown out, as it were in passing, a seventh important argument based on the existence of apparently useless or vestigial organs in animals and plants, such as the rudimentary wings of some insects.[18]

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[15] Ibid., pp.233–41
[16] Ibid., p.245
[17] Erasmus, *The Botanic Garden*, p.43
[18] Ibid., p.11

All these arguments would re-emerge in the early decades of the nineteenth century as important components of influential transformist theories. The argument from unity of plan in particular, probably derived from Buffon, would go on to play a central role in the development of the transformist theories of Étienne Geoffroy Saint-Hilaire and his followers, as would the argument based on monstrosities. In connection with the latter, Darwin had already speculated in his *Economy of Vegetation* that such monstrosities may be either 'remains of their habits of production in their former less perfect state, or attempts towards greater perfection.'[19] The fifth argument, based on the transmission of acquired traits to offspring probably inspired James Harrison to describe Darwin as the 'Lamarckian he undoubtedly was'.[20] However, it should be remembered that the *Zoonomia* was published before Lamarck's conversion to transformism in around 1800. It would therefore make more sense to call Lamarck a 'Darwinian', although there is no evidence that he was in fact aware of Erasmus Darwin's works. It is more likely that both drew their ideas from a common fund of hereditarian beliefs that were widely held in medical and agricultural circles in the late eighteenth century and early nineteenth centuries.[21]

In the *Zoonomia* Darwin generally confined himself to suggesting separate origins for the major groups of animals, for example, asking:

> would it be too bold to imagine, that all warm-blooded animals have arisen from one living filament, which the GREAT FIRST CAUSE endued with animality, with the power of acquiring new parts, attended with new propensities, directed by irritations, sensations, volitions, and associations; and thus possessing the faculty of continuing to improve by its own inherent

[19] Ibid., p.11
[20] James Harrison, 'Erasmus Darwin's View of Evolution', *Journal of the History of Ideas* 32: 2 (1971), p.247
[21] See, for example, John C. Waller, 'Ideas of heredity, reproduction and eugenics in Britain, 1800–1875', *Studies in the History and Philosophy of the Biological and Biomedical Sciences* 32: 3 (2001), pp.457–89

activity, and of delivering down those improvements by generation to its posterity, world without end?[22]

Only a few pages later his speculations grew bolder, and he was prepared to countenance the idea of a common origin for all life on earth, although he typically couched his speculations in the form of questions:

> Shall we then say that the vegetable living filament was originally different from each tribe of animals above described? And that the productive living filament of each of those tribes was originally different from the other? Or, as the earth and ocean were probably peopled with vegetable productions long before the existence of animals; and many families of these animals long before other families of them, shall we conjecture that one and the same kind of living filaments is and has been the cause of all organic life?[23]

These speculations may have seemed bold to Darwin's contemporaries, but the ideas that he chose to express in poetic form in *The Temple of Nature*, first published in 1803, went significantly further. Up to now, Darwin had not addressed the question of the first origins of life, but in this work he openly advocated spontaneous generation:

> Hence without parent by spontaneous birth
> Rise the first specks of animated earth;
> From Nature's womb the plant or insect swims,
> And buds or breathes, with microscopic limbs. [24]
> [Canto 1, lines 247–50]

Elsewhere in the poem Darwin made clear that he believed that 'Organic Life began beneath the waves [Canto 1, line 234, p.21]'.[25] This must be so, he claimed in an accompanying footnote, as the evidence of geology showed that the entire earth was originally covered by water. The *Temple of Nature* contains Darwin's last published

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[22] Darwin, *Zoonomia*, vol. 2, p.240
[23] Ibid., p.244
[24] Erasmus Darwin, 'The Temple of Nature', in *The Poetical Works of Erasmus Darwin* (London, 1806), p.24
[25] Ibid., p.21

statement on transformism, 'Note VIII – Reproduction' in the notes at the end of the book, which gives both a summary of his transformist beliefs and a strong sense of his highly speculative approach to the subject, emphasised here as elsewhere by his repeated rhetorical use of questions.

> But it may appear too bold in the present state of our knowledge on this subject, to suppose that all vegetables and animals now existing were originally derived from the smallest microscopic ones, formed by spontaneous vitality? and that they have, by innumerable reproductions, during innumerable centuries of time, gradually acquired the size, strength, and excellence of form and faculties, which they now possess? and that such amazing powers were originally impressed on matter and spirit by the great Parent of Parents! Cause of Causes! Ens Entium![26]

Although Darwin never developed a fully articulated transformist system such as Lamarck's, which I will be discussing later in this chapter, he did bring together, often only as hints or asides, many of the elements from which more fully developed theories were to be constructed. Most of his ideas were not original, Buffon and Linnaeus being particularly obvious and often cited influences, but he brought them together under the framework of an overarching, if in places somewhat sketchy, theory of transformism. While there is little evidence that the French philosophical naturalists to whom I will now be turning knew or were influenced by his work, his ideas were known in Great Britain, and may well have influenced the reception of later continental transformist thinkers, whose theories, derived from many of the same sources, would have seemed to echo his.

George-Louis Leclerc, Comte de Buffon (1707–88)

Buffon came from a wealthy family from Montbard, in the Bourgogne region of eastern France. After studying at the University of Angers he came to Paris in 1732, where at first he devoted himself to mathematics. In 1739, with the help of

[26] Ibid., pp.244–5

influential friends, he was appointed intendant (director) of the Jardin du Roi, a position he was to hold for the rest of his life. It was in this post that he devoted himself to writing his truly encyclopaedic *Histoire Naturelle* (1749–1804), which ran to 36 volumes in his lifetime, with a further eight published after his death. In this great work he speculated widely on the nature of species, the generation of living things, the history of the earth and a multitude of other subjects. Towards the end of his life he published the *Époques de la Nature* (1779), in which he attempted to produce a new chronology of the history of the globe, divided into seven epochs. This new chronology suggested that the earth was at least 75,000 years old, although Buffon made it clear that he considered it likely to be very much older than that.[27] In the *Histoire Naturelle* Buffon has much to say that is profoundly relevant to contemporary and later theories of transformism. However, his ideas developed and changed over the years, and at times he even seems to contradict his earlier work in later writings. Here I will attempt to give a broad outline of his ideas relating to transformism, before concentrating on the overarching theory of the history of life found in his late work, the *Époques de la Nature*.

Buffon gave the following famous and influential definition of a species in the second volume of the *Histoire Naturelle*, published in 1749: 'we should regard as the same species those which, by means of copulation, perpetuate and conserve the likeness of the species, and as different species those which, by the same means, can produce nothing together'.[28] Buffon went on to propose a theory to explain how the form of the species was perpetuated and conserved. For him the process of generation was analogous to the growth of the individual organism. To explain growth, Buffon speculated that the 'body of the animal is a type of internal mould,

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[27] Georges-Louis Leclerc, Comte de Buffon, *Histoire Naturelle, Générale et Particulière* : *Les Epoques de la Nature, supplément*, vol. 5 (Paris, 1778), p.73

[28] 'qu'on doit regarder comme la même espèce celle qui, au moyen de la copulation, se perpétue et conserve la similitude de cette espèce, et comme des espèces différentes celles qui, par les mêmes moyens, ne peuvent rien produire ensemble'. Georges-Louis Leclerc, Comte de Buffon, *Histoire Naturelle, Générale et Particulière, avec la Description du Cabinet du Roy,* vol. 2 (Paris, 1749), pp.10–11

on which the matter which serves for its growth models itself and is assimilated to the whole.'[29] This concept of an internal mould was also used by Buffon to explain reproduction:

> In the same way that we can make moulds by which we can give the exterior of things whatever shape we please, let us suppose that Nature can make moulds by which she gives not only an external shape, but also an internal form, would that not be a means by which reproduction could operate?[30]

In the cases of both growth and reproduction the internal mould performed the function of organising the 'organic molecules' that the organism ingested in its food: 'The material which serves for the nutrition and the reproduction of animals and plants is therefore the same; it is a productive and universal substance composed of organic molecules, always existing, always active, of which the union produces the bodies of organised beings.'[31] These were not molecules in the modern sense, but rather the indivisible, living and eternal particles which Buffon considered made up organic matter, which were in a state of eternal flux around the natural world.[32]

Both the theory of internal moulds, which ensured that organisms bred true, and Buffon's definition of a species as a group of organisms that were capable of

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[29] 'Le corps d'un animal est une espèce de moule intérieur, dans lequel la matière qui sert à son accroissement se modèle et s'assimile au total'. Buffon, *Histoire Naturelle*, vol. 2, p.41

[30] 'De la même façon que nous pouvons faire des moules par lesquels nous donnons à l'extérieur des corps telle figure qu'il nous plaît, supposons que la Nature puisse faire des moules par lesquels elle donne non seulement la figure extérieure, mais aussi la forme intérieure, ne seroit-ce pas un moyen par lequel la reproduction pourroit être opérée ?' Ibid., p.34

[31] 'La matière qui sert à la nutrition et à la reproduction des animaux et des végétaux, est donc la même ; c'est une substance productive et universelle composée de molécules organiques toûjours existantes, toûjours actives, dont la réunion produit les corps organisez.' Ibid., p.306

[32] It may not be readily apparent how Buffon's theory of internal moulds explained both parents' roles in sexual reproduction. His answer to this question is rather complex, and as it does not bear directly on his attitude towards transformism, I will not be dealing with it here. However, for a fuller account of Buffon's theory of generation, I would direct the interested reader to the account in Elizabeth Gasking's, *Investigations into Generation, 1651–1828* (Baltimore, MA, 1967), pp.86–96

breeding together to produce offspring, would seem to present insurmountable problems for any theory of transformism. However, the possibility of the transmutation of species clearly intrigued Buffon, and in the fourth volume of his *Histoire Naturelle* he mustered arguments both for and against its possibility in a chapter on the natural history of horses. For Buffon, the question of common descent was raised principally on the basis of the obvious unity of plan evident among vertebrate animals. He observed that:

> If, in the immense variety presented to us by all the animated beings which people the universe, we choose an animal, or even the body of man, to serve as the basis for our understanding, and relate it, by way of comparison, to all the other organised beings, we will find that, although all these beings exist separately, and they all vary by differences that are infinitely graduated, at the same time there exists a general and primitive design which can be followed very far.[33]

He went on to suggest that this common plan strongly suggested common descent, not only among all vertebrates, but among all animals and all plants :

> If it is once admitted that there are families among plants and animals, that the ass is of the family of the horse, and that the one only differs from the other because it has degenerated, it could equally be said that the ape is of the family of man, that it is a degenerated man, that man and the ape had a common origin like the horse and the ass, that each family, among the animals as among the plants, has only had a single root, and even that all the animals have come from a single animal, which in the succession of time, has produced, by becoming more perfect or degenerating, all the races of other animals.[34]

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[33] 'Si, dans l'immense variété que nous présentent tous les êtres animés qui peuplent l'Univers, nous choisissons un animal, ou même le corps de l'homme pour servir de base à nos connoissances, et y rapporter, par la voie de la comparaison, les autres êtres organisés, nous trouverons que, quoique tous ces êtres existent solitairement, et que tous varient par des différences graduées à l'infini, il existe en même temps un dessein primitif et général qu'on peut suivre très-loin'. Buffon, *Histoire Naturelle*, vol. 4, p.379

[34] 'si l'on admet une fois qu'il y ait des familles dans les plantes et dans les animaux, que l'âne soit de la famille du cheval, et qu'il n'en diffère que parce qu'il a dégénéré, on pourra dire également que le singe est de la famille de l'homme, que c'est un homme dégénéré, que l'homme et le singe ont eu une origine commune comme le cheval et l'âne, que chaque famille, tant dans les animaux que dans les végétaux, n'a eu qu'une seule souche, et même

Having presented the evidence for common descent, Buffon proceeded to give five arguments against the transmutation of species. Firstly, he argued that this contradicted revelation, which makes it clear that all the currently existing species participated in the creation.[35] Secondly, historical evidence suggests that since the time of Aristotle no change has been observed in species.[36] Thirdly, if animals were of common descent, they should be able to interbreed, but this is not the case: 'if they came from the same source, if they were in effect of the same family, it would be possible to bring them together, to unite them again, and in time undo what time had done.'[37] Fourthly, Buffon considered it almost impossible that two individuals of a species would have 'both degenerated to precisely the same point, and to that point at which they could only reproduce together'.[38] Arthur O. Lovejoy has admitted that the logic of this argument seemed to him 'a trifle obscure'.[39] I would argue that it makes sense if it is assumed that Buffon imagined a saltatory model of transformism, where new species, incapable of interbreeding with the parent species, are constantly arising within a single generation, so that a female of one species could give birth to an individual of an entirely new species. Under such circumstances the chance production of two individuals, male and female, which had changed in exactly the same manner so as to be able to interbreed as a new species seemed to Buffon an extraordinarily unlikely occurrence. Fifthly, Buffon argued that the absence of intermediate forms was evidence against the

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que tous les animaux sont venus d'un seul animal, qui, dans la succession des temps, a produit, en se perfectionnant et en dégénérant, toutes les races des autres animaux.' Ibid., p.382

35 Ibid., p.382

36 Ibid., p.383

37 's'ils venoient de la même souche, s'ils étoient en effet de la même famille, on pourroit les rapprocher, les allier de nouveau, et défaire avec le temps ce que le temps auroit fait.' Ibid., p.383

38 'deux animaux, mâle et femelle, d'une certaine espèce, ont non seulement assez dégénéré pour n'être plus de cette espèce, c'est-à-dire, pour ne pouvoir plus produire avec ceux auxquels ils étoient semblables, mais encore dégénéré tous deux précisément au même point, et à ce point nécessaire pour ne pouvoir produire qu'ensemble'. Ibid., p.389

39 Glass, Temkin and Straus, *Forerunners of Darwin*, p.99

transmutation of species: 'if the species of the ass came from the species of the horse, that could only have come about through successive slight changes; there would therefore exist a large number of intermediate animals between the horse and the ass.'[40] This gradualist model seems to conflict with the model of transformism that is implied by Buffon's fourth argument. However, it is possible that he imagined each of the intermediate forms as a species in its own right, although to what extent such an intermediate species would be unable to interbreed with the parent species, and therefore qualify as a separate species, would surely be doubtful.

Although Buffon had provided five compelling arguments against transformism, he was clearly unable to completely banish the idea from his mind, as speculations about the transmutation of species recur in later volumes of the *Histoire Naturelle*. In volume 9, for example, published in 1761, Buffon speculated about the possible relationship between the animals of the old and the new worlds in terms that left it in no doubt that he considered the transmutation of species a real possibility.

> It would not be impossible that, even without inverting the order of Nature, all the animals of the new world are, in the end, only those of the old world, from which they have long ago taken their origin … . But that ought not to stop us from regarding them as animals of different species; from whatever cause the difference has come, whether it was produced by time, by the climate and the environment, or whether it dates from the creation, it is none the less real.[41]

Again in his *Histoire Naturelle des Oiseaux*, volume 16 of the *Histoire Naturelle*, published in 1770, he makes a very similar argument for the possibility of the

[40] 'si l'espèce de l'âne vient de l'espèce du cheval, cela n'a pû se faire que successivement et par nuances, il y auroit eu entre le cheval et l'âne un grand nombre d'animaux intermédiaires', Buffon, *Histoire Naturelle*, vol. 4, p.390

[41] Il ne seroit donc pas impossible, que, même sans intervertir l'ordre de la Nature, tous ces animaux du nouveau monde ne fussent dans le fond les mêmes que ceux de l'ancien, desquels ils auroient autrefois tiré leur origine. … Mais cela ne doit pas nous empêcher de les regarder aujourd'hui comme des animaux d'espèces différentes : de quelque cause que vienne cette différence, qu'elle ait été produite par le temps, le climat et la terre, ou qu'elle soit de même date que la création, elle n'en est pas moins réelle'. Buffon, *Histoire Naturelle*, vol. 9, p.127

common origin of families of birds, claiming that 'these neighbouring species are probably separated from one another by the influence of the climate, of food, and by the passage of time which brings about every possible combination and brings into evidence all the means of variation, perfection, alternation and degeneration.'[42] But yet between these two examples, in volume 13 of the *Histoire Naturelle*, published in 1765, we find the bald statement that 'the form of each species is a type of which the principle traits are engraved in indelible and forever permanent characters'.[43] It seems from this apparent contradiction that Buffon's opinion on the subject of transformism was far from settled, and changed repeatedly over time. Perhaps the most telling comment on his attitude to transformism comes from volume three of the supplement to the *Histoire Naturelle*, published in 1776, where Buffon admits that 'the relations between species is one of the profound mysteries of Nature, which man will only be able to sound by means of experiments as often repeated as they will be long and difficult.'[44]

Before leaving Buffon, it is necessary to say something about the picture of the history of life and the relationship between species that Buffon evoked in his *Époques de la Nature*, published in 1778, which is quite different from those found in his earlier writings. This famous and influential work provided a sweeping overview of the history of the earth from its beginning to the present day and beyond. For Buffon, the cooling of the earth from its original molten state was the main factor driving development of both the physical globe and life. Life first appeared on the earth during the third of Buffon's seven epochs, during which the

[42] 'ces espèces voisines ne se sont probablement séparées les unes des autres que par les influences du climat, de la nourriture, et par la succession du temps qui amène toutes les combinaisons possibles et met au jour tous les moyens de variété, de perfection, d'altération et de dégénération.' Buffon, *Histoire Naturelle*, vol. 16, p.xxi

[43] 'L'empreinte de chaque espèce est un type dont les principaux traits sont gravés en caractères ineffaçables et permanens à jamais' Buffon, *Histoire Naturelle*, *supplément*, vol. 3, p.32

[44] 'En général, la parenté d'espèce est un de ces mystères profonds de la Nature que l'homme ne pourra sonder qu'à force d'expériences aussi réitérées que longues et difficiles.' Buffon, *Histoire Naturelle*, vol. 13, p.ix

globe was covered by a universal ocean. The first forms of life were therefore marine. However, the waters were still too hot for modern species to survive:

> This great heat could only be suitable for shellfish and fish of other natures; and in consequence it was in the early period of this epoch, that is to say from 30 to 40 thousand years after the formation of the earth, to which can be attributed the existence of lost species for which living analogues cannot be found anywhere.[45]

These early, heat-tolerant species became extinct as the earth cooled. The first terrestrial animals, which Buffon envisaged as including the elephants, rhinoceroses and hippopotamuses whose remains had been found in superficial deposits in northern Europe, Siberia and North America, had to wait until the fifth epoch until conditions were suitable for their appearance. These were larger than modern forms, as the higher temperatures meant that all organic processes operated with more force. As the earth cooled, these creatures migrated south to their current homes in equatorial climates, while they were replaced in the north by new, cold-resistant forms, in a succession that was still continuing:

> The reindeer and other animals which can only survive in the coldest climates came last, and who knows if in the course of time, when the earth will be even colder, there will not appear new species of which the character will differ from that of the reindeer as much as the nature of the reindeer differs from that of the elephant?[46]

At the same time, Buffon still held to his belief that the internal moulds maintained the immutability of species. As he noted in the chapter on the second epoch in the

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[45] 'Cette grande chaleur ne pouvait convenir qu'à d'autres natures de coquillages et de poissions ; et par conséquent c'est aux premiers temps de cette époque, c'est-à-dire depuis trente jusqu'à quarante mille ans de la formation de la terre, que l'on doit rapporter l'existence des espèces perdues dont on ne trouve nulle part les analogues vivants.' Buffon, *Les Epoques de la Nature, Histoire Naturelle, supplément*, vol. 5, pp.94–5

[46] 'Les rennes et les autres animaux que ne peuvent subsister que dans les climats les plus froids sont venus les derniers, et qui sait si par succession de temps, lorsque la terre sera plus refroidie, il ne paraîtra pas de nouvelles espèces dont le tempérament différera de celui du renne autant que la nature du renne diffère à cet égard de celle de l'éléphant ?' Ibid., p.176

Époques de la Nature, 'the type of each species does not change; the internal mould conserves its form and does not vary.'[47] How did Buffon reconcile that immutability of species with the succession of species evident in the history of the earth? In the *Époques de la Nature* he suggested that organic molecules are produced by the action of heat on 'ductile materials'.[48] This provided an ample supply of the raw materials for life, especially in the earlier epochs, when the surface of the globe was hotter. Without saying so explicitly, Buffon seemed to believe that living things could come into being from spontaneously generated organic matter without the need for an internal mould. He claimed, for example, that the animals of the tropical Americas are smaller than those of Africa because they came into being independently at a later date, when the generative powers of the earth had lost some of their vigour.[49] Two years before, in volume 15 of the *Histoire Naturelle*, Buffon had implied that new species which arose were not always successful, and that those forms not well adapted to their environment were ultimately eliminated: 'these imperfect sketches, a thousand times proposed and executed by Nature, which, having scarcely the ability to exist, only survived for a time, have since been struck from the list of beings.'[50] in *Époques de la Nature* nature seemed, however, to only have a limited repertoire of species, as identical conditions would produce identical species. Writing about the distribution of plants on the globe, Buffon wrote that 'in general the same temperature, that is the same degree of heat, produces everywhere the same plants without them having to have been transported there.'[51]

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[47] 'le type de chaque espèce n'a point changé ; le moule intérieur a conservé sa forme et n'a point varié.' Ibid., p.27
[48] Ibid., p.186
[49] Ibid., pp.184–5
[50] 'ces ébauches imparfaïtes, mille fois projetées, exécutées par la Nature, qui ayant à peine la faculté d'exister n'ont dû subsister qu'un temps, et ont été depuis effacés de la liste des êtres.' Buffon, *Histoire Naturelle*, vol. 15, p.cclx
[51] 'en général la même température, c'est-à-dire le même degré de chaleur, produit partout les mêmes plantes sans qu'elles y aient été transportées.' Buffon, *Les Epoques de la Nature, Histoire Naturelle, supplément*, vol. 5, p.187

Jacques Roger has argued that it is impossible to make Buffon a precursor of the transformism of Lamarck and Geoffroy.[52] While it is true that he himself cannot be regarded as a transformist, it cannot be denied that his ideas were a powerful influence on later transformist thinkers. As Lovejoy has rightly pointed out, 'he put the hypothesis of organic evolution before his contemporaries in clear and definite form. He called to their attention, also, the facts of comparative anatomy which constitute one of the principle evidences for the hypothesis.'[53] Buffon's prodigious oeuvre provided an almost inexhaustible mine of ideas on the origin and transmutation of species for later theorists, most notably his successors at the Jardin du Roi, and its post-revolutionary reincarnation as the Jardin des Plantes, Lamarck and Geoffroy. Throughout his career he struggled with the question of the transmutation of species. In later work he created a synthesis which allowed for immutable species to appear and disappear from the slowly cooling surface of the earth without the necessity for transformism as such. Buffon bequeathed to his successors a vision of nature in 'continual flux'; all that was possible for the student of nature was 'to catch her in the instant of his own time, and to cast backwards and forwards a few glances to try to glimpse what she formerly could have been and what in the future she could become.'[54]

Jean-Baptiste Lamarck (1744–1829)

Jean-Baptiste Pierre Antoine de Monet, Chevalier de Lamarck, to give him his full name, was born into a family of minor nobility in Bazentin-le-Petit in Picardy. After a brief military career cut short by an injury resulting from horse-play among his

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[52] Jaques Roger, *Les Sciences de la Vie dans la Pensée Française au XVIIIe Siècle* (Paris, 1993), p.577

[53] Arthur O. Lovejoy, 'Buffon and the problem of species', in Bentley Glass, Owsei Temkin and William L. Straus, *Forerunners of Darwin, 1745–1859* (Baltimore, MA, 1968), pp.111–12

[54] 'la Nature, je l'avoue, est dans un mouvement de flux continuel ; mais c'est assez pour l'homme de la saisir dans l'instant de son siècle, et de jeter quelques regards en arrière et en avant, pour tâcher d'entrevoir ce que jadis elle pouvoit être, et ce que dans la suite elle pourroit devenir.' Buffon, *Histoire Naturelle*, vol. 9, p.127

comrades, he came to Paris in 1771 with the intention of studying medicine and botany. He attended lectures at the Jardin du Roi, where he came to the attention of Buffon, at that time its director. Buffon was impressed by Lamarck's botanical work, and particularly by his non-Linnaean approach to plant classification.[55] Buffon arranged to have Lamarck's work published as the *Flore Françoise* in 1778 and secured him a vacancy in the botanical section of the Académie des Sciences. In 1781 he was made Correspondant du Jardin et Cabinet du Roi, charged with visiting the main European scientific establishments to report back on developments. Lamarck's career was going from strength to strength, until the upheavals engendered by the Revolution led to a sharp change of direction. Remarkably, the Jardin itself was the only savant institution to survive the Revolutionary period largely unscathed. As E.C. Spary has shown in her masterly study of the strategies adopted by the savants in the face of the vicissitudes of the revolutionary period, they were able to skilfully 'fashion a rhetoric of community and fraternity', which successfully deflected criticism during the difficult years 1790–93.[56] In the process, the Jardin du Roi was transformed into the Muséum d'Histoire Naturelle and Lamarck became its professor of the zoology of insects, worms and microscopic animals. Lamarck the botanist had become a zoologist.

The period following his appointment to his new post also saw dramatic development in Lamarck's ideas about the natural world. In his *Recherches sur les Causes des Principaux Faits Physiques*, published in 1794, the same year as the Muséum reopened for classes, he had made clear his belief in the fixity of species and the impossibility of ever knowing anything about the origins of living things, stating categorically that:

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[55] Richard W. Burkhardt, Jr, *The Spirit of System: Lamarck and Evolutionary Biology* (Cambridge, MA, 1977), p.23

[56] E.C. Spary, *Utopia's Garden: French Natural History from Old Regime to Revolution* (Chicago, 2000), p.191

> All the individuals of this nature which exist take their origin from individuals similar to themselves, which taken together constitute the entire species. I therefore believe it is as impossible for man to know the physical cause of the first individual of each species, as to find the physical cause of the existence of matter or of the entire universe. This is at least what my knowledge and reflections lead me to think. If many varieties are produced through the effects of circumstances, these variations do not completely change the species, although we doubtless often make mistakes in identifying as species what are only varieties …[57]

By 1800 Lamarck had completely reversed his opinions on both the origins of life and the mutability of species. According to Richard Burkhardt, the above quotation from 1794 was Lamarck's last definitive statement on the immutability of species.[58]

Both Burkhardt and L.J. Jordanova have suggested that Lamarck's work in conchology was pivotal in his conversion to transformism.[59] They point to the embarrassing failure of naturalists to find living representatives of many of the species found in the fossil record as a major stimulus. Lamarck's uniformitarian geological views made him hostile to catastrophism as a mechanism for extinction, and he could not conceive of another natural mechanism through which species could be lost; his only option was to hypothesise that species had changed their form over millennia rather than disappeared. This is borne out by a passage from his *Système des Animaux sans Vertèbres* of 1801, where Lamarck stated that 'although many of the fossil shells are different from all known marine shells, that in no way

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[57] 'Tous les individus de cette nature, qui existent, proviennent d'individus semblables qui tous ensemble constituent l'espèce entière. Or, je crois qu'il est aussi impossible à l'homme de connoître la cause physique du premier individu du chaque espèce, que d'assigner aussi physiquement la cause de l'existence de la matière ou de l'univers entier. C'est au moins ce que le résultat de mes connoissances et de mes réflexions, me porte à penser. S'il existe beaucoup de variétés produites par l'effet de circonstances, ces variétés ne dénaturent point les espèces : mais on se trompe sans doute souvent, en indiquant comme espèce, ce qui n'est que variété'. Jean-Baptiste Lamarck, *Recherches sur les Causes des Principaux Faits Physiques* vol. 2, (Paris,1794), p.214

[58] Richard W. Burkhardt, Jr, The inspiration of Lamarck's belief in evolution', *Journal of the History of Biology* 5: 2 (1972), p.417

[59] See, for example L.J. Jordanova, *Lamarck* (Oxford, 1984), pp.35–6 and Burkhardt, 'The inspiration of Lamarck's belief in evolution', p.422

proves that these species of shells have become extinct, but only that the species have changed over time, and that currently they have forms different from those of the individuals we find in the fossil record.' [60] Later, in his *Philosophie Zoologique* (1809), he would go on to conclude that: 'If some species are really lost, they can only be, without doubt, those among the large animals that live in dry parts of the globe, where man, through the absolute empire that he exercises, has been able to destroy all the individuals of some of them that he has not wanted to conserve or reduce to domesticity.'[61]

Goulven Laurent has drawn attention to the importance of close analogies found between some fossil shells and living species for the development of Lamarck' s theories.[62] In a series of papers in the *Annales* of the Museum in the first decade of the nineteenth century, as well as in his later taxonomic works on invertebrates, he attempted to identify the living 'analogues' of fossil species. As early as 1799 Lamarck had stated his belief that 'it is absolutely essential to research and determine the living or marine analogues of the great number of fossil shellfish which are found buried in the middle of our vast continents' .[63] The existence of such analogies was, for Lamarck, strong evidence against the reality of the periodic catastrophes and subsequent mass extinctions envisaged by Cuvier and other catastrophist geologists. That some of these living species, while clearly still

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[60] 'quoique beaucoup de coquilles fossiles soient différentes de toutes coquilles marines connues, cela ne prouve nullement que les espèces de ces coquilles soient anéanties, mais seulement que ces espèces ont changé à la suite des temps, et qu'actuellement elles ont des formes différentes de celles qu'avoient les individus dont nous retrouvons les dépouilles fossiles.' Jean-Baptiste Lamarck, *Système des Animaux sans Vertèbres* (Paris, 1801), pp.408–9

[61] 'S'il y a des espèces réellement perdues, ce ne peut être, sans doute, que parmi les grands animaux qui vivent sur les parties sèches du globe, où l'homme, par l'empire absolu qu'il y exerce, a pu parvenir à détruire tous les individus de quelques-unes de celles qu'il n'a pas voulu conserver ni réduire à la domesticité.' Jean-Baptiste Lamarck, *Philosophie Zoologique*, vol.1 (Paris, 1802), p.76

[62] Goulven Laurent, 'Paléontologie(s) et évolution au début du XIXe siècle : Cuvier et Lamarck' *Asclepio* 52: 2 (2000), p.147

[63] 'il est très-essentiel de rechercher et de déterminer les analogues vivans ou marins du grand nombre de coquilles fossiles qu'on trouve enfouies au milieu même de nos vastes continens'. Jean-Baptiste Lamarck, 'Prodrome d'une nouvelle classification des Coquilles', *Mémoires de la Société d'Histoire Naturelle de Paris* 1 (1799) p. 63.

showing an identity with their fossil equivalents, had undergone minor changes in form, was only to be expected on Lamarck's transformist principles; for example, in a paper published in the *Annales* of the Museum in 1802, he noted that he suspected that the fossil mollusc species *Voluta musicalis* 'was the analogue of the *volute musica* of Linnaeus, a little changed in the course of time'.[64]

Of course, Lamarck did not formulate his theories in a vacuum. As Pietro Corsi has pointed out, 'in the late eighteenth-century Parisian scientific community, there was extensive discussion on the origin of life, on the possibility of explaining vital-function characteristics in physical terms, and on interpreting the succession of life forms on earth in evolutionary terms.'[65] Jean-Claude Delamétrie (1743–1817), Pierre-Jean-Georges Cabanis (1757–1808) and Phillipe Bertrand (c.1730–1811), among others, had all speculated on the subject of the transmutation of species. However, Lamarck's position as a professor at the Muséum and the prestige of his work on invertebrate taxonomy put him in a better position than most to elaborate and promote his own brand of transformism. The first hint of Lamarck's new views appeared in the 'Discours d'ouverture' for his course at the Muséum in 1800. His *Système des Animaux sans Vertèbres* published the following year then further elaborated his ideas. The first fully worked out account of the theory appeared in his *Recherches sur l'Organisation des Corps Vivants* in 1802. There was then a gap of some years before the publication of a yet more extensive, detailed and developed exposition of his theory of transformism in the *Philosophie Zoologique* (1809). Lamarck himself stated that this work was 'nothing but a new edition, revised,

[64] 'J'ai plusieurs fois soupçonné que cette coquille fossile étoit l'analogue du *voluta musica* de Linné, un peu changé par la suite du temps'. Jean-Baptiste Lamarck, 'Sur les fossiles des environs de Paris, comprenant des espèces qui appartiennent aux animaux marins sans vertèbres, dont la plupart sont figurés dans la collection des vélins du Muséum', *Annales du Muséum d'Histoire Naturelle* 1 (1802), p.477

[65] Pietro Corsi, *The Age of Lamarck: Evolutionary Theories in France, 1790–1830* (Berkeley, CA, 1988), p.89

corrected and greatly enlarged of my *Recherches sur les Corps Vivants*.'[66] The final major restatement of his theory appeared in his *Histoire Naturelle des Animaux sans Vertèbres* (1815–22). This important work appeared in seven volumes, the first of which was almost entirely given over to an introduction which gave a very full and detailed account of Lamarck's theories of spontaneous generation and transformism. As Pietro Corsi has pointed out, of all of Lamarck's works it was this one, along with his botanical work and memoirs on conchology, that 'won general acceptance among European naturalists'.[67] Burkhardt has also noted that this was 'the work his contemporaries valued the most'.[68] It was therefore through this work that many zoologists throughout Europe were introduced to Lamarck's transformist theories. According to Corsi, its 'Europe-wide prestige contributed powerfully to establishing the scientific worthiness of the transformist hypothesis' among those willing to accept it.[69] As we will see in subsequent chapters, this certainly seems to have been the case in Edinburgh.[70] As Burkhardt has noted, the 'basic structure of Lamarck's theory remained constant from 1802 through the last exposition of his view'.[71] Given the relative stability of Lamarck's thought, I will draw evidence from all three of the major statements of his theory to build up a picture of his ideas, although perhaps with rather more emphasis on the *Histoire Naturelle*, as this is the work which seems to have reached the widest audience internationally in the 1820s and early 1830s.

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[66] 'La *Philosophie zoologique* n'est autre chose qu'une nouvelle édition refondue, corrigé et fort augmenté de mon ouvrage intitulé : *Recherches sur les Corps vivants*.' Lamarck, *Philosophie Zoologique*, p.73

[67] Corsi, 'The importance of French transformist ideas', p.222

[68] Burkhardt, *Spirit of System*, p.40

[69] Corsi, *Age of Lamarck*, p.211

[70] Although, in an important exception to the general neglect of the *Philosophe Zoologique* in Britain on its publication, a surprisingly favourable review of this book by Lockhart Muirhead, the professor of natural history at the University of Glasgow, appeared in 1811 in the *Monthly Review* ([Lockhart Muirhead], Review of Lamarck's *Philosophie Zoologique*, *Monthly Review* 55 (August 1811), pp.473–84). For a more detailed discussion of this review, see Jonathan R. Topham, 'Science, print and crossing borders: Importing French science books into Britain, 1789–1815, in David N. Livingstone and Charles W.J. Withers, *Geographies of Nineteenth-Century Science* (Chicago, 2011), pp.329–30

[71] Burkhardt, *Spirit of System*, p.144

Before examining Lamarck's theories regarding spontaneous generation and transformism, it seems important to take a look at how his ideas were underpinned by his understanding of the meanings of 'nature' and 'life'. In the *Histoire Naturelle* Lamarck defines nature as 'an order of things, alien to matter, determinable through the observation of bodies, and of which the whole constitutes in its essence an inalterable power, constrained in all its actions, and constantly acting in all parts of the universe.'[72] Based on this description, it seems clear that the Newtonian laws of gravitation would have provided an ideal illustration of Lamarck's nature in action, and this may well have been the model he had in mind. Earlier in the same work he discussed the operation of nature on living things, describing it as 'the power, in some manner mechanical, which has given existence to the diversity of animals, and of necessity made them what they are.'[73] Living things were therefore subject to unchanging natural laws in the same way that the planets were constrained in their orbits by the laws of gravitation. Lamarck made it clear that nature was not an intelligence, but merely applied mechanically the laws of nature established by God; 'an intermediary between GOD and the physical parts of the universe, for the execution of the divine will'.[74] While Corsi has stated the opinion that 'there is little doubt that he was not a fervent nor even a lukewarm Christian, and I personally doubt he was even a deist', in the absence of any unambiguous evidence for his atheism, and given the abundant references to the 'supreme author' and discussion of the role of the divine will throughout Lamarck's work, it seems rash to rule out altogether the possibility that he was sincere in his deism.[75] Jordanova's conclusion

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[72] 'La *nature* est un ordre des choses, étranger à la matière, déterminable par l'observation des corps, et dont l'ensemble constitue une puissance inaltérable dans son essence, assujétie dans tous ses actes, et constamment agissante sur toutes les parties de l'univers.' Jean-Baptiste Lamarck, *Histoire Naturelle des Animaux sans Vertèbres* (Paris, 1815), p.317
[73] 'la puissance, en quelque sorte mécanique, que a donné l'existence aux animaux divers, et que les a fait nécessairement ce qu'ils sont.' Ibid., p.26
[74] 'un intermédiaire entre DIEU et les parties de l'univers physique, pour l'exécution de la volonté divine.' Ibid., p.331
[75] Pietro Corsi, 'Idola Tribus: Lamarck, politics and religion in the early nineteenth century', in Aldo Fasolo (ed.), *The Theory of Evolution and its Impact* (Milan, 2012), p.22

that, although we can know little of his religious beliefs due to the paucity of surviving evidence, 'it is fair to assume that he was not a devout Catholic' does, however, seem a fair one.[76]

We turn now to Lamarck's concept of organic life. As he stated in his *Histoire Naturelle*, for Lamarck, 'every action or phenomenon observed in a living body is at the same time a physical action or phenomenon, and the product of organisation.'[77] This conception of the nature of life is fundamentally opposed to vitalism; life is not the manifestation of an immaterial essence, but rather the product of the organisation of matter acting in accordance with essentially mechanical laws of nature. It is therefore a radically materialist theory of life. Lamarck goes on to explain how nature goes about organising non-living matter to create life. This process is directly analogous to the normal process of fertilisation which takes place during sexual reproduction. Lamarck, who favoured the theory of epigenesis in the then current debate between epigenesists and preformationists, believed that a 'subtle vapour' emanating from the seminal fluid acted as an organising principle that transformed the ovum into a living being. Supplied with the right materials by nature, Lamarck believed that 'subtle fluids', principally heat or electricity, present in the environment, could create life from non-living matter. As he himself put it:

> Why may not heat and electricity which, in certain parts of the world and during certain seasons are so abundant in nature, above all at the surface of the earth, act on certain substances in a favourable state and circumstances which it finds there, and perform that which the subtle fertilising vapour performs on the embryos which it organises and prepares to enjoy life?[78]

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[76] Ludmilla Jordanova, 'Nature's powers : A reading of Lamarck's distinction between creation and production', in James R. Moore (ed), *History, Humanity and Evolution: Essays for John C. Greene* (Cambridge, 1989), p.73

[77] 'Tout fait ou phénomène observé dans un corps vivant, est à-la-fois un fait ou phénomène physique, et un produit de la organisation.' Lamarck, *Histoire Naturelle*, p.12

[78] 'Pourquoi la *chaleur* et l'*électricité* qui, dans certaines contrées et dans certaines saisons, se trouvent si abondamment répandues dans la nature, sur-tout à la surface du globe, n'y opéreroient-elles pas sur certaines matières qui se rencontrent dans l'état et les circonstances favorables, ce que la *vapeur subtile* des matières fécondantes exécute sur les embryons qu'elle

In his *Organisation des Corps Vivants* the subtle fluids were given as heat, which he usually referred to in the guise of caloric, and electricity. In his *Histoire Naturelle* twelve years later he included the magnetic fluid and possibly light to the list, but the basic theory remained the same.[79] Indeed Lamarck considered that all of these fluids may simply be different states of the same substance.[80] These fluids were 'subtle' because they were *incontenable*, in other words they could not be contained by surrounding matter, but could flow through it. They were considered to be so tenuous that they could not be perceived directly by the senses, but only known from their effects.[81] One of these effects was manifested in their ability to act on *contenable* fluids, such as water or the bodily fluids of living things. The rotation of the earth on its axis and its movement around the sun caused continual fluxes of subtle fluids on the surface of the planet, whose action on suitable matter could bring life into being.[82] It was the action of these subtle fluids on tiny, spontaneously-generated gelatinous bodies that transformed them into the simplest living beings. In the *Organisation des Corps Vivants* Lamarck explained how the organisation of living beings comes about in more detail: 'In this way the uncontainable fluids trace the first traits of the simplest organisation, and then the containable fluids develop it by their movements and other influences'.[83] In this way the simplest forms of life were, and continue to be, produced from inanimate matter through the operation of natural laws. These first 'sketches of life', as Lamarck calls them, were therefore at the same time both the oldest and the youngest of living things. Lamarck believed that, as the most simple organisms found in nature lived in water, 'it can be

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organise et rend propres à jouir de la vie ?' Jean-Baptiste Lamarck, *Recherches sur l'Organisation des Corps Vivants* (Paris, 1802), p.101

[79] Lamarck, *Histoire Naturelle*, p.43

[80] Lamarck, *Organisation des Corps Vivants*, p.196

[81] Ibid., p.159

[82] Lamarck, *Histoire Naturelle*, p.45

[83] 'Ainsi les fluides *incontenables* tracent d'abord les premiers traits de la plus simple organisation, et ensuite les fluides *contenables* par leurs mouvements et leurs autres influences la développent.' Lamarck, *Organisation des Corps Vivants*, pp.107–8

regarded as a fact that it is uniquely in water that the animal kingdom had its origin.'[84]

These 'living points' had no organs, could not move independently, and relied for nutrition on the deposition of materials brought into their bodies by the fluxes of containable fluids, driven ultimately by the movements of the subtle fluids which surrounded them in their environment. However, the process of development did not stop at this point, but continued to complicate the structures of these simplest of living things. The first organ to appear was the digestive tract, at first with only one opening, as in polyps. As living things became more complex, the fluxes of subtle fluids became internalised, so that the further development of the organism was less dependent on external influences, and living things were less at the mercy of the environment for their existence. Further development then brought other organs into being in a step-wise manner. With these new organs came new faculties, which could not exist without the organs appropriate to them. Changes that occurred during the lifetime of an organism were transmitted to its offspring, and so the upward trend of development continued generation after generation. All things being equal, organisms would tend to ascend through a fixed series of increasingly complex forms over time. However, according to Lamarck, all things were not equal, as the environment had a strong effect on the development of the bodies of animals. Changes induced by the vicissitudes of the environment would tend to push animals off the fixed developmental track they would otherwise be constrained to follow. As Lamarck puts it in his *Philosophie Zoologique*:

> It will, in effect, be evident that the state in which we see all animals is, on one hand, the product of the increasing *composition* of their organisation, which tends to form a regular gradation, and, on the other, the product of a multitude of very different circumstances, which tend continually to destroy

[84] 'on peut regarder comme une vérité de fait, que c'est uniquement dans l'eau que le règne animal a pris son origine.' Lamarck, *Organisation des Corps Vivants*, p.105

> the regularity of the gradations of their increasing composition of organisation.[85]

As Burkhardt has pointed out, there is no simple relationship between an ideal hierarchy and the external influences which tend to deform it, as environmental influences also perform a vital role at least in the early stages of development of all living things in promoting increasing complexity and organisation; Lamarck's 'explanation of the production of the simpler invertebrates demonstrates that in his view the power of life was not opposed to environmental influences but, on the contrary, grew directly out of them.'[86] In the *Histoire Naturelle*, Lamarck sums up his theory in four laws:

> *First law*: Life, through its own forces, tends continually to increase the volume of every body which possesses it, and to extend the dimensions of these parts up to a pre-determined point.
> *Second law*: The production of a new organ in an animal body results from a new need which arises and continues to make itself felt and of a new movement which derives from and is maintained by this need.
> *Third law*: There is a direct relationship between the development and power of action of organs and their continuing use.
> *Fourth law*: All that has been acquired, developed or changed in the organisation of individuals during their lives is conserved during reproduction and transmitted to the new individuals which arise from those which underwent the changes.[87]

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[85] 'Il sera, en effet, évident que l'état où nous voyons tous les animaux est, d'une parte, le produit de la *composition* croissante de l'organisation qui tend à former une *gradation régulière*; et, de l'autre parte, que est celui des influences d'une multitude de circonstances très différentes que tendent continuellement à détruire la régularité dans la gradation de la composition croissante de l'organisation.' Lamarck, *Philosophie Zoologique*, vol. 1, p.221

[86] Burkhardt, *Spirit of System*, p.157

[87] '*Première loi*: La vie, par ses propres forces, tend continuellement à accroître le volume de tout corps que la possède, et à étendre les dimensions de ces parties, jusqu'à un terme qu'elle amène elle-même.
Deuxième loi: La production d'un nouvel organe dans un corps animal, résulte d'un nouveau besoin survenu qui continue de se faire sentir, et d'un nouveau mouvement que ce besoin fait naître et entretient.
Troisième loi: Le développement des organes et leur force d'action sont constamment en raison de l'emploi de ces organes.
Quatrième loi: Tout ce qui a été acquis, tracé ou changé, dans l'organisation des individus, pendant le cours de leur vie, est conservé par la génération, et transmis aux nouveaux

This turns the argument for design on its head, since animals are not designed to suit their environments, but rather their environments, and the needs that they find themselves subject to, have moulded them into what they are. As Lamarck puts it in the *Organisation des Corps Vivants*:

> It is not organs, that is to say the nature and forms of the parts of the animal body, which have given rise to its habits and faculties; but, on the contrary, the habits, way of life and circumstances which its ancestors have encountered which have over time determined the form of its body, the number and state of its organs, and finally the faculties of which it enjoys the use.[88]

Thus new needs, brought about either by the increasing level of organisation of the organism or by environmental change, produce new habits, which in turn lead to the development of existing organs or the production of new ones to meet that need. Conversely, organs which are no longer required and consequently no longer used, will diminish and ultimately disappear. This all takes place in an entirely mechanical fashion through the flow of fluids to the appropriate organ, stimulating its growth and development. Dismissing the term 'inheritance of acquired characteristics' often used by later commentators, but never by Lamarck himself, Corsi points out that what are inherited are not physical characteristics but biological processes; 'it was not "characters" that were acquired during the lifetime of the organism, but only a higher or lower degree of organic fluid flow or, in

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individus qui proviennent de ceux qui ont éprouvé ces changemens.' Lamarck, *Histoire Naturelle*, pp.181–2

[88] Ce ne sont pas les organes, c'est-à-dire la nature et la forme des parties du corps d'un animal, qui ont donné lieu à ses habitudes et à ses facultés ; mais ce sont au contraire ses habitudes, sa manière de vivre, et les circonstances dans lesquelles se sont rencontrés les individus dont il provient, qui ont avec le temps constitué la forme de son corps, le nombre et l'état de ses organes, enfin les facultés dont il jouit. Lamarck, *Organisation des Corps Vivants*, p.50

general, a small difference in fluid distribution patterns.'[89] It is often forgotten that Lamarck provided a materialist explanation for the development or loss of organs through use and disuse.

Lamarck's belief in the mutability of species led him to propose that they did not exist in nature outside the context of a particular moment in geological time; they 'only have a relative permanence, and are only temporarily invariable.'[90] Species only seemed stable over the relatively short period of human history; in the much longer time-frame of geological time, this apparent stability would disappear.[91] The classification of living things into species by naturalists was therefore entirely artificial. Nonetheless, the species concept was useful in the study of living things and Lamarck did not propose abandoning it altogether. He suggested that 'it was useful to give the name "species" to all collections of similar individuals, which reproduction has perpetuated in the same state as long as their circumstances do not change enough to vary their habits, their character and their form.'[92] Classification therefore became simply a tool for the naturalist, but did not reveal the true order of nature, which could only be understood as a process unfolding over vast spans of time. In a memorable passage from a memoir published in the annals of the Museum for 1802, Lamarck compared natural historians who did not take account of the vastness of the history of the earth to a group of insects living in a building:

> I seem to hear some of those little insects who live for only a year, living in some corner of a building, and who one can imagine busy among themselves consulting their traditions to pronounce on the age of the building in which

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[89] Pietro Corsi, 'Jean-Baptiste Lamarck, "From Myth to History"', in Snait B. Gissis and Eva Jablonka (eds), *Transformations of Lamarckism: From Subtle Fluids to Molecular Biology* (Cambridge, MA, 2011), p.12

[90] 'les *espèces*, parmi eux, n'ont qu'une constance relative, et ne sont invariables que temporairement.' Lamarck, *Philosophie Zoologique*, p.113

[91] Lamarck, *Philosophie Zoologique*, p.112

[92] 'il est utile de donner le nom d'*espèces* à toute collection d'individus semblables, que la génération perpétue dans le même état tant que les circonstances de leur situation ne changent pas assez pour faire varier leurs habitudes, leur caractère et leur forme.' Lamarck, *Philosophie Zoologique*, pp.113–14

> they find themselves; going back through their paltry history as far as the twenty-fifth generation, they unanimously decide that the building that shelters them is eternal and that it has always existed, as it has always appeared the same to them, and they have never heard it said that it had any beginning.[93]

Lamarck's theory also has important implication for the overall shape of the history of life on earth. Firstly, he considered that animals and plants had entirely separate origins; both owed their origins to fluxes of subtle fluids acting on matter, but differed in the nature of the matter in the two cases. If the tiny mass of matter into which organisation was to be introduced was gelatinous, it would become an animal, if it was mucilaginous, it would become a plant.[94] For this reason plants lacked the fundamental property of irritability possessed by all forms of animal life. In consequence Lamarck denied absolutely the existence of intermediate plant-animals, or zoophytes, which many natural historians viewed as a link between the animal and vegetable kingdoms.

Not only did Lamarck consider that plants and animals had separate origins, but he also envisaged that the animal kingdom could be classified into two main branches with separate origins. One, consisting of the infusoria, polyps and radiaria (and in later versions also the acephala and the mollusca) were ultimately descended from the first 'living points' which had come into being through spontaneous generation from non-living matter. The second branch, consisting of all the remaining invertebrates, including insects, crustaceans and annelids, owed their origin to the spontaneous generation of parasitic worms within the bodies of the animals

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[93] 'Il me semble entendre ces petits insectes que ne vivent qu'une année, qui habitent quelque coin d'un bâtiment, et que l'on supposeroit occupés à consulter parmi eux la tradition, pour prononcer sur la durée de l'édifice où ils se trouvent : remontant dans leur chétive histoire jusqu'à la vingt-cinquième génération, ils décideroient unanimement que la bâtiment à qui leur sert d'asile est éternel et qu'il a toujours existé ; car ils l'ont toujours vu le même, et ils n'ont jamais entendu dire qu'il ait eu un commencement.' Lamarck, 'Sur les fossiles des environs de Paris', p.303

[94] Lamarck, *Philosophie Zoologique*, p.397

belonging to the other, older branch. [95] This shown clearly in the diagram from figure 1, from the *Histoire Naturelle des Animaux sans Vertèbres*, which illustrates the two main branches of the invertebrates. The relationship of the vertebrates is left somewhat ambiguous in this diagram. In the earlier diagram from the *Philosophie Zoologique* shown in figure 2 Lamarck had made the position of the vertebrates less ambiguous. This reflects changes in Lamarck's thinking between 1809 and 1815. Figure 2 reflects Lamarck's ideas as they stood in 1809, when he published his *Philosophie Zoologique*. Here the vertebrates are clearly shown as part of the series descending from the parasitic worms. However, by 1815 he had become uncertain of where the connection lay between invertebrates and vertebrates, and could only conclude that 'this transition is still unknown'.[96] Among other less significant changes, Lamarck also changed his mind about the place of the molluscs between 1809 and 1815, moving them from the second to the first series. However, despite these changes, the overall pattern remained largely unchanged.

[95] Lamarck, *Histoire naturelle*, p.455
[96] Ibid., p.460

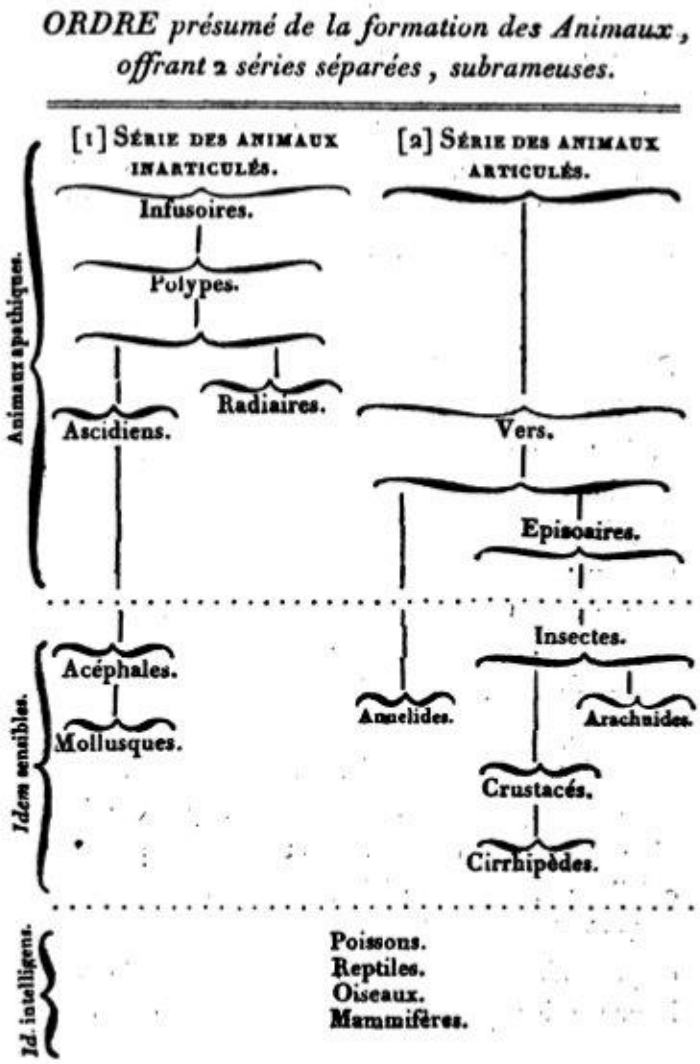

Figure 1 'Presumed order of the formation of animals in two separate series.' From Lamarck's *Histoire Naturelles des Animaux sans Vertèbres*.[97]

[97] Lamarck, *Histoire Naturelle*, p.457

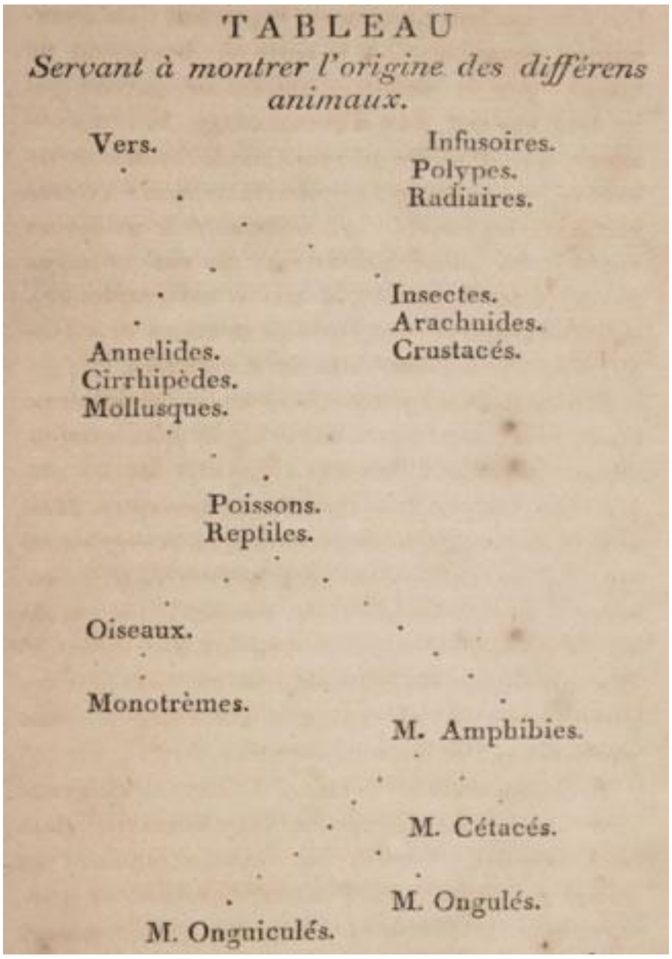

Figure 2 'The origins of the main subdivisions of the animal kingdom.' From Lamarck's *Philosophie Zoologique*.[98]

It is tempting to view the diagrams in figures 1 and 2 as genealogical trees. However, as Lamarck believed that new animals were continuously coming into being through spontaneous generation even at the present time, which then proceeded through the series of stages of Lamarck's system, it would be more accurate to think of them as developmental pathways that have been followed, and continue to be followed, by all animal forms as they pass through the different levels of organisation. The branching pattern results from the influence of environmental factors distorting the series, as discussed above.

One feature of Lamarck's arguments which may be surprising to modern readers is the almost total absence of evidence drawn from the fossil record in his major

98 Lamarck, *Philosophie Zoologique*, vol.2 (Paris, 1809), p.463

theoretical works. This is all the more surprising given the care he had taken in his work in conchology to identify fossil 'analogues' for modern species. Jordanova has commented that fossils 'were certainly an important element in the *genesis* of his historical approach to nature, but a detailed examination of the fossil record had no place in his arguments for transformism.'[99] Corsi has explained this as a result of Lamarck's rejection of extinction, suggesting that:

> Lamarck never relied on paleontological data, convinced as he was that all fossils, with a few exceptions (essentially the remains of animals destroyed by man), were still alive somewhere on Earth or at the bottom of the seas. For Lamarck, as for a number of his followers, the beautiful fossil ammonites found embedded in rocks were still thriving in the oceans.[100]

Although there may be some merit in this argument, it is also true that Lamarck did believe that, although species did not become extinct, they might be drastically transformed over time; although the direct ancestors of the ammonites might still have been with us, they might not be readily recognisable as such after such a great span of time. I would also argue that Lamarck's failure to use evidence from the fossil record is entirely in keeping with his style of working, which was highly speculative and showed little concern with finding factual evidence to back up his theories. Burkhardt has commented that his inability to convince large numbers of his contemporaries of the truth of his theories was at least in part owning to the fact he was: 'Almost flamboyant, – considering the circumstances – in his inattention to the sort of details needed to give his theory some semblance of legitimacy in the eyes of most of his contemporaries, he left his work open to ridicule.'[101]

Before leaving Lamarck, I will take a brief glance at how his work was received both in France and internationally. There has been some debate about the extent to which Lamarck's ideas were taken up, rejected or ignored during his lifetime as well as

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[99] Jordanova, *Lamarck*, p.35
[100] Corsi, 'Lamarck, "From Myth to History"', p. 13
[101] Burkhardt, 'Lamarck, evolution, and the politics of science', p.296

after his death. Some historians, notably Adrian Desmond, have argued that Lamarck's transformist theories were only ever acceptable to a fringe of extreme radical thinkers.[102] Others, such as Pietro Corsi, stress, that in the 1820s at least, Lamarck's ideas were widely known and discussed, even if they caused anxiety in some more conservative intellectual circles. Contrary to the view of Lamarck as an isolated thinker, taken seriously by only a small number of marginalised radical thinkers, Corsi has argued that:

> During the 1820s, the golden decade for Lamarck's reputation in France and Europe, his doctrines were subjected to a variety of criticisms and only a few commentators insisted on the dangerous leaning of his teaching and theorizing. From Edinburgh to Göttingen, from Turin to Paris, it was common to pay homage to the old naturalist, who had left a monument of taxonomic achievement such as the *Histoire naturelle des animaux sans vertèbres*.[103]

Burkhardt has also argued that in the 1820s transformism was not considered as dangerous a doctrine as some scholars have suggested. According to Burkhardt:

> Lamarck's theory of evolution was rejected not because the idea of organic mutability was virtually unthinkable at the time, but because Lamarck's support of that idea was unconvincing and because more generally, the kind of speculative venture Lamarck had embarked upon did not correspond with contemporary views of the kind of work a naturalist should be doing.[104]

In this view, where Lamarck's ideas were rejected, it was not because they were considered morally, politically or philosophically dangerous, but because they seemed to be the fanciful daydreams of a system builder, which were not sufficiently grounded in experiment and observation; criticisms which, as we have seen, are not entirely without foundation.

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102 See, for example, Desmond, *The Politics of Evolution*
103 Corsi, 'Idola tribus', p.27
104 Burkhardt, *Spirit of System*, pp.201–2

Lamarck also had powerful enemies in the world of French natural history, in particular his younger colleague at the Muséum, Georges Cuvier, who seized any opportunity to sully his reputation and discredit his theories. Burkhardt acknowledges that 'Georges Cuvier's magisterial and disapproving presence has long been recognized as a factor in the poor reception of Lamarck's evolutionary theory by his contemporaries.'[105] A quite breathtakingly mean-spirited eulogy to Lamarck by Cuvier was delivered to the Académie des Sciences on 26 November 1832. Cuvier was strongly opposed to transformism, and, judging from his eulogy, seems also to have had a strong personal dislike for Lamarck. A translation of this eulogy into English was later published in the *Edinburgh New Philosophical Journal* of January 1836. The caricature of Lamarck's theories which it presented significantly shaped their reception in the English-speaking world, and continues to do so to this day. Madeleine Barthélemy Madaule has noted that 'Cuvier's eulogy, as disseminated by the Edinburgh school, certainly is at the root of a simplistic interpretation of Lamarck.'[106] Cuvier's main strategy was to misrepresent Lamarck's theory as suggesting that animals somehow willed themselves to evolve. In his eulogy he stated quite falsely that Lamarck had said that 'it is the desire of flying that has converted the arms of all birds into wings, and their hairs and scales into feathers.'[107] This impression was reinforced by the similar reading of Lamarck's theory to be found in influential *Nouveau Dictionnaire d'Histoire Naturelle* (first edition 1803–4, second edition 1816–19) of Julien-Joseph Virey (1775–1847). Corsi has pointed out that Virey 'helped to perpetuate the myth that Lamarck believed in the animal's "will" to change its anatomic and organic structure', and that there is evidence that some British natural historians, such as William Kirby (1759–1850), were seriously misled by his work.[108]

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105 Burkhardt, *Spirit of System*, p.292

106 Madeleine Barthélemy Madaule (trans. M.H. Shank), *Lamarck the Mythical Precursor: A Study of the Relations between Science and Ideology* (Cambridge, MA, 1982), p.77

107 Georges Cuvier, 'Memoir of M. de Lamarck', *The Edinburgh New Philosophical Journal* 20: 39 (1836), pp.14–15

108 Corsi, *Age of Lamarck*, p.177

With a little more justice, Cuvier also claimed that Lamarck's theories are largely unsupported by evidence, and implied that they are not compatible with the findings of comparative anatomy:

> A system established on such foundations may amuse the imagination of a poet; a metaphysician may derive from it an entirely new series of systems; but it cannot for a moment bear the examination of any one who has dissected a hand, a viscus, or even a feather.[109]

However, Corsi's belief that Cuvier's opinion of Lamarck and his theories was not shared by a large number of his contemporaries is borne out by the fact that his scurrilous eulogy 'caused uproar and indignation'.[110] Several members of the Academy asked him to make changes to it to soften its tone, but he refused. [111] It was finally presented to the Academy of Sciences only in 1832, after Cuvier's own death.

In addition, Corsi has argued that the image of Lamarck as a neglected martyr of science in his own time was partly a myth created by his supporters in reaction to the attacks of Cuvier and his other enemies and perpetuated by subsequent generations of historians, and that in fact

> the myth of the poor, isolated, and blind Lamarck was created and diffused by his supporters at the height of his success with the reading public and the intellectual elites: there is no doubt that during the 1820s, Lamarck reached the peak of his popularity in France and in the rest of Europe. [112]

To sum up, we have seen that Lamarck's transformism was grounded in the belief that life, which was entirely a consequence of the organisation of matter, was

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[109] Cuvier, 'Memoir of M. de Lamarck', p.15
[110] Corsi, 'Lamarck, "From Myth to History"', p.16
[111] Toby A. Appel, *The Cuvier–Geoffroy Debate: French Biology in the Decades before Darwin* (New York, 1987), p.169

[112] Ibid., p. 15

constantly coming into being as a result of the action of fluxes of subtle fluids on non-living matter. Depending on the nature of the matter in question, that life would be either vegetable or animal. All animal life was further subdivided into those organisms that could claim descent from simple monads, and those descended from parasitic worms formed inside the bodies of already existing creatures. Two forces were at work which directed the further development of living things. The first was an inherent tendency to increase in complexity over time, which operated quite mechanically. The second depended on the influence of the environment, which distorted the otherwise simple progress of organisation. Environmental change and increasing complexity led to the emergence of new needs, which generated new habits in the animal, which in turn led to the further development of already existing organs or the production of entirely new ones. These changes were then transmitted to subsequent generations by inheritance. This, in very schematic form, was Lamarck's theory of transformism, which he maintained in more or less this form from 1800 until the end of his life. While he was promoting and elaborating this theory, one of his younger colleagues at the Muséum, Étienne Geoffroy Saint-Hilaire was developing his own, rather different, transformist theory. It is to him that we turn next.

Étienne Geoffroy Saint-Hilaire (1772–1844)

Originally destined for the church by his family in Étampes, near Paris, the outbreak of the Revolution led Geoffroy to change his plans, and instead he became a student of medicine in Paris at the College of Cardinal Lemoine, where he became a friend and protégé of the mineralogist René-Just Haüy (1743–1822). Haüy was to have good reason to be glad of his friendship with Geoffroy, who helped him escape from prison in 1792 just before the massacres of prisoners of September of that year, probably saving his life. Haüy was able to repay his debt to Geoffroy when he helped him gain the post of sub-curator and sub-demonstrator at the Jardin des

Plantes in 1793.[113] When the Jardin was transformed into the Muséum d'Histoire Naturelle later the same year Geoffroy was appointed to one of the zoology chairs, responsible for quadrupeds, cetaceans, reptiles, birds and fish, despite his almost complete ignorance of zoology and at the young age of 21. However, he did not take long to get to grips with his new position, and in 1796 he published a 'Memoir on the natural relations of the makis lemur L.', in which, building on the ideas of Buffon, he first proposed the principle of unity of plan in vertebrate anatomy that was to become one of the central tenets of his work.

In 1798 he left the Muséum to accept an invitation to join the party of scholars and scientists that accompanied Napoleon on his ill-fated expedition to Egypt. He was to remain in Egypt until 1801. His stay there was to have a significant impact on his ideas; Toby Appel has noted that the 'metamorphosis of Geoffroy's thought took place largely as a result of the three years he spent in Egypt'.[114] It was certainly here that he first became acquainted with the anatomy of crocodiles, which was to play a large role in his later work. He also amassed a significant collection of mummified animals from Ancient Egyptian tombs, which would also play a significant role in debates on transformism back in Paris. On his return to Paris, Geoffroy returned to his post at the Muséum. In 1808 he was sent on a mission to inspect natural history collections in Portugal, which he completed with notable success, and in 1809 he was appointed to the chair of zoology at the Faculty of Sciences in Paris.

In 1818 Geoffroy published the first volume of what was to be his most influential work, his *Anatomie Philosophique*. In this work he stated that 'it is fairly easy to reduce to the unity of composition the diverse forms of organization of the

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[113] Hervé le Guyader, *Geoffroy Saint-Hilaire: A Visionary Naturalist* (trans. Marjorie Grene) (Chicago and London, 2004), p.5
[114] Appel, *The Cuvier–Geoffroy Debate*, p.71

vertebrates'.[115] His belief that the anatomies of all vertebrates conformed to a single ideal plan led him to reject the idea of a single scale of being. While comparative anatomy had traditionally made the human body its point of reference for the study of the 'lower' animals, which could be arranged in a series of decreasing perfection from the human form, Geoffroy's theories led him 'not to give preference to any anatomy in particular, but to consider the organs first where they are at the *maximum* of their development, in order to follow them step by step to the zero of their existence.'[116] This was a radical departure from conventional notions of the relationships between different animal groups, including those of his colleague at the Muséum Lamarck. In the second volume of the *Anatomie Philosophique* Geoffroy went on to propose four fundamental principles for the study of comparative anatomy, these were the 'theory of analogues, the principle of connections, the elective affinities of organic elements, the balancement of organs'.[117] I will discuss these principles in terms of their application to the skeletons of animals, as this is where most of Geoffroy's work was focussed, but the same principles applied to other organ systems too. By the 'analogues', Geoffroy meant what we would now call homologues, meaning that anatomists should expect to find homologous bones in the bodies of all vertebrates. The principle of connection dictated that, while bones may differ in shape and size between species so as to become virtually unrecognisable, the ways in which they were connected would remain constant. The principle of elective affinities was derived from the theory proposed of Geoffroy's friend and colleague Étienne Serres (1786–1868), who proposed that all organs had primitively been double, and that non-paired organs were the result of the fusion of a symmetrical pair of organs during foetal development.[118] Finally, by balancement

[115] Geoffroy Saint-Hilaire, 'Preliminary discourse' from *Anatomical Philosophy: On the Respiratory Organs with Respect to the Determination and the Identity of their Bony Parts* (1818), in Guyader, *Geoffroy Saint-Hilaire*, p.34
[116] Ibid., p.34
[117] Geoffroy Saint-Hilaire, 'Preliminary discourse' from *Anatomical Philosophy: Of Human Monstrosities* (1822), in Guyader, *Geoffroy Saint-Hilaire*, p.45
[118] See, for example, Étienne Serres, *Recherches d'Anatomie Transcendantal et Pathologique* (Paris, 1832), p.17

of organs, Geoffroy meant to suggest that when one bone was very highly developed, the surrounding bones would be proportionally underdeveloped. Based on these principles, Geoffroy claimed that there was indeed only one animal, modified in a myriad different ways: 'all being formed of homologous organs, do they not seem to you to be modifications of the same being, and this abstract being, or common type, which it is always possible to designate under the same name, and which currently you call the vertebrate animal?'[119]

Between the publication of the two volumes of the *Anatomie Philosophique*, Geoffroy announced a radical extension of his theory of unity of plan to insects in his lecture course at the Faculty of Science in 1820. For Geoffroy, the exoskeleton of the insect was homologous with the spinal column of vertebrates. The internal organs of insects were therefore enclosed within their spines. As Geoffroy put it: 'In the last analysis, we arrive at this result: every animal lives inside or outside its vertebral column.'[120] Geoffroy had already embraced Serres' theory that higher vertebrates passed consecutively through forms that represented the adult forms each of the main divisions of the lower vertebrates during foetal development. He now extended this theory to include insects: 'insects occupy a place in the series of the ages and of the developments of the higher vertebrates, that is to say, that they actualize one of the conditions of their embryo, as the fishes do for one of those of their fetal stages.'[121]

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[119] 'tous, comme étant formés d'organes analogues, ne vous semblent-ils pas les modifications d'un même être, de cet être abstrait, ou type commun, qu'il est toujours possible de désigner par un même nom, et que présentement vous appeliez animal vertébré?' Étienne Geoffroy Saint-Hilaire and Étienne Serres, 'Rapport fait à l'Académie royale des Sciences sur une mémoire de M. Roulin, ayant pour titre : Sur quelques changemens observés dans les animaux domestiques transportés de m'ancien monde dans le nouveau continent', *Mémoires du Muséum d'Histoire Naturelle* 17 (1828), p.211

[120] Étienne Geoffroy Saint-Hilaire, 'First memoir', from *Memoirs on the Organization of Insects* (1820), in Guyader, *Geoffroy Saint-Hilaire*, p.58

[121] Étienne Geoffroy Saint-Hilaire, 'General considerations on the vertebra' (1822), in Guyader, *Geoffroy Saint-Hilaire*, p.71

In 1830 a paper was submitted to the Academy of Sciences by Pierre Stanislas Meyranx and M. Laurencet, the latter being a man so obscure his first name is now unknown. This paper suggested that the bodies of molluscs, specifically cephalopods, also partook of the same unity of plan as vertebrates. Geoffroy was delighted with this further extension of the principle of unity of plan. It was Geoffroy's espousal of this further extension of his theory that was to lead to a heated debate between him and Georges Cuvier that caused a Europe-wide controversy. In Weimar the aged Goethe was even moved to exclaim 'The volcano has come to an eruption; everything is in flames'.[122] Relations between Geoffroy and Cuvier, who had once been close friends, had been deteriorating for some time, and for Cuvier Geoffroy's reaction to Meyranx and Laurencet's paper was the final straw. While debate was highly politicised and brought to a head many of the simmering tensions of the period, both scientific and social, it is unfortunately beyond the scope of my thesis to fully explore these issues, and I can only direct the reader to Toby Appel's excellent account of the debate.[123]

The acrimonious dispute between Cuvier and Geoffroy has in the past often been presented as turning on the attitudes of the participants towards transformism, although as Appel has pointed out 'recent studies, however, have minimized the relevance of the evolution controversy, for evolution was scarcely mentioned in the proceedings of 1830.'[124] Although transformism played little or no part in the debate, Geoffroy had in fact been an avowed transformist since at least 1825. There is no evidence to suggest that he was a transformist prior to this date, although this does not prove that he did not toy with the idea earlier.[125] It is not too difficult to see how a belief in a common body plan for all animals could lead to the idea of common descent. Comparative anatomy, was, however, only one of the four main pillars of Geoffroy's transformism. The others were Serres' embryology, the evidence of

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[122] Quoted in Appel, *The Cuvier–Geoffroy Debate*, p.1
[123] Ibid.
[124] Ibid., p.3
[125] Ibid., p.130

vertebrate paleontology and Geoffroy's own experiments in teratology, the study of congenital abnormalities, or 'monstrosities' as they were known in the early nineteenth century.

Right from his first paper on the subject in 1825, part of whose extremely long title read 'on the question of whether the gavials, today spread throughout the eastern parts of Asia, descend, by way of uninterrupted generation, from the antediluvian gavials', Geoffroy's theories were grounded in paleontological discoveries. The principal fossil evidence used by Geoffroy in support of this theories was the remains of fossil reptiles found at Caen, Le Havre and Honfleur. In his key 1825 paper, published in the *Memoires du Muséum d'Histoire Naturelle*, Geoffroy suggested that these fossil reptiles were the ancestors of the modern crocodiles he had gained first-hand experience of in Egypt. Cuvier had in fact used the apparent identity of the species recovered from the Egyptian mummies with modern species of crocodiles as evidence against transformism. However, like Lamarck, Geoffroy considered that the 'several thousands of years that had flowed by since the earth took its present form do not constitute a sufficiently long lapse of time to have brought about significant and permanent variations in the organisation of living beings.'[126] Nonetheless, he went on to claim that he had detected differences between the modern and the mummified crocodiles that, although slight, provided evidence of an evolutionary relationship between the fossil reptile Teleosaurus and modern crocodiles, since 'the points of variation which I believe I have found there relate to the organic system in which also resides the differences between

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[126] 'plusieurs milliers d'années qui se sont écoulées depuis que notre globe a pris sa forme actuelle, ne comprennent pas un laps de temps suffisamment considérable, pour avoir introduit des variations importantes et permanentes dans l'organisation des êtres.' Étienne Geoffroy Saint-Hilaire, 'Recherches sur l'organisation des gavials ; Sur les affinités naturelles desquelles résulte la nécessité d'une autre distribution générique, *Gavialis*, *Teleosaurus* et *Steneosaurus* ; et sur cette question, si les Gavials (*Gavialis*), aujourd'hui répandus dans les parties orientales de l'Asie, descendent, par voie non interrompue de génération, des Gavials antidiluviens, soit des Gavials fossiles, dits Crocodiles de Caen (*Teleosaurus*), soit des Gavials fossiles du Havre et de Honfleur (*Stenosaurus*), *Memoires du Muséum d'Histoire Naturelle* 12 (1825), p.154

Teleosaurus and the crocodiles'.[127] In other words, the mummified crocodiles represent an intermediate form between modern crocodiles and the fossil Teleosaurus, while being very much closer to the former, due to the relatively short time that had elapsed since the mummified crocodiles had been alive.

In an 1828 report to the Royal Academy of Sciences on a paper co-authored with Serres on modifications observed among domestic animals transported to different environments by François Désiré Roulin (1796–1874), Geoffroy considerably developed his ideas beyond the relationship between Teleosaurus and modern crocodiles. He also left Roulin's relatively modest claims far behind, asserting that Roulin's research 'leads to an understanding of the way in which extinct animals are, through uninterrupted generations and successive modifications, the ancestors of the animals of the present world.'[128] He also went so far as to propose the following, to modern eyes rather bizarre, 'progressive series': 'Icthyosaurus, Plesiosaurus, Pterodactylus, Mososaurus, Teleosaurus, Megalonix, Megatherium, Anoplotherium, Paleotherium, etc.'[129] Teleosaurus, meaning 'completed lizard', was so named by Geoffroy because he considered it a transitional form, showing mammalian characteristics; in the series above it appropriately forms the link between reptiles and mammals.

Fossil bones provided evidence for the transmutation of species, but Geoffroy also developed a detailed theory to explain the mechanism that brought it about. Teratology played an important role in this theory. Geoffroy believed that the same forces that led to the production of monstrosities were those that drove the transmutation of species. As he put it in his 1825 paper on fossil reptiles: 'That

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[127] 'les points de variation que je crois y avoir saisis se rapportent au système organique, sur lequel portent les différences du *Teleosaurus* à l'égard des Crocodiles', Ibid., pp.154–5
[128] elles portent à comprendre de quelle manière les animaux perdus sont, par voie non interrompue de générations et de modifications successives, les ancêtres des animaux du monde actuel.' Ibid., p.208
[129] Geoffroy and Serres, 'Rapport fait à l'Académie royale des Sciences sur une mémoire de M. Roulin', p.215

which, in the great operations of nature, demands a considerable span of time, is nevertheless accessible to our senses, and can be found reproduced in miniature and under our eyes in the spectacle of monstrosities, whether produced accidentally or deliberately.'[130] According to Geoffroy, these changes were brought about by modifications in the environmental conditions experienced by animals in the course of their foetal development. In order to test these ideas, he resorted to what one historian has aptly described as 'experimental transformism'.[131] These experiments were conducted on chicken eggs in a hatchery in the village of Auteuil. Believing that the constitution of the atmosphere had a profound effect on development, Geoffroy attempted to provoke monstrosities by varying the exposure of the foetus to atmospheric gases; for example he would file or prick the shell to facilitate the entry of gases, or coat parts of the shell in wax to reduce it. Although the results were, unsurprisingly, inconclusive, Geoffroy claimed to have had some success in producing monstrosities, reporting that:

> So there I made monsters at will, and even better, it is clear that by varying my procedure, and through the success of various attempts and trial and error, I was able to produce them with one quality or another. Changing the conditions of the external modifiers, while directing on the egg more or less of the elastic fluids that constitute its normal atmosphere, I brought about development in unaccustomed ways; and finally, what was the predicted and looked for object of the experiment, I did not have the chicken that would normally be expected, or at least not all the organs that characterise a chicken in its normal state.[132]

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[130] 'Ce qui, dans les grandes opérations de la nature, exige un temps quelconque considérable, est toutefois accessible à nos sens et se trouve produit en petit et sous nos yeux dans le spectacle des monstruosités, soit accidentelles, soit volontairement provoquées.' Geoffroy, 'Recherches sur l'organisation des gavials', p.152

[131] Jean Rostand, 'Etienne Geoffroy Saint-Hilaire et la tératogenèse expérimentale', *Revue d'Histoire des Sciences et de leurs Applications* 17: 1 (1964), pp. 42

[132] 'Là donc j'ai fait des monstres à volonté; et mieux, c'est qu'éclairé par la variété de mes procédés et le succès de plusieurs essais et tâtonnemens, je les ai fait de telles et telles qualités. Changeant,
les conditions des modificateurs externes, en dirigeant sur l'oeuf plus ou moins des fluides élastiques qui sont son ordinaire atmosphère, j'entraînai les développemens dans une voie inaccoutumée ; et finalement je n'avois point, objet prévu et cherché expérimentalement, je n'avois point le poulet attendu, ou du moins tous les organes qui caractérisent un poulet

Geoffroy believed that the constitution of the atmosphere had changed gradually over geological time. This change played a double role in the history of life; on the one hand it provoked the production of mutations, on the other it gradually made the earth uninhabitable for older forms, which were replaced by new forms arising from the mutations that these changes themselves generated. Of course, not all mutations were favourable, and many 'monsters' produced would not survive. The changing environment therefore imposed a selective pressure which determined the forms that would survive and flourish.

> The imperceptible changes from one century to the next end up accumulating and reaching a certain point at which respiration becomes difficult and finally impossible for certain organ systems: it therefore requires and creates for itself another arrangement, perfecting or altering the pulmonary cells in which it operates; modifications which may be beneficial or harmful. These then propagate and spread through the rest of the animal economy. If those modifications lead to harmful effects, the animals which undergo them will cease to exist, to be replaced by others, with forms modified to suit the new circumstances.[133]

In an article in the *Revue Encyclopédique* of 1833, Geoffroy made clear that 'the decreasing quantity of oxygen relative to the other components of the atmosphere'

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dans l'état régulier.' Geoffroy and Serres, 'Rapport fait à l'Académie royale des Sciences sur une mémoire de M. Roulin, p.227

[133] 'Les modifications insensibles d'un siècle à un autre finissent par s'ajouter et se réunissent en une somme quelconque ; d'où il arrive que la respiration devient d'une exécution difficile et finalement impossible quant à de certaines systèmes d'organes : elle nécessite alors et se crée è elle-même un autre arrangement, perfectionnant ou altérant les cellules pulmonaires, dans lesquelles elle opère ; modifications heureuses ou funestes, qui se propagent et qui influent dans tout le reste de l'économie animal. Car si ces modifications amènent des effets nuisibles, les animaux que les éprouvent cessent d'exister, pour être remplacés par d'autres, avec des formes un peu changées, et changées à la convenance des nouvelles circonstances.' Étienne Geoffroy Saint-Hilaire, 'Quatrième mémoire, lu à l'académie des sciences, le 28 mars 1831', *Recherches sur de Grands Sauriens Trouvés à l'État Fossile vers les Confins Maritimes de la Basse Normandie, Attribués d'abord au Crocodile, puis Déterminés sous les Noms de Téléosaurus et Sténosaurus* (Paris, 1831), p.79

is the most important factor.[134] He explained why the oxygen content of the air had been decreasing over geological time through recourse to a geo-chemical explanation:

> the imagination cannot fail to be frightened by the prodigious volume of shells produced since the first appearance of molluscs and by the thickness that their remains has added to the crust of the globe. Yet, in the final analysis, all the shells are reduced to insignificance by the calcium saturated in the principle of combustion, that is to say chalk, earth, in great part formed of fixed oxygen. [135]

The oxygen lost to the atmosphere was therefore locked up over geological time in rocks in the form of calcium carbonate (rather picturesquely designated as 'calcium saturé du principe comburant' by Geoffroy), the production of which in the oceans of the world was a gradual but continuous process.

Opposing the causes of variation in animals was a power called the *nisus formativus*, a concept, at least as far as its role in reproduction is concerned, which played a similar role to Buffon's internal mould. Geoffroy defined this as 'the principle which presides over the successive order of generations, which compels the return to the same forms, and in consequence the reappearance of the same species, that is to say the tendency to regular development.'[136] So the history of life on earth could be explained by the struggle between these two competing forces; the *nisus formativus*

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[134] Etienne Geoffroy Saint-Hilaire, 'Palaeontographie : Considérations sur des ossemens fossiles la plupart inconnus, trouvés et observés dans les bassins de l'Auvergne', in *Revue Encyclopédique,* vol. 59 (eds H. Carrot and P. Leroux) (Paris, 1833), p.82

[135] 'et l'imagination ne peut que s'effrayer du volume prodigieux des coquilles produites depuis la naissance des mollusques, et de l'épaisseur que les dépouilles de ces animaux ont donnée à la croûte du globe. Or, en dernière analyses, toutes ces coquilles se réduisent, à très peu de chose près, à du calcium saturé du principe comburant, c'est-à-dire à de la chaux, terre, en très grande partie, formée d'oxygène concret.' Étienne Geoffroy Saint-Hilaire, *Études Progressives d'un Naturaliste pendant les Années 1834 et 1835* (Paris, 1835), p.118

[136] 'le principe qui préside à l'ordre successif des générations, au retour obligé des mêmes formes, et par conséquent à la réapparition des mêmes espèces, c'est-à-dire cette tendance à des développemens réguliers'. Geoffroy and Serres, 'Rapport fait à l'Académie royale des Sciences sur une mémoire de M. Roulin', p.214

promoting stability and environmental factors compelling change. Geoffroy's transformism therefore shared with Lamarck the idea of two competing principles. The major difference was that in Lamarck's thought both principles promoted change, although in different directions. For Geoffroy the two principles were antagonistic to one another, one promoted change while the other promoted stability. Lamarck's transformism was driven by an innate tendency towards perfection that was built into the living organism; for Geoffroy, progressive change was entirely the result of external factors acting on living things.

Although they were both transformists, Geoffroy's theory differed in other significant ways from Lamarck's, both in scope and in its details. Unlike Lamarck Geoffroy does not concern himself with the origins of life. Geoffroy shows no apparent interest in the question of spontaneous generation. He only addressed how already existing organisms were modified by their changing environment. This has led Goulven Laurent to suggest that for Geoffroy, 'nature does not start, as for Lamarck, by creating very simple animals, in order to then progressively complicate them, but she commences with a "plan of construction", with an "ideal being"'.[137] Unlike Lamarck, Geoffroy's model proposed a direct role for environmental influences acting on animals in the course of foetal development, while Lamarck saw them as acting indirectly through development of new habits after birth.[138] Their very different approaches to transformism may be in part explained by the significant age difference between the two men. Lamarck had been trained in the philosophical, system-building tradition of Buffon, who had been his patron at the Museum of Natural History in his early days there, while Geoffroy was of the same generation as Cuvier, and therefore schooled in a more rigidly inductive approach.

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[137] 'La Nature en effet, aux yeux de Geoffroy Saint-Hilaire, ne commence pas, comme chez Lamarck, par créer des animaux très simples, pour les compliquer progressivement ensuite; mais elle commence, pour ainsi dire, par la conception du "plan de construction" d'un "être idéal". Laurent Goulven, 'Le cheminement d'Etienne Geoffroy Saint-Hilaire (1772–1844) vers un transformisme scientifique', *Revue d'histoire des sciences* 30: 1 (1977), p.51

[138] Frank Bourdier, 'Lamarck et Geoffroy Saint-Hilaire face au problème de l'évolution biologique', *Revue d'histoire des sciences*, 25: 4 (1972), pp. 325

Although his transformism significantly differed from that of Lamarck in important respects, Geoffroy generally wrote approvingly of his older colleague's work. In his 1825 paper on fossil reptiles he quoted with approval the two laws laid out in the second volume of his *Philosophie Zoologique* (1809).[139] However, Corsi has argued that 'Geoffroy had read precious little of Lamarck, for whom he nevertheless felt considerable human and political sympathy.'[140] He goes on to suggest that his attitude to Lamarck and his theories changed over time, references to his work becoming increasingly hostile after 1831. It is certainly the case that in his *Études Progressives d'un Naturaliste*, published in 1835, he compared Lamarck's theories to the theory of transformism 'without proof and without dignity' contained in the *Telliamed* of Benoît de Maillet (1656–1738).[141] However, although it may be true to say that Geoffroy was at times a little vague on the details of Lamarck's theory, and may have increasingly tried to distance himself to some extent from it after the death of Lamarck, it is undeniable that they had a strong influence on him, as the regular and generally positive references to Lamarck in his work testify.

Appel has rightly remarked that Geoffroy's theories were not as comprehensive as Lamarck's.[142] The more limited scope of Geoffroy's transformism may have made it more acceptable to contemporaries than Lamarck' all-encompassing system building. The early decades of the nineteenth century saw a shift in the nature of scientific discourse away from system-building towards a more inductive model of scientific practice. Lamarck's theory-heavy, but evidence-poor system looked increasingly like a throwback to eighteenth-century models of scientific practice. The new inductive model was intensively promoted in France by the immensely influential Cuvier, who deeply distrusted theory in general and was violently

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139 See Jean-Baptiste Lamarck, *Philosophie Zoologique* (Paris, 1809), vol. 1, p.235
140 Pietro Corsi, 'The revolutions of evolution: Geoffroy and Lamarck, 1825–1840' (2011), [http://hsmt.history.ox.ac.uk/staff/documents/Corsi_Lamarckinthe1830s_Oct2011.pdf Accessed 6 May 2014], p.12
141 Geoffroy, *Études Progressive*, p.104
142 Appel, *The Cuvier–Geoffroy Debate*, p.130

hostile to transformism in particular. Unlike Lamarck's theories, Geoffroy's were based on detailed anatomical research and experimentation, which made his theories harder to dismiss as armchair speculation. Despite this, Geoffroy was determined to defend his philosophical approach to scientific practice against what he saw as Cuvier's advocacy of sterile fact collection. In two of his published works he used the following parable to illustrate the importance of theory over dry fact gathering; once in a memoir read to the Academy of Sciences in 1831, and again in *Études Progressives d'un Naturaliste pendant les Années 1834 et 1835* in 1835.

> Paul has the desire and the means to procure all the pleasures of life: he is intelligent, inventive, and he applies himself to find and gather together that which he supposes ought to be necessary for him. He stocks his cellar with the best wines, he fills his woodshed with all the wood necessary to keep him warm: he acts with the same discernment in regard to all the other probable items for his consumption. They are chosen for their good quality and conveniently arranged and a wise order reigns everywhere. But when he has achieved this, Paul stops. He will not drink this wine, he will not warm himself with this wood, he won't use any of the items he has assembled. – *But*, you will say to me, *your Paul is a madman*. – I agree.[143]

Unfortunately, Geoffroy's philosophical brand of science was to lead him in directions that would be damaging for his reputation. From the mid-1830s Geoffroy's writings became increasingly dominated by the law of *soit pour soit* ('like for like'), a principle of affinity which he considered to be an extension of the Newtonian law of gravitation but applied to the smallest particles of matter. These

[143]' Paul a le désir et les moyens de se procurer toutes les jouissances de la vie : il est intelligent, inventif, et il s'est appliqué à rechercher et à rassembler ce qu'il suppose lui devoir être nécessaire. Il approvisionne son cellier des meilleurs vins ; il remplit son bûcher de tout le bois que réclamera son chauffage : il agit avec le même discernement pour tous les autres objets de sa consommation probable. Les qualités sont bien choisies, les objets habilement rangés, et un ordre savant règne partout. Mais arrivé là, Paul s'arrête. De ce vin, il ne boira pas ; de ce bois il ne se chauffera pas ; de toutes les autres pièces de son mobilier, il n'usera pas. – *Mais*, me direz-vous, *votre Paul est un fou*. – Je l'accorde.' Étienne Geoffroy Saint-Hilaire, *Recherches sur de Grands Sauriens Trouvés à l'État Fossile vers les Confins Maritimes de la Basse Normandie, Attribués d'abord au Crocodile, puis Déterminés sous les Noms de Téléosaurus et Sténosaurus* (Paris, 1831), p.137 and Geoffroy, *Études Progressives d'un Naturaliste*, pp.85–6

ideas did not make many converts, and he found himself increasingly isolated as his writings became more and more abstruse and metaphysical. Although he continued to make strenuous efforts to promote his ideas, Appel has noted that, 'his erratic and unprofessional behaviour increasingly alienated him from his colleagues'.[144] According to the botanist and zoologist Alfred Moquin-Tandon (1804–63), by 1834 some of his colleagues were even 'beginning to feel that Geoffroy was losing his grip on reality'.[145] Nonetheless, he was still honoured for his very real contributions to comparative anatomy, even by those who had no time for his transformist ideas. His very public confrontation with Cuvier only helped to make his ideas more widely known, and he became a hero for many of those in French and European society who held progressive political and philosophical views. His ideas had a strong influence on a number of contemporary literary figures, including George Sand (1804–76), Honoré de Balzac (1799–1850) and Edgar Quinet (1803–75).[146]

Geoffroy's philosophical anatomy was tremendously influential among anatomists and other medical men throughout Europe. A generation of philosophical anatomists, including Richard Owen (1804–92) and William Benjamin Carpenter (1813–85) in England, Robert Knox in Scotland and Henri Milne-Edwards (1800–85) in France, to name just a few, were profoundly influenced by his work. Many of these did not follow him as far as his belief in transformism, but some certainly did. To establish the nature and extent of the influence of Geoffroy's transformist theories in Edinburgh, that great centre of medical education in the early decades of the nineteenth century, along with those of other contemporary and earlier transformist thinkers, will be the central concern in the chapters that follow.

144 Appel, *The Cuvier–Geoffroy Debate*, p.178
145 Ibid., p.180
146 Ibid., p.188

Conclusion

In conclusion, I am going to look at some themes that run through the theories of the origin and transmutation of species which have been explored in this chapter and which we will meet again in the context of Edinburgh natural history in subsequent chapters. It is not surprising that many common themes crop up in the theories I have been examining. Darwin, Lamarck and Geoffroy were certainly all familiar with the work of Linnaeus and Buffon, and Buffon equally certainly knew the work of Linnaeus, even if he was unimpressed with his system of classification. As for Lamarck and Geoffroy, they must have known each other well and Geoffroy could at times be quite protective of the reputation of his older colleague, at least in his earlier work. In addition, all of these figures would certainly have been connected to networks of like-minded individuals within which ideas such as the transmutation of species would have been widely known and discussed.

One of the most important common strands running through most of the different transformist theories is unity of plan, which was first met in a transformist context in the work of Buffon. It was also one of the arguments for transformism seized upon by Erasmus Darwin, before becoming a central plank of Geoffroy's theories. The concept of unity of plan was to have enormous influence on the study of physiology and anatomy well beyond transformist circles. It was, for example, central to the transcendental anatomy of Richard Owen (1804–92), although he was certainly no transformist. But its most lasting impact was to be as one of the most important pieces of evidence supporting the concept of common descent.

Another recurring theme is the importance of the fossil record. Buffon made extensive use of this, even if his knowledge seems to have been limited to the existence of extinct species of fossil shells and the bones of large mammals found in the superficial deposits of Europe, Siberia and North America. Darwin also drew on fossil evidence, although probably at second hand, while Lamarck was something of an expert on fossil shells, which played an important role in the genesis of his ideas, although in his major works on transformism he made little use of the fossil record. Geoffroy's theories were more heavily grounded in vertebrate palaeontology, and particularly the remains of the fossil reptiles that had recently been found in France. His comparative studies of these and of the crocodiles he had encountered in his time in Egypt was a main support for his theory.

All but Linnaeus thought about transformism in the context of an all-encompassing system. They were, to one extent or another, system builders of the kind that many influential scientists of the early and mid-nineteenth centuries, such as George Cuvier, liked to pour scorn on. The speculative nature of their theories made them seem to many like fanciful musings when compared with the rigorous inductive approach that was being promoted as the only correct way to do science at the time. In the 1820s and 1830s Geoffroy had to actively defend the role of the philosopher-naturalist, as in the parable of Paul quoted above. In Britain, the inductive ideology was if anything more deeply ingrained than in France by this time, creating a formidable obstacle to the acceptance of the systems of the transformists.

A common feature of the theories of the earth espoused by most of the thinkers discussed here was their directional nature, which played an important role in their theories of transformism. Darwin was a little vague about the driving force behind his idea of progressive change, but for Buffon it was the cooling of the earth that was driving a process that was in fact more degeneration than progress. As the earth cooled the vital forces diminished, and even the size of organisms decreased. For Geoffroy too, the change was a negative one; the gradual fall in the oxygen content of the air. In his case, however, this was the motive force for an increase in the level of organisation of living things, as they struggled to adapt to an increasingly oxygen-poor atmosphere. Lamarck, in contrast, was a uniformitarian. It was not a changing environment that drove increasing perfection but an innate tendency within organic life. Fluxes of subtle fluids in the environment played an important role in stimulating development in the early stages of the history of life, but this was not due to any directional change in the environment itself. For Lamarck environmental change did play a role in disrupting the simple progress of life, but these diversions from the straight path of progress had no overall direction.

Darwin and Lamarck both advocated spontaneous generation as an integral part of their theories. They considered that life had arisen from inorganic matter, and Lamarck certainly believed that it continued to do so. Buffon also concluded in his *Époques de la Nature* that organic molecules could, at least in the early history of the earth, have arisen spontaneously through the action of heat on inorganic matter, although he was rather vague about how these organic molecules could give rise to the first living things in the absence of an internal mould. Although there is no necessary connection between transformism and spontaneous generation, they would continue to be closely associated in the theories of many transformist thinkers.

Lastly, most of the thinkers I have discussed used their transformist theories to construct genealogies of living organisms, and to make sense of the order which seemed to lie behind the diversity of life. Linnaeus speculated that living things could be divided into natural families, derived from a limited number of ancestors by hybridisation. Buffon at times also seems to be thinking in terms of natural families of living things, such as the sparrow family among the birds, which had diverged from each other through the influence of their environments. Lamarck took the important step in both the *Philosophie Zoologique* and the *Histoire Naturelle des Animaux sans Vertèbres* of drawing branching diagrams to indicate the lines of descent of the major subdivisions of the animal kingdom. Geoffroy took the equally important step of trying to determine the positions of fossil forms in the tree of life, even if some of his suggestions seem extremely fanciful to modern eyes. I will be exploring in later chapters which of these ideas were to be picked up in Edinburgh in the early decades of the nineteenth century, but first I must turn to examine the institutional and social contexts in which these ideas would be received.

Chapter 3: The Edinburgh context

Introduction

The University of Edinburgh in the early decades of the nineteenth century was still a centre of excellence in medical education that attracted students from across Europe, the English-speaking world and beyond, although its reputation had slipped somewhat from its late eighteenth-century apogee. That it did not require any religious tests before matriculation of the kind that existed at Oxford and Cambridge added to the diversity of its student body. The natural history course taught first by John Walker (1731–1803) and then from 1804 by Robert Jameson attracted a wide range of students and covered all aspects of natural history, including the latest developments in geology and zoology. Some evidence for the content of their lectures survives in the form of a series of published syllabuses. These, however, only give an outline of what was actually said in lectures. Much more detailed information can be gleaned from the various sets of students' notes which survive. In this chapter I will be giving an overview of the extent and nature of this valuable source of information. Detailed discussion of their contents will have to wait for subsequent chapters, where I will be drawing on the testimony of some of these sets of notes to examine how doctrines relating to transformism were presented to students by Walker and Jameson.

Outside the university itself there existed a number of important extra-mural anatomy schools. The most successful of these school, that of John Barclay, employed two of the most original thinkers in British natural history at the time, Robert Grant and Robert Knox. Both were in close contact with current developments in natural history in Paris, which was at the epicentre of innovative thinking in the natural sciences in this period. Knox in particular influenced generations of medical students with his unconventional opinions. Sadly, little evidence survives for the content of their lectures, but a good picture of their views can be gleaned from their published works. A plethora of scientific and medical clubs and societies provided a place to discuss and debate new ideas in Edinburgh. The minutes and transactions from a number of the most significant of these survive. Finally, the scholarly journals published in Edinburgh made the opinions discussed in natural history circles in the city accessible to a wider audience nationally and internationally. They can also shed light on the opinions and editorial policies of their editors, who were generally significant figures in their respective fields. In this chapter my aim will be to present a broad overview of the Edinburgh context in which new ideas on the origin and transmutation of species were received and developed, as revealed through these published and unpublished sources. This contextual overview will then underpin the detailed discussion of the reception and further development of these ideas in the following two chapters.

The Edinburgh University medical school

In the eighteenth century Edinburgh had become one of the most important medical schools in Europe. This was in part due to the excellence of the teaching of the professor of anatomy from 1720 to 1758, Alexander Monro primus (1697–1767), who had himself studied in Leiden with the great Herman Boerhaave (1668–1738). When Monro primus retired, his place was filled by his son, Alexander Monro secundus (1733–1817), who also proved to be a highly competent professor of anatomy and medicine from 1758 to 1817. The Edinburgh medical school continued to flourish. In addition to the teaching of the first two Monros, Edinburgh benefitted from being free from the Anglican Confession of Faith demanded as a requirement for matriculation by the English universities. As Alexander Bower noted in 1822, at Edinburgh 'no oath, nor subscription to any article of religion, nor Confession of Faith, are required, as is the case at the universities of Oxford and Cambridge. Persons of every profession of religion are freely admitted, whether Catholics or

Protestants, and no questions asked.'[1] This made Edinburgh a popular choice with English students from nonconformist families, as well as those from abroad. Sadly, the grandson of Monro primus, Alexander Monro tertius (1773–1859), did not continue the tradition of excellence in teaching established by his father and grandfather. Darwin describes how Monro 'made his lectures as dull, as he was himself'.[2] It was even reported that Monro read his grandfather's lecture notes verbatim to his class, not even taking the trouble to excise references such as 'When I was a student in Leiden in 1719…'.[3] Although the authenticity of this account is questionable, the fact that this story was in contemporary circulation gives a strong sense of how Alexander was regarded by his students and peers. The poor quality of lecturing in anatomy and surgery meant that those students who took their studies seriously often felt they had to turn to the large number of extra-mural teachers to supplement the teaching within the University.

The number of chairs in the Faculty of Medicine grew over time. Between 1720 and 1770, eight chairs belonging to the Faculty were established: anatomy, institutes of medicine, practice of physic, chemistry, midwifery, botany, materia medica, and natural history.[4] A chair of clinical surgery was established in 1803, a chair of military surgery in 1806 and a chair of medical jurisprudence in 1807. Then in 1831 chairs in pathology and a separate chair of surgery were established.[5] The establishment of a separate chair of surgery had been a bone of contention between the Town Council and the University, and was forced on the University by the Government, against the loud opposition of Monro tertius, who, supported by the

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[1] Alexander Bower, *The Edinburgh Student's Guide: Or an Account of the Classes of the University, Arranged under Four Faculties; with a Detail of What Is Taught in Each* (Edinburgh, 1822), p.6
[2] Charles Darwin, '1876 May 31 – Recollections of the development of my mind and character', in Charles Darwin, *Autobiographies* (London, 2002), p.22
[3] Brown, *Charles Darwin: Voyaging*, p.56
[4] Alexander Grant, *The Story of the University of Edinburgh during its First Three Hundred Years* (London, 1884), vol. 1, p.320. There is some disagreement among authors as to whether natural history belonged in the Faculty of Arts or Faculty of Medicine. Grant suggests that it was included in the Medicine, while according to Bower it was in the Arts Faculty.
[5] Ibid., p.327

University, was determined to retain the chairs of both anatomy and surgery for himself. The Monros did not have a monopoly on nepotism within the Faculty of Medicine; between 1786 and 1807, of the ten appointments made to medical chairs, eight were also sons of Edinburgh professors.[6] Despite the 'comparative incompetence' of Monro tertius, student numbers continued to grow into the mid-1820s; in 1824 140 students took the M.D. degree, and in 1827 the high water mark of 150 graduates was reached, a number that was not to be exceeded until after the Universities Act of 1858.[7]

It had been conflicts of the kind noted above between the Town Council of Edinburgh, which had been responsible for founding the university, and still controlled appointments to the majority of the University chairs, and the University authorities which played a major role in the decision of Robert Peel, the then Home Secretary, to establish the Scottish Universities Commission in July 1826. The Commission published its Report in 1831.[8] The section of the report relating to the University of Edinburgh provides a fascinating snapshot of the university and its professors in the mid-1820s. Morrell has neatly summed up the principal revelations of the report, noting that 'the importance of personal connexions and personal whims, as opposed to qualifications and rules of procedure, produced unexpected and unpredictable behaviour; professors were at least as loyal to their clients as to the University; and the location and extent of various sorts of authority were not sharply defined'.[9] As we will see below, this was at least as true of the chair of natural history as of the rest of the University.

[6] D.B. Horn, *A Short History of the University of Edinburgh 1556–1889* (Edinburgh, 1967), p.108
[7] Grant, *Story of the University of Edinburgh*, p.329
[8] J. B. Morrell, 'Science and Scottish University Reform: Edinburgh in 1826', *The British Journal for the History of Science* 6: 1 (1972), p.40
[9] Ibid., p.42

The chair of natural history at the University of Edinburgh

At the beginning of the nineteenth century the chair of natural history at Edinburgh was occupied by the venerable second professor of the subject, John Walker, who held the chair from 1779 until his death in 1803. Walker had graduated from the University of Edinburgh himself in 1749 and had become a minister in the Church of Scotland. From his parishes first in Glencorse and then from 1764 in Moffat he pursued his interest in natural history, which brought him into contact with two men who were to become important patrons, William Cullen (1710–90), professor of chemistry at Edinburgh, and Henry Home, Lord Kames (1696–1782). It was with their help, and that of other influential acquaintances, that he was able to secure the post of Regius Professor at the University of Edinburgh in 1779. His main rival for the chair when it became vacant on the death of Robert Ramsay (1735–78), Edinburgh's first professor of natural history, had been William Smellie (1740–95). Smellie was a successful Edinburgh publisher and the 'editor and author of works that helped to define the Scottish Enlightenment'.[10] He also owed his success in part to the patronage of Kames, who encouraged him to write his two-volume *Philosophy of Natural History* (1790–91). Smellie also produced a translation of Buffon's *Natural History* (1780) that was still being reprinted well into the nineteenth century.

Walker's chief interest was mineralogy. He had little time for the speculative theories of the earth that formed an important component of natural-historical discourse in the period. As Matthew D. Eddy has noted in a recent study, 'Buffon's cosmology typified two things that most irritated him; firstly, unconfirmed and therefore potentially erroneous information, and secondly, a love of theoretical systems.'[11] He fully accepted a relatively recent date for the creation of the earth, in line with orthodox interpretations of the Biblical chronology, and his approach to

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10 S.W. Brown, 'Smellie, William (1740–1795)', *Oxford Dictionary of National Biography*, Oxford University Press, 2004; online ed., May 2008 [http://www.oxforddnb.com/view/article/25753, accessed 18 Oct 2012], p.2

11 Matthew D. Eddy, *The Language of Mineralogy: John Walker, Chemistry and the Edinburgh Medical School, 1750–1800* (Farnham, 2008), p.165

his subject was largely non-historical. Despite his hostility towards system building, on the few occasions he did deign to speculate on the history of the globe, he seems to have believed that the oldest, primary strata of the earth had precipitated out from an aqueous solution, which, however, he did not equate with the Biblical flood.[12] He shared this belief with Abraham Gottlob Werner (1749–1817), with whom his successor in the chair of natural history, Robert Jameson, was to study in Freiberg. He may well have come to this opinion independently, however, as Eddy has found no reference to Werner in any of Walker's lectures or personal notes.[13]

Walker appears to have adopted a style of teaching that he bequeathed to his successor Jameson, along with the outlines of his syllabus. As Jameson was also to do, he organised field trips for his students and held tutorials in the Natural History Museum of the university, of which he was the keeper. He also encouraged his students to found societies, including the Natural History Society in 1782 and the Chemical Society in 1785. The interest he showed in stimulating the enthusiasm of his students through these additional activities is likely to have contributed to the popularity of his class with students from a wide variety of backgrounds. The largest single group attending his lectures were medical students, making up around half of the total number. The rest were students from the other faculties of law, divinity and arts, aristocrats with an interest in natural history or individuals with a professional interest in the subject matter of his course, such as apothecaries or jewellers.[14] The matriculation policy of the university, which allowed students to pay for their studies on a class-by-class basis and to attend lectures that were not required for their degrees may help to explain the diversity of the audiences his lectures attracted.

Robert Jameson, who had been a student of Walker's, and helped him with his lectures when his eyesight began to fail, succeeded him as regius professor of

[12] Ibid., p.32
[13] Ibid., p.130
[14] Ibid., p.44

natural history in 1804. He was to hold the chair for half a century, until his own death in 1854. Like Walker, he was also the keeper of the University's natural history museum, which he built into one of the best and most extensive natural history collections in the country in the course of his career. However, his importance goes far beyond his chair at the University. As the founder and president of the prestigious Wernerian Natural History Society and editor of one of the most important scientific journals in Europe he was a pivotal figure in Edinburgh natural history. As well as being the author of several important geology texts, including *Elements of Geognosy* (1809), he also edited and provided extensive notes for the extremely influential English translation of Cuvier's *Theory of the Earth*.[15] His connections in the natural history world and wide network of former students put him at the centre of an extensive web of correspondents across which information and specimens were exchanged around the globe. It has been claimed that Jameson was sympathetic to transformism, if not an open advocate of it.[16] To what extent this was true will be one of the main themes of the chapters which follow.

[15] Georges Cuvier, *Essay on the Theory of the* Earth (trans. Robert Kerr) (Edinburgh, 1815)
[16] Secord, 'Edinburgh Lamarckians', pp.1–18.

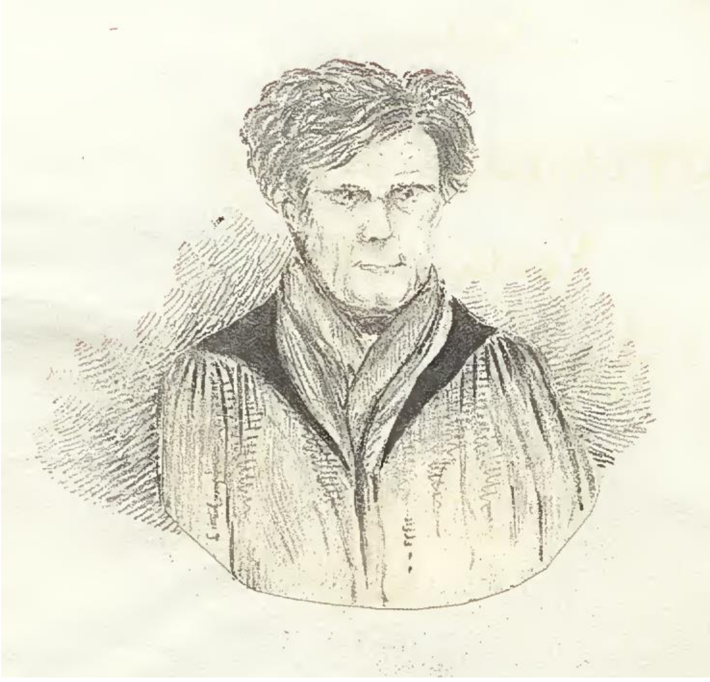

Figure 3 Robert Jameson through the eyes of one of his students. A caricature of Jameson from an anonymous set of student notes dated 1831–2.[17] (Reproduced courtesy of the National Library of Scotland.)

Jameson was the perpetual President of the Wernerian Natural History Society, which he founded in 1808. This society provided a forum for natural historians from Scotland, Great Britain and beyond to exchange findings and opinions. He was also 'Senior Honorary Member' of the Plinian Natural History Society (1823–41) for undergraduate students at the University. Along with David Brewster (1781–1868), he co-edited the *Edinburgh Philosophical Journal* until 1824 and became the sole editor of its successor the *Edinburgh New Philosophical Journal* in 1826. As a result of the diverse roles he played Jameson was the centre of a network of friendship and patronage that included almost every significant figure in natural history in Scotland in the first half of the nineteenth century. He acted, for example, as a patron to his younger associates Robert Grant and Robert Knox, whose careers as lecturers in the Edinburgh extra-mural anatomy schools he played a large part in fostering. He also provided these two key transformist figures in Edinburgh in the 1820s with a platform for their ideas through his editorship of the *Edinburgh New*

[17] Robert Jameson, Notes on natural history lectures (1831–32) (anonymous student) (National Library of Scotland Ms.3936), f.3 verso

Philosophical Journal and the *Memoirs of the Wernerian Natural History Society*. In a later chapter I will attempt to determine whether and to what extent Jameson himself might have shared their transformist opinions. With around 200 students in his natural history class at its peak, he was in a position to influence a very large number of individuals, many of whom went on to become significant figures in the scientific world. For example, two students who were later to go on to develop noteworthy ideas relating to the transmutation of species were the botanist Hewitt Cottrell Watson (1804–81), who was medical student at Edinburgh from 1828 to 1832, and the ethnologist James Cowles Prichard (1786–1848), who was a student at Edinburgh between 1805 and 1808 and whose election to the Wernerian Society was supported by Jameson.[18]

Like Walker, Jameson was not from an especially privileged background, being the son of a soap maker from Leith. He had been an unenthusiastic student at grammar school, and had had to be dissuaded by friends from going to sea rather than continuing his studies after he left school.[19] Instead, he became the assistant to the surgeon John Cheyne in Leith. According to the *Biographical Memoir* of Jameson written by his nephew Laurence Jameson, at this time Jameson became acquainted with Charles Anderson, who had produced a translation of Abraham Gottlob Werner's *Theory of the Formation of Veins*.[20] He also attended courses of lectures by Walker in 1792 and 1793. Jameson often accompanied Walker on dredging expeditions in the Firth of Forth. In 1793 he visited London, where he spent much time in the museums and met a number of leading naturalists, including Joseph Banks and other members of the Linnean Society.[21] This inspired him to give up his medical training to devote himself instead to natural history.

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[18] H.F. Augstein, *James Cowles Prichard: Remaking the Science of Man in Early Nineteenth Century Britain* (Atlanta, GA, 1999), p.11
[19] Laurence Jameson, *Biographical Memoir of the Late Professor Jameson* (Edinburgh, 1854), p.6
[20] Ibid., p.6
[21] Ibid., p.8

In 1793 Jameson also made a trip to Ireland where his interest in Werner's theories and rejection of the rival model of the history of the earth proposed by Jameson Hutton were encouraged by the Irish geologist Richard Kirwan, who pointed out to him 'several strong fails [sic] against the Huttonian theory'.[22] By 1796 Jameson had fully embraced the Neptunian theory of the earth of Werner, which had much in common with the geological opinions that he would have heard from his friend and patron Walker. Evidence for this is to be found in two papers he read to the Royal Medical Society in that year, in which he expressed an uncompromisingly Wernerian view of the history of the earth.[23] He was to become the principal champion in Scotland of Werner's theories in the conflict that took place in the early decades of the nineteenth century between the disciples of Werner and the followers of Hutton. While Werner's theory interpreted the geological record as showing a clear pattern of progressive change over time, Hutton's theory was radically ahistorical, centred on a uniformitarian model of the history of the earth, with, as he famously put it, 'no vestige of a beginning, – no prospect of an end.'[24] In 1800 Jameson travelled to Freiberg to study mineralogy and geology with Werner himself. On his return to Scotland Jameson assisted Walker, who was now old and in poor health, with his classes. While still a student he was given charge of the University's Natural History Museum by Walker.[25] When the old professor died in December 1803, Jameson succeeded him in the chair of natural history at the University. As well as the leading Wernerian geologist in Britain, Jameson was also the most important interpreter of the ideas of Cuvier for a British audience. He contributed prefaces and notes to successive editions of Robert Kerr's translation of Cuvier's *Theory of the Earth* (1813). After Kerr's death in 1813 subsequent editions

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[22] Robert Jameson, quoted in Jessie M. Sweet, 'Robert Jameson's Irish journal, 1797', *Annals of Science* 23: 2 (1967), p.110

[23] Jessie M. Sweet and Charles D. Waterston, 'Robert Jameson's approach to the Wernerian theory of the Earth, 1796', *Annals of Science* 23: 2 (1967), pp.81–95

[24] James Hutton, 'Theory of the Earth; or an investigation of the laws observable in the composition, dissolution and restoration of land upon the globe', *Transactions of the Royal Society of Edinburgh* 1 (1788) p.304

[25] V.A. Eyles, 'Robert Jameson and the Royal Scottish Museum' *Discovery* 15: 4 (April 1954), p.157

were entirely Jameson's work and his version of Cuvier became the most influential popular geology in Britain between 1813 and 1830.[26]

Jameson's reputation has in the past been blighted by a remark made by Charles Darwin in a letter to J.D. Hooker in 1854, where he described Jameson as 'that old, brown dry stick Jameson'.[27] Hardly more flattering were Darwin's remarks in his 'Recollections' in 1876, where he described Jameson's lectures as 'incredibly dull' and noted that 'the sole effect they produced on me was the determination never as long as I lived to read a book on Geology or in any way to study the science.'[28] Not everyone, however, agreed with Darwin. Indeed, it was the effusive praise of Jameson from Edward Forbes, Jameson's successor as professor of natural history at Edinburgh, that prompted Darwin's remark in his letter to Hooker quoted above. In his inaugural address, published in *The Scotsman*, Forbes said:

> Who, that in time past was his pupil and found pleasure in the study of any department of Natural History, can ever forget his enthusiastic zeal, his wonderful acquaintance with scientific literature, his affection for all his friends and pupils who manifested a sincere interest in his favourite studies. When, in after life, their fates scattered them far and wide over the world, some settling amid the civilised security of rural seclusion; some rambling to the far ends of the earth to sift and explore wild and savage regions; some plunging into the boiling and noisy whirlpool of metropolitan activity, none who remained constant to the beautiful studies of his pupilhood was ever forgotten by the kind and wise philosopher, whose quick and cheering perception of early merit had perpetuated tastes that might have speedily perished if unobserved and unencouraged.[29]

Although, it is, of course, unlikely that Forbes would have used his inaugural address to damn the memory of his former teacher and predecessor, there is no

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[26] Dennis R. Dean, 'Jameson, Robert (1774–1854)', *Oxford Dictionary of National Biography*, Oxford University Press, 2004 [http://www.oxforddnb.com/view/article/14633, accessed 5 July 2012]

[27] Charles Darwin to JD Hooker, 29 [May 1854], Darwin Correspondence Database, http://www.darwinproject.ac.uk/entry-1575 [accessed on 19 March 2013]

[28] Darwin, *Autobiographies*, pp.25–6

[29] Edward Forbes, 'Professor Forbes inaugural lecture', *The Scotsman*, 17 May 1854, p.4

reason to believe that the sentiments expressed were not sincere. It would certainly have been difficult for Forbes to have provided a more glowing eulogy for Jameson than he did. Likewise, in the 'Biographical sketch of Robert Edmond Grant' that appeared in *The Lancet* in 1850, probably by Thomas Wakley, there is a reference to 'the highly attractive and invaluable lectures on Natural History of Professor Jameson'.[30] Perhaps more likely to be impartial than friends and former students, the Scottish Universities Commissioners who interviewed Jameson in 1826 testified not only to the personal qualities of Jameson, but to the popularity of his course, reporting that: 'The average number of Students is stated at 200, being a great increase under the present very able and enlightened Professor – his first course of Lectures in the College having been attended only by 35.'[31] Although not everyone found Jameson an inspiring lecturer, despite his evident enthusiasm for his subject, he generally seems to have been a popular one. Robert Christison later wrote of his time as a student of Jameson in 1816 that his 'lectures were numerously attended in spite of a dry manner, and although attendance in Natural History was not enforced for any University honour or for any profession, the popularity of his subject, his earnestness as a lecturer, his enthusiasm as an investigator, and the great museum he had collected for illustrating his teaching, were together the causes of his success'.[32] As Jameson, like the other Edinburgh professors, received most of his income from the fees from students who attended his course he had every reason to make his course as attractive as possible to students, and added inducements such as free access to the museum and field trips must have helped him to do this.

Not only does the report of the 1826 Scottish University Commission provide evidence for Jameson's character, it also provides a valuable insight into his role as professor of natural history in the mid-1820s. His class met for 'one hour each day,

[30] [Thomas Wakley], 'Biographical Sketch of Robert Edmond Grant, M.D. F.R.S. L. & E. &c. Professor of Comparative Anatomy and Zoology in University College, London', *The Lancet* 56 (1850), p.689

[31] Scottish Universities Commission (1826), *Report Relative to the University of Edinburgh* (Edinburgh, 1830), p.46

[32] Quoted in Eyles, 'Robert Jameson and the Royal Scottish Museum', p.159

on five days of the week, for five months in the course of the Winter, and for one hour a-day during three months in the Summer Session.'[33] According to the report, the 'mode of teaching in the Natural History Class is by Lectures and Demonstrations of the objects of Natural History; and with the view of impressing the details upon the minds of the pupils, the Professor makes it a practice to converse with them an hour before the Lecture, and very frequently after the Lecture.'[34] In addition to the lectures, Jameson also met with his students in the Museum three or occasionally six times a week for more informal discussions of subjects of natural historical interest. He also took his students on regular field excursions. While the commissioners had nothing but praise for Jameson's teaching, his management of the museum did come in for some criticism. His decisions to grant or deny access to the collections could be arbitrary and based on personal prejudices. The Commission had received several complaints about Jameson's behaviour, which they considered to be well founded. In particular, they felt that 'very serious evils have resulted from the extraordinary difficulties experienced by men of Science on obtaining access to the Museum, and the general prohibition enforced against the fair use of it.'[35] That Jameson could be a difficult character is without doubt. In his *Memoir* of the life of John Fleming, John Duns quotes Fleming, probably sometime in the mid-1820s, complaining of his treatment by Jameson and comparing him unfavourably with David Brewster, with whom Jameson had shared the editorship of the *Edinburgh Philosophical Journal* until 1826:

> I have found Dr B.'s [Brewsters's] friendship uniform, and kind, and intimate – 'the council's' [Jameson's] irregular, cold, and distant. As men of science there can be no competition. 'The council' is bolstered by his professorial chair and the museum. Dr B. stands on a broad foundation of discovery and generalisation. Dr B. has mentioned my name on suitable

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[33] Scottish Universities Commission, *University of Edinburgh*, p.46
[34] Ibid., p.47
[35] Scottish Universities Commission (1826), *Minutes of evidence taken before the Commissioners for visiting the universities and colleges in Scotland: University of Edinburgh 1826, 1827, 1830* (Edinburgh?, 1830?), p.100

> occasions with respect; the Prof. has erased mine from his editions because it was coupled with Thomson's Annals of Phil.[36]

In the early 1830s there emerged a challenge to Jameson's authority from an unexpected source. The attack came from a medical student at the University, Henry H. Cheek (1807–33), who was also one of the two editors of the *Edinburgh Journal of Natural and Geographical Science* (1829–31), a publication that will be discussed in more detail in a later section of this chapter. Cheek used his journal to launch a campaign against the Wernerian Natural History Society and against Jameson, its president and founder. The Wernerian Society counted among its members most of the important figures in natural history in Scotland in the first half of the nineteenth century. However, according to Cheek, who was not himself a member, all was not well at the Society. In May 1830 the editors of the *Edinburgh Journal of Natural and Geographical Science* congratulated themselves on ' having instigated the present investigation of the independent members of the Wernerian Society into the singular condition of their mis-directed institution.'[37] A further tirade against the direction of the Society followed in the July number. Among a catalogue of complaints that Cheek directed at the Society, the main issue around which the dispute crystallised was the lack of access of members to the Society's library, which, he claimed, appeared to be reserved for the sole use of Jameson. There followed a very frank exchange of views between Cheek and Patrick Neill, the secretary of the Wernerian Society, who came to the defence of Jameson. This controversy was conducted largely through a series of pamphlets. The tone of the exchange rapidly became very personal. In response to Neill's first reply to his journal article in an 'Address to the Members of the Wernerian Natural History Society', Cheek attacked Jameson personally in his position as professor of natural history and keeper of the Natural History Museum:

[36] Quoted in John Duns, *Memoir*, in John Fleming, *The Lithology of Edinburgh* (Edinburgh, 1859), p.xl
[37] [Henry H. Cheek], 'On the present state of science abroad: No.1 Scientific coteries of Paris', *Edinburgh Journal of Natural and Geographical Science* 2 (May 1830), p.118

> I can declare that, during the four years of my residence in Edinburgh, I have been grieved to see the Museum of the University closed to the student who did not purchase certain nominal privileges at an exorbitant price, and, what was more disgraceful, the total uselessness of that establishment to the man of science; – I have felt indignant at the perusal of the syllabus of lectures which the Professor of Natural History puts into the hands of his pupils, and which is only calculated to delude; and I have beheld with disgust a coterie brooding like a night-mare over the Wernerian Natural History Society, till there was little remaining of it but the mockery cast by its name, upon opinions which are now only to be found in the pages of the history of error.[38]

In the 'miscellaneous information' section of the December 1830 number of the journal there then appeared a paragraph by Cheek noting that the Royal Commission for Visiting Scottish Universities had just given its report; the editors announced that they 'look anxiously for the judgment which may be passed upon those flagrant malpractice which we have already exposed', presumably a reference to Jameson's conduct over the Museum, which was indeed roundly criticised in the report of the Commission.[39] In his second pamphlet, Neil accused Cheek of 'doing all in his power (fortunately little) to hold up to contempt and infamy either its President or Secretary, or both, by the grossest imputations'.[40] He also accused Robert Knox, who had supported Cheek, of gross ingratitude to Jameson, who had helped him in his early career.

The passage quoted above suggests that, from Cheek's perspective at least, there were three main complaints regarding Jameson's conduct as professor of natural history. Firstly, was the issue of Jameson's management of the Museum, which he seems to have considered his own private domain, to which he could grant or deny access in an entirely arbitrary fashion. The justice of this compliant would seem to be confirmed by the report of the Scottish Universities Commission. The second

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[38] Henry H. Cheek, *An Answer to Certain Statements Contained in Mr Neill's 'Address to the Members of the Wernerian Natural History Society. 25 September 1830* (Edinburgh, 1830), pp3–4
[39] [Henry Cheek], 'Miscellaneous intelligence: Edinburgh University', *Magazine of Natural History and Journal of Zoology, Botany, Mineralogy, Geology and Meteorology* 3 (1831), p.77
[40] Ibid., p.4

concerns Jameson's teaching, which it is implied did not live up to the promise of the published syllabus. Unfortunately Cheek does not provide any more detailed evidence to back up his assertions, and they are not confirmed by other sources. Thirdly, Cheek claims that Jameson and his cronies mismanaged the affairs of the Wernerian Society, and, in particular, that he treated the valuable library of the Society as his personal property. He also claimed that Jameson poached papers read at the Society's meetings for his *Edinburgh New Philosophical Journal*, thus depriving *Memoirs* of the Society of these papers. It is certainly true that the *Memoirs* were only published erratically and at increasingly long intervals, only running to eight volumes between 1808 and 1838.

Taken as a whole, the dispute gives us an invaluable insight into the ambiguous figure cut by Jameson in the first years of the 1830s. Viewed in conjunction with the other evidence available, what emerges from the various, and often apparently contradictory, accounts of Jameson's character and professional conduct is the picture of a hard-working and dedicated professor who devoted his life to his subject and his students, but who could at times be cantankerous, jealous of his own prerogatives and inclined to let personal animosities affect his judgement.

The natural history syllabus at Edinburgh

Eight sets of students' lecture notes from the period 1782 to 1797 from Walker's natural history course survive in the library of the University of Edinburgh, as do many of his notebooks. He also published a detailed syllabus for his course, entitled *Institutes of Natural History*.[41] These provide valuable evidence for the content of the natural history syllabus in the later decades of the eighteenth century. Walker's lectures were organised under six main headings: meteorology, hydrography,

[41]John Walker, *Institutes of natural history : containing the heads of the lectures in natural history, delivered by Dr Walker, in the University of Edinburgh* (Edinburgh, 1792)

geology, mineralogy, botany and zoology.[42] When Jameson became professor of natural history, he adopted this basic structure more or less unchanged. However, if we make a comparison of the syllabuses in detail, it quickly becomes evident that Jameson's 1826 syllabus is much more detailed that Walker's in 1797. If we compare, for example, the material presented on insects, it seems that Jameson covered the subject in greater detail. The topics covered by Walker were as follows:

> Insects,
> 1. History,
> 2. Class,
> 3. Numbers,
> 4. Sex,
> 5. Generation,
> 6. Metamorphosis,
> 7. Food,
> 8. Hybernation,
> 9. Respiration,
> 10. Senses,
> 11. External Characters,
> 12. Orders,
> 13. Habitation,
> 14. Uses,
> 15. Noxious Effects,
> 16. Vulgar Errors.[43]

In his 1826 syllabus Jameson covered insects under 23 separate headings rather than Walker's 16. His approach seems to have been more methodical, and he systematically went through all the different organ systems of the insect body, as well as covering behaviour, distribution and economic and medical importance before moving on to insect taxonomy and the fossil record of the order:

> *II. Insecta.* – 1. Cutaneous System – 2. Muscular System – 3. Circulating System – 4. Respiratory System – 5. Digestive System – 6. Generative System – 7. Organs of the Senses – 8. Nervous System – 9. Organs of Locomotion –

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[42] John Walker, Notes on natural history lectures (1797) (taken by David Pollock), Edinburgh University Library, Gen.703, vol. 1, f.1
[43] Ibid., ff.5–6

10. Number – 11. Longevity – 12. States of Insects – 13. Societies – 14. Noises – 15. Luminosity – 16. Hybernation – 17. Instincts – 18. Geographical and Physical Distribution – 19. Uses in the Economy of Nature – 20. Domestication – 21. Dietetical Uses – 22. Diseases – 23. Diseases in Man and other Animals, also in Plants, occasioned by Insects.

I. Account of the Characters used in the description, arrangement, and determination of Species.
II. Account of the Orders, Genera, and Species, as far as is necessary for the Student.
III. Description and History of Fossil Insects.[44]

Printed syllabuses survive for Jameson's natural history course from at least four sessions. One of these, for 1826, is preserved in the report of the Scottish Universities Commission.[45] Corrected proofs for two further printed syllabuses for the natural history course at Edinburgh survive among Jameson's papers in Edinburgh University Library.[46] These are undated. However, amendments made by hand to one appear in print in the other, so the first must predate the second. Both must be later than the 1826 syllabus, as the 1826 version does not include any of the corrections marked on the two sets of proofs from Jameson's papers. The two sets of proofs probably both date from the early to mid-1830s, as they were found wrapped in a page from the *Edinburgh Evening Courant* dated December 1830. A fourth syllabus was published in Laurence Jameson's 1854 *Biographical Memoir* of his uncle.[47] Although this is undated, it is likely to be from the early 1850s.

All the syllabuses are divided into the same six sections as Walker's had been: meteorology, hydrology, mineralogy, geology, botany and zoology. For assessing Jameson's opinions on transformism, the last three of these are the most interesting.

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[44] Robert Jameson, 'Syllabus of lectures on natural history' (1826), in Scottish Universities Commission, 'Appendix to No. XIII', *Returns, Papers, and Examinations Printed by Order of the Royal Commissioners for Visiting the Universities and Colleges of Scotland* (Edinburgh, 1830?), p.13
[45] Scottish Universities Commission (1826), 'Appendix to No. XIII, University of Edinburgh, 26th September 1826', *Returns, Papers, and Examinations,* pp.9–14
[46] I will refer to these as 'Syllabus of lectures on natural history (183? a)' and 'Syllabus of lectures on natural history (183? b)' (University of Edinburgh Library, Gen.130)
[47] Jameson, *Biographical Memoir*, pp.21–6

Starting with the geology section, the only reference to the history of life is in part 9 of the 1826 syllabus, 'On the Fossil Organic Remains, especially their Geognostical Distribution in the Crust of the Earth, and the connection by their distribution with the state of the Earth during different periods of its formation.'[48] This section appears in all of the first three syllabuses, but is absent entirely from the final one, to be replaced by a reference to 'General Palaeontology'.[49] While this tells us that, at least in the 1820s and 30s, Jameson talked about the history of life on earth and its relationship to changes in the state of the planet, the summary is so vague as to say practically nothing about Jameson's views on the history of life.

Part 13 was entitled 'On the Deluge and the Age of the World' in 1826. In later syllabuses this became 'Deluges, and *The Deluge* explained' and finally 'Deluges, considered', before vanishing altogether in the 1850s. This strongly suggests that Jameson took the Biblical Deluge seriously, a fact confirmed by the evidence of the student notes discussed below. The botany section is very short; this was clearly not an area that particularly interested Jameson. We do find an intriguing reference here to 'Deductions illustrative of Gradual Change in the Heat of the Earth, and of Alteration in Climate, as disclosed by the facts in the Physical and Geographical Distribution of Fossil and Living Plants'.[50] This strongly suggests that Jameson discussed Buffon's theory that the earth had gradually cooled from a molten state over geological time. This part became a discussion of 'changes in the climate of the Earth' in the next syllabus we have and then stays that way into the 1850s, suggesting perhaps a reduced emphasis on a cooling earth.[51]

Jameson then moved on to Zoology, giving an exhaustive account of the classification of animals in the conventional order from man to the Protozoa.

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[48] Jameson, 'Syllabus of lectures on natural history' (1826), in Scottish Universities Commission, 'Appendix to No. XIII', *Returns, Papers, and Examination*, p.11
[49] Jameson, *Biographical Memoir*, p.24
[50] Jameson, 'Syllabus of lectures on natural history' (1826), in Scottish Universities Commission, 'Appendix to No. XIII', *Returns, Papers, and Examination*, p.11
[51] Jameson, 'Syllabus of lectures on natural history (183? a), p.9

Jameson's classification from the 1826 syllabus accords broadly with the influential schemes of classification of Lamarck and Cuvier. As we might expect, Jameson's classification scheme changes gradually in line with developments in the science as we pass from the earliest to the latest versions of the syllabus. Of more interest is the final section of the zoology syllabus, entitled 'Philosophy of Zoology'. In the 1826 version, and the earliest of the two syllabuses found in Jameson's papers, this section starts with the intriguing subheading 'Origin of the Species of Animals', although sadly the evidence gleaned from the surviving student notes does not shed any further light on the content of this suggestively titled section.[52] In these two syllabuses, we then find an intriguing reference to the 'various Revolutions or Changes the Animal World has experienced, from the first creation to the present time'. That Jameson clearly acknowledged that 'revolutions' or 'changes' had taken place in the animal world is, although it is not possible to determine from this evidence the nature and causes of the changes he had in mind. In this period the appearance of new species was first beginning to be perceived as problematical as a result of paleontological discoveries. The presence of this section in Jameson's syllabus shows that he was very much aware of this important question. The entire section on the 'Philosophy of Zoology' appears to have been dropped by Jameson after the second of the surviving syllabuses, probably dating from some time in the mid-1830s.

To flesh out the content of the lectures for which these syllabuses provided the framework, we must turn to the surviving student notes. Here I will only give a brief overview of the extent and nature of the notes that survive. In subsequent chapters I will be drawing on these further as an invaluable source of information on the views on the nature and origins of species and the history of life on earth that Jameson transmitted to his students in the course of his lectures. At least twelve bound sets of hand-written notes from Jameson's natural history lectures survive,

[52] Jameson, 'Syllabus of lectures on natural history' (1826), in Scottish Universities Commission, 'Appendix to No. XIII', *Returns, Papers, and Examination*, p.14 and Jameson, 'Syllabus of lectures on natural history (183? a), p.15

four in Edinburgh University Library, three in the National Library of Scotland, one in Glasgow University Library, one in the Wellcome Library in London and two in Elgin Museum. The notes span a period of 30 years. As we have seen from the syllabuses, Jameson repeatedly modified the content of his lectures in reaction to developments in natural history during this time. A comparison of these lecture notes should therefore provide some evidence for areas of development and continuity in Jameson's ideas over three decades. The earliest set of notes dates from 1806 and the latest from 1835/6. They therefore cover the first three decades of Jameson's career at Edinburgh. Nine of these sets can be confidently dated to 1806, 1816/17, 1822/3, 1828/9 (two sets), 1830, 1830/1, 1831/2 and 1835/6. The remaining two are not dated, but the paper they were written on bears watermarks from 1813/14 and 1827 respectively. This can be taken as setting the earliest possible dates for these notes. From internal evidence the first of these two sets could not have been written before 1826, as it contains a reference to a paper published in that year. The watermarks therefore cannot be taken to give a reliable indication of the actual date of writing, which may be much later. As the authorship of four sets is unknown, I will refer to them here by date of composition, or watermark date, suffixed with a question mark when the date of composition is unknown. The handwriting of most of the sets of notes is clear and easily legible. However, the set of 1827? is almost entirely illegible. The notes with an attribution can be ascribed to the following students: John Borwick (1806), George Gordon (1822/3 and 1828/9), Alexander Turnbull Christie (1828/9), W.S. Walker (1830), Robert M'Cormick (1830/1) and David Blair Ramsay (1835/6).[53] The 1827? set contains an autograph signature on the first folio, but unfortunately it is illegible. The remaining two sets (1816/17 and 1831/2) are anonymous. I will be drawing on the evidence of some of

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[53] George Gordon, and Church of Scotland minister from Moray, was such an enthusiastic amateur geologist that he came to Edinburgh to study geology with Jameson twice in the 1820s. See Michael Collie, 'Hugh Miller's dealings with contemporary scientists', in Lester Borley (ed.), *Celebrating the Life and Times of Hugh Miller: Scotland in the Early 19th Century, Ethnography and Folklore, Geology and Natural History, Church and Society* (Cromarty, 2002), p.229

these sets of notes to explore the reception of theories of life and organisation in Edinburgh in later chapters.

The extra-mural medical schools

As we have seen above, deficiencies of some of the teaching at the University in the early nineteenth century created a lively demand for extra-mural classes.[54] In 1822 Bower listed 23 extra-mural teachers offering their services to Edinburgh's medical students.[55] The need for additional tuition must have been keenly felt, as students were obliged to pay for class tickets for the University professors, and so would end up paying twice over for subjects for which they chose to attend extra-mural classes. I will concentrate my attention here principally on the school run by John Barclay. Not only was his the most successful of the extra-mural anatomy schools in Edinburgh, but both Robert Knox and Robert Grant taught there, and Knox went on to take over the running of the school after Barclay's death.

Barclay taught anatomy in Edinburgh from 1797 to 1825. As a young man he had studied for the ministry at the University of St Andrews before coming to Edinburgh to study medicine, and he remained a deeply religious man all his life. He was a great success as a lecturer; Browne has estimated that Barclay's classes accommodated four or five hundred students a year.[56] Apart from the quality of his lectures, Barclay's popularity also owed something to the close proximity of his lecture hall to the Royal Medical Society and Surgeon's Hall, making it very convenient for students.[57] In order to cope with demand, he had to give two lectures a day, one in the morning and one in the evening. Such was Barclay's reputation as a comparative anatomist, that in 1817 there was a move to create a chair of

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54 M.H. Kaufman, 'John Barclay (1758–1826) extra-mural teacher of anatomy in Edinburgh: honorary fellow of the Royal College of Surgeons of Edinburgh', *Surgeon* 4: 2 (2006), p.95
55 Bower, *The Edinburgh Student's Guide*, pp.142–6
56 Browne, *Charles Darwin*, p.57
57 Lisa Rosner, 'Barclay, John (1758–1826)', *Oxford Dictionary of National Biography*, Oxford University Press, 2004 [http://www.oxforddnb.com/view/article/1345, accessed 5 July 2012]

comparative anatomy at the University for him, which was defeated by the professors of the university, jealous of their prerogatives. [58]

Barclay's M.D. thesis had been entitled *De Anima, seu Principio Vitali*, and the existence of a vital principle was an abiding interest for him throughout his career.[59] He was strongly anti-materialist, and his last published work, *An Enquiry in to the Opinions, Ancient and Modern, Concerning, Life and Organization* (1822), consisted of a sustained attack on a rogues gallery of thinkers whose views Barclay disagreed with, from Epicurus and Aristotle to Buffon and Erasmus Darwin.[60] In the preface to his book, Barclay gave the following motive for writing it: 'As young men entering on the studies of anatomy and chemistry, are naturally let to investigate the causes of organization, and are frequently apt to form hypotheses on grounds of information not the most ample, it was imagined that such an inquiry might be particularly useful to them.'[61] The book was intended to be a guide for students to the different theories on the nature of life that were current at the time, and it therefore provides a valuable insight into both Barclay's own reading and the theories that were likely to have been discussed in medical circles in Edinburgh in the 1820s. I will have more to say on Barclay's critique of Erasmus Darwin in the following chapter. However, the overall purpose of the book was a spirited defence of the existence of a vital principle in all living things, 'a principle which, independent of either configuration or matter, extends its influence over the nutritious particles around it, as fire does over combustible materials, and which assimilates these particles in a regular manner'.[62] In a letter dated 31 December 1821 from Johann Spurzheim to the Edinburgh phrenologist George Combe, Spurzheim

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[58] Philip F. Rehbock, *The Philosophical Naturalists: Themes in Early Nineteenth-Century British Biology* (Madison, WI, 1983), p.34
[59] George Ballingall, 'The life of Dr Barclay', in John Barclay, *Introductory Lectures to a Course of Anatomy, delivered by John Barclay, M.D. F.R.S.E. with a Memoir of the Life of the Author* (Edinburgh, 1827), p.v
[60] John Barclay, *An Enquiry in to the Opinions, Ancient and Modern, Concerning, Life and Organization* (Edinburgh, 1822)
[61] Barclay, *Life and Organization*, p.x
[62] Barclay, *Life and Organization*, p.236–7

noted that Barclay's hostility to phrenology was a result of his attachment to the idea of the vital principle, concluding that 'Dr Barclay has not sufficient reason to believe in Phrenology, since it does not agree with his vivifying principle, which builds the organisation.'[63] When Barclay became ill shortly before his death in 1826 his classes were taken over by Robert Knox, who subsequently became the proprietor of the school.

Knox was a colourful and controversial figure who graduated M.D. from the University of Edinburgh in 1814. During his studies he attended Barclay's extra-mural anatomy lectures. He inherited radical political views from his father, who had been a member of the Jacobin-inspired Friends of the People. After his studies he spent some years as an army doctor. He served at the Cape of Good Hope from between 1817 and 1820, an experience which helped him develop his unorthodox views on race and his strongly anti-colonial attitude. In 1821/2 he spent a year studying in Paris, where he attended courses by Cuvier, De Blainville and Larrey, and most importantly for the development of his opinions on comparative anatomy, Geoffroy St-Hilaire.[64] During his time in Paris he became a steadfast convert to Geoffroy's brand of philosophical anatomy. Back in Edinburgh, he was an active member of the Wernerian Society and published widely on various topics in comparative anatomy. Knox's biographer and former student, Henry Lonsdale, notes that during this period 'His studies at the Museum of Natural History made him known to Professor Jameson, who was glad to receive the aid of a promising naturalist for his *Quarterly Philosophical Journal* [sic]'.[65] Jameson regularly entrusted Knox with rare specimens of exotic animals for dissection, which formed the basis of

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[63] Johann Spurzheim to George Combe, 31 December 1831, National Library of Scotland, Ms7207, f.63

[64] Clare L. Taylor, 'Knox, Robert (1791–1862)', *Oxford Dictionary of National Biography*, Oxford University Press, 2004 [http://www.oxforddnb.com/view/article/15787, accessed 5 July 2012], p.1

[65] Henry Lonsdale, *A Sketch of the Life and Writings of Robert Knox the Anatomist* (London, 1870), p.36. Presumably Lonsdale is referring here to the *Edinburgh New Philosophical Journal*, edited by Jameson.

a number of papers given by Knox to the Wernerian Society. According to the *Memoirs* of the Society for 1824, Knox had reason to thank Jameson for access to specimens of an assortment of reptiles, an *Ornithorynchus* (duck-billed platypus), a chameleon and a collection of human skulls.[66] Knox also had the opportunity to renew his acquaintance with Barclay, and on 2 March 1825 Knox signed an agreement of partnership in Barclay's extra-mural anatomy school.[67] The high quality, colourful style and often controversial content of Knox's classes made him a very popular lecturer with medical students. In only three years Knox increased the attendance of 300 students at Barclay's school to more than 500. This figure suggests that around two-thirds of Edinburgh medical students attended Knox's course, and makes it the largest anatomy class in British history.[68] In 1825 he also became a fellow of the Royal College of Surgeons of Edinburgh, where he was appointed the conservator of their museum the same year. Barclay died in 1826, leaving Knox as sole proprietor of his anatomy school. Unfortunately for Knox, his involvement in the scandal surrounding Burke and Hare's infamous series of murders, committed to meet the need of the anatomy schools for fresh cadavers, cast a blight over Knox's once flourishing career. Knox had bought twelve of the bodies from the murderers, and his name became indelibly associated with their crimes in the public mind. His career subsequently went into a long decline that led him to move to London in the early 1840s, where he supported himself and his family largely by writing and giving public lecture tours until his death in 1862.

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[66] Robert Knox, 'An account of the *Foramen centrale* of the retina generally called the *Foramen of Soemmering*, as seen in the eyes of certain reptiles', *Memoirs of the Wernerian Society* 5: 1 (1824), pp.1–7; Robert Knox, Observations on the duck-billed animal of New South Wales, the *Ornithorynchus paradoxus* of naturalists: Memoir I. On the organs of sense, and on the anatomy of the poison gland and spur, 5: 1 (1824), pp. 26-41; Robert Knox, 'Additional observations relative to the *foramen centrale* of the retina in reptiles', *Memoirs of the Wernerian Society* 5: 1 (1824), pp.104-6; and Robert Knox,' Inquiry into the origin and characteristic differences of the native races inhabiting the extra-tropical part of southern Africa', *Memoirs of the Wernerian Society* 5: 1 (1824), pp.206–19

[67] Lonsdale, *Life and Writings of Robert Knox*, p.44

[68] Rehbock, *The Philosophical Naturalists*, p.44

We know surprisingly little of Knox's views in the 1820s. His publications during this period were generally rigorously factual, and give very little indication of his theoretical stance. As Evelleen Richards attests, Knox himself destroyed the bulk of his papers before his death, and the material utilised by his biographer Henry Lonsdale and Knox's correspondence with bodies such as the Edinburgh Royal College of Surgeons have been lost, depriving the historian of potentially valuable sources of information on his early career.[69] His adherence to Geoffroy's philosophical anatomy and rejection of Cuvier's functionalist approach is hinted at in a few of the publications from his Edinburgh years, for example in a paper he gave in 1826 on the vestigial spur found in the foot of the female Echidna he noted that 'The physiological anatomist can have no difficulty in comprehending that this organ must bear to the male spur the same relation that the human male breast does to the female.'[70] Otherwise his opinions on comparative anatomy and transformism in the 1820s have to be reconstructed from biographical sources and writings from decades later, when Knox was living in London. The potential dangers of such an approach can be illustrated by an example taken from a comparison of his stated views on the effect of climate on the human species in 1824 with the opinion he gave on the same subject in 1850. In 1824, he had this to say on the subject of the origins of the different races:

> We may view the human race as derived originally from one stock, to which the arbitrary name of Caucasian has been given. This species, influenced by climate and civilization, assumed, at a very early period, five distinct forms, which have also been arbitrarily designated by the names Caucasian, Mongolic, Ethiopian, American, and Malay.[71]

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[69] Evelleen Richards, 'The "Moral Anatomy" of Robert Knox: The Interplay between Biological and Social Thought in Victorian Scientific Naturalism', *Journal of the History of Biology* 22: 3 (1989), p.377

[70] 'Notice respecting the Presence of a Rudimentary Spur in the Female Echidna of New Holland', *Edinburgh New Philosophical Journal* 1 (1826), pp. 130–2

[71] Robert Knox, 'Inquiry into the origin and characteristic differences of the native races inhabiting the extra-tropical part of southern Africa', *Memoirs of the Wernerian Society* 5: 1 (1824), p.210

By 1850, however, he considered that never ‘has climate or external circumstances effected any serious changes, produced any new species, any new groups of animal or vegetable life, any new varieties of mankind.’[72] It seems that between 1824 and 1850 Knox had completely reversed his opinion on the role of the environment in generating races. It is quite possible that other opinions, undocumented in his writings of the 1820s, had undergone similar alterations in the intervening decades. It is with this proviso that I will attempt in subsequent chapters to give an account of Knox’s views on transmutation, based principally on the evidence of his later writings. Where these opinions appear to be derived from ideas found in Geoffroy’s writings of the 1820s, we might be on slightly safer ground, and it is certainly the case that one area of evident continuity in Knox’s thought was his continuing adherence to the doctrines of transcendental anatomy and unity of plan he had learned from Geoffroy in Paris. Nevertheless, the danger of assuming that Knox’s views had remained unchanged during a period of twenty-five years still cannot be overstated.

Another important figure who taught at Barclay’s school was Robert Edmond Grant, who completed his medical studies at the University of Edinburgh in 1814. An inheritance from his father, who died in 1808, had left him at liberty to spend the next twenty years studying and travelling extensively in western Europe. In 1815 Grant was in Paris, studying under Henri de Blainville, from where his studies took him to Italy and Germany. In 1820 he was back in Edinburgh, where he practiced medicine for a while. He also attended the natural history classes of Robert Jameson in 1823, whose lectures he had first experienced as a medical student in 1810.[73] In the same year as his return to Edinburgh he became a member of the Linnaean Society, helped by a recommendation from Jameson, and was elected a member of the Royal Society of Edinburgh in 1824.[74] He also became a member of the Wernerian Society

[72] Robert Knox, *The Races of Men: A Fragment* (London, 1850), p.90
[73] [Wakley], 'Biographical Sketch of Robert Edmond Grant, p.689
[74] Ibid., p.690

and an honorary member of the Plinian Society.[75] During the 1820s he visited Paris regularly in the summer and he was personally acquainted with many of the major figures of French natural history, including Geoffroy and Cuvier. He was also a friend of the influential natural historian and Church of Scotland minister John Fleming, who named a genus of sponge *Grantia* after him, 'to commemorate his valuable services in elucidating the physiology of sponges'.[76] Grant had attended John Barclay's lecture on comparative anatomy in 1821,[77] and in 1824 he was invited by Barclay to teach the part of his course that dealt with invertebrates, which was Grant's particular area of expertise. In 1827 Grant left Edinburgh to take up the chair of zoology at University College, London, a post for which he was recommended by an impressive list of Scottish scientific luminaries from both inside and outside the University of Edinburgh, including Robert Jameson, David Brewster, John Fleming and Alexander Monro tertius. Sadly, beyond his published work, very few sources exist for Grant. Not only do we know he was in the habit of burning manuscripts of his work,[78] but very few of his letters seem to have survived, although we do know that he kept bound volumes of his correspondence during his lifetime. These volumes should have been bequeathed to University College London on his death, but do not seem to have ever come into the possession of the University.[79] As a result, the outlines of his life and thought have to be reconstructed largely from published sources.

In addition to Barclay, Knox and Grant, there was one other extra-mural anatomy teacher active in the 1820s and 30s for whom we have significant evidence regarding his attitudes towards transformism, and that is John Fletcher (1792–1836). Fletcher

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[75] Ibid., p.691
[76] John Fleming, *A History of British Animals, Exhibiting the Descriptive Characters and Systematical Arrangement of the Genera and Species of Quadrupeds, Birds, Reptiles, Fishes, Mollusca, and Radiata of the United Kingdom* (Edinburgh, 1828), p.524
[77] Robert Grant, Essays on medical subjects (UCL library, MS ADD 28 (box 17)), ff.88–179
[78] [Wakley], Biographical Sketch of Robert Edmond Grant, p.692
[79] Adrian Desmond and Sarah E. Parker, 'The bibliography of Robert Edmond Grant (1793–1874): illustrated with a previously unpublished photograph', *Archives of Natural History* 33: 1 (2006), p. 204

had graduated from Edinburgh in 1816 and taught at the Argyle Square Medical School from 1828 to 1836. He was an admirer of Geoffroy and an active promoter of unity of plan and recapitulation theory. His lectures were published as *Rudiments of Physiology* (1835–7), in what was 'the first book completed on higher anatomy in Britain.'[80] He frequently cited Grant in his book and was clearly also aware of the ideas of Knox. Although he was an advocate of the new philosophical anatomy, it becomes quickly apparent on reading *Rudiments of Physiology* that he was no transformist. He did, however, discuss transformist theories at some length in this work. It would therefore seem that an exposition and critique of these theories formed part of the syllabus that Fletcher taught his students, as the book is largely based on his teaching.

Scientific, medical and natural history societies

In the early decades of the nineteenth century, Edinburgh was home to a wide variety of medical and scientific societies, drawing their membership from diverse professional and social groups. In all there were fifteen such societies in Edinburgh active between 1800 and 1844, although this figure includes some which only came into existence during this period, and others which were dissolved before its end. There were four natural history societies, including the Wernerian Natural History Society (1808–58) and Plinian Natural History Society (1823–41), as well as the more specialised Edinburgh Geological Society (founded 1834) and Botanical Society of Edinburgh (founded 1836). The Royal Society of Edinburgh (founded 1783) was something of a special case among the scientific societies. It was the most socially prestigious of the societies and a large proportion of its members were interested amateurs from among Edinburgh's social elite, along with many of the most influential scientists of the day. The Physico-Chemical Society of Edinburgh (1819–22) was a student society for which no records survive. The Royal Medical Society (founded 1737) and Royal Physical Society (founded 1771, date of dissolution

[80] Desmond, *The Politics of Evolution*, p.71

unknown), although largely dealing with medical matters, also concerned themselves from time to time with natural history topics. Edinburgh's Royal Colleges of Surgeons and Physicians were concerned mainly with the regulation and advancement of their respective professions. Several other medical societies were in existence during this period, including the Hunterian Medical Society, the Anatomical Society of Edinburgh, the Aesculapian Club and Edinburgh Harveian Society. The activities of the last two were more social than scholarly in nature. Some were student societies, such as the Royal Medical Society of Edinburgh and the Plinian Natural History Society, some were for professional groups, such as the Medico-Chirurgical Society of Edinburgh and the Royal Colleges of Surgeons and Physicians, while others were open to enthusiastic amateurs, such as the Edinburgh Geological Society and the Botanical Society of Edinburgh.

A significant number of these societies acted as clearing houses for the latest scientific ideas. Some of them also published volumes of proceedings or transactions to provide a permanent record of their members research and make it known to a wider audience. It can reasonably be assumed that all of them would have kept minute books in which the papers that were presented to the society and the discussions that took place at their meetings were recorded. Many minute books and other papers relating to these societies survive in the library of the University of Edinburgh, the National Library of Scotland or elsewhere. These records provide a valuable resource for the investigation of how new scientific ideas and theories were received and transmitted in Edinburgh in the first half of the nineteenth century. They are also an important resource for any investigation of the prevalence of transformist theories in the Edinburgh of the period. I will only be looking in detail here at those societies where there is positive evidence that transformism or related topics were discussed in their meetings, starting with the Plinian Natural History Society.

While visiting Paris in the late 1820s, a recent graduate of Edinburgh University and member of the Plinian Society by the name of John Coldstream was afflicted by a severe moral and religious crisis. William Mackenzie, a Scottish doctor who knew Coldstream in Paris, explained his crisis in the following manner: 'Though a young man, I believe, of blameless life, still he was more or less in the dark on the vital question of religion, and was troubled with doubts arising from certain Materialist views, which are, alas! too common among Medical students.'[81] While a medical student at Edinburgh, Coldstream had been an active member of the Plinian Society, which he had joined on 18 March 1823. Charles Darwin knew him in Edinburgh and described him as 'prim, formal, highly religious and most kind-hearted'.[82] While he steered away from controversial subjects in his own contributions to the Society, it has been suggested that his Paris crisis had something to do with the materialist ideas current among students that Mackenzie referred to, ideas for which the Plinian Society provided a forum for discussion, and to which Coldstream would have been exposed at its meetings.[83]

The Plinian Natural History Society was founded in 1823 by a group of undergraduates at the University of Edinburgh. It has in the past often been incorrectly stated that it was founded by Robert Jameson, the professor of natural history at the University. This misapprehension can be traced back to Darwin, who suggested that Jameson was the founder in his 'Recollections',[84] and this has subsequently been repeated as fact by a number of scholars, including Desmond and Moore in their biography of Darwin.[85] Although Jameson was an honorary member of the Society, he never actually attended any of its meetings. When asked about the Plinian and his relationship with it by the commissioners of the Royal Commission on the Scottish Universities in 1826, he described it as a forum 'where

[81] John Hutton Balfour, *Biographical Sketch of the Late John Coldstream* (Edinburgh, 1864), p.13
[82] Charles Darwin, 'Recollections of the development of my mind and character', p.23
[83] See, for example, Desmond and Moore, *Darwin*, p.41
[84] Charles Darwin, 'Recollections of the development of my mind and character', p.24
[85] Desmond and Moore, *Darwin*, p.31

young men can discuss subjects which they have heard in the class of Natural History, or in the other classes where such subjects are considered.'[86] While he stated that the society had been founded 'under his countenance', he denied that he had 'any particular controul [sic]' over it. However, he went on to affirm that in his opinion the members 'distinctly confine themselves to the proper subjects of investigation'.[87] As most, if not all, members would have been Jameson's students, his teaching undoubtedly had a significant influence on the debates that took place at the Society's meetings.

According to a biography of John Baird, one of the founders of the society, Baird and his associates, 'feeling the want of a society where younger students of nature could meet and discuss their views freely among themselves, unawed by the older and more mature naturalists of the day, resolved to institute a society for the advancement of the study of natural history, antiquities, and the physical sciences in general'.[88] Foremost among the founders of the society was Baird, along with his brothers Andrew and William. Both William Baird's *Memoir of the Late Rev. John Baird* (1862) and a *Nature* article on 'Local scientific societies' published in 1873 credited John Baird as the founder of the society.[89] According to the society's minutes, it was also Baird who announced the 'proposed plan and objectives' of the society at its first meeting on 14 January 1823.[90] By June 1826 its membership had risen steadily to 106. It also had 25 corresponding members and ten honorary members. The honorary members were Edinburgh University professors Robert Jameson (natural history), Thomas Hope (chemistry), Robert Graham (botany), Andrew Duncan senior (institutions of medicine) and Andrew Duncan junior

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86 Scottish Universities Commission (1826), *Minutes of evidence taken before the Commissioners*, p.92

87 Ibid., p.92

88 William Baird, *Memoir of the Late Rev, John Baird, Minister of Yetholm, Roxburghshire; with an Account of his Labours in Reforming the Gypsy Population of that Parish* (London, 1862), p.63

89 Ibid, p.63; Anon, 'Local scientific societies', *Nature* 9 (1873), p.38

90 Anon, *Abstract of the proceedings of the Plinian Society from its first meeting Jan 14, 1823, to July 25, 1826* (Edinburgh, 1829), p.33

(materia medica), George Husband Baird, the Principal of the University, and from outside the University John Fleming, Sir Walter Scott, Sir Humphrey Davy and Sir James Hall. In 1829, the *Magazine of Natural History* included the following description of the objectives of the Plinian Society:

> The principal intention of the founders of the Plinian society was to promote natural history; but antiquarian researches, and the advancement of all the physical sciences, have also been included among its professed objects. The means which have been adopted for the prosecution of the views of the Society are, the reading of papers, debates, the formation of a museum and library, and excursions to the country, for the examination and collection of objects of natural history. Papers have been read on subjects connected with all the departments of natural science, more especially on the zoology, botany, geology, mineralogy, meteorology, and antiquities of Scotland. [91]

In June 1826 the minutes of the Society noted that a deputation to Professor Jameson could report on 'the very flourishing condition of the Society – that there are at present about 150 members on its list and that although we have no compulsory laws and impose no fines, we have had a good attendance of members all session and an abundant supply of papers.'[92] The Society continued to flourish and attract new members throughout the 1820s, its weekly meetings between November and July regularly attracting around twenty members. As the outline of the Society's activities above indicate, papers given at the society addressed a very wide range of topics, from mineralogy and ornithology to more controversial topics, such as phrenology and the existence of a vital principle; papers were even read to the society on subjects such as apparitions and astrology. Below we will be looking in more detail on the contributions of members of the society to the debates on spontaneous generation, and the origins and transmutation of species. In 1829 the *Magazine of Natural History* could still report that 'It will be unnecessary to remark

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[91] Anon, 'Natural History in Scotland', *The Magazine of Natural History and Journal of Zoology, Botany, Mineralogy, Geology, and Meteorology* 1 (1829), pp.291–2

[92] Minutes of the Plinian Society, 1826–1841, vol 1 (Library of the University of Edinburgh Dc.2.53), f.25 verso

[92] Ibid, f.99 verso

on the flourishing state of this Society, when it is stated that it is at present composed of upward of 180 members.'[93]

The Society, however, went into a marked decline after 1830. The minutes for 6 March 1832 refer to the 'declining state' of the Society,[94] and in those for the next meeting on 3 April the Convenor of the Committee, Henry Cheek, reported that the Committee considered that 'the Plinian Society is not in a condition to be continued any longer as a separate institution', a situation that he ascribed to the 'apparent apathy in this University to the cultivation of those Branches of natural science for the prosecution of which this Society was originally founded.'[95] The society did, however, continue to meet at increasingly irregular intervals until 19 May 1835, after which there was a lapse of almost six years until January 1841, when the minutes note that since 'there has been no meeting of the Society since 1835 and there was no prospect of any revival of it taking place, it was moved by Dr Spittal that the Society be dissolved.'[96]

The evidence of the Society's minutes sheds much light on the prevalence among its members of the kind of materialist ideas that seem to have so much shocked Coldstream. For example, in a famous and widely discussed incident, the account of a paper given at the Plinian Society by William A.F. Browne on 27 March 1827 was struck from the minutes of the Society by an unknown hand.[97] The announcement that the paper was to be given was also deleted from the minutes of the previous week's meeting. If the intention of the unknown person was to erase all trace of the paper from the minutes, he did his work badly, for the account of the paper is still

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[93] Anon, 'Natural History in Scotland', *The Magazine of Natural History and Journal of Zoology, Botany, Mineralogy, Geology, and Meteorology* 1 (1829), pp.291–2
[94] Minutes of the Plinian Society, 1826–1841, vol 2 (Library of the University of Edinburgh Dc.2.54), f.98 recto
[95] Ibid., f.99 verso
[96] Ibid., f.125 verso
[97] See, for example, Howard E. Gruber, *Darwin on Man: A Psychological Study of Scientific Creativity* (London, 1974), p.39; Desmond and Moore, *Darwin*, p.38; and Janet Brown, *Charles Darwin: Voyaging* (London, 2003), p.77

clearly legible. Curiously, the following week's minutes record that those from the previous week's meeting had been approved, with no suggestion of any debate or controversy regarding Browne's paper. Unfortunately, in the absence of further evidence, no firm conclusion can be drawn regarding the circumstances of the deletion.

In 1827 Browne had recently qualified as a licentiate of the Royal College of Surgeons of Edinburgh.[98] He was also a member of the Phrenological Society and a friend and follower of George Combe. At a Phrenological Society dinner in 1829, Combe described Browne as 'a member of the Society, who on all occasions had displayed much zeal in the cause of Phrenology, and who at the present time had been particularly active and successful in bringing before the medical students of Edinburgh the importance of Phrenology as the doctrine of the functions of the brain'.[99] As the superintendent of first Montrose Asylum and then Crichton Royal Asylum in Dumfries, Browne was to go on to have a distinguished career as a bold innovator in the treatment of mental illness.

The account of Browne's paper in the Society's minutes is as follows:

> Mr Browne then read his paper on organization as connected with life & mind in which he endeavoured to exhibit the following propositions.
> I. That all matter is organized
> II. That it is the gradually increased perfection in the arrangement of the parts constituting organization which is the source of the distinction perceptible in the various objects of nature & not specific differences.
> III. That life is the abstract of the qualities inherent in these modes of arranging matter.
> IV. That mind is to be distinguished from life, being neither one of the functions or combination of qualities, by the concatenation of which life is constituted nor a term indicating a similar idea.

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[98] Andrew Scull, 'Browne, William Alexander Francis (1805–1885)', *Oxford Dictionary of National Biography*, Oxford University Press, 2004; online ed., Sept 2010 [http://www.oxforddnb.com/view/article/46958, accessed 5 July 2012]
[99] Anon, 'Dinner by the Phrenological Society to Dr Spurzheim', *Phrenological Journal* (1829) 5, p.142

And V That mind as far as one individual sense & consciousness are concerned, is material.

A discussion ensued between Messrs Binns, Greg, Mr Grant, Ainsworth & Browne.[100]

Although the account of it is somewhat cryptically worded in places, Browne's March 1827 paper appears to have been strikingly materialist in essence. That Browne considered the human mind to be a product of the physical organisation of the brain is unquestionable on the evidence of the fifth point. In harmony with his attempts to banish immaterial agents from explanations of mental phenomena, Browne had previously given a paper on 25 April 1826 to the Society which presented a materialist explanation of apparitions. In it he attempted to prove 'that, there exists in the mind a primitive feeling of belief in spectral appearances, that as a superabundant circulation of the blood in some parts of the brain induce a state of mind calculated for receiving these'.[101] Thus an immaterial, spiritual phenomenon could be reduced to the manifestation of a purely physical process within the brain. His explanation of the phenomenon of life as 'the abstract of the qualities inherent in these modes of arranging matter' is perhaps less clearly stated than his views on the mind. However, the second point seems to imply that the differences between living and non-living things are the result of greater or lesser 'perfection' of organisation, rather than being evidence of a qualitative, 'specific' difference. In the light of this, it seems fair to conclude that Browne also believed that life too was not a manifestation of an immaterial 'vital principle', but rather a product of organisation. Such radical materialism was perhaps not surprising from an enthusiastic young disciple of phrenology, but would have been deeply shocking to the more conservative among Browne's fellow students, probably including the 'highly religious' John Coldstream. However, Browne does not seem to have been alone in attributing life and mind to material causes. On 25 March 1828 Thomas Shapter

[100] Minutes of the Plinian Society, 1826–1841, vol 1 (Library of the University of Edinburgh Dc.2.53), f.57
[101]Ibid., f.11 recto

presented a paper to the Society in which 'he stated his opinion to be that vital principle was the result of organization'.[102] It was perhaps to combat such views that John Symonds presented a copy of John Barclay's fierce defence of the vital principle, *An Inquiry Into the Opinions, Ancient and Modern, Concerning Life and Organization* (1822), to the society on 19 June 1827.[103] Based on the evidence of the Society's minutes, it is clear that a number of members flirted with materialist theories of the nature of organic life and the human mind, and that such ideas were openly discussed at the Society's meetings, even if some members were clearly actively hostile to them, as we can see from the case of Browne's February 1827 paper. It should perhaps not be surprising then that, as we will see in subsequent chapters, a number of members also touched on transformist themes in the meetings of the Society.

The Wernerian Natural History Society was quite a different beast from the Plinian Society. It was founded on 12 January 1808 by Robert Jameson, who was also its president until his death in 1854. Its founder members were Jameson, William Wright, Thomas Macknight, John Barclay, Thomas Thomson, Col. Stewart Murray Fullerton, Charles Anderson, Patrick Neil and Patrick Walker. These nine '*resolved to associate themselves into a society for the purpose of promoting the study of natural history*; and in honour of the illustrious Werner of Freyberg [sic], to assume the name of the Wernerian Natural History Society'.[104] Jameson had studied under Werner at Freiberg, and he was the most important of Werner's disciples in Britain. It is clear that one major aim of the society in its early days was to combat the theory of the earth proposed by James Hutton, and popularised in John Playfair's *Illustrations of the Huttonian Theory of the Earth* (1802).[105] Werner's theory of the earth was the main rival to that of James Hutton, leading to a lively polemic between the supporters of

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[102] Ibid., f.99 verso
[103] Ibid., f.71 recto
[104] Anon, 'Some account of the Wernerian Natural History Society of Edinburgh', *Blackwood's Edinburgh Magazine* 3: 1 (June 1817), p.233
[105] Minutes of the Wernerian Society, 1808–58, vol. 1 (University of Edinburgh Library, Dc.2.55), p.232

the two philosophers in Edinburgh. Eight volumes of the *Memoirs of the Wernerian Natural History Society* were published between 1811 and 1839. As its name would suggest, the focus of the society was on mineralogy and geology, although the *Memoirs* also contain a significant proportion of papers on zoological and botanical subjects. Active members during the late 1820s and 1830s included Robert Grant, Robert Knox, John Fleming and William Macgillivray, all of whom regularly contributed papers to the *Memoirs* of the Society during its heyday in the 1820s. The Society went into a decline, along with the health of its president, in the 1840s, and was finally formally wound up in 1858, four years after Jameson's death. If the minutes of the society are to be trusted, the choice of topics addressed at the Society's meetings were rather more conservative than those addressed at the meetings of the Plinian Society. This should be no surprise, as the members of the Wernerian Society were older, more established figures, and had little to gain from courting controversy.

The Royal Medical Society is a society for medical students at the University of Edinburgh. The Society is still in existence and holds in its library a complete set of minute books going back to its foundation in 1737. These, however, record only the private business of the Society, dealing principally with issues of membership, finance and dealings with other societies. However, its library also holds a complete set of dissertations read to the Society from its inception, which, in contrast, is an extremely valuable source of information on the interests of the members. The rules for providing subjects and delivering papers were laid down in the code of laws in 1781. According to these rules, each ordinary member had to provide 'the History of a Case, a Medical or Philosophical Question, and an Aphorism of Hippocrates'.[106] The committee then selected 36 of these as the subjects for dissertations. The rules then laid down that the president would order 'each member who intended to write during the next session to declare his intention at the next meeting, when each of the thirty-six oldest members who intended to write was required, from numbers on

[106] James Gray, *History of the Royal Medical Society, 1737–1937* (Edinburgh, 1952), p.73

slips of paper, corresponding with those marked on the sets, to draw one.'[107] In 1796 the aphorism was dropped, as was the case study in 1816, leaving only the medical or philosophical question to be answered in the dissertation. In practice the system was not as rigid as these rules would suggest, and students were very often allowed to write on subjects of their own choosing.[108]

While the bulk of dissertations presented to the society were on medical subjects, a substantial minority were on natural-historical topics. These included papers on the debate between Wernerians and Huttonians in geology, theories of generation, the relationship between mind and organisation and the origins and nature of the human races. The latter was one of the more popular non-medical subjects for dissertations. Six dissertations were presented to the Society on the subject between 1800 and 1836. Out of these, five supported the monogenist view that all the races had a common origin, against only one polygenist essay. As we will see in subsequent chapters, one of these dissertations on race, by Henry H. Cheek, also presented an outline of a theory of the transmutation of species. The topics dealt with are similar to many of those found in the minutes of the Plinian Society, with whom the Royal Medical Society seems to have shared a significant number of members. The dissertations read before the Royal Medical Society, however, provide a much richer source of information on the opinions of members than the comparatively brief entries found in the minute books of the Plinian Society.

The Royal Physical Society of Edinburgh was founded in 1771. Its constitution described it as 'exclusively devoted to Natural History and the Physical Sciences'.[109] Despite this, until 1827 the Society chiefly concerned itself with medical subjects; after this date it apparently broadened its interests to encompass science and natural history more generally.[110] Sadly, the Centre for Research Collections at the

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[107] Ibid., p.73
[108] Ibid., p.74
[109] A. Hume, *The Learned Societies and Printing Clubs of Great Britain* (London, 1853), p.172
[110] Jacqueline Jenkinson, *Scottish Medical Societies, 1731–1939* (Edinburgh, 1993), p.150

Edinburgh University library now only hold books of dissertations up to volume 36 (1822/3), later volumes presumably having been lost. Like the Royal Medical Society, the Royal Physical Society, founded in 1771, provided a forum for the debate between monogenists a polygenists over race. The only explicit reference to transformism as such is in a dissertation given by Ralph Smith Stewart on 18 October 1822. In his paper on theories of the earth, he gave a somewhat mocking account of the transformist theories of the seventeenth century French philosopher Benoît de Maillet, to which he added that

> Many bold imaginations have followed up this opinion, and have ascribed the existence of animals to the universal fluid, which were at first of the simplest kind, as the monads and other infusory microscopic animals: that in process of time these animals became complicated and assumed that diversity of nature and character in which they now exist.[111]

Stewart himself considered it 'strange that these systems which are as apparently as wild as those of the illustrious philosophers of Laputa, have been generally conceived by men of science and genius'.[112] Although does not name the men of science he is referring too, it is evident that he was aware of the transformist theories in circulation at the time, Erasmus Darwin being one likely candidate as the target of his mockery.

The Edinburgh natural history journals

Three significant journals which included articles touching on transformism were published in Edinburgh in the first half of the nineteenth century. The first two, the *Edinburgh New Philosophical Journal* and the *Edinburgh Journal of Science* were edited by two established figures in scientific and natural history circles in Scotland, Robert Jameson and David Brewster. The third was a slightly unusual venture edited by

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[111] Ralph Stewart Smith, 'On the different theories of the earth', Royal Physical Society, Dissertations [35] (1821–2)(Centre for Research Collections, University of Edinburgh library, Da.67 Phys), p.507

[112] Ibid., p.508

Henry H. Cheek and William Francis Ainsworth, both medical students at the university at that time, and entitled the *Edinburgh Journal of Natural and Geographical Science*. The most important of the three in terms of the number and variety of the transformist articles it published was the *Edinburgh New Philosophical Journal*, so it is with this periodical that I will begin.

The *Edinburgh New Philosophical Journal* is notable for publishing a significant number of transformist articles in the late 1820s. From 1819 to 1824 Robert Jameson and David Brewster coedited the journal's predecessor, the *Edinburgh Philosophical Journal*. Jameson then edited this journal on his own from 1824 until 1826, when the title was changed slightly to the *Edinburgh New Philosophical Journal.* Under this new guise it continued publication until 1852. This journal was one of the most important periodicals in its field and attracted contributions from many significant figures in natural history from across Europe and beyond. The full original title of the journal was *The Edinburgh Philosophical Journal Exhibiting a View of the Progress of Discovery in Natural Philosophy, Chemistry, Natural History, Practical Mechanics, Geography, Navigation, Statistics, and the Fine and Useful Arts*, which gives an accurate sense of its scope. It generally included around 25 articles on extremely diverse subjects, followed by the proceedings of major Scottish scientific societies, then a section of 'scientific intelligence' under various subject headings, and finally lists of patents. The journal published not only articles by important figures in Edinburgh natural history circles, such as Robert Grant, Robert Knox, John Fleming, William Macgillivray and Robert Graham, but by major national and international figures such as Alexander von Humboldt, Georges Cuvier and John Audubon. To give a flavour of the eclectic nature of the journal, volume 8, number 25 (1825) contained the following among its articles: 'On the construction of oil and gas burners', by Robert Christison, professor of medical jurisprudence at the University of Edinburgh; 'A table of the geographical positions of several places in India by James Franklin; 'Observations and experiments on the structure and function of the sponge', by Robert Edmond Grant; and 'Sketch of the geology of Sicily' by Charles

Daubeny, professor of chemistry at the University of Oxford. A significant proportion of authors were current or former students of Jameson's natural history class. After Jameson became the sole editor in 1824, it was widely referred to in natural history circles simply as 'Jameson's journal'.[113] Apart from the slight change to the title after 1826, the editorial policy of the journal seems to have been largely unaffected by Brewster's withdrawal from the editorship. Under Jameson's sole editorship it was notable for the appearance of a number of transformist articles. The sudden appearance of articles dealing with the transformation of species might suggest that Jameson had a more sympathetic attitude to transformist ideas than Brewster, who was severely evangelical in his religious views and became a great opponent of transformism in the 1840s, but it is impossible to know for sure.

Between 1826 and 1829 Jameson's journal published five openly transformist articles. Two of these were published under Grant's name, so there can be no doubt about their authorship, while three were published anonymously.[114] Three of the five articles, including both the articles by Grant, were published in the same year, 1826, one in the last volume of the *Edinburgh Philosophical Journal* and two in the first volume of the *Edinburgh New Philosophical Journal*. The first anonymous article, published in 1826, was entitled 'Observations on the nature and importance of geology'.[115] A second anonymous article of a decidedly transformist tendency entitled 'Of the changes which life has experienced on the globe' was published in 1827.[116] The final anonymous article, published in the *Edinburgh New Philosophical Journal* in 1829, was an English-language summary of a paper by Geoffroy Saint-

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[113] It was, for example, regularly referred to in this way in the pages of the *Magazine of Natural History* in the 1830s, and by Robert Chambers in both his *Vestiges of the Natural History of Creation* (London, 1844), p.172, and *Ancient Sea-margins, as Memorials of Changes in the Relative Level of Sea and Land* (Edinburgh, 1848), p.286

[114] Robert Grant, 'On the structure and nature of the Spongilla friabilis', *Edinburgh Philosophical Journal* 14 (1826), pp. 270–84 and Robert Grant, 'Observations on the structure of some silicious sponges', *Edinburgh New Philosophical Journal* 1 (1826), pp.341–51

[115] Anon, Observations on the nature and importance of geology', *Edinburgh New Philosophical Journal* 1 (1826), pp.293–302

[116] Anon, 'Of the changes which life has experienced on the globe', *Edinburgh New Philosophical Journal* 3 (1827), pp.298–301

Hilaire, published the previous year in the *Mémoires du Muséum d'Histoire Naturelle*.[117] It would be wrong to read any special significance into the anonymous publication of these articles; apparently unexceptional and uncontroversial articles entitled 'On the comparative nutritive properties of different kinds of food' and 'On the discovery of native iron in Canaan, Connecticut, North America' were also published anonymously in Jameson's journal, to give just two of many examples.[118]

There has been some debate about the authorship of the first anonymous article, 'Observations on the nature and importance of geology', perhaps the most explicitly transformist of the three, and the only one to refer directly to Lamarck. Many earlier works attributed it firmly to Grant. [119] However, in a paper published in 1991, James A. Secord made a case that the paper was not by Grant but by Jameson.[120] A third candidate proposed by Pietro Corsi is the Austrian geologist Ami Boué (1794–1881).[121] In support of his contention that the article was by Jameson, Secord musters much evidence against Grant's authorship. However, most of the arguments in favour of Jameson would hold equally well for Boué, who had attended Jameson's classes when he was a medical student in Edinburgh between 1814 and 1817 and like Jameson became a Wernerian geologist. After leaving Edinburgh, he continued to publish articles in Jameson's journal, and was a member of the Wernerian Society. He is known to have been an admirer of Lamarck and Geoffroy and an advocate of

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[117] Anon, 'Of the continuity of the animal kingdom by mean of generation from the first ages of the world to the present times: On the relations of organic structure and parentage that may exist between animals of the historic ages and those at present living, and the antediluvian and extinct species', *Edinburgh New Philosophical Journal* 7 (1829), pp.152–6. For the original article, see Étienne Geoffroy Saint-Hilaire, 'Mémoire: Où l'on propose de rechercher dans quelles rapports de structure organique et de parenté sont entre eux les animaux des âges historiques, et vivant actuellement, et les espèces antédiluviennes et perdues', *Mémoires du Muséum d'Histoire Naturelle* 26 (1828), p.209–29

[118] See respectively *Edinburgh New Philosophical Journal* 3, p.140 and *Edinburgh New Philosophical Journal* 4, pp.154–6

[119] See, for example, Loren Eiseley, *Darwin's Century: Evolution and the Men Who Discovered It* (Garden City, NY, 1958), pp.146–7 and Desmond, *Politics of Evolution*, p.69. Desmond later accepted Secord's attribution of the paper to Jameson in Desmond and Parker, 'The bibliography of Robert Edmond Grant', p.206

[120] Secord, 'Edinburgh Lamarckians'

[121] Pietro Corsi, 'The revolutions of evolution', p.17

spontaneous generation.[122] In addition, in the same year he had published a 'Geological Observation' in the *Edinburgh New Philosophical Journal* on a similar theme, the style and vocabulary of which bear close comparison with the anonymous article, as I will explore in greater detail in a later chapter.[123] His credentials as a transformist are left in little doubt by a 'résumé of the progress of geological sciences during the year 1833' for the *Bulletin de la Société Geologique de France*. In this work he states:

> The naturalist who restricts the circle of his ideas to the short duration of his life will necessarily be directed to the ancient idea of the species as a being *sui generis* formed once for all time, which must perpetuate itself as such, at least as long as the present laws of nature remain in effect. The authority of scholastic writings and the most ancient legislators also corroborate this opinion, engraved in the memory from the most tender infancy. On the other hand, in examining the whole scale of creations, living as well as fossil, in ignoring individual instances in order to see the whole, set in motion by a subtle material that is disseminated everywhere, one easily arrives with the Lamarcks, the Geoffroys, and other great naturalists, at an entirely different conclusion.[124]

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[122] Goulven Laurent, 'Ami Boué (1794–1881) : Sa vie et son œuvre', *Travaux du Comité Français d'Histoire de la Géologie*, troisième série T.VIII (1993) (http://www.annales.org/archives/cofrhigeo/ami-boue.html [accessed 12 March 2013])

[123] Ami Boué, 'Geological Observation, – 1. On Alluvial Rocks: 2. On Formations: 3. On the Changes that appear to have taken place during the different periods of the Earth's formation on the Climate of our Globe, and in the nature and the physical and the geographical distribution of its Animals and Plants', *Edinburgh New Philosophical Journal* 1 (1826), p.90

[124] 'Le naturaliste qui restreint le cercle de ses idées à la courte durée de sa vie, sera nécessairement porté à l'idée ancienne que l'espèce est un être *sui generis* formé une fois pour toutes, et devant se perpétuer tel, aussi long-temps du moins que dureront les lois actuelles de la nature. L'autorité des écrits scolastiques
Et des législateurs les plus anciens vient encore corroborer cette opinion gravée dans la mémoire de la plus tendre enfance. D'un autre côté, en parcourant toute l'échelle des créations, tant vivantes que fossiles, et en négligeant les individualités pour ne voir qu'un tout mis en mouvement par une matière subtile disséminée partout, on arrive aisément avec les Lamarck, les Geoffroy, et autres grands naturalistes, à une tout autre conclusion.' Ami Boué, *Bulletin de la Société Géologique de France: Résumé des Progrès de Sciences Géologiques pendant l'année 1833* (Paris, 1834), pp.113–14

There are several important arguments against Grant's authorship of the article. Firstly, Grant published almost exclusively on invertebrate zoology in this period; if Grant were to have written a piece on Lamarck at this time it would be most surprising for it to be couched as a defence of geology, whereas for Jameson or Boué this would be entirely natural. What is more, the main thrust of the argument in defence of geology is almost identical to that found in Jameson's preface to the fifth edition of Cuvier's *Theory of the Earth* (1827), written only a matter of weeks after the publication of the article.[125] Secord also points out that the geological ideas and the terminology in which they are expressed are a more or less perfect fit with those to be found in other publications by Jameson from around the same date. For example, the use of the terms 'geognosy' and 'oryctognosy', and the generally Wernerian cast of the article are very characteristic of Jameson's writing, much of which would have appeared quite anachronistic by 1826. Once again, however, the same could be said for Jameson's disciple Boué. Finally the author of the anonymous article presents Lamarck's theory as 'the logical consequence of Werner's', to which both Jameson and Boué were deeply committed. In contrast, there is no obvious reason why Grant would have been interested in making a connection between Wernerian geology and Lamarckian transformism. In addition to the arguments based on the ideas expressed in the article, Secord points out numerous close textual parallels between the anonymous article and Jameson's notes and preface to the fifth edition of the *Theory of the Earth*.[126] However, perhaps unsurprisingly, similar textual parallels can also be found in Boué's earlier paper of 1826. While it is quite possible the article was by Jameson, the evidence from other sources for Boué's transformist ideas make him seem at least as likely a candidate.[127]

After he relinquished the editorship of the *Edinburgh Philosophical Journal*, David Brewster established his own journal, *The Edinburgh Journal of Science* in 1824, which

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125 Georges Cuvier, *Essay on the Theory of the Earth* (Edinburgh, 1827), pp.v–viii
126 Ibid., pp.11–13
127 See, for example, Ami Boué, 'Résumé des progrès des Sciences géologiques pendant l'année 1833', *Bulletin des Sciences naturelles et géologiques* 5: 1 (1834), pp.112–21

he continued to publish until 1832. Brewster's editorial policy in his new journal was very close to that of the *Edinburgh Philosophical Journal* and it published a similar range of articles. According to Brewster, the 'same kind of information which has been given in the Edinburgh Philosophical Journal, will be given in the present one, with various improvements, both in the embellishments, and in the scientific character of the work.'[128] Editorial assistance was provided in a number of areas by a group of experts, including Robert Knox in zoology and comparative anatomy, and Samuel Hibbert in geology and antiquities.[129] Some writers, including both Knox and Grant, continued to publish articles in both Brewster's and Jameson's journal. Isidore Geoffroy St Hilaire even published very similar articles on female pheasants which took on the plumage of the male in both Brewster's and Jameson's journals.[130] Grant published four articles on invertebrate zoology in Brewster's journal in 1827 and 1828, of which one contains a brief reference to the transmutation of plant species.

Between October 1829 and June 1831 Henry H. Cheek edited a short-lived journal entitled the *Edinburgh Journal of Natural and Geographical Science*. For the first two volumes his co-editor was William Francis Ainsworth, his friend, fellow medical student and member of the Plinian Society. The final volume was edited by Cheek alone, with editorial assistance in specific areas provided by Sir William Jardine, G.A. Walker Arnott, John Scouler, Robert Knox and J.F.W. Johnston, who 'have undertaken the entire direction of their several Departments'.[131] In total 21 monthly issues of the journal were produced. The *Journal* was published in Edinburgh but also distributed in London and Dublin.[132] It seems that Cheek may have published

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[128] [David Brewster], 'Advertisement', *Edinburgh Journal of Science* 1 (1824), pp.viii–ix
[129] Ibid., p.viii
[130] 'On female pheasants assuming the male plumage', *Edinburgh New Philosophical Journal* 1: 1 (1826). pp.302–10 and 'On the females of pheasants which assume the plumage of the male', *Edinburgh Journal of Science* 6: 9 (1827), pp.9–17. It is not recorded if this occasioned any friction between the respective editors.
[131] Title page, *Edinburgh Journal of Natural and Geographical Science* 3 (1831), p.1
[132] Charles W.J. Withers, 'Towards a history of geography', *History of Science* 37 (1999), p.63

this journal from his own house in Gardner's Crescent, Edinburgh, as Patrick Neill, the Edinburgh printer and secretary of the Wernerian Natural History society, referred to 'his converting his dwelling-house into a printing office' in a polemical pamphlet he wrote attacking Cheek in 1830.[133]

The journal published articles on a wide variety of scientific and geographical subjects, including some by notable figures in Edinburgh natural history circles, such as William Macgillivray, Sir William Jardine and John Fleming. Despite the youth and inexperience of its founders, the journal therefore seemed to have the support of a number of important figures in Edinburgh natural history. It was similar in scope and format to Jameson's *Edinburgh New Philosophical Journal* and may have been conceived as a competitor to it. As we have already seen, Cheek had used the pages of his journal to openly criticise Jameson in no uncertain terms, prompting a brief pamphlet war with Patrick Neill, who accused Cheek of trying to set himself up as 'the Baron Cuvier of Edinburgh'. [134] As we will see in later chapters, Cheek seems to have used the journal not only to attack those he perceived as his enemies, but as a platform for his own views on the transmutation of species.

Cheek's bitter attacks on Neill and Jameson must surely have irreparably soured relations with many of the leading figures in the natural history establishment in Edinburgh. The dispute gives us an invaluable window into Cheek's position in Edinburgh natural history circles in the first years of the 1830s. It seems that Cheek was at the centre of a circle of younger natural historians who met for regular conversaziones at his house in Gardner's Crescent, from which he also published his journal. From this position he seems to have felt confident enough to, perhaps unwisely, challenge the authority of the Wernerian Society, and of its president, Robert Jameson, Edinburgh's professor of natural history. It might be speculated

133 Patrick Neill, *Supplement to an Address to the Wernerian Natural History Society, Dated July 1830; Containing a Reply by Mr Neill to Mr Cheek's Answer. November 1830* (Edinburgh, 1830), p.16
134 Neill, *Supplement to an Address to the Wernerian Natural History Society,* pp.15–16

that this controversy, in which Neill seems to have had the last word, may have led, directly or indirectly, to the winding up of Cheek's journal after its next volume and hastened his departure from Edinburgh after his graduation in 1832, but it is impossible to know for certain.[135]

Conclusion

We have seen how there existed a network of interconnected institutions in Edinburgh in the early decades of the nineteenth century which acted as a focus for the dissemination and development of new ideas in natural history. At the centre of this network was Robert Jameson, the professor of natural history. He was the patron and mentor for generations of natural historians, including such notable figures as Robert Grant, Robert Knox and Ami Boué. For half a century his role as professor of natural history put him in a key position to shape the opinions of the countless students who passed through his class. His influence was also felt through his role as president of the Wernerian Society and editor of the *Edinburgh New Philosophical Journal*. The latter provided a particularly important channel for the transmission of transformist ideas. Jameson was also plugged into an extensive international network of natural historians, across which ideas and specimens could be exchanged. Not only did international students, such as Boué, come to study at Edinburgh, but Edinburgh students, such as Grant, Knox and Jameson himself, went to study abroad, bringing back with them the exciting new theories of thinkers such as Werner, Lamarck and Geoffroy. Beyond Jameson's immediate circle of influence, the Plinian Society, the Royal Physical Society and the Royal Medical Society provided undergraduates with fora for the discussion of new and often controversial ideas in the natural sciences.

Setting himself up in opposition to the established circle around Jameson was the somewhat enigmatic figure of Henry Cheek. Although he seems to have maintained

135 For an excellent account of this controversy, see Forbes W. Robertson, *Patrick Neil: Doyen of Scottish Horticulture* (Dunbeath, Caithness, 2011), pp.46–7

close links with some established figures, especially Knox, his position in the Edinburgh natural history world must have been rendered extremely problematic due to his unbridled criticism of Jameson. Nonetheless, he too promoted transformist ideas both in the student societies to which he belonged and through his editorship of the *Edinburgh Journal of Natural and Geographical Science*. He seems also to have been at the centre of a group of students who met at his house in Gardner's Crescent to discuss the latest theories in natural history. It seems likely that only his tragically early death in 1833 prevented him from becoming a significant figure in natural history in the decades that followed.

We have also seen how even some apparently more conservative figures in Edinburgh natural history, such as Barclay, Fleming and Brewster supported and encouraged such unconventional thinkers as Grant and Knox during the 1820s and 30s. This may seem all the more surprising when it is remembered that both Fleming and Brewster went on the become fierce critics of transformism after the publication of *Vestiges of the Natural History of Creation* in 1844. To give just a few examples of their perhaps surprising openness to talented individuals with unconventional ideas, Brewster published the work of Grant and Knox in his journal, Barclay employed them both in his extra-mural anatomy school, while both Brewster and Fleming provided glowing references for Grant to support his candidacy for the post of professor of zoology at University College London, where he was to continue to promote his transformist theories in this new context.

The theories of students and graduates of the University, such as Knox, Grant, Boué and Cheek, were then spread far and wide in the pages of the Edinburgh-based scientific journals. That this period of efflorescence is not better known may in part be due to the fact that none of the Edinburgh transformists wrote a major work on the subject, and their ideas were rapidly forgotten in the changing and increasingly hostile intellectual climate of the late 1830s and 1840s. It is not inconceivable, however, that the Edinburgh transformists, through the seeds that were planted in the minds of the many scientists and medical men who had studied at the University, may well have had a profound influence on the re-emergence of evolutionary ideas as a subject for serious scientific debate after 1859.

Chapter 4: The reception of transformist theories in Edinburgh

Introduction

In the preceding two chapters I have examined the transformist theories that were prevalent in the late eighteenth and early nineteenth centuries and the Edinburgh contexts in which there is some evidence these ideas were received and discussed. In this chapter I will look at the wide variety of responses to transformist theories by figures from Edinburgh's natural history circles that can be reconstructed from the surviving evidence. To this end I have explored the teaching of natural history and other related subjects at the University, the records of the medical, scientific and natural history societies of the city and books and articles published by natural historians. First, I will examine the impact of the eighteenth-century natural historians whose theories were explored in chapter 2. Then I will turn to the two most important transformist thinkers of the early nineteenth century, Jean-Baptiste Lamarck and Étienne Geoffroy Saint-Hilaire, to establish how widely known their theories were in Edinburgh and how their ideas were received. As reactions to Lamarck appear to have been markedly different among evangelicals from the responses of those who did not share their religious perspective, I will deal with critiques of Lamarck based on characteristically evangelical considerations in a separate section.

It has been suggested by some historians that transformism was seen from the first as a threat to religion and society because any challenge to the established view of the natural order in the sphere of natural history would inevitably have been seen as a dangerous threat to the natural order of society. Adrian Desmond, for example, has typified Lamarckism as 'a progressive anti-establishment science'.[1] In the context of Edinburgh in the 1820s, Desmond and James Moore have specifically

[1] Adrian Desmond, 'Artisan resistance and evolution in Britain, 1819–1848', *Osiris, 2nd Series* 3 (1987), pp.77–110

identified the members of the Plinian Natural History Society as 'fiery, freethinking democrats who demanded that science be based on physical causes, not supernatural forces.'[2] While not going as far as Desmond, James Secord has also conceded that these 'new approaches were considered subversive in certain quarters'.[3] It is a key concern of this chapter to establish to what extent this suggested association in the minds of contemporaries between radical politics and transformist theorising is borne out by the evidence. Do we find advocates of transformism drawing political or social morals from the theories of Lamarck and Geoffroy? Is there evidence for a moral panic regarding transformism on the part of the academic and medical establishment, fearing the spread of subversive ideas about the natural order among disaffected students and working class radicals in Edinburgh in the early decades of the nineteenth century? These will be some of the questions that this chapter will aim to address.

Eighteenth-century thinkers: Linnaeus, Darwin and Buffon

Before addressing the reception of transformist thinkers in Edinburgh at the end of the eighteenth and beginning of the nineteenth centuries, it is necessary to briefly review some of the characteristics of late eighteenth-century thought that conditioned responses to their theories. A dominant concept in discourse on the natural order in the eighteenth century was the 'great chain of being'. This concept was underpinned by what Arthur O. Lovejoy has called the Principle of Plenitude, which he defined as the principle that 'no genuine potentiality of being can remain unfulfilled'.[4] Lovejoy has traced this principle back to the ancient Greeks, but in its eighteenth century form it was based on the idea that it was impossible that a deity whose attributes were infinite would not create a universe where all potentiality for being was realised. Thus nature was envisaged as an infinite hierarchical chain of being ascending from inanimate matter to the deity himself, as is so memorably

[2] Desmond and Moore, *Darwin*, p.31
[3] Secord, 'Edinburgh Lamarckians', p.15
[4] Arthur O. Lovejoy, *The Great Chain of Being* (Cambridge, MA, 1964), p.52

illustrated in Pope's *Essay on Man*. Each link in the chain was locked in place in such a way that 'Where, one step broken, the great scale's destroyed'.[5] In Lovejoy's words, it represented 'an absolutely rigid static scheme of things'.[6] It was therefore fundamentally incompatible with any suggestion of change or progress in the natural world. The concept of the great chain of being as it existed in the late eighteenth century is effectively illustrated in the final chapter of volume I of the *Philosophy of Natural History* (1790) by William Smellie, Walker's rival for the chair of natural history in 1779, which is entitled 'Of the Progressive Scale of Animals'. Here Smellie wrote that 'There is a graduated scale or chain of existence, not a link of which, however seemingly insignificant, could be broken without affecting the whole.'[7] The natural order, at least in the case of living things, did not allow for any gaps, and 'the gradations from one species to another are so imperceptible that to discover the marks of their discrimination requires the most minute attention.'[8] Organisms were locked into an ascending natural order that was complete and unchanging.

Although there was little love lost between Smellie and Walker as a result of their rivalry for the chair of natural history,[9] they were of one mind on the question of the order of nature. In a set of student's lecture notes from Walker's course in 1791 can be found the following exposition of the chain of being: 'There is undoubtedly a continued chain in nature from its lowest subject up to the human species, & which it is to be supposed proceeds from him to his maker, all being linked as in the moral world by the most beautiful & regular gradation; for nothing is more certain than the maxim "Natura nunquam fit saltus".'[10] It is worth comparing these words with a classic formulation of the same idea from the *Contemplation de la Nature* (1764) of

[5] Alexander Pope, *An Essay on Man* (Glasgow, 1768), p.9
[6] Lovejoy, *Great Chain of Being*, p.242
[7] William Smellie, *The Philosophy of Natural History*, vol. 1 (Edinburgh, 1790), p.520
[8] Ibid., p.55
[9] Brown, 'Smellie, William (1740–1795)', p.3
[10] John Walker, Notes on natural history lectures (1791) (anonymous student) (Edinburgh University Library, Dc.10.33), f.37

Charles Bonnet (1720–93), whose work Walker was certainly familiar with: 'Between the lowest degree and the highest degree of physical or spiritual perfection there is an almost infinite number of intermediate degrees. The series of these degrees constitutes the *Universal Chain*.'[11]

The continuity that Smellie and Walker imagined as an attribute of the chain of being has another interesting consequence for Smellie in his *Philosophy of Natural History*. The seamless unity of the natural order can also be used to explain the obvious family resemblances between some species. These ideas on the relations between species seem in some ways to prefigure the concept of unity of plan that was to become the central concept of philosophical anatomy in France and Britain in the early decades of the nineteenth century. Smellie noted, for example, that 'in the creation of animals, the Supreme Being seems to have employed only one great idea, and, at the same time, to have diversified it in every possible manner, that men might have the opportunity of admiring equally the magnificence of the execution and the simplicity of the design.'[12] It may have been that he was influenced by the ideas on unity of plan of Buffon, whose work Smellie had translated; in volume 8 of Smellie's translation of Buffon's *Histoire Naturelle* we find the following passage: '… in all of them [vertebrate animals] he found a solid structure composed of the same pieces, and nearly situated in the same manner. This plan proceeds uniformly from man to ape, from ape to quadrupeds, from quadrupeds to cetaceous animals to birds, to fishes, and to reptiles'.[13] In Smellie's vision of the natural world, this unity of plan was a natural consequence of the imperceptibly close gradations that existed between the links in the chain of being. Following this line of reasoning, he went on to claim in his *Philosophy of Natural History* that 'Man, in his lowest condition, is evidently linked, both in the form of his body and the capacity of his mind, to the

[11] 'Entre le dégré le plus bas & le dégré le plus élevé de la Perfection corporelle ou spirituelle, il est un nombre presqu'infini de dégrés intermédiaires. La suite de ces dégrés compose la *Chaîne universelle*', Charles Bonnet, *Contemplation de la Nature* (Paris, 1764), p.27
[12] Smellie, *Philosophy of Natural History*, p.55
[13] Comte de Buffon, *Natural History, General and Particular* (trans William Smellie) (London, 1785), vol 8, pp.62–3

large and small orang-outangs.'[14] However, this is emphatically not an evolutionary connection, but simply the result of close proximity in the progressive scale of being. To suggest that the orang-utan could one day become human would be to imply that it was possible to break the chain of being, destroying the integrity of the natural order.

In his lectures, Walker made very clear that he considered species to be immutable. In a student's notes from his lectures in 1782 we find him stating that it is 'probable that no species of P[lant] or An[imal] changes into another & no species lost or new formed'.[15] In 1790 he stated categorically that 'Both in the Vegetable and Animal Kingdom there are a certain number of distinct species which have remained without addition or diminution since the creation.' [16] In 1797 he is still telling very much the same story, although in even more emphatic terms: 'There therefore appears to be no Species of Plants or Animals entirely lost, or any new Species formed. The Transmutation of Species either in Plants or Animals, is a Vulgar Error.'[17] That Walker found it necessary to refute transformism, and to continue to do so from the early 1780s through to the later 1790s, shows that he was well aware of these theories, and felt it was an influential enough idea to require categorical refutation.

For Walker, therefore, species were fixed for all eternity. Nonetheless, as we will see in the next chapter, the same was not necessarily true for varieties of plants and animals, which he defined as 'beings belonging to a species, and differing from it in some trifling circumstance.'[18] Such varieties were simply the result of environmental

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[14] Smellie, *The Philosophy of Natural History*, p.523
[15] John Walker, Notes on natural history lectures (1782) (taken by Charles Stewart) (Edinburgh University Library, Dc.2.22), f.7 verso
[16] John Walker, Notes on natural history lectures (1790) (anonymous student), vol. 4 (Edinburgh University Library, Dc.2.25), f.162
[17] John Walker, Notes on natural history lectures (1797) (taken by David Pollock), vol. 9 (Edinburgh University Library, Gen.711), f.135
[18] Ibid., f.164

factors, such as the climate or the availability of food, acting on individuals of the species. Walker's ideas about the nature of varieties are likely also to have been influenced by Buffon, whose works Walker cited regularly in his lectures. In volume 6 of his *Histoire Naturelle*, quoted here in Smellie's translation of 1785, Buffon gave an account of the effect of the environment on varieties of animals.

> And, if we examine each species in different climates, we shall find sensible varieties both in size and figure. These changes are produced in a slow and imperceptible manner. Time is the great workman of Nature. He moves with regular and uniform steps. He performs no operation suddenly; but by degrees, or successive impressions, nothing can resist his power; and those changes which at first are imperceptible, become gradually sensible, and at last are marked by results too conspicuous to be misapprehended. [19]

It is not clear whether Buffon is here proposing a mechanism for the development of new species as well as varieties, although it is quite evident that Walker was thinking only of the generation of varieties. In any case, the manner and mechanism of the transmutation appear to be the same.

In contrast to Walker, Alexander Monro, Secundus, the professor of medicine and anatomy at the University of Edinburgh, was thoroughly hostile to Buffon's ideas on even the transmutation of varieties. In his anatomy lectures for 1774–5 he gave a summary of Buffon's account of the development of new varieties of dog through the influence of environmental factors. He went on to challenge his ideas, claiming that '*Buffon* refutes himself by the very accurate enumeration he gives. If the variety of Dogs depends upon the Circumstances he supposes, how comes he to find there is a certain No. only, the No. should have been endless, considering of the succession of Ages'.[20] Even if not everyone accepted Buffon's theories on the production of new varieties through the influence of the environment, it is clear that

[19] Buffon, *Natural History*, vol. 4, pp.70–1
[20] Alexander Monro, Secundus, Notes on lectures on anatomy (1774–5) (taken by James Johnson) (Edinburgh University Library, Gen.573), f.197

they were widely known, and that those who disagreed with them considered them at least worthy of refutation.

As we saw in chapter 2, in Carl Linnaeus's *Disquisitio de sexu plantarum* of 1760 he suggested not only that hybridisation was possible, but that interspecific hybrids could be fertile. He went on to propose that that new species could arise in this manner. In his *Disquisitio* he wrote that 'It is impossible to doubt that there are new species produced by hybrid generation'.[21] Walker seems to have been aware of these views, as in a lecture of 1790 he said that 'Linnæus, lately thought, that all plants changed to their present dissimilar forms during the process of time from a single species. He and Bonnet have thought too that plants may and do continue to start up during the course of Ages. An Opinion in which few will be willing to follow him.'[22] Presumably it was Linnaeus's theory of the production of new species through hybridisation that Walker had in mind here, although there is the suggestion that Linnaeus went further than this to propose a common origin for all species of plants. In 1797 Walker explicitly denied that inter-specific hybrids could ever be fertile, stating that 'if Mules were fertile the whole Vegetable and Animal Creation, would run into Confusion and Disorder, so that this Infertility of the Mules, may be presumed to be intended by the wise Creator of the Universe, to preserve the Order and Regularity, which is every where so conspicuous over the Globe'.[23] The infertility of hybrids is therefore presented as a mechanism of divine providence for keeping the chain of being in order. Monro also emphasised the reduced fertility of hybrids even among different breeds of dog, claiming that any hybrid breed would 'generate a certain No. of Times, but after that the spurious breed wears out, & hence the varieties come to be marked so plainly.'[24]

[21] Translation by H.F. Roberts. Quoted from Bentley Glass, 'Heredity and variation', p.149
[22] Walker, Lecture notes (1790), vol. 4, f.163
[23] Walker, Lecture notes (1797), vol. 9, f.140
[24] Monro, Lecture notes (1774–5), f.197

At times Walker even seemed to doubt whether inter-specific hybridisation was possible at all. We know from the 1790 lecture notes that Walker knew about Linnaeus's claim published in 1749 that the newly discovered species *Peloria* was a hybrid, but that he insisted that this was not the case and that it was only a remarkable variety of an existing species.[25] Walker went on to recount how the Swedish naturalist and disciple of Linnaeus Daniel Solander had personally shown him a supposed example of a hybrid between *Mirabilis talapa* and *Mirabilis longiflora* plants which had been sown in the same bed, but dismissed his claims as he regarded the parent plants as varieties of the same species. He was also aware of the experiments on hybridisation in plants performed by Joseph Gottlieb Koelreuter, who in 1761 produced the first well-authenticated inter-specific hybrid.[26] Again he dismissed these, as in his opinion the parent plants were 'only varieties tho delivered by Linnæus as different species'.[27] Although seemingly determined to reject all claims to have produced a race of hybrid plants, he was, however, prepared to concede that 'it would appear that the production of hybrid plants requires more experiments before any conclusion can be formed.'[28]

While Walker doubted the possibility of inter-specific hybrids, for him hybridisation between different varieties of the same species could take place and produce fertile progeny. At the end of lecture 42 of his course in 1790 Walker has something very intriguing to say about hybrids between different varieties of peacock:

> It is certain likewise that in some animals a variety will miss one generation and take place in the next. I have seen a white peacock and pea hen, which, when they bred produced peacocks and pea hens of the common sort, yet these, when they bred produced white peacocks and pea hens, but here Gentlemen I find the hour is elapsed.[29]

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[25] Walker, Lecture notes (1790), vol. 4, ff.168–9

[26] Bentley Glass, 'Heredity and variation in the eighteenth century concept of the species', in Glass, Tempkin and Strauss, *Forerunners of Darwin*, p.158

[27] Walker, Lecture notes (1791), f.19

[28] Ibid., f.19

[29]Walker, Lecture notes (1790), vol. 4, f.167–8

Walker does not offer any explanation for this phenomenon, which is presented merely as an anecdote to round off his lecture. Maupertuis had addressed the question of heredity in cases such as this in his *Vénus Physique* (1745), where he discussed the inheritance of albinism in humans at length.[30] We know that Walker had probably read some of the works of Maupertuis, or had at least heard about them at second hand; in one of his lectures in 1797 he made a reference to his speculations about the possible impact of comets on the history of life on earth in his 'Essai de la Cosmologie' (1751).[31] However, if Walker knew of Maupertuis's ideas on heredity, or their relevance to the phenomenon he describes, we have no evidence that he said anything about it in his lectures.

In his course Walker gave a detailed account of different theories of generation. First he dealt with spontaneous generation, or as it was generally known at the time, 'equivocal generation', an idea that he traces back to Aristotle. Equivocal generation was the idea that life could arise spontaneously from non-living matter; to quote some of the examples given by Walker, 'that silk worms were generated from putrid mulberry leaves' and 'most other insects, from corrupted vegetables and animals'.[32] In the notes from his lectures in 1782 he said that the 'Doctrine of equivocal Gen[eration is] to be reviewed and rejected & that of univocal established.'[33] In 1791 he continued to declare in more or less the same words that 'The doctrine of Equivocal generation is here to be considered & rejected upon review'.[34] He went on to discuss and reject the opinions of Buffon on generation, which he presented in the following fashion:

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[30] Bentley Glass, 'Maupertuis, pioneer of genetics and evolution', in Glass, Tempkin and Strauss, *Forerunners of Darwin*, p.70–1

[31] Walker, Lecture notes (1797), vol. 3, f.135–6; Pierre-Louis Maupertuis, 'Essai de la Cosmologie', *Les Oeuvres de Monsieur de Maupertuis* (Paris, 1752), pp.35–6

[32] Walker, Lecture notes (1790), vol. 4, f.159

[33] Walker, Lecture notes (1782), f.7 verso

[34] Walker, Lecture notes (1791), f.45

> 1st That every where in nature there are certain Particulars [sic] Organiques.
> 2d That there are in nature certain external and internal moults [sic] in which these particles are formed into living existences and
> 3d That there is in nature a force productive sufficient to bring to life the particles fashioned by the Moults, thus, this differs very little from the Hypothesis of Equivocal generation[.][35]

After this description of Buffon's system, he went on to reject his ubiquitous organic molecules as chimeras. He finally settled on a theory he attributed to William Harvey as closest to the truth, stating that the most likely explanation of generation was 'that a mixture of male and female liquors formed the Fœtus.'[36] He also quoted with approval Harvey's dictum 'omne animal ex ovo', thereby denying the possibility of equivocal generation. However he also claimed in his 1790 lectures that 'Bonnet and Haller have proved that the embryo of the animals is in the female'.[37] The theory of Bonnet and Haller referred to here was a version of preformationism, in that it suggested that the adult form of the animal was contained within the egg of the mother, entirely formed, although tiny. There is clearly some contradiction here between the epigenetic argument for the formation of the embryo from a mixture of the 'liquors' of the two parents and the idea that the embryo derives from the female. Which of these two mutually incompatible alternatives reflected Walker's own views is not clear. However, his ideas did not appear to have changed much by 1797, when he was still declaring himself in agreement with the opinions of Harvey and hostile to the idea of equivocal generation, which was central to the theories of many transformist thinkers, such as his contemporary Erasmus Darwin.[38]

In the preface to his influential *Philosophy of Zoology* (1822) the Church of Scotland minister and natural historian John Fleming warmly recommended a book by John Barclay published in the same year and entitled *An Enquiry into the Opinions Ancient*

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[35] Walker, Lecture notes (1790), vol. 4, f.161
[36] Ibid, f.160
[37] Ibid, f.161–2
[38] Walker, Lecture notes (1797), vol. 9, f.128–9

and Modern, Concerning Life and Organization. In Fleming's opinion Barclay's book 'should be perused with care by every student of Anatomy and Natural History, as an effective preservative against the doctrines of Materialism, and deserves a place as well in the library of the Divine as in that of the Physiologist.'[39] Barclay's religious faith strongly coloured the views he expressed in *Life and Organization*, which at times reads more like an evangelical sermon than a scientific work. Some flavour of this rhetorical style can be gained from the following rapturous definition of the deity taken from a long passage laden with theological argument found in the 'Summary View' at the end of the book; according to Barclay God is: 'the first and the last, the self-existent, the supreme in heaven and supreme on earth, the being who, in the opinion of Aristotle, could not possibly owe his existence to matter, though matter might derive its existence from him'.[40]

In accord with Barclay's motives for writing *Life and Organization*, the book was organised as a kind of gazetteer of key thinkers whose ideas on the nature of life he wanted to critique or refute. It contains sections on the opinions of philosophers and scientists in the following order: Ocellus Lucanus, Democritus, Epicurus, Lucretius, Paracelsus, Jean-Baptiste Fray-Fournier, Erasmus Darwin, Leibnitz, Joseph Priestley, Albrecht von Haller, Buffon, John Turberville Needham, Maupertuis, Jean-Baptiste Robinet, Johann Friedrich Blumenbach, Pierre Gassendi, Georges Cuvier, William Lawrence, Pierre Jean Cabanis, Jean-Antoine Chaptal, Thomas Thomson, René Descartes, Aristotle, William Harvey, Thomas Willis, John Hunter, John Abernethy, Joseph Philippe François Deleuze and Nehemiah Grew. The order is clearly not historical, but is intended to be thematic. Barclay divided the book into the following sections: materialist philosophers of antiquity (Ocellus Lucanus to Lucretius), 'those who, since the revival of learning have treated of the causes of organisation, and ascribed the principal phenomena of life to organic structure' (Paracelsus to Descartes) and 'those who suppose a living internal principle distinct

[39] John Fleming, *The Philosophy of Zoology or a General View of the Structure, Functions, and Classification of Animals,* vol. 1 (Edinburgh, 1822), p.xiii
[40] Barclay, *Life and Organization*, p.529

from the body and likewise the cause of organization' (Aristotle to Grew). Although several of these figures had written on the transmutation of species, including Lawrence, Fray-Fournier and Robinet, Barclay had little to say about this aspect of their thought. His interests lay more with their opinions on materialism and the vital principle. One name is conspicuous for its absence from this list, and that is Lamarck. This is most surprising, as the list does include some of Lamarck's younger contemporaries, such as Cuvier and Fray-Fournier. It seems hard to believe that Lamarck's radically materialist ideas on the nature and origin of life did not attract the wrath of Barclay, as did the theories of many of his contemporaries. Perhaps the most likely explanation is that in 1822 he was simply unaware of them.

One figure whose transformism Barclay did allude to was Erasmus Darwin. For his critique of Darwin, Barclay seems to have relied largely on the third edition of *Zoonomia* (1801). After giving the reader a detailed account of Darwin's theories, noting his conclusion that all life may trace its origin to a single living filament, and drawing parallels with the ideas of Jean-Baptiste Fray-Fournier, the French military surgeon and amateur naturalist, Barclay proceeded to give his own critique of his theories. His criticism of Darwin centred around the inability of the 'living filaments', which for him were the primordial form at the origin of all life, to show 'that degree of intelligence and foresight necessary to qualify them for organizing a complicated structure' and bemoans his failure to 'assist them with the laws of fate or necessity, the wisdom of nature, or the providence of a deity.'[41] Ultimately, it would seem that it was the overly mechanistic nature of Darwin's theory, and his failure to provide a clear role for divine providence operating through the agency of an immaterial vital principle that made his ideas unacceptable. The strength of Barclay's hostility towards Darwin and his ideas can be gauged by the following assessment of *Zoonomia* from the final paragraph of the section on the 'Opinions of Darwin' in *Life and Organization*: 'If redundancy of fancy, novelty of terms, unbounded extravagance of supposition, and multitudes of theories, with numerous

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[41] Barclay, *Life and Organization*, p.147

facts, curious and important, but ill-arranged and ill-understood, were to recommend the Zoonomia, it should certainly stand high in the public opinion.'[42]

However, not everyone in Edinburgh in the early nineteenth century was so ready to dismiss Erasmus Darwin's ideas. Robert Grant, for example, was first introduced to transformism through the writings of Darwin before he encountered the theories of Lamarck. He seems to have been aware of Darwin's transformist theories from at least his years as an undergraduate at the University of Edinburgh, as he made reference to Darwin's *Zoonomia* in an undergraduate dissertation published in 1814.[43] Much later, in the dedication to Charles Darwin in Grant's *Tabular View of the Primary Divisions of the Animal Kingdom* (1861) he wrote that 'More than fifty year have now elapsed since the "Zoonomia" of your illustrious ancestor, Dr. Erasmus Darwin, first opened my mind to some of "the laws of organic life".[44] For Grant, at least, the theories of Erasmus Darwin proved to be the catalyst for a life-long advocacy of the transmutation of species.

The reception of Lamarck in university and medical circles

There is some evidence that Lamarck's theories were known among a group of medical practitioners identified by L.S. Jacyna as 'philosophic Whigs,' who were active in Edinburgh in the early decades of the nineteenth century. Jacyna has shown that John Thomson (1765–1846), professor of surgery at the Royal College of Surgeons 1804–21, regius professor of military surgery at the University from 1806 and professor of general pathology from 1832 to 1841, presented a 'carefully edited version of Lamarckism' in his lectures.[45] However, despite 'his numerous references

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[42] Ibid., p.148

[43] Robertus E. Grant, *Dissertatio Physiologica Inauguralis, de Circuitu Sanguinis in Foetu* (Edinburgh, 1814), p.8

[44] Robert Edmond Grant, *Tabular View of the Primary Divisions of the Animal Kingdom, Intended to Serve as an Outline of an Elementary Course of Recent Zoology* (London, 1861), p.v

[45] L.S. Jacyna, *Philosophic Whigs: Medicine, Science and Citizenship in Edinburgh, 1789–1848* (London, 1994)

to Lamarck … he never hinted at the possibility of transformism'.[46] For evidence of the open discussion of the transformist theories of Lamarck and his contemporaries we have to turn to the natural history circles surrounding Robert Jameson, Edinburgh's professor of natural history.

Although he would have been aware of older, eighteenth-century varieties of transformism, which he would have learned about in John Walker's lectures, there is no evidence that Jameson knew about the work of Lamarck until around 1813. His preface to the first English edition of Cuvier's *Discours sur les Révolutions de la Surface du Globe*, published under the title of *Essay on the Theory of the Earth* in that year, contained an explicit reference to Lamarck. Here, Jameson stated that:

> Some naturalists, as La Mark [sic], having maintained that the present existing races of quadrupeds are mere modifications or varieties of these ancient races which we now find in a fossil state, modifications which may have been produced by change of climate, and other local circumstances, and since brought to the present great difference by the operation of similar causes during a long succession of ages, – Cuvier shews that the difference between the fossil species and those which now exist, is bounded by certain limits; that these limits are a great deal more extensive than those which now distinguish the varieties of the same species; and, consequently, that the extinct species of quadrupeds are not varieties of the presently existing ones. [47]

It seems likely that Jameson's acquaintance with Lamarck's thought at this time was relatively superficial and probably at second hand for two reasons. The first is his doubly incorrect spelling of Lamarck's name, which would seem unlikely if Jameson had been well acquainted with his works. The second is his apparently limited knowledge of Lamarck's theory. The quotation above argues that fossil forms cannot be the ancestors of existing animals because they are too different to be varieties of the modern species. In doing this Jameson credited Lamarck only with the idea that fossil animals are 'mere modifications or varieties of these ancient

[46] Ibid., p.138
47 Robert Jameson, 'Preface', in Cuvier, *Theory of the Earth*, 1st ed., p.viii.

races'. In consequence, the argument presented by Jameson turned solely on whether the modern forms could or could not be varieties of the fossil animals. As we have noted above in relation to John Walker's lectures, the production of new varieties through the influence of the environment was relatively uncontroversial in this period. For Jameson, the 'present great difference' between living and fossil forms clinched the argument that these were not varieties of the same species. The possibility that one species could have transmuted into another completely distinct species was not even addressed. His argument curiously neglects Lamarck's belief that transmutation was possible between rather than just within species, and indeed that this was central to his theory: a curious omission if Jameson was thoroughly familiar with Lamarck's work at this time. It would be easy to conclude on the basis of this passage that Jameson had entirely missed the true import of Lamarck's theory. It is, of course, conceivable that Jameson was simply making rather loose use of the term 'variety', but this seems unlikely given the clear distinction made by Walker.

In the same work in which Jameson dismissed Lamarck's transformism can be found other speculations on the transmutation of species from an unexpected source, which Jameson can hardly have been ignorant of. Cuvier, the author of the *Essay on the Theory of the Earth*, is himself generally regarded as a fierce opponent of transformism. However, R. Hooykaas has argued that Cuvier 'was not so dogmatic in his biological anti-transmutationism as he is generally believed to have been.'[48] Hooykaas based this argument on evidence that Cuvier's objection to transformism rested largely on the empirical grounds that no changes could be observed in animals during historical time and on the apparent absence of intermediate forms in the fossil record. This holds true for land animals; however, Cuvier seems to have allowed for the possibility that marine organisms had undergone transmutations brought about by changes in the properties of the medium in which they lived.

[48] R Hooykaas, *Natural Law and Divine Miracle: The Principle of Uniformity in Geology, Biology and Theology* (Leiden, 1963), p.71

There is striking evidence for this in Jameson's translation of the fifth edition of *Discours sur les Révolutions de la Surface du Globe* (1827), where, closely following the original French text, it is noted that: 'There has, therefore, been a succession of variations in the economy of organic nature, which has been occasioned by those of the fluid in which the animals lived, or which at least corresponded with them; and these variations have gradually conducted the classes of aquatic animals to their present state'.[49] This passage is much less ambiguous in its statement that the changes in marine animals are likely to have been brought about by change in the environment than its counterpart in the previous edition, which read: 'In animal nature, therefore, there has been a succession of changes corresponding to those which have taken place in the chemical nature of the fluid; and when the sea last receded from our continent, its inhabitants were not very different from those which it still continues to support.'[50] As Jameson was responsible for the revised translation, the original translator, Robert Kerr, having died in 1813, he is unlikely to have missed the significance of this passage. Ironically, despite his violent hostility to Lamarck's theories, Cuvier may have indirectly raised the possibility of the transmutation of species in some minds, at least in regard to marine organisms.

Among Jameson's papers, held at the library of the University of Edinburgh, there are to be found some clues as to how and when Jameson may have first come into direct contact with Lamarck's transformist theories. Two receipts survive there for volumes of Lamarck's *Histoire Naturelle des Animaux sans Vertèbres* from Treuttel & Co of London, dated 25 May 1822 and 11 April 1823.[51] We therefore know that

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[49] Georges Cuvier, *Essay on the Theory of the Earth* (trans. Robert Jameson) (Edinburgh, 1827), p.12. The original French texts reads: 'Il y a donc eu dans la nature une succession de variations que ont été occasionnées par celles du liquide dans lequel les animaux vivaient ou que du moins leur ont correspondu ; et ces variations ont conduit par degrés les classes des animaux aquatiques à leur état actuel', Georges Cuvier, *Discours sur les Révolutions de la Surface du Globe* (Paris, 1825), p.14

[50] Cuvier, *Theory of the Earth*, 4th ed., p.13. This paragraph was also identical in the previous two editions.

[51] Treuttel & Co receipts, Jameson papers, University of Edinburgh library (Gen.130). Jonathan R. Topham has identified Treuttel & Co has one of the most important importers of

Jameson certainly owned a copy of this work, which devoted almost its entire first volume to a detailed exposition of Lamarck's theory of the transmutation of species. According to remarks by George Johnston on John Fleming's *History of British Animals* (1828), published in the *Edinburgh New Philosophical Journal* in 1828, Lamarck's *Histoire Naturelle* was 'in general use among naturalists in this country; and it is necessary that the student should be acquainted with its language or synonymes, whether he may choose to adopt them or not.'[52] Jameson was therefore not alone in appreciating the value of this work. Remarkably, a handwritten translation of parts of it into English in the handwriting of Jameson's assistant at this period, William MacGillivray, also survives among Jameson's papers.[53] The paper this is written on is watermarked 1821, so it probably does not long post-date the purchase of the book. It is certainly not likely that it was written after August 1831, when MacGillivray succeeded Robert Knox as conservator of the museum of the Royal College of Surgeons of Edinburgh. [54] The translation was made from the first French edition (1815–22), as a page reference to a 'table of articulated and inarticulated animals' on page 457 in Chapter XII (f.5) refers to the pagination of that edition, which differs from that of the second edition. We can safely conclude that Jameson bought his copy soon after the publication of the final volume. The first folio begins mid-sentence with material from page 400 of volume 1, from which it is evident that part of the original manuscript has been lost. It is not possible to determine whether the entire book was ever translated, as each chapter has been paginated separately in the copy. What is apparent from the pagination, however, is that there are a number of lacunae in the manuscript. From the surviving sections,

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continental books into Britain in the years following the Napoleonic Wars. See Jonathan R. Topham, 'Science, print and crossing borders: Importing French science books into Britain, 1789–1815, in David N. Livingstone and Charles W.J. Withers, *Geographies of Nineteenth-Century Science* (Chicago, 2011), p.313

[52] George Johnston, 'A few remarks on the class Mollusca, in Dr Fleming's work on British animals; with descriptions of some new species', *Edinburgh New Philosophical Journal* 5 (1828), p.75

[53] Fragments of a translation of Jean-Baptiste Lamarck, *Histoire Naturelle des Animaux sans Vertèbres*, vols 1–6.1 (1815–22), Jameson papers (University of Edinburgh Library Gen.124)

[54] Isobel Rae, *Knox: The Anatomist* (Edinburgh, 1964), p.109

we can, however, say that it included at least parts of volume 1 to volume 6, part 1. Despite its fragmentary nature, the existence of this translation is conclusive evidence for Jameson's interest in the work of Lamarck in the early 1820s.

As mentioned in chapter 3, an anonymous Lamarckian paper entitled 'Observations on the nature and importance of geology' appeared in the *Edinburgh New Philosophical Journal* in 1826. This paper was long considered to be by Robert Grant, but as I have argued in an earlier chapter, it was probably written by Ami Boué, the former student and disciple of Jameson, if not by Jameson himself. This paper is particularly significant in that, as Secord has pointed out, it is was the 'earliest favorable reaction to Lamarck in a British scientific periodical.'[55] What did this important paper have to say about the theories of Lamarck? First the author observed that, in the past, naturalists had attempted 'to arrange the species of animals, sometimes according to a scale of gradation, and sometimes according to a reticulated form, without giving any distinct account of the meaning of such an arrangement.'[56] The question then, was to give meaning to the relationships that had been observed between the different species of animals. He then introduced the theory of Lamarck, 'one of the most sagacious naturalists of our day', as a possible answer to this problem.[57] Then followed a brief account of Lamarck's views on the spontaneous generation of infusory animals and simple worms and the evolution of all existing animals from these first primitive forms under the influence of external circumstances.[58] Later in the article the author went on to introduce the evidence of domestic animals and cultivated plants in support of Lamarck's theory, and against the supposed immutability of species.[59] He then sounded a note of caution, before

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[55] Secord, 'Edinburgh Lamarckians', p.1
[56] Anon, 'Nature and importance of geology', p.296
[57] Ibid., p.296
[58] It is worthy of note that Jameson himself used the term 'evolved from' here to describe the process by which new species are derived from pre-existing ones. This is a very early use of this term in its modern sense.
[59] Anon, 'Nature and importance of geology', p.298

concluding that Lamarck's ideas offered the best explanation of the relations between the species of animals currently available:

> Although it should not be forgotten, that this meritorious philosopher, more in conformity with his own hypothesis than is permitted in the province of physical science, has resigned himself to the influence of imagination, and attempted explanations, which, from the present state of our knowledge, we are incapable of giving, we nevertheless feel ourselves drawn towards it, as these notions of the progressive formation of the organic world, must be found more worthy of its first Great Author than the limited conceptions that we commonly entertain.[60]

It might be expected that the members of the Plinian Natural History Society, many of whom had already espoused the materialist ideas found in the minutes of the Society, would also have been drawn to the theories of Lamarck. Robert Grant, the most openly transformist thinker in Edinburgh, after all, was an active member in the mid-1820s, and there is certainly substantial evidence from other sources that Lamarck's theories were read and discussed in Edinburgh at the time. However, except for two references to the purchase of his *Histoire Naturelle des Animaux Sans Vertèbres* for the society's library, where it was to be found from the summer of 1827, there are no direct references to Lamarck in the Society's minutes or proceedings.[61] We do have one piece of evidence from the minute books of the Society that the subject of transformism was not unknown to its members. From the minutes for the meeting of 18 December 1827 comes the only direct reference to transformism to be found in the records of the Society. Here it is noted that William A.F. Browne 'remarks on the question whether any change might be produced on vegetables sufficient to constitute new genera and species' in the course of a paper on the botany of the Edinburgh area.[62] The reading of the paper was followed by a discussion involving Browne, one of the Baird brothers, Henry Cheek, Edward Binns and Allen Thomson. Unfortunately, the record of this paper is absent from the

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[60] Ibid., p.297
[61] Minutes of the Plinian Society, vol. 1, f.57.
[62] Ibid., f.85 verso.

published Proceedings of the Society for 18 December, which might have provided further details. Although the wording of the minutes is rather vague, it is at least evident that the transmutation of species was a topic familiar to members of the Society.

Significantly more positive evidence for the reception of Lamarck's theories can be gleaned from sources relating to some of the lecturers who taught in Edinburgh's extra-mural medical schools in the 1820s and 1830s.The extra-mural teacher for whose transformist views we have the most evidence is Robert Grant. Despite not making explicit reference to Lamarck in his published work, we know that Grant was sympathetic to Lamarck's transformist theories not only from his own open espousal of transformism but also from the testimony of Charles Darwin. In his autobiography Darwin, who got to know Grant well in his time at the University of Edinburgh, recalled how they used to go on invertebrate-collecting trips together on the Firth of Forth. He recounted how one day while they were on such a collecting trip, probably in late 1826 or early 1827, Grant 'burst forth in admiration of Lamarck and his views on evolution.'[63] I will be exploring how Grant developed his own transformist theories in detail in the next chapter.

Not all followers of the new philosophical anatomy in the extra-mural schools were as enthusiastic about Lamarck's theories as Grant. According to Philip F. Rehbock, Robert Knox 'did not feel comfortable with Lamarck's or Geoffroy's views of evolutionary descent'.[64] This may be true as far as it goes, but Evelleen Richards has demonstrated that, while Knox might not have subscribed to Lamarckian transformism, he seems to have had his own rather eccentric theory of organic descent, which will be explored in chapter 5.[65] His belief in the transmutation of species and his rejection of Lamarckian transformism were both acknowledged by Baden Powell in the mid-1850s when he wrote that Knox, 'one of the most zealous

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[63] Darwin, *Autobiographies*, p.24
[64] Rehbock, *The Philosophical Naturalists*, p.50.
[65] Richards, '"Moral Anatomy" of Robert Knox', p.399

supporters of the principle of transmutation in this country, speaks very slightingly of Lamarck'.[66] And indeed he did; in an article published in *The Lancet* in 1855 Knox gave the opinion that 'The wild conjectures of Le Methrie [sic] and Lamarck were written in a style of romance, excluding them from the sober field of science.'[67] As we will see, rejection of Lamarck, however, need not imply an absolute rejection of transformism.

The hostility of Knox to the theories of Lamarck still, however, needs to be explained. There are two possible reasons for his response. Firstly, Knox was profoundly hostile to the notion of progress in the natural world, and the place of man at the pinnacle of creation. He looked forward to a time when the 'boast about the higher characters of the present organic races will be abandoned, and the law of development and progress simply stated as it is, without a reference to successive *improvement*; for *successive improvement* implies a final purpose'.[68] Later in the same book he went on to write, in his usual acerbic manner, that the 'world, for countless thousands of years, was inhabited only by fishes; could they have spoken, and left us records, we should have found, no doubt, that they considered themselves as the most perfect of all Nature's works, and the beings for whom the seas, at least, if not the dry land, had been made.'[69] It seems that Lamarck's rather triumphalist account of the ascent of man may have sat rather ill with Knox's distrust of theories of progressive development. Knox's second problem with the theory of Lamarck was that it made use of the inheritance of acquired characteristics, the possibility of which Knox expressly denied. This denial that over time species could be modified by their environments was linked to his belief in the unshakable permanence of human races, which came to preoccupy him more and more in later decades. In *Great Artists and Great Anatomists* he dismisses Lamarck's mechanism for the

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[66] Baden Powell, *Essays on the Spirit of the Inductive Philosophy, the Unity of Worlds, and the Philosophy of Creation* (London, 1855), p.395
[67] Robert Knox, 'Introduction to Enquiries into the Philosophy of Zoology', *The Lancet* 7: 1 (1855), p.625
[68] Ibid., pp.190–1
[69] Ibid., p.369

transmutation of species in the following words: 'Lamarck's idea was that organization was the result of function, and not function the necessary result of form; that an animal was aquatic, not by the nature of its organs but became so, acquiring a fitting organization by its being forced to live in water. This view was wholly theoretical and met with no respect.'[70] Knox's choice of words here is intriguing. To say that Lamarck's ideas 'met with no respect' is by no means the same as saying they were unworthy of respect. As is often the case with Knox's writings, his true opinions are often veiled behind a highly rhetorical, deliberately provocative style, with frequent apparent inconsistencies and contradictions. Having said that, it is clear that Lamarck's particular brand of transformism did not meet with his approval. However, as we will see in the next chapter, Knox's dismissal of Lamarckian transformism should not be taken to imply that he was a confirmed opponent of transmutation *per se*.

Interest in Lamarck's theories among Edinburgh's extra-mural lecturers was not confined to Grant and Knox. John Fletcher also wrote a detailed critique of Lamarck's theories in his *Rudiments of Physiology* (1835–7). The picture painted by Fletcher is the skewed one found in the writings of many of Lamarck's critics, which may ultimately be traced back to Julien-Joseph Virey's influential *Nouveau Dictionnaire d'Histoire Naturelle* (second edition 1816–19) and Cuvier's mean-spirited eulogy to Lamarck, published in the *Edinburgh New Philosophical Journal* in 1836. Corsi has established that other British natural historians had been misled by Virey's account of Lamarck's theory.[71] Fletcher wrote that Lamarck 'traces all tribes of animals to the lowest zoophyte, and ascribes all the differences which they now display entirely to the different instincts which they have experienced, and the different efforts which they have severally made to gratify them'.[72] In common with many other critics of Lamarck, Fletcher's critique relied on an incomplete and

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[70] Robert Knox, *Great Artists and Great Anatomists: A Biographical and Philosophical Study* (London, 1852), p.72
[71] Corsi, *The Age of Lamarck*, p.177
[72] John Fletcher, *Rudiments of Physiology, in Three Parts*, vol. 1 (Edinburgh, 1835), p.12

inaccurate account of his theories. To cite another example of the superficiality of Fletcher's critique, he classed Lamarck as among 'those who conceive that every form of organized matter consists of a congeries of monads or organic molecules of precisely the same nature, and competent therefore to enter into the composition of any organized being'.[73] According to Fletcher, Lamarck shared this view with Turberville Needham, Buffon, Gottfried Reinhold Treviranus and Friedrich Tiedemann, among others. Of these figures, perhaps the one whose ideas are most closely represented by Fletcher's generalisation is Buffon, but the concept of identical 'organic molecules' plays no part in the theories of Lamarck. Although Fleming's own religious affiliations are unknown, like many of the evangelical critics of Lamarck whose opinions I will be examining in the next section his rejection of Lamarck seems to have been determined to some extent by his religious convictions, as is made clear from this following passage from one of the footnotes to the *Rudiments of Physiology* :

> The study of nature is the study of God's nature; and it is only they who have stopped on the threshold of this study, and have let in only light enough to render darkness visible, or who are evidently wrong-headed – and such men have existed from Epicurus to Lamarck – that have indulged in those flippant and irreverent remarks, the object of which is to shake our faith in truths which it must distress us to doubt, and wither us to disbelieve.[74]

However Fletcher's religious arguments are rather different from those of the evangelical critics of Lamarck and do not bear the distinctive stamp of evangelical theological preoccupations. They appear rather to be of a more moderate, natural-theological character.

[73] Ibid., p.12
[74] Ibid., pp.64–5

Evangelical critiques of Lamarck

During the first half of the nineteenth century Edinburgh was home to an extraordinary group of individuals belonging to the Evangelical Party of the Church of Scotland who took a strong interest in science and natural history.[75] These included such figures as the minister and natural historian John Fleming, the natural philosopher David Brewster, the minister and reformer Thomas Chalmers and the geologist and journalist Hugh Miller. Their religious views had a strong influence on their reactions to the theories of Lamarck, so I will be devoting this separate section to their critiques of his ideas, as well as the opinions of others who shared their evangelical perspective. The Evangelical Party had been in competition with the Moderate Party for the soul of the established Church of Scotland since far back in the previous century.[76] However, during the first few decades of the nineteenth century the Evangelicals started to gain significant ground over the previously dominant Moderates. It was in May 1834 that the Evangelicals finally wrested control of the general assembly of the Church from their opponents.[77] Thus began the 'Ten Year's Conflict' between the two parties, which centred on the right of local landowners to impose ministers on congregations against the will of the parishioners, a right fiercely opposed by the Evangelicals, but upheld by the courts. This was to lead to the Disruption of 1843, in which the majority of Evangelical ministers, around 40 percent of the total, left the Church to found the Free Church of Scotland. As we will see, the growing assertiveness of the Evangelical Party seems to have been paralleled by the growing readiness among those of its members with scientific interests to oppose transformist theories, which they found objectionable from both a scientific and religious standpoint. In 1845, in the aftermath of the Disruption, the Free Church established a chair of natural history at New College in

[75] For a more detailed study of this group of scientific Evangelicals, see Paul Baxter, 'Deism and development: Disruptive forces in Scottish natural theology', in Stewart J. Brown and Michael Fry (eds), *Scotland in the Age of the Disruption* (Edinburgh, 1993), pp.98–112

[76] To differentiate members of the Evangelical Party from other evangelicals who shared many of their theological perspectives, but may not have been members of the Evangelical Party as such, I will give Evangelical an initial capital letter when referring to the Evangelical Party, but not when discussing evangelical Christians more generally.

[77] Stewart J. Brown, *Thomas Chalmers and the Godly Commonwealth* (Oxford, 1982),p.233

Edinburgh, its newly created Edinburgh theological college, in order to equip the new generation of Free Church ministers for the struggle for the soul of science. This chair was awarded to the Evangelical minister, zoologist and former professor of natural philosophy at the University of Aberdeen, John Fleming. David Brewster observed with some satisfaction that the purpose of this new chair was 'to give such a complete course of geology and natural history, that the student will find himself armed at all points' for combat against the doctrines of transformism and natural law.[78]

Evangelical theology was seen as being incompatible with transformism for two main reasons. Firstly, evangelicals generally viewed the world as fundamentally corrupted and depraved as a result of the Fall. Many evangelicals therefore came to deny any possibility of progress, whether in the natural world or in human society, other than personal spiritual progress, or the amelioration of society that they believed resulted from it.[79] Secondly, transformism seemed to suggest that God was content to rely on secondary causes in his governance of the world and appeared to deny him the power to intervene in the natural world by supernatural means. This conflicted with the evangelical emphasis on the absolute power of God. Although these were not the only theological arguments against transformism, they were the ones most generally emphasised by evangelical critics. These two theological arguments came very much to the fore in the evangelical attacks on Robert Chambers' transformist *Vestiges of the Natural History of Creation* (1844), the reception of which is sadly beyond the scope of this study, but similar considerations were certainly behind many of the earlier evangelical reactions to transformism.[80] While some of the figures I will be discussing in this section were prominent members of the Evangelical Party of the Church of Scotland, such as Hugh Miller and John

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[78] David Brewster, Review of [Robert Chambers], *Vestiges of the Natural History of Creation*, *North British Review*, 3: 6 (1845), p.506

[79] See, for example, William Scott, *The Harmony of Phrenology with Scripture: Shewn in a Refutation of the Philosophical Errors Contained in Mr Combe's 'Constitution of Man'* (Edinburgh, 1836)

[80] For an excellent account of evangelical reactions to *Vestiges* see Secord, *Victorian Sensation*

Fleming, the exact religious affiliations of others, notably John Stark, have proved more difficult to establish. When this is the case, I have included their arguments in this section where these are clearly grounded in characteristically evangelical theological concerns, even if the writers may not have been associated with the Evangelical Party, or may have belonged to churches other than the established Church of Scotland.

An early Evangelical response to Lamarck comes from the *Memoirs of the Wernerian Society* and constitutes the only explicit reference to Lamarck to be found there. This was contained in a paper by the Evangelical minister James Grierson (1791–1875) given to the Society in February 1824. Here Grierson made reference to 'The original or infinitely small monadic animals and vegetables, Lamarck, and others, who hold the same system, tell us, gradually acquired different habits, became larger and more diverse from one another; and hence all the animals and vegetables we have now.'[81] He then goes on to dismiss Lamarck's ideas 'which, if they do not evince much power of observation, or great accuracy of deduction, certainly shew no deficiency in power of fancy.' While Grierson was clearly no follower of Lamarck, his remarks at least demonstrate that his theories were known and discussed by members of the Society. Lamarck's theories were dismissed by Grierson principally on the grounds of being fanciful speculation and therefore bad science, with no indication that any political or religious anxieties were aroused by the implications of his theories.

John Fleming the prominent natural historian and Evangelical minister has left us considerable evidence in print of his thoughts on Lamarck's theories. Along with Jameson, Fleming was a founder member of the Wernerian Natural History Society.

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[81] James Grierson, 'General observations on geology and geognosy, and the nature of these respective studies', *Memoirs of the Wernerian Natural History Society* 5 (1823–24), p.404

There is good evidence that he knew Jameson well.[82] Jameson joined with John Playfair and David Brewster in successfully proposing Fleming as a fellow of the Royal Society of Edinburgh in 1814. In the mid-1820s Fleming became involved in a lively polemic with William Buckland, reader in geology at the University of Oxford.[83] Fleming took great exception to Buckland's views on the supposed geological evidence for the Biblical Deluge and his catastrophist interpretation of the history of the Earth, as laid out in his *Reliquiæ Diluvianæ* (1823). In 1826 Fleming published a provocative article in the *Edinburgh Philosophical Journal* entitled 'The Geological Deluge, as interpreted by Baron Cuvier and Professor Buckland inconsistent with the testimony of Moses and the phenomena of nature', which caused a storm of controversy.[84] However, in spite of the shadow cast over his career by this episode he was to go on to be appointed professor of natural philosophy at University and King's College, Aberdeen in 1834. As the leading natural historian of the Evangelical party of the Church of Scotland, after the Disruption he was the natural choice for the newly established chair of natural history at the Free Church college in Edinburgh in 1845.

As Pietro Corsi has made clear in a paper published in 1978 on the importance of Lamarck's work for Lyell's *Principles of* Geology, Fleming shows himself to be surprisingly sympathetic to Lamarck's theories in his writings, while not necessarily sharing his conclusions.[85] In April 1820 a review of Lamarck's newly published *Histoire Naturelle des Animaux sans Vertèbres* by Fleming appeared in the *Edinburgh Review*. He started by praising Lamarck, recognising that 'His writings, which are

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[82] D. T. Moore, 'Fleming, John (1785–1857)', *Oxford Dictionary of National Biography*, Oxford University Press, 2004; online ed., Oct 2009 [http://www.oxforddnb.com/view/article/9705, accessed 5 July 2012]
[83] James Burns, 'John Fleming and the geological deluge', *British Journal for the History of Science* (2007) 40: 2, pp.205–25
[84] John Fleming, 'The Geological Deluge, as interpreted by Baron Cuvier and Professor Buckland inconsistent with the testimony of Moses and the phenomena of nature', *Edinburgh Philosophical Journal* (1826), 14, pp.204–39
[85] Pietro Corsi, 'The importance of French transformist ideas for the second volume of Lyell's *Principles of Geology*', *The British Journal for the History of Science*, 11: 3 (1978), pp.222–4

now voluminous, are generally characterized by the research and ingenuity of his speculations, and by the clear and perspicuous language in which he has embodied them', but he went on to add that 'they also betray a decided propensity to generalize on assumed or deceptive premises, and they are all, more or less, tinctured with the influence of a few leading and favourite doctrines, which seldom rest on very stable foundations.'[86] It soon emerges that the key doctrine that Fleming objected to was Lamarck's materialism. In particular, jumping to the fourth section of the introduction to Lamarck's book, he took issue with his materialist explanation of mental phenomena. He criticised Lamarck for his supposed advocacy of the principle that the faculties of the mind are localised in particular areas of the brain, noting that 'we find him confidently asserting the doctrine, that every mental faculty has its appropriate organ, without which it cannot exist: but the nature of the union of matter and spirit in our own constitution is too mysterious to enable us implicitly to adopt any such proposition.'[87] It soon becomes apparent that Fleming's profound hostility to these ideas was rooted principally in his religious convictions. According to Fleming, Lamarck's ideas were 'subversive of those sublime and consoling views of religion which teach us, that mind may exist and act independently of matter altogether, and may be combined with it, or detached from it, at the will of the Sovereign ruler.'[88] As evidence that the mind is not a product of the material structure of the human brain, Fleming notes that 'the intellect, for example, has been observed to continue unimpaired, when a large portion of the brain has been obliterated or removed'.[89]

At this point Fleming seemed to be declining to turn from Lamarck's theory of the mind to address the transformist theories that Lamarck builds on the foundations of his materialist premises. Instead he stated that 'impressed as we are by the

[86] [John Fleming], Review of Jean-Baptiste Lamark, *Histoire Naturelle des Animaux sans Vertèbre*s, *Edinburgh Review* 3:4 (April 1820), pp.403–4
[87] Ibid., p.406
[88] Ibid., pp.406–7. The emphasis on divine power here is typical of evangelical critics.
[89] Ibid., pp.406

conviction, that the authors premises are often extravagant, or erroneous, we are the less solicitous to accompany him, step by step, in this preliminary dissertation.'[90] But then, despite his avowed hesitancy, he did in fact go on to launch the following concerted assault on Lamarck's transformism:

> Even thought and imagination are represented as mere physical appearances; and new organs are formed, by mechanical means, *in consequence of a strong feeling of their need*, of the performance of the functions to which they are destined. But does a physical feeling create a physical organ? or, if this strong feeling is not physical, then are not *all* the phenomena of mind physical? Be it, however, what it may, when did the most intense wish, or feeling, of any human being, generate an additional organ to his original frame? or when did the most ardent desire of an unfortunate culprit to fly from the pursuit of justice, furnish him with wings? Farther, the existence of an ascending scale, from the more simple to the more complex animal structures does not necessarily imply, that the different tribes of living creatures were successively produced in that order, or that nature was compelled to limit her efforts to the scanty and imperfect, before she could progressively advance to the more ample and finished forms.[91]

It is interesting to note that, like Fletcher's argument discussed above, Fleming's misrepresentation of Lamarck's theory as implying that animals in some sense will themselves to evolve is identical to the argument that Cuvier was to use to damn Lamarck's ideas in his notorious eulogy to him in 1832, later published in translation in the *Edinburgh New Philosophical Journal*.[92] As Lamarck was still alive in 1820, Cuvier cannot have been his source, and Fleming may instead have picked up this interpretation from the second edition of Julien-Joseph Virey's *Nouveau dictionnaire d'histoire naturelle* (1816–19). Fleming's criticism of Lamarck's transformism in 1820 was not based principally on religious principles, or its lack of conformity with the Mosaic account of creation, but rather on a sustained critique from a scientific perspective of Lamarck's mechanism for the transmutation of species and his vision of the history of life, based on Fleming's own rather skewed

[90] [Fleming], Review of Lamarck, *Animaux sans Vertèbre*s, p.408
[91] Ibid., pp.409–10
[92] Cuvier, 'Memoir of M. de Lamarck', pp.14–15

interpretation of his theories. When he did attack Lamarck's theory as incompatible with true religion earlier in his review it is clear is that he was criticising Lamarck's materialistic theory of the mind, not his transformism. His main concern there was to defend the existence of the vital principle against Lamarck's materialism on religious grounds, a preoccupation he shared with Barclay. Finally, it is important to note that in this review Fleming's appraisal of Lamarck's work was far from being altogether negative. He does not seem to consider the ideas contained in it as dangerous, or counsel his audience against reading the book. On the contrary, he concludes that this is 'a work, which, for all its defects, promises to hold a distinguished station in the library of the zoologist, and to impart both an impulse and facility to the study of the various tribes of beings of which it treats.'[93]

Two years after the publication of his review of the *Histoire Naturelle* Fleming published his *Philosophy of Zoology* (1822), the title of which strangely echoes that of Lamarck's *Philosophie Zoologique* of 1809. Once again, we find Fleming concerned to defend the existence of an immaterial vital principle against Lamarck's conception of life as a consequence of the organisation of matter. For Fleming as for Barclay, the vital principle consisted of an immaterial essence which gave life to inanimate matter, a view fundamentally opposed to Lamarck's theory, which made life a result of the action of the same laws of nature that applied to non-living matter. In chapter 2 of his book, 'On the peculiar characters of organized bodies', Fleming remarked that in his *Histoire Naturelle des Animaux sans Vertèbres* Lamarck 'refers some of the movements which are here considered as indicating the existence of irritability in plants, to the influence of the mechanical or chemical powers'; however, in Fleming's opinion 'these different actions ... occur in connection with the vital principle, and their entire dependence on the laws of inorganic matter is a gratuitous assumption.'[94] In opposition to Lamarck's materialist vision of life, Fleming then went on to expound his own vitalist theory of the 'Living or Vital

[93] [Fleming], Review of Lamark, *Animaux sans Vertèbre*s, p.418
[94] Ibid., p.14

Principle', which, 'so far as appears to our senses, can only reside in organized bodies. For The connection is temporary, and may be dissolved by various circumstances and it is capable of being divided or multiplied by the process of generation.'[95] It is responsible for the generation and development of the organism and 'in the formation of an organized body, acts in direct opposition to the laws of chemistry or mechanics.' [96] For Fleming the vital principle seemed to correspond to the essence of a species in an Aristotelian sense; as a consequence there were 'as many different kinds of vital principles, as there are species in nature.'[97]

In the same chapter Fleming devoted three pages to a detailed discussion of the relationship between the species of animals alive today and those found preserved in the fossil record, although without making explicit reference to Lamarck's theory of transformism. He noted that the discoveries of contemporary geologists indicated that 'the organic remains found in the older rocks differ from those which occur in the more recent strata, and that they are all different from the plants and animals which now exist on the surface of the globe'.[98] Without saying who proposed the idea, although it can reasonably be conjectured that the theories he was discussing were those of Lamarck, he noted that some had suggested that fossil organisms were the ancestors of those now alive on the planet, having been modified by the effects of their changing environments. He went on to concede that the evidence from the domestication of animals and plants and from the study of the races of men could be seen to support this supposition. His counter argument was similar to that noted above from Jameson's preface to Cuvier's *Theory of the Earth*, with the crucial exception that here Fleming did make explicit reference to the transmutation of species:

[95] Ibid., pp.22–3
[96] Ibid., p.39
[97] Ibid., pp.23
[98] Ibid., p.26

> The effect of circumstances on the appearance of living beings, is circumscribed within certain limits, so that no transmutation of species was ever ascertained to take place; – and it is well known, that the fossil species differ as much, nay more, from the recent kinds, as these last do from one another. It remains, likewise, for the abettors of this opinion, to connect the extinct with the living races, by ascertaining the intermediate links or transitions. This task, we fear, will not be executed speedily.[99]

His opinion that there were strictly circumscribed limits to the changes which could be brought about by the environment was reinforced in his discussion of varieties in volume 2 of the book, where he stated that 'the vital principle of every animal is restrained, in all its operations, within certain limits, peculiar to each species'.[100] The implication that the task of tracing the connections between extinct and living forms could be completed successfully at all was an astonishing concession from a natural historian who seemed otherwise to be firmly opposed to transformism. As noted above, Fleming was even prepared to concede that there was some concrete evidence in its favour. He then went on to consider an even more surprising possibility that:

> If the seeds of some plants, and the eggs of certain animals, be so minute as to be excluded with difficulty from any place to which air and water have access, and if they are capable of retaining, for an indefinite length of time, the vital principle, when circumstances are not favourable to its evolution, the crust of the earth may be considered as a mere receptacle of germs, each of which is ready to expand into vegetable or animal forms, upon the occurrence of those conditions necessary to its growth.[101]

This would seem to be an idea closely akin to Buffon's theory of 'organic molecules', which Fleming was no doubt familiar with. He doubted, however, if this explanation would be viable for the higher animals, such as mammoths, which seemed to have appeared and then disappeared from the face of the earth over geological time. For mammals, which depend entirely on their mothers for the first

[99] Ibid., p.27
[100] Fleming, *Philosophy of Zoology*, vol. 2, p.150
[101] Fleming, *Philosophy of Zoology*, vol. 1, p.27

part of their development, this presented obvious practical problems. Fleming's treatment of the subject concluded with a series of questions, which not only leave the problem of explaining the fossil record unresolved, but give an impression of Fleming as surprisingly open minded on the subject of the history of life on earth. He asked: 'Is the generation of organized beings simultaneous or successive? Have they all been created at once; but, in the progress of time, so modified by the influence of external agents, as now to appear under different forms? Or have they been called into being at different periods, according as the state of the earth become suitable for their reception.'[102] He therefore seems to be prepared to at least consider both a version of transformism driven by environmental change similar to the one proposed by Geoffroy and one closer to Buffon's model in which species were summoned into existence by a changing environment, as expressed in his *Époques de la Nature*. In this major work of the early 1820s Fleming seemed prepared to coolly consider arguments which it might be expected an Evangelical natural historian would have dismissed out of hand, if viewed in the light of the Evangelical reaction to Chamber's *Vestiges* twenty years later.

In 1829 Fleming again addressed the theories of Lamarck in a review of J.E. Bichino's *Systems and Methods in Natural History* (1827), published in the *Quarterly Review*. In this review Fleming's views seem to have subtly hardened against transformism. After giving a lengthy account of Lamarck's theories, Fleming introduced a number of arguments against them. Firstly, he argued that Lamarck introduced needless complication to the story of life, asking why God could not have created 'Man directly, as easily as a Monas'.[103] Evincing a typically evangelical hostility to the possibility of progress, he then claimed that the fossil record did not present a picture of progressive development, but that in fact the remains of 'zoophytes and mollusca, along with the bones of *vertebrated animals*, and the stems of *dicotyledonous plants*' could all be found in rocks from every geological epoch

[102] Ibid., p.28
[103] [John Fleming], Review of J.E. Bichino, *On Systems and Methods in Natural History*, *The Quarterly Review* 42 (Nov 1829), p.320

where there was evidence of life.[104] His position on the history of life therefore seems to have changed significantly since he wrote his *Philosophy of Zoology*, where he noted that the fossil evidence seemed to suggest that a succession of living forms had inhabited the earth over geological time. This move towards a denial of progress in the geological record was to become familiar from the evangelical critiques of Chambers' *Vestiges* in the mid-1840s.[105] As we will see, this evangelical argument against progressive development was also to be employed by other critics of Lamarck in the course of the 1830s. While Fleming acknowledges that Lamarck had 'succeeded in making some converts', including several respected naturalists who he did not name, he ultimately concluded that 'the whole scheme, as an exposition of the plan of procedure, is so obviously a dream of the imagination, that one may well be surprised to find it occupying a place in the records of science.'[106]

To sum up Fleming's critique of transformism, it must first be noted that in the early 1820s he was prepared to engage constructively with transformist theories rather than condemn them out of hand, especially in his *Philosophy of Zoology*. He not only seemed prepared to consider the possibility that there was some merit in transformist ideas, but conceded that they had some evidence in their favour. It is also worthy of comment that his criticisms of transformism itself, as opposed to materialism, were based not on religious, moral or political criteria, but on the grounds of scientific plausibility and lack of fit with empirical evidence. Although he came down against the transformism of Lamarck, he clearly had immense respect for his work as a zoologist and was very far from condemning him as a dangerous radical. This relatively tolerant attitude seems to have given way to a more inflexible attitude by the end of the 1820s. Fleming's moderate stance on Lamarckism during the 1820s is entirely consonant with his friendship with Robert Grant, the most openly transformist of Edinburgh natural historians, as we saw in

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104 Ibid., p.321
105 See, for example, Hugh Miller, *Foot-Prints of the Creator: or the Asterolepis of Stromness* (London, 1849)
106 [Fleming], Review of J.E. Bichino, pp.320–1

chapter 3. Later critiques of Lamarck from evangelical sources in the 1830s and 1840s would become increasingly hostile and put more emphasis on religious as opposed to scientific arguments.

Another Edinburgh-based natural historian, member of the Wernerian Society and minister of evangelical opinions who expressed his views on Lamarck in print was James Duncan (1804–61), who wrote a 'memoir of Lamarck' to accompany a volume on 'Foreign Butterflies' he wrote for William Jardine's *Naturalist's Library* (1837). His approach to Lamarck's transformism bears comparison to that of Fleming during the previous decade, although Duncan did not seem to have been prepared to accept that Lamarck's theories had any concrete evidence in their favour. He admitted to finding it 'difficult, indeed, to conceive how Lamarck could advance a theory so utterly opposed to observation and probability, and at the same time succeed so effectually in convincing himself of its truth'.[107] Despite the fact that Lamarck's speculative theories were contradicted by the evidence, Duncan nonetheless admitted that 'they merit attention as the production of a mind remarkable for originality and penetration, as well as for extensive and varied knowledge.'[108] Duncan seems to have been thoroughly familiar with the first volume of Lamarck's *Histoire Naturelle des Animaux sans Vertèbres*, which he paraphrased at length, while his scientific criticisms of Lamarck's transformism seem largely to have been derived from volume 2 of Lyell's *Principles of Geology* (1832). Like so many others, he also repeated Cuvier's misrepresentation of Lamarck's theory as one in which 'efforts and desires engender organs'.[109] It is evident that Duncan also derived most of his biographical information on his subject from Cuvier's mean-spirited eulogy to Lamarck.[110] Duncan, however, is not just hostile to Lamarck's transformist on scientific grounds, for according to him

[107] James Duncan, 'Memoir of Lamarck', in *The Naturalist's Library. Entomology. Vol. V. Foreign Butterflies* (Edinburgh, 1837), p.37
[108] Ibid., pp.17–18
[109] Ibid., p.36
[110] Cuvier, 'Memoir of M. de Lamarck', pp.1–22

they were 'at once absurd and impious – alike opposed to reason and religion.'[111] His critique of Lamarck on theological grounds is based on Duncan's typically evangelical argument that his theory denies God a continuing role in the world after the creation:

> While thus admitting the existence of the Deity, any direct interference in the affairs of the universe is wholly denied to him. His sovereignty is reduced to a mere nominal supremacy, as he is supposed to take no care or thought for the worlds which he authorized or permitted to be created, and can have no sympathy for the creatures which inhabit them.[112]

This critique was to be typical of evangelical responses to transformism in the 1840s, where we find very similar sentiments expressed by Evangelical scientists and natural historians such as David Brewster and Hugh Miller in their critiques of Robert Chamber's *Vestiges of the Natural History of Creation* (1844).[113] As a minister of the Reformed Presbyterian Church of Scotland, it is perhaps unsurprising that Duncan shared with the Evangelicals Brewster and Miller their emphasis on the power of God to actively intervene in the world rather than relying on secondary causes. Nonetheless, his critique of Lamarck in the late 1830s lacks the sense of imminent moral danger present in evangelical denunciations of *Vestiges* a decade later. The harshest thing Duncan has to say of Lamarck's theories is that they have a 'hurtful tendency'.[114] He still believed they 'merit attention' and enthusiastically endorsed Lamarck's less controversial work on invertebrate taxonomy. Despite his damning critique of Lamarck on both scientific and religions grounds, Duncan acknowledged his 'pre-eminent excellence in the ordinary subjects of natural history',[115] and that his *Histoire Naturelle* was 'the most valuable system that has ever appeared of the invertebrate animals; and it has formed the guide to most authors

[111] Duncan, 'Memoir of Lamarck', p.63
[112] Ibid., p.45
[113] See, for example, David Brewster, Review of [Robert Chambers], *Vestiges of the Natural History of Creation*, *North British Review,* 3:6 (1845), p.374; and Miller, *Foot-Prints of the Creator*, p.15
[114] Ibid., p.17
[115] Ibid., p.63

who have since written on the subject.'[116] There is certainly no indication that his works were to be shunned because of their dangerous content, or that his ideas posed a threat to the established order.

A much less nuanced attack on Lamarckian transformism was read to the Royal Society of Edinburgh on 1 March 1841 by John Stark (1779–1849), a successful Edinburgh printer, natural historian, member of the Wernerian Society and author of *Elements of Natural History* (1828). In this paper on the 'supposed progress of human society', Stark noted that 'LAMARCK more than hints that some species of Quadrumanous animals, or Apes, may, from the exigencies of their situation, have given up their natural propensities, and learned to walk, and speak, and think, by some fancied necessity of a progressive development of faculties.'[117] He backed up his argument with reference to particular passages from Lamarck's *Philosophie Zoologique*, a work he clearly had at least a passing acquaintance with. The main thrust of his argument, and the objective for his attack on Lamarck becomes abundantly clear in the following passage:

> Though the habit of flying from predaceous animals did, according to LAMARCK, lengthen the limbs and quicken the pace of the gazelles and antelopes, so as to produce, through ages of practice, the present handsome and light forms which these animals now bear; yet, extravagant as this theory is, it would not be more so than that which would suppose a naked and fruit-eating savage, with no instinctive propensities for blood and animal fibre, no means of pursuit, and no implements of chase, to discover that the animals which fled him would serve as food.[118]

It is not transformism that Stark is attacking *per se*, but rather the idea of progressive development in general, and with particular reference to the progress of human civilization. Although he was approaching the question from a diametrically

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[116] Ibid., p.51

[117] John Stark, 'On the supposed progress of human society from savage to civilized life, as connected with the domestication of animals and the cultivation of Cerealia', *Transactions of the Royal Society of Edinburgh* 15 (1844), p.181

[118] Ibid., p.183

opposed viewpoint, he shared Knox's distaste for the idea of progress. Although Stark was clearly a deeply religious man, while Knox, according to his biographer and former student Henry Lonsdale, viewed 'all religious sects' as 'having their origin in idolatrous credulity and ignorance', they both shared a distrust of the idea of increasing perfection in the natural world.[119] In place of Knox's opposition to any suggestion of purpose in the universe, Stark's dislike for the concept of progress was inspired by the evangelical assertion that real progress was impossible in a fallen world. An almost identical attack on progress in nature and society from an evangelical perspective can be found in the evangelical Edinburgh phrenologist William Scott's *Harmony of Phrenology with Scripture* (1836), written as a refutation of the progressivist doctrines contained in George Combe's *Constitution of Man* (1828).[120] For Stark, there is no way in which humanity could have raised themselves above the beasts; 'it does not appear how their knowledge of digging the soil for edible roots, or cultivating the most simple herbs, could originate without supernatural aid; and if man had been created a savage, without the knowledge of speech, a savage he might have for ever remained among the beasts of Eden'.[121] Not only is progress impossible except through supernatural agency, but In Stark's rather gloomy view, we even face the possibility of future degeneration, and he ends his article with a call to arms:

> It is the business of the philosopher, the duty of the statesman – the object of all – to inquire into the causes of the apparently fated decline of all human communities; and to ascertain whether moral degradation, like the same cause among the antediluvians, may not be the forerunner of national ruin.[122]

Hugh Miller (1802–56), the geologist and editor of the Evangelical newspaper *The Witness* was another stern critic of Lamarck. In Miller's first major publication on geology, *The Old Red Sandstone* (1841), which had appeared the previous year in

[119] Lonsdale, *Life and Writings of Robert Knox*, p.404
[120] Scott, *The Harmony of Phrenology with Scripture*
[121] John Stark, 'On the supposed progress of human society', p.191
[122] Ibid., p.209

serial form in *The Witness*, he launched a scathing attack on Lamarckian transformism. Characteristically, he brought a satirical touch to his critique of progressive development, asking whether in some remote epoch:

> The descendents of the *ourang-outang*, for instance, may be employed writing treatises on Geology, in which they shall have to describe the remains of the *quadrumana*, as belonging to an extinct order. Lamarck himself, when bearing home in triumph with him the skeleton of some huge salamander or crocodile of the Lias, might indulge, consistently with his theory, in the pleasing belief that he had possessed himself of the bones of his grandfather [.][123]

Like Fleming, Miller's concrete criticisms of Lamarck's transformism in *The Old Red Sandstone* bore principally on its nature as unscientific speculation rather than because it was contrary to revelation, although he made no secret of the fact that his rejection of transformism was inspired by his evangelical faith. The relationship between his critique of Lamarck's theories and his religious beliefs is neatly illustrated in his assertion that 'there is no lack of faith among infidels; their "vaulting" credulity o'erleaps revelation, and "falls on the other side."'[124]

Miller saw God's creation as being composed of a continuous spectrum of living forms. However, these forms were not all alive at the same time. For him the 'perfection of the works of the Deity is a perfection entire in its components, and yet these are not contemporaneous, but successive: it is a perfection which includes the dead as well as the living, and bears relation in its completeness, not to time, but to eternity.'[125] It was this spectrum of creation that Lamarck and his followers had mistaken for evidence of development: 'They confound gradation with progress.'[126] However, Miller was a more sophisticated thinker than Duncan or Stark, and his ideas were far more nuanced. In *The Old Red Sandstone* he did not deny the

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[123] Hugh Miller, *The Old Red Sandstone; or New Walks in an Old Field* (Edinburgh, 1841), p.39
[124] Ibid., p.41
[125] Ibid., p.45
[126] Ibid., p.42

progressive nature of the history of life altogether, but conceded that the appearance of the major divisions of the animal kingdom as revealed by the fossil record had been progressive. However, he did not believe that there was any evidence for progress within these higher divisions:

> The mammifer takes precedence of the bird, the bird of the reptile, the reptile of the fish; there is progression in the scale; the arrangement of the classes is consecutive, not parallel. But in this great division there is no such progression; the osseous fish takes no precedence of the cartilaginous fish, or the cartilaginous, as a series, of the osseous.[127]

In general, though, the trend was upward, and this was reflected in the increasing physical size of animals: 'We begin with an age of dwarfs, – we end with an age of giants. The march of Nature is an onward and an ascending march'.[128] However, this progress was radically discontinuous: 'The curtain drops at [God's] command over one scene of existence full of wisdom and beauty – it rises again, and all is glorious, wise and beautiful as before, and all is new.'[129] Each fresh creation included higher forms of life than the preceding one, but there was no continuity between these entirely separate exertions of God's creative power. The fundamentally discontinuous nature of the fossil record was Miller's most important source of evidence against transformism.

The critiques of transformism by Stark, Duncan and Miller in the late 1830s and early 1840s differed from earlier criticisms in that they placed much greater emphasis on religious rather than purely scientific criteria. Even Fleming, who was a prominent figure in the Evangelical Party of the Church of Scotland, did not depend principally on religious arguments in his critiques of transformism in the early 1820s, even while he used them freely against Lamarck's materialistic theory of the mind. It seems likely that the tone of Stark's attack on transformism reflected

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[127] Ibid., p.62
[128] Ibid., p.269
[129] Ibid., p.102

a new philosophical and religious climate that had set in by the late 1830s and early 1840s, which was more actively hostile to transformist theories. And this hostility was increasingly rooted in theological considerations. The growing assertiveness of the Evangelical Party of the Church of Scotland in the decade before the Disruption of 1843 may well have led those of its members who took an interest in natural history to go on the offensive against transformism, and this could also have encouraged other evangelical critics. As Baxter has shown, the Evangelicals became increasingly assertive in the decade before the Disruption and science became yet another battleground in their struggle with the Moderates.[130] For those with scientific interests, it became imperative to claim true science as well as true religion for the Evangelical cause. As transformism began to reach a wider audience after the publication of volume 2 of the Lyell's *Principles of Geology* in 1832, it became more important for Evangelical natural historians to ensure that views of the history of the earth and of life consonant with Evangelical theological positions were promoted and defended against the perceived threats of progressivism and transformism. The violent reaction in Evangelical quarters to the publication of Robert Chambers' *Vestiges of the Natural History of Creation* in 1844 would seem to bear this out.

Reception of the transformist theories of Geoffroy

An anonymous paper entitled 'Of the continuity of the animal kingdom by mean of generation from the first ages of the world to the present times' was published in the *Edinburgh New Philosophical Journal* in 1829.[131] It is an abstract of a memoir read before the French Academy of Sciences by Étienne Geoffroy St Hilaire and published in the *Mémoires du Muséum d'Histoire Naturelle* the previous year.[132] This article was attributed to Grant by Desmond in a 1984 paper.[133] There is no direct

[130] See Baxter, 'Deism and development'
[131] Anon, 'Of the continuity of the animal kingdom', pp.152–6
[132] Geoffroy, 'Mémoire', pp.209–29
[133] Desmond, 'Robert E. Grant', pp.201–2

evidence to support this attribution, although Grant's known transformism and his status as one of the most important disciples of Geoffroy are points in his favour as a possible author. However, as we will see below, Grant is not the only candidate. The article gives a full summary of Geoffroy's paper, but gives significantly more space to his arguments concerning the relationship between living and extinct animals, while giving a very condensed versions of Geoffroy's rather convoluted refutation of the theory of preformation and account of his experiments on chicken eggs at the hatchery in Auteuil, which are developed at considerably more length in the original paper. It is often more or less a direct translation of the French text, with one or two intriguing exceptions that I will explore below.

The paper begins with an introductory paragraph which has no counterpart in the original article. Here the question is posed:

> Are the animals whose remains occur buried in the earth, and which almost all belong to species or genera which are not observed in the living state, to be considered as having been ancestors of those which now people the earth, and as having been modified by the influence of time, and of the changes that have supervened in the state of the globe? Or is the contrary opinion to be adopted? Are we to believe, that, after the occurrence of great cataclysms, new beings were produced by a new exertion of creative power, – in short, to make use of M. Geoffroy St Hilaire's expression, that *the six days work was resumed*.[134]

I would suggest that there are one or two rhetorical touches in this paragraph that indicate what conclusion the author wanted the reader to reach with regard to the question in hand. When the reader is asked to consider the transformist option, the question simply takes the form 'Are the animals …?', but when it comes to the alternative view, his question is couched in the form 'Are we to believe …? For transformism it is a question of establishing the truth of a fact. For multiple creations, the readers are asked to consider if this is something that it is reasonable to *believe*. I would argue that the author is here subtly implying that the transformist

134 Anon, 'Of the continuity of the animal kingdom', p.154

answer is the more rational one. I would also suggest that the reference to Geoffroy's resumption of the six days of creation carries a strong implication that the concept of multiple creations is contrary not just to reason but to scripture, which makes no allowance for multiple supernatural creations.

The following two paragraphs are a close summary of the first three pages of Geoffroy's paper, often coming close to a literal translation in some passages. The author then missed out a long section on monstrosities, foetal development and the mechanisms which produce and limit variation in living things and rejoined Geoffroy four pages later for a discussion of the idea that lines of filiation can be discerned among series of fossil species. The author then skipped over a passage on the effect of changes in the environment on the development of living things and went straight to an appreciation of the work of Lamarck. In the following key passage, the anonymous author seems to clearly reveal his own opinion:

> To establish M. Geoffroy's opinion in a solid manner, the important point is to demonstrate that the differences of atmospheric conditions may have been sufficiently great and powerful to bring the different species and general, from the types which they originally presented, to what we now see them to be. Now, of this the author thinks no doubt can be entertained. Let attention be paid to the modification which the species may still undergo, in consequence of a mere transportation from one latitude to another, – modifications which have been determined by Dr Roulin with respect to the animals transported from Europe to America. Let the important facts, in particular, which the study of monstrosity presents, be attended to, and there will appear nothing surprising in the modifications produced in the animal species, by the succession of ages, any more than in modifications induced in the agents under the influence of which animals are developed. [135]

The above paragraph introduces a section on Geoffroy's experiments on chickens at Auteuil. A comparison of this passage with its much shorter equivalent in the original article is illuminating. The original passaged reads: 'Aware of the

[135] Ibid., pp.154–55

inadequacy of research in that direction, I did not wait for the work of M.Roulin to enlighten us, and I had already considered that some experiments in physiology could be undertaken to shed light on the questions of antediluvian geology; to these I consecrated all of the spring of the year 1826.'[136] Both texts then go straight into an account of Geoffroy's experiments. This is the only reference in Geoffroy's paper to Roulin. It can be clearly seen that the anonymous author seems to have been rather better informed about Roulin's work than Geoffroy was, and discourses at length on it as a proof of the transmutation of species. There can be little doubt that the anonymous author was himself a transformist. It is evident throughout his account of the article that he presented Geoffroy and Lamarck's theories in the most favourable light possible. For example, the original French text mentions criticisms that the facts on which Lamarck's theory is based have been proved false: 'Should we straight away conclude that the inadequacy of the facts brought as proof condemns the doctrine that they ought to support? This consequence seems correct, but is nevertheless not necessarily the conclusion in all cases.'[137] When the anonymous author comes to render this into English, he beefs it up considerably, stating that 'The particular facts on which M. de Lamarck rests his grand idea are far from being perfectly correct. Perhaps there is not even one of them that is not blemished by some inaccuracy; and yet the conclusion which he draws from them is true – such is the power of genius in foreseeing the great truths of nature.'[138]

Geoffroy's original paper concludes with a further ten pages of detailed description and discussion of his experiments on chicken eggs. However, the anonymous

[136] Geoffroy, 'Mémoire', p.219. The original French text reads: 'Fixé sur l'insuffisance des recherches dans cette direction, je n'avois pas attendu que les travaux de M. Roulin nous donnassent l'éveil, et j'avois déjà pensé que quelques expériences de physiologie pourroient être entreprises au profit des questions de la géologie antédiluvienne ; j'y consacrai tout le printemps de l'année 1826.'
[137] Ibid., p.218. The original French text reads: 'Irons-nous aussitôt conclure de l'insuffisance de ces faits apportés en preuve à la condamnation de la doctrine qui'ils devoit étayer? Cette conséquence paroît de droit, mais n'est cependant point l'obligée conclusion de tous les cas.'
[138] Anon, 'Of the continuity of the animal kingdom', p.154

author of the article concluded his article much more concisely in one page, including the following commentary:

> M. Geoffroy St Hilaire, by varying the phenomena of heat, dryness, and motion, not only produced monstrosities at pleasure, but even produced a given species of monstrosity by means of a particular precaution. And let it not be objected that the monstrous species thus produced in an artificial manner, were incapable of being reproduced and perpetuated. Nature, aided by time, which he had not at his disposal, acting by more numerous and gentler modifications, could have done what will always be impossible in the most judiciously conducted experiments.[139]

I think that on reading this article, no reader could long be in doubt that the anonymous author was sympathetic to Geoffroy and broadly in agreement with his theories.

Jameson not only published articles relating to Geoffroy's transformist theories in his journal. On 25 April 1829, he 'gave an account of the doctrines of Geoffroy St Hilaire on the analogy between extinct animals and those now living' to the Wernerian Society.[140] Sadly no record of exactly what Jameson had to say about Geoffroy's theories has survived, but it seems unlikely that Jameson could have discussed Geoffroy's theories on the relationship between existing and extinct species without touching on his theory of the transmutation of species, first fully articulated in a paper in the *Mémoires du Muséum d'Histoire Naturelle* in 1825.[141] Indeed, given the coincidence of dates between Jameson's paper to the Wernerian Society and the publication of 'Of the continuity of the animal kingdom' in his journal, which appeared in the April–June 1829 number, it seems more than likely that the paper he gave was based on that article, which may even have been written by Jameson himself rather than Grant.

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[139] Ibid., p.155
[140] Wernerian Society, Minutes of the Wernerian Society, vol. 1, f.297
[141] Geoffroy, 'Recherches sur l'organisation des gavials', pp.97–155

We have seen above that Robert Knox could at times be quite disparaging regarding the theories of Lamarck. However, an early reference to transformism in Knox's writings which can give us a more nuanced view of his attitude towards transformism appeared in his translation of the lectures of De Blainville on comparative osteology, published in *The Lancet* in 1839. Although Lamarck is mentioned here, the focus is on the ideas of Geoffroy. In a note appended to these lectures, which merits quotation in its entirety, Knox states that:

> The lapse of time passing for nothing with modern geologists, Lamack [sic], Geoffroy (St. Hilaire), and others, took advantage of such views, by proposing a doctrine in which the specific distinctions of animals was set aside. The fossil Sawrians [sic] became in their eyes, the progenitors of the recent crocadile [sic]; and although such theories were altogether *speculative*, and positively contradicted by all human experience and chronology, yet both being, as it were, but a drop in the great ocean of time, these distinguished naturalists conceived themselves to be at liberty to set aside all human experience and human records, and appealed to the obvious changes which have taken place in the surface of the globe, as indicative of an immeasurable antiquity, and of a sufficient lapse of time and change of circumstance to effect all manner of alterations in the primitive creation.[142]

The statement in the passage quoted above that the transformism of Lamarck and Geoffroy was 'altogether speculative, and positively contradicted by all human experience and chronology' was used by Rehbock to illustrate Knox's hostility to transformism.[143] Put back in its context, we can see that things are not quite so simple. What Knox in fact said was that, although transformism was 'contradicted by all human experience and chronology', the vast span of geological time makes the time frame of recorded history quite inadequate when discussing the history of life on earth. Despite the typically slightly mocking tone of this note, the only concrete criticism that is raised against transformism is that it is 'speculative', which hardly constitutes a conclusive refutation. This passage in fact is a good sample of

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[142] Robert Knox, 'Lectures of M. De Blainville on comparative osteology. The comparative osteography of the skeleton and dentar system, in the five classes of vertebral animals, recent and fossil, by M.H.M. Ducrotay de Blainville', *The Lancet* 1 (26 October 1839), p.137

[143] Rehbock, *Philosophical Naturalists*, p.50.

Knox's rather elliptical rhetorical style, which often leaves his true opinion difficult to fathom.

What is clear from this passage is that Knox was familiar with Geoffroy's 1825 paper on his 'Recherches sur l'organisation des gavials', which suggested that modern crocodiles were the descendants of the fossil species found in France.[144] This paper was something of a landmark for Geoffroy for, as Toby Appel has remarked, before 1825 'Geoffroy had neither explicitly adopted special creationism nor mentioned evolution.'[145] Appel has concluded that there was no reason to believe Geoffroy was a convinced transformist before 1825, although he had doubtless discussed the subject with his colleague Lamarck before then. It cannot therefore be assumed that Geoffroy's disciples, such as Knox and Grant, were aware of his transformist views before this date. Knox refers to this paper not only here, but repeatedly in some of his later works.[146] In his *Great Artists and Great Anatomists* (1852) Knox gave a quite differently nuanced account of his views on Geoffroy's theories from the one quoted above from his translation of De Blainville's lectures:

> To Cuvier's theory of the 'Fixity of Species,' as demonstrated by the drawings on the Egyptian tombs, Geoffroy objected, that 'as the surrounding circumstances had not changed, there existed no reason for a change in the Fauna.' He might have added, that the period referred to by Cuvier, in proof of his views, was but an instant in the duration of the globe.
>
> Convinced of the soundness of the basis on which Autenrieth, Goethe, and Geoffroy had constructed the great theories of Transcendental Anatomy, I hesitated not applying then constantly in all my researches on zoology, from 1820 inclusive: these principles were fully explained by me in three courses of lectures on Comparative Anatomy delivered to distinguished classes in 1825-26-27.[147]

[144] Geoffroy, 'Recherches sur l'organisation des gavials', pp.97–155
[145] Appel, *Cuvier–Geoffroy Debate*, p.130
[146] For example Knox, *The Races of Men*, pp.176, 478
[147] Knox, *Great Artists and Great Anatomists*, pp.211–12

Although, as often with Knox, the exact meaning of these two consecutive paragraphs when taken together is not altogether clear, two conclusions can safely be drawn from the arguments of each of them. From the first we know that Knox was aware that Cuvier's arguments against transformism based on historical evidence were fatally flawed. From the following paragraph we learn that Knox was a convinced disciple of Geoffroy during the 1820s. Knox nowhere explicitly said that he was a follower of Geoffroy's transformist theories, as opposed to his transcendental anatomy, but the juxtaposition of these two paragraphs is extremely suggestive, especially when later in the same book Knox stated explicitly 'I believe all animals to be descended from primitive forms of life'.[148] A few years later in an article in *The Lancet* he wrote: 'A last question remains – the origin of natural families: Have they been distinct from all time? I think not.'[149] These would appear to be unambiguous avowals, that at least by the early 1850s Knox believed in some form of transformism. That, with Geoffroy, he was converted to a belief in the transmutation of species in the mid-1820s seems highly probable.

Although not a transformist, John Fletcher was an enthusiastic advocate of Geoffroy's transcendental anatomy among Edinburgh's extra-mural medical lecturers in the late 1820s and 1830s. For Fletcher, the 'classification of animals upon the principles, not of their general similarities of form and function, but of the fundamental structure of their organs, as proposed by Geoffroy St Hilaire, De Blainville and others, is unquestionably much more profound and scientific than that of Cuvier'.[150] He went on to describe Geoffroy as the 'chief' of those who held that 'if not all plants, certainly all animals consist of the same number of organs, and these all fundamentally the same.'[151] Fleming acknowledged that Geoffroy attributed the different types of animals 'to the greater or lesser degree of original

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[148] Ibid., p.109
[149] Robert Knox, 'The philosophy of zoology, with special reference to the natural history of man', *The Lancet* 7:2 (1855), pp.218
[150] John Fletcher, *Rudiments of Physiology, in Three Parts* (Edinburgh, 1835), pp.9–10
[151] Ibid., p.12

development of a certain primitive type common to all'.[152] He went on to note that for Geoffroy this primitive type was not simply an ideal form, but had actually existed:

> Nor is the prototype on which each plant and animal, however elevated, is supposed to be based, conceived to be merely traceable in imagination through all the adventitious forms of the less perfect fabric; but to have actually existed, first preparatory to the general creation successively of the various tribes of organized beings, and again preparatory to the development of the embryo of each individual in its generation.[153]

This seems to be a rather garbled account of Geoffroy's transformist theories, although Fletcher correctly identifies the central role of foetal development in them. It is not clear from Fletcher's account that he had fully understood the details of Geoffroy's theory, or the nature of the mechanism for the transmutation of species that he had proposed. As will be discussed in chapter 5, Fletcher did, however, go on to give a more detailed account of transformism in the same work, without attributing it directly to either Lamarck or Geoffroy. The theory he outlined does, however, appears to draw elements from the ideas of both these thinkers, as well as from Buffon's concept of 'organic molecules'. Despite the detailed transformist theory he had presented, Fletcher finally rejected the idea, concluding that:

> while it has nothing but the most vague and rambling presumptions in its favour, it is quite inconsistent with the generally immutable character of each tribe from the earliest periods of which we have any records; nor does the fact of many tribes being known to have formerly existed which have now perished from the face of the earth, any more than that of many others now inheriting the earth which probably at one time had no existence, afford any proof of their mutual convertibility, or indicate any thing more than that the character of its inhabitants has, at different times, varied with that of the globe.[154]

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[152] Ibid., p.12
[153] Ibid., p.12
[154] Ibid., pp.14–15

Perhaps unsurprisingly, given the enthusiasm for his theories in the extra-mural medical schools, there is also evidence from the minutes of the Plinian Society to indicate that a significant number of the medical students at the University of Edinburgh were taking an interest in Geoffroy's transcendental anatomy in the early 1830s. On 15 February 1831 Thomas Shapter 'read a paper giving an analysis of Geoffroy St. Hilaire's Views of Unity of Organisation', after which Allen Thomson is reported to have 'read another paper on the same subject',[155] and on 25 April 'Mr French announced for next meeting a paper on Unity of Organization'.[156] Clearly Geoffroy's theories were both well-known and widely discussed in student circles in Edinburgh in the late 1820s and early 1830s. While the often scanty evidence of the minutes of the Society tell us nothing about the reception of Geoffroy's transformist theories by its members, as opposed to his ideas on unity of plan, they must certainly have been aware of them.

One member of the Plinian Society who we know from other sources certainly took a positive interest in the theories of Geoffroy was Henry H. Cheek, editor of the *Edinburgh Journal of Natural and Geographical Science*. As we noted in chapter 2, in 1830 a famous and acrimonious debate took place on the subject of unity of plan in the Academy of Science in Paris between Geoffroy, the great champion of the concept, and Georges Cuvier, its most famous opponent.[157] The confrontation between the two great comparative anatomists became something of a cause celebre in intellectual circles across Europe and was widely reported. Cheek's journal devoted considerable space to 'a controversy which has arisen between the two first zoologists of the age'.[158] The April number contained a detailed account of Cuvier's

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[155] Minutes of the Plinian Society, 1826–1841, vol. 2 (Library of the University of Edinburgh Dc.2.54), f.85 recto
[156] Ibid., f.93 verso
[157] For a comprehensive and detailed account of the debate and its impact, see Appel, *The Cuvier–Geoffroy Debate*
[158] [Henry Cheek], 'Review of the recent discussion before the Academy of Sciences in Paris, on the "unity of organization". Part I,– Baron Cuvier's Views', *Edinburgh Journal of Natural and Geographical Science* 2 (April 1830), p.37

arguments against Geoffroy. In May followed Geoffroy's answer to Cuvier's charges. According to Toby Appel, 'even the supporters of Geoffroy agreed that Cuvier had had the upper hand in the debate' and that 'Cuvier could be said to have won the day'.[159] Although the articles in Cheek's journal evince a healthy respect for both of the great comparative anatomists, the reader is left in little doubt that the author favours Geoffroy's arguments. Rather than acknowledge Cuvier the victor, the second article reports that 'M. St. Hilaire has consented to relinquish the discussion in the Academy; but, confident in the truth and novelty of his conclusions, he has determined to write a work, wherein he will controvert the opinions of M. Cuvier.'[160] In the event, Geoffroy's *Principles of Philosophical Zoology*, rushed into print in April 1830, did not definitively put an end to the dispute. Only the death of Cuvier in May 1832 finally prevented any further continuation of hostilities. There is little doubt where Cheek's sympathies lay, and there is much evidence for his espousal of both Geoffroy's principle of unity of plan and his transformist theories to be found throughout Cheek's journal.

Conclusion

We have seen that there is substantial evidence that transformist theories were well known in Edinburgh natural history circles in the first half of the nineteenth century. John Walker discussed the theories of Linnaeus and Buffon in his lectures, and we know that Erasmus Darwin's ideas were known from the writings of Barclay and Grant. Lamarck's transformist theories were discussed in print by Jameson, Fleming, Duncan, Stark, Knox, Fletcher and Miller, seven of the most significant figures in natural history in Edinburgh. We have Darwin's word for it that they were also advocated by Grant. Geoffroy's theories were debated at the Plinian and Wernerian Societies and in the pages of the *Edinburgh New Philosophical Journal* and the *Edinburgh Journal of Natural and Geographical Science*. Jameson took an active interest in his ideas, and three of Geoffroy's most important disciples in the

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159 Appel, *The Cuvier–Geoffroy Debate*, p.171
160 [Henry Cheek], 'On the present state of science abroad', p.116

English-speaking world, Grant, Fletcher and Knox, were Edinburgh graduates and resident there for most of the period. All three were also extra-mural medical lecturers in the city, and doubtless contributed to the spread of these ideas among the medical students of the University. We have seen that reactions to transformism ranged from outright rejection to enthusiastic acceptance. Many reactions are surprisingly nuanced; Fleming, for example, acknowledged the strength of Lamarck's theories and the evidence that existed in their favour, while ultimately rejecting them on carefully considered scientific and philosophical grounds. Duncan too, although his reaction was more hostile, acknowledged that Lamarck's transformism merited attention.

There appears to have been a shift in the perception of transformism, which can be dated to the mid-1830s. Criticism before this seems to have been primarily based on scientific or philosophical criteria. Walker presented transformism as a 'vulgar error', which contradicted the doctrine of the 'great chain of being' that still dominated discourse in natural history in the late eighteenth century. Although Barclay's criticism of Erasmus Darwin's theories did have a religious dimension, they were principally condemned as being contrary to his cherished doctrine of the vital principle. Whatever Jameson may have come to believe later, in 1813 his criticism of Lamarckian transformism was simply that it overstepped the limits to variation established by Cuvier. In his earlier writings on Lamarck Fleming dismissed his theories as excessively speculative, as well as contrary to the evidence of the fossil record, although latterly he seems to shift onto more theological territory in his criticisms. Knox, the great anti-clerical enemy of the idea of purpose in the universe, attacked Lamarck because he saw his theories as teleological. Although Lamarck's transformism came in for a wide range of criticism, Geoffroy's theories seem to have largely escaped negative comment, perhaps because they were less well known to natural historians outside the circle of comparative anatomists and their students.

The one thing that most of the critiques of transformism had in common until the end of the 1820s was that they were based firmly on scientific or philosophical criteria. There seems to have been a general consensus that it was upon this terrain that the transmutation of species was to be judged. At least until the mid-1830s, the evidence seems to suggest that the most important question in the minds of natural historians in Edinburgh was whether transformism was good or bad science, not whether it posed a threat to the established religious, social or political order. If religious or political concerns were indeed behind the openly expressed scientific criticisms, there is little evidence to show it. Even after this, there is never any suggestion that transformist ideas are politically dangerous or threaten the established social order. When religious criticisms do emerge strongly in the later 1830s and 1840s, they come generally from among natural historians with evangelical religious views, suggesting a likely connection with the more assertive and militant attitude that became apparent within the Evangelical Party in the decade between the rise to dominance of the Evangelicals within the Church of Scotland in 1834 and the Disruption of 1843. Although evangelical critics of transformism, such as Grierson and Fleming, existed in the 1820s, they were less strident, and lacked the focus on specifically theological considerations that became apparent later. From the mid-1830s attacks tended to centre on specifically evangelical theological concerns, such as the impossibility of progress in a fallen world, as with Stark, or the power and willingness of God to intervene directly in nature rather than relying on secondary causes, the major preoccupation of Duncan.

In this chapter I have confined myself to examining the response in Edinburgh natural history circles to the transformist theories coming from outside their city. However, a significant number of natural historians based in Edinburgh, who had studied in the city, or who were members of its natural history societies and published in its journals, also came to formulate their own theories of the transmutation of species rather than simply accepting or rejecting the ideas of earlier transformist thinkers. They formulated a fascinating body of diverse but interrelated theories regarding the true nature and origins of genera, species and varieties and the history of life on earth; it is to these individuals and their theories that I will turn my attention in the next chapter.

Chapter 5: Models of transformism in Edinburgh

Introduction

In a footnote to the introduction to his *System of Mineralogy* (1804), Robert Jameson laid out the main problems of natural history as he saw them as the nineteenth century began. For Jameson, the most important questions included: 'Were all animals and plants originally created as we at present find them, or have they by degrees assumed the specific forms they now possess? Are certain species become extinct? In what order and whither have they migrated? What change has climate produced?'[1] Right at the very beginning of his career as professor of natural history at the University of Edinburgh, Jameson was therefore already raising important questions regarding the history of life on earth. First among these questions was that of the transmutation of species. In this chapter I will explore how a number of important figures in Edinburgh natural history went about searching for answers to this question.

Natural historians in the late eighteenth and early nineteenth centuries were faced with emerging evidence from the geological record which suggested that some fossil species had become extinct. In the eighteenth century it had still been possible to argue that apparently lost fossil species might still be alive in some unexplored corner of the globe or in the uncharted depths of the oceans, but by the early nineteenth century exploration had made the continuing existence of these species seem increasingly unlikely.[2] It was particularly hard to believe that the enormous animals discovered in alluvial deposits in Europe, Siberia and North America and the even more ancient mammals catalogued by Georges Cuvier from the Paris basin were still lurking in some forgotten corner of the world.

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[1] Robert Jameson, *System of* Mineralogy, vol. 1 (Edinburgh, 1804), pp.xix–xx

[2] For a more detailed account of this debate, see Martin J.S. Rudwick, *The Meaning of Fossils: Episodes in the History of Palaeontology* (Chicago, 1985), especially pp.105–9

Not only was it becoming increasingly clear that some fossil species had been entirely lost, but as the fossil record became better known it became evident that new species had come into being long after the first appearance of life on earth. No fossil of a terrestrial animal or plant had been discovered in transition rocks, while mammals only made their appearance in tertiary deposits. Geologists had the delicate task of accounting for this apparent evidence of multiple creations while avoiding a complete rupture with the Biblical account.[3] For followers of theories of the earth which proposed directional change over geological time, such as those of Buffon or Abraham Gottlob Werner, it must have seemed natural to link the story of life to the changing physical conditions on the surface of the earth. As we have seen in chapter 2, Buffon had proposed in his *Époques de la Nature* that new species had been summoned into existence in some unexplained way by changes in the environment of the earth linked to the cooling of the globe. Cuvier suggested that a series of catastrophes had wiped out the inhabitants of the earth, each of which had been followed by the appearance of an entirely new flora and fauna. Another possible answer to this problem was some form of transformism.

In this chapter I will first explore how directional theories of the earth, and in particular that of Werner, may have paved the way in Edinburgh for the acceptance of transformist interpretations of the history of life. I will then investigate how transformist thinkers in Edinburgh envisaged the broad sweep of the history of life and the forces which impelled it forwards. In doing this I will also attempt to determine where they owed a debt to earlier transformists, and in what ways they differed from them. Turning from what would in modern parlance be termed 'macroevolution', I will next address theories regarding the nature of species and varieties and how these were thought to come into being. [4] Finally, I will take a look

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[3] For a classic account of the relationship between geology and scripture in this period see Charles Coulson Gillispie, *Genesis and Geology* (Cambridge, MA, 1996), chapters 3 and 4
[4] The proposed link suggested in this chapter between Wernerian neptunism and transformism goes some way, I hope, to reconciling the apparent contradiction between these two positions noted by Secord (See Secord, 'Edinburgh Lamarckians', p.18.)

at two thinkers whose views are radically different from those of most of the figures I have been considering, Robert Knox and Hewett Cottrell Watson. Their theories were unusual in that, while they both accepted the transmutation of species as fact, both appear to have questioned the progressive nature of the history of life. It should be noted, however, that Knox at least did not deny that directional change had occurred, but only that this change was towards increasing perfection. Indeed Knox was resolutely opposed to any theory that he saw as teleological, or in which the human species was represented as the pinnacle of creation. But before looking at these exceptions to the dominant progressive model, I must first establish how the idea of a directional history of life became established.

The history of the earth and the story of life

Born in 1731 and dying in 1803, John Walker, Edinburgh's second professor of natural history, lived through a momentous period in the history of geology. His teaching reflected this, touching as it did on the seventeenth-century theories of the earth of John Ray (1627–1705) and Thomas Burnet (*c*.1635–1715), soon to be regarded as mere historical curiosities, as well as on the more recent ideas of Buffon and Linnaeus. In his 1790 lecture course, Walker considers three possible theories of the earth as explanations of how 'extraneous fossils' had found their way 'to the tops of the greatest heights and the profoundest Caverns of the Earth.'[5] The first was that which he associated with Thomas Burnet and John Woodward, who interpreted this phenomenon as evidence of the Biblical deluge; the second was the theory of the recession of a primordial universal ocean, associated by Walker with Linnaeus and Buffon; and the third relies on the action of volcanoes and earthquakes as a mechanism, associated by Walker with John Ray and Robert Hooke, among others. Of these three Walker tells us that the 'Volcanic Theorists make a poor Story of the

[5] John Walker, Notes on natural history lectures, vol. 4 (1790) (anonymous student) (Edinburgh University Library, Dc.2.25), f.145. It should be noted that the word 'fossil' in this period did not have its current meaning, but could refer to rocks and minerals as well as the remains of living things.

matter', although he declines to give an opinion on the relative merits of the first two. As for the age of the earth, in 1790 Walker did not seem ready to assign a great antiquity to the world. He suggested that in determining the age of the globe 'a sure monument is the formation of *Vegetable Soil* which is no where of such depth as to persuade us this Globe is of very remote antiquity'.[6]

Along with most geologists of the period, Walker divided the rocks into primitive and secondary strata. He noted in his lectures in 1790 that fossils are only found in the secondary rocks. However, within the fossil-bearing strata he does not seem to have recognised any directionality in the story told by the organic remains they contained. On the contrary, he went on to say in a later lecture the same year that 'Both in the Vegetable and Animal Kingdom there are a certain number of distinct species which have remained without addition or diminution since the creation.'[7] In 1797 he explicitly linked the history of earth with the history of life by adding 'that the primitive strata have been formed previous, but the secondary strata posterior, to the Existence of organized Bodies.'[8] This would suggest that Walker believed that the primitive strata existed before the creation of life and that this creation had taken place once and for all at the beginning of the deposition of the secondary strata.

In notes from 1797 we see essentially the same story; no new species had been created and none had become extinct since the creation: 'the true Species of Plants and Animals exist in the same Manner and Number in the present Day as they did in ages past. There is no Example of a Species of Animal being entirely lost, though they may be extinguished in a particular Tract of Country.'[9] Nevertheless, he did

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[6] Ibid, f.147
[7] Walker, Lecture notes (1790), vol. 4, f.162
[8] John Walker, Notes on natural history lectures (1797) (taken by David Pollock), vol. 3 (Edinburgh University Library, Gen.711), ff.38–9
[9] Walker, Lecture notes (1797), vol. 9, f.132

concede that many fossils were 'unlike any species now known to live'.[10] Although Walker seems to have continued to doubt that any species had ever become extinct, by the last decade of the eighteenth century it was becoming apparent to many natural historians that some fossil species were indeed lost forever. William Smellie, for example, wrote in the first volume of his *Philosophy of Natural History* (1790) about the discovery of bones of the mammoth, 'whose species is supposed to be extinguished'.[11] John Playfair, in his *Illustrations of the Huttonian Theory of the Earth* (1802), was a great deal more certain on the subject of extinction. According to Playfair 'The inhabitants of the globe, then, like all the other parts of it, are subject to change: It is not only the individual that perishes, but whole *species*, and even perhaps *genera*, are extinguished.'[12] In Walker's Edinburgh, then, there was a dawning realisation that many species had disappeared from the world. However, Walker himself, at least, seems to have continued to believe that no new species had been added after the creation of life, and that these species were immutable. These views of Walker's were to be challenged not long after Robert Jameson succeeded him as professor of natural history in 1804.

As Martin Rudwick has chronicled, by the early decades of the nineteenth century there had developed a general consensus among geologists that the history of life was directional, or even progressive.[13] At the same time, exploration and discovery were making it seem more and more implausible that all the fossil species that had been discovered in the rocks were still alive somewhere in the world. Increasingly geologists were coming to take the view that the world was very much older than had previously been believed and that over this vastly extended expanse of geological time some species must have become extinct and been replaced by entirely new species. This was the view of Cuvier, whose extremely influential

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[10] Walker, Lecture notes (1790), vol. 4, f.145
[11] Smellie, *Philosophy of Natural History*, vol. 1, p.79
[12] John Playfair, *Illustrations of the Huttonian Theory of the Earth* (Edinburgh, 1802), p.469
[13] Martin Rudwick, *Worlds before Adam: The Reconstruction of Geohistory in the Age of* Reform (Chicago, 2008), p.49

studies of fossil mammals from the Paris basin had convinced him that extinction was a regular part of the workings of nature. Robert Jameson, Walker's successor in the chair of natural history at Edinburgh, seems to have taken an active interest in these developments and kept abreast of the latest theories.

Jameson was the leading proponent in Britain of the Neptunist theories of Abraham Gottlob Werner, with whom he had studied in Freiberg. From his published work it is apparent that Jameson inherited a fundamentally progressive vision of the history of the earth from Werner. According to Werner's theory, the early earth had been covered by a universal ocean, from which the oldest Primitive rocks were deposited by chemical precipitation. As the waters receded and more and more dry land emerged, the mechanical deposition of sedimentary rocks became increasingly important as the earth gradually took on its current form. This progressive narrative of the history of the earth led Jameson to posit a similarly progressive history for life. In his *Elements of Geognosy* (1808) he wrote that 'As the water diminished, it appears to have become gradually more fitted for the support of animals and vegetables, as we find them increasing in number, variety and perfection, and approaching more to the nature of those in the present seas'.[14] According to the Wernerian model, no terrestrial life could have existed before the emergence of dry land, which only occurred after marine life was well established. It seems highly likely that Jameson's pre-established acceptance of a progressive version of life's history could have made it easier for him to accept transformist explanations for the progress apparent in the fossil record. There was no need for him to step over the hurdle that a fundamentally static view of the history of life, such as the one that emerges from the notes from Walker's lectures, would have presented. This is likely to have made transformism more acceptable to a Wernerian such as Jameson than to supporters of the other great school of geology in this period, the Huttonians. Hutton's steady-state model of geological processes precluded any mechanism of

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[14] Robert Jameson, *Elements of Geognosy* (Edinburgh, 1808), p.82

universal, directional environmental change which could explain progressive change in the living world.

Jameson's thoughts on the relationship between the history of life and the history of the earth can be charted from the surviving sets of lecture notes written by his students. In his lectures on mineralogy, Jameson divided up the different series of strata by age and type into Primitive, Transition, Secondary, Tertiary and Alluvial following Werner's system. According to a set of notes written by John Borwick taken from Jameson's geology lectures in 1806, the Primitive rocks contained no trace of life, while in the Transition rocks the first evidence of life appeared in the form of corals. Fish and then quadrupeds follow in the Secondary and Tertiary. Quadrupeds similar or identical to modern forms finally made their appearance in the Alluvial. There is thus a clear directionality in Jameson's view of the history of life: 'Corals have been the first animals, fish & last of all animals man.'[15] This is very much the presentation of the history of life on earth that Jameson continued to give throughout his career, according to the notes which survive from his lectures.

The earliest rocks were seen to have been deposited in a universal sea, from which land gradually emerged. In this Jameson closely followed the theories of Werner. According to this model, only marine life could have existed during the formation of the Transition rocks. Terrestrial life had to wait for the emergence of dry land at the beginning of the Secondary period. The picture of the history of life that emerges is essentially the same as presented in Jameson's *Elements of Geognosy*. A set of notes which may be by William Dansey include a detailed description of this process:

> In the oldest of the Transition rocks, which appear to have been formed while the earth was still covered with water, we find the remains only of marine plants and animals, but not a trace of terrestrial organization has ever been discovered. We first meet with such relics in the newest rocks of this

[15] Robert Jameson, Notes on natural history lectures (1806) (taken by John Borwick) (University of Edinburgh Library, Gen.847), f.109

> class, which were formed after a portion of the land was uncovered & capable of supporting vegetation on its surface. From this period to the newest or alluvial, the quantity of vegetable remains increased as well as the variety; & this is further confirmed by a correspondent increase of coal.[16]

Jameson suggested in his lectures that significant climatic change had also occurred over geological time, and that this was reflected in fossil faunas and floras. For example, according to a set of notes made by W.S. Walker in 1830 he used the evidence of fossil tree ferns from the Edinburgh area to deduce that 'at one period the climate was very different from what it is at present and that at the time Britain was calculated to produce plants and animals requiring a much more considerable temperature then the Island possesses at present.'[17] Jameson also suggested that, along with the emergence of dry land as the universal Wernerian ocean receded, directional change in the climate of the world was an important factor in bringing about the changes in plants and animals witnessed by the fossil record. Like Buffon, Jameson seems to have believed that the decreasing temperature at the surface of the earth played an important role in moving forward the story of life. This opinion is confirmed by a note written on the back of a library card for 1824/5. Here Jameson wrote that Transition rocks contained evidence 'of such a temp[erature] as to allow of the growth of organic bodies all of which appear to have been marine.'[18] It seems that Jameson considered that a falling temperature was a prerequisite for the development of life in the oceans. It appears, therefore, that Jameson considered that changes both in temperature and in sea level had played important roles in driving progressive change in the living world. Jameson was far from unique in making a connection between the retreat of a universal ocean and a progressive, or even transformist, history of life. As Pietro Corsi has shown, in France and elsewhere in Europe the 'common problematic horizon shared by the participants in this first

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[16] Robert Jameson, Notes on natural history lectures (1813/14?) (taken by William Dansey?), vol. 3 (University of Edinburgh Library, Dc.3.33), f.254

[17] Robert Jameson, Notes on natural history lectures (1830) (taken by W.S. Walker) (National Library of Scotland, Ms.14148), f.5

[18] [Robert Jameson], Note on the verso of 'Civis Bibliotecae Academiae Edinburgenae a die 12 Octobris 1824, ad diem 12 Octobris 1825' (Edinburgh University Library, Gen.1999/2/3)

debate on the history of life comprised a belief in the aqueous origin of the terrestrial globe, the decrease of the sea level, the emergence of land and the adaptation of marine life to surface and atmospheric condition.'[19]

For the changes apparent from the fossil record to have taken place Jameson believed that a vastly greater period of time than that represented by the records of human history would have been required. In an untitled and undated manuscript fragment in Jameson's handwriting found among his papers, he concluded that it should therefore be no surprise to find that the mummified ibises brought back from Egypt by Geoffroy were identical to modern ones, since they were simply not old enough for any significant change to have occurred. He observed that:

> The characteristic forms of the animals and vegetables ~~which the surface of the earth~~ that now inhabit the surface of the Earth do not appear to have experienced any changes for the most remote antiquity. The Ibis which we find embalmed in the catacombs of Egypt, whose antiquity is almost equally great with that of the pyramids is identical with that which fishes at present upon the banks of the Nile; an identity which evidently proves that the enormous remains of fossil animals found buried in the bosom of the earth do not belong to any of the varieties of the present existing species, but to an order of things very different from that under which we at present live & far beyond the reach of our traditions.[20]

It is worth noting that this is essentially the same argument as was used by both Lamarck and Geoffroy to defend transformism against Cuvier's claim that the mummified Egyptian animals were proof that the transmutation of species did not occur.

Like Cuvier, but unlike Lamarck, Jameson taught that many species found in the fossil record were now extinct. This is quite contrary to Lamarck's opinion that

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[19] Corsi, 'Before Darwin', p.75

[20] [Robert Jameson], untitled manuscript fragment (no date), Jameson Papers (Edinburgh University Library Gen.1999/5), f.2. The corrections present in the manuscript suggest that this was written by Jameson himself rather than being copied from an unidentified source.

fossil species were either the ancestors of existing species or were still awaiting discovery in living form somewhere in the world.[21] Cuvier, on the other hand, was quite sure that his fossil quadrupeds were gone forever and concluded that 'we shall easily perceive that there is very little chance indeed of our ever finding alive those which have only been seen in a fossil state'.[22] Coupled with Cuvier's belief that 'the ancient and now extinct species were as permanent in their forms and characters as those which exist at present',[23] this made it highly probable to him that many of the otherwise unknown species found only in fossil form were indeed extinct. Jameson seems to have come down firmly on the side of Cuvier on the reality of extinction: 'Altho some naturalists will not allow it, it may be proved that several species of organic beings have become extinct.'[24]

Again, echoing Cuvier's model of the history of life, Jameson sometimes seems to have suggested in his lectures that each epoch of geological history had its own fauna and flora and that the forms present in each succeeding epoch were more 'perfect' than those of the last. In a set of lecture notes taken by David Blair Ramsay in 1835/6 it is asserted that in the Secondary rocks the 'animals and plants found here [are] higher in scale than those in transition rocks.'[25] At times the evidence suggests that he discussed the possibility that Cuvier's belief that there was a complete turnover of species between different geological formations was correct. For example, in a set of notes taken in 1830/1 by R.M. M'Cormick he seems to have suggested that between the older and newer parts of the Alluvial formation there was a more or less complete replacement of the fauna: 'The Alluvial Formation

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[21] 'Si il y a des espèces réellement perdues, ce ne peut être, sans doute, que parmi les grands animaux que vivent sur les parties sèches du globe, où l'homme, par l'empire absolu qu'il y exerce, a pu parvenir à détruire tous les individus du quelque-unes de celles qu'il n'a pas voulu conserver ni réduire à la domesticité.', Jean-Baptiste Lamarck, *Philosophie Zoologique* (Paris, 1809), p.76

[22] Cuvier, *Theory of the* Earth, 1st ed., p.61

[23] Ibid., p.115

[24] Jameson, Lecture notes (1806), f.105

[25] Robert Jameson, Notes on natural history lectures (1835/6) (taken by David Blair Ramsay), vol. 2 (University of Glasgow Library MS Cullen 281, f.90

consists of two deposits the deepest or oldest, and the uppermost or newest – the former frequently contains the remains of animals now extinct and the latter of fossil plants and animals of the same species as exist at the present time.'[26] This picture of a stepwise approach to contemporary forms over geological time is also evident in volume 3 of Jameson's *System of Mineralogy* (1808). Here, starting from the earliest rocks containing fossils, he charts the progress of life from its beginnings:

> The organic creation during that period appears to have had a totally different aspect from what it assumed in the succeeding. In the newer formations we find the remains of known genera, and in the newest of all the remains of organic species resembling those found in the present seas.[27]

Jameson did not generally refer to catastrophes to explain the extinction of species, although he did make repeated reference to the Deluge; however, according to M'Cormick's notes he stated that the 'Opinion of Geologists that the Deluge as not universal [is] founded on geological phenomena'.[28] In these notes Jameson accounted for the absence of fossils of antediluvian humans by suggesting that Europe was uninhabited before the Deluge, and Asia, acknowledged to be the cradle of man, had not yet been fully explored by geologists.[29] It is interesting to note that this was also the opinion of William Buckland, who wrote in 1819 that 'The examination of diluvian gravel Central and South Asia would probably lead to the discovery of human bones which is almost the only fact now wanting in the series of phenomena which have resulted from the Mosaic Deluge.'[30] As this quotation is from an unpublished manuscript by Buckland, it may be that Jameson came to this conclusion independently. Alternatively, as Rupke points out, as Buckland cited the authority of Edward Stillingfleet (1635–1699) in support of this idea, it is possible

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[26] Robert Jameson, Notes on natural history lectures (1830/1) (taken by R. M'Cormick) (Wellcome Library, Ms.3358), p.135

[27] Robert Jameson, *System of* Mineralogy, vol. 3 (Edinburgh, 1808), p.81

[28] Jameson, Lecture notes (1830/1), f.150

[29] Jameson, Lecture notes (1830), f.151

[30] Quoted in Nicolaas Rupke, *The Great Chain of History: William Buckland and the English School of Geology (1814–1849)* (Oxford, 1983), p.91

that Jameson's ideas derived directly or indirectly from the same source.[31] According to the set of notes which may be by William Dansey and which are watermarked 1813/14, Jameson seems to have been at some pains to demonstrate the religious orthodoxy of his views and the harmony of natural history and orthodox religion; in one of his relatively rare references to scripture here he emphasised 'the agreement of the Mosaic account, in all its stages with modern mineralogical discoveries'.[32] Nevertheless, the views he presented in his lectures were very far from a literalist reading of the seven days of creation, or even from the modified view of creation espoused by Walker.

Not only is there abundant evidence from Jameson's writings and lecture notes that a Wernerian theory of the earth and a progressive model of the history of life were closely linked in Jameson's thought, but a significant number of articles that developed more or less the same model appeared in Jameson's *Edinburgh New Philosophical Journal*. As noted in chapter 3, the *Journal* published five unabashedly transformist articles between 1826 and 1829. However, in addition to these articles, there were also a number of papers published around this time which presented a view of the history of life fully compatible with transformism, but which did not speculate regarding the mechanisms behind the changes observed in the geological record. These articles generally present the fossil record as exhibiting gradual, progressive change. Often they suggest that environmental change was in some way the motor for the changes observed in fossil animals and plants. I have identified three articles that meet these criteria published between 1826 and 1830. The first paper entitled 'Geological Observations'(1826) was by Ami Boué, who is known to have been an admirer of Lamarck and Geoffroy and an advocate of spontaneous generation.[33] After noting that 'the farther we penetrate into the crust of the earth, the more simplicity do we observe in the vegetable and animal productions', he

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[31] E. Stillingfleet, *Origines Sacrae: or a Rational Account of the Grounds of Natural and Revealed Religion*, 2 vols (Oxford 1797)
32 Jameson, Lecture notes (1813/14), vol 1, f.5
[33] Laurent, 'Ami Boué'

speculated that this was due to a greater equality of temperature across the globe before concluding that as 'the zones and climates gradually became established, the vegetables and animals became diversified.'[34] Boué does not elaborate on what he means by 'diversified', but it seem logical to conclude that he imagined new species arose in some manner in response to changes in their physical environment. In the following year Jens Esmark (1762–1839), the Danish-Norwegian mineralogist, published a piece in which he speculated that the earth might have been devoid of life for several thousand years after the creation, and that 'organisation did not begin till this long period was completed, which the earth required to the full development of its own constitution; that, after it began, it proceeded by successive steps from the less to the more perfect formations, ending with man as the head of the whole.'[35] Again, we are presented with a model where the appearance of life was made possible by changes in the physical environment; after life became established, it then gradually increased in complexity. A similar paper was published anonymously in 1830. Here again we find the same gradual, progressive history of life as we have seen in the preceding papers:

> It is, notwithstanding, always of much importance to be able to look into the facts already established, and to observe that the gradual development of organic bodies in the animal and vegetable kingdom has followed precisely the same progress. While the simplest organised kinds of both kingdoms first appear, we also find repeated throughout the same gradations, as regards the gradual appearance and increase of the most perfectly organised beings in the strata of the earth's crust.[36]

In 1835 a paper by Alphonse de Candolle on the fossil history of plants appeared which shared this view of the progressive development of life. In it de Candolle stated that:

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[34] Boué, 'Geological Observation', p.90

[35] Jens Esmark, 'Remarks tending to explain the Geological History of the Earth', *Edinburgh New Philosophical Journal* 2 (1827), pp.120–1

[36] Anon, 'Remarks on the Ancient Flora of the Earth', *Edinburgh New Philosophical Journal* 8 (1830), p.127

> With these results before us, we can recognise with M. Brongniart, that the greater number of organs, and those the most distinct, have succeeded to the less perfect ones; in other words, that the vegetable kingdom appears to have been gradually becoming more perfect. This law of gradual development would hence appear to exist in the vegetable, as it has been supposed to exist in the animal kingdom.[37]

Speculations about the progressive nature of the history of life on earth were not restricted to the *Edinburgh New Philosophical Journal*. In volume 7 of the Memoirs of the Wernerian Natural History Society, which covers the period 1831–37, we find a lengthy article by Robert James Hay Cunningham presenting an unambiguously progressive picture of the history of life. This article, entitled 'On the geology of the Lothian', won the prize offered by the Society in 1836 for the 'best geological report of the Lothians'. In his paper Cunningham stated that

> If geologists have, in the course of their investigations, come to any certainty concerning the ancient state of our globe, there is certainly no one doctrine supported by a greater number of facts, than that of progressive development. Many remains have been adduced as belonging to beings, which held a place in the zoological scale, higher than was consistent with this theory. With one exception, however, all these remains have been found, on more accurate and better conducted examination, to be, instead of dissentient facts beautiful proofs of its truth.[38]

Cunningham was in no doubt that this one controversial exception, the famous Stonesfield mammal found in sediments considered too old to contain mammalian remains, would sooner or later be satisfactorily explained. While no mechanism is proposed here for the 'progressive development' evident in the fossil record, this paper is clearly open to a transformist interpretation. It was presumably on these grounds that John Fleming was later to criticise the progressivist aspect of this paper in his *Lithology of Edinburgh* (1859).[39]

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[37] Alphonse de Candolle, 'On the history of fossil vegetables', *Edinburgh New Philosophical Journal* 18 (1835), pp.98–9
[38] Robert James Hay Cunningham, 'On the geology of the Lothians', *Memoirs of the Wernerian Natural History Society* 7 (1831–37), p.9
[39] Fleming, *The Lithology of Edinburgh*, p.15

The flow of articles speculating on the history of life in Edinburgh natural history journals seems to have slowed after the mid-1830s, but in 1850, we find Fleming, at that time the newly appointed professor of natural science at the New College of the Free Church, attacking both the *Edinburgh New Philosophical Journal* and Jameson himself for their support of transformism in his inaugural lecture. In this lecture he explicitly linked Jameson's Wernerian geology with transformism. The motive for his assault was his perception that Jameson and his journal were sympathetic to the transformist speculations embodied in *Vestiges of the Natural History of Creation* (1844) and its sequel, *Explanations* (1845). A short but generally positive comment on *Vestiges* had appeared in the 'New publications received' section of Jameson's journal. In his lecture Fleming commented that:

> In the only scientific periodical in Scotland, *The Edinburgh New Philosophical Review* [sic], conducted by Professor Jameson, this work was noticed in the following laudatory terms:– 'Although we do not agree with the ingenuous author of this interesting volume in several of his speculations, yet we can safely recommend it to the attention of our readers, who will perceive, from the subjoined table of contents, that the subjects discussed are of an attractive kind.'[40]

To make matters worse, the journal had again pronounced a favourable verdict on *Explanations*, commenting that the author had resolved many of the faults that had marred his original work. Fleming was not impressed: 'Yet again we have the same *Edinburgh New Philosophical Journal* declaring:– "These 'Explanations' sufficiently prove that the author has met with great effect the arguments of his distinguished opponents! [Exclamation mark added by Fleming.]"'[41] Even though these two very brief comments on *Vestiges* and *Explanations* hardly count as a wholesale

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[40] John Fleming, 'Natural science', in *Inauguration of the New College of the Free Church, Edinburgh, November, M.DCCC.L. with Introductory Lectures on Theology, Philosophy and Natural Science* (Edinburgh, 1851), p.218. Fleming's quotation comes from [Robert Jameson], 'New publications received', *Edinburgh New Philosophical Journal* 38 (1845), p.186

[41] Ibid, p.218. Fleming's quotation comes from [Robert Jameson], 'New publications received', *Edinburgh New Philosophical Journal* 40 (1846), p.400

endorsement of transformism, they certainly indicate that the journal, and its editor, continued to be surprisingly open to new ideas at a time when there was a violent controversy raging around the subject. Fleming was in no doubt what was the source of Jameson's indulgent attitude towards the development hypothesis, as Chambers' brand of transformism was generally known. He linked it directly to Jameson's Wernerian geology, of which Fleming had himself been a supporter in his younger days.

> Subsequent to the rise of this Scottish geology of Hutton, the German geology of Werner was introduced, and for a while appeared to triumph. This system, equally indifferent to the truths of palaeontology, and outraging all philosophy by the extravagance of its assumptions, paved the way for those reveries of progressive development with which of late years we have been inundated. In Jameson's "Geognosy," it is stated, in reference to organic remains, "Those which occur in the earliest periods belong to the lowest and most imperfect class of animals, the zoophytes. In the newer and newer formations, we meet with quantities of shells and fish, and these are accompanied by a variety of marine plants."[42]

Fleming then included several more quotations from Jameson's *Elements of Geognosy* to illustrate that the Wernerian theory was by its nature progressive, and hence was in accord with progressive, transformist models of the history of life, such as the theory developed in *Vestiges*. As we have seen, the gradual appearance of increasingly perfect forms of life as the waters retreated and made the earth gradually fit to receive them is quite explicitly stated in Jameson's writings on geology.[43] It may well be that what Fleming was trying to do here is to discredit transformism by linking it to what was, by 1850, a thoroughly discredited theory of the earth. Although his comments were meant as a damning judgement on both theories, as we have already noted above, they also establish a link between the progressive Wernerian theory of the earth and transformism in Fleming's mind, which must also have been apparent to his contemporaries. Having examined some

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[42] Ibid., p.216. Fleming's quotation comes from Jameson, *Elements of Geognosy*, p.80

[43] Jameson, *Elements of Geognosy*, pp.80–2

of the articles from the *Edinburgh New Philosophical Journal* that hinted at a transformist interpretation of the history of life, I turn now to those which proclaimed it openly.

Transformist interpretations of the fossil record and the history of life

The best known of the anonymous transformist articles published in the *Edinburgh New Philosophical Journal* was entitled 'Observations on the nature and importance of geology' (1826). There has been considerable debate over the authorship of this article. However, for the reasons discussed in chapter 3, it is probably reasonable to assume that it was either by Jameson or by Ami Boué, his former student and fellow Wernerian geologist. The paper is unusual for transformist writings from the period in that it directly and explicitly engaged with Lamarck's theories, while the others generally avoid naming their sources. The anonymous author made a strong case that geology alone could give concrete evidence for the truth of Lamarck's theory and noted that the gradual appearance of increasingly perfect forms over geological time already provided strong support for the transmutation of species. He then tackled the subject of extinction, first asking if changes in environmental factors might have led to the extinction of some species, whether 'from a change in the media in which organic creatures lived, and from powerful causes operating upon them, their power of propagation may be weakened, and at length become perfectly extinct?'[44] This is counter to the view of Lamarck, who considered that no species ever became extinct, except perhaps for those occasionally driven to extinction by human agency. Instead, according to Lamarck, apparently lost fossil species had either transformed into new ones over time, or had simply not yet been discovered in living form. [45] No firm conclusion was reached by the author on the subject of extinction, however, and he was also prepared to countenance Lamarck's idea that 'many fossil species to which no originals can be found, may not be extinct, but have

[44] Ibid., p.298
[45] Lamarck, *Philosophie Zoologique*, p.76

gradually passed into others.'[46] Finally, and before moving on to discuss the utility of geology for solving the problems of biogeography, he addressed the observation made by Cuvier that the mummified ibises brought back by Geoffroy in the aftermath of Napoleon's ill-fated expedition to Egypt were identical to the modern species.[47] The author dismissed this objection utilising the same argument we have already seen used elsewhere by Jameson, Geoffroy and Lamarck, that the time that had passed since the time of the pharaohs was insufficient for any change to be evident: 'what are a few thousand years to which the mummy refers, in comparison with the age of the world, as its history is related by geology.'[48] What emerges is a broadly Lamarckian model of transformism, although the role played by environmental change is more reminiscent of Geoffroy's theories.

The following year, the *Edinburgh New Philosophical Journal* published an article entitled 'Of the changes which life has experienced on the globe'. This has received less attention than the 1826 article discussed above, although it has been suggested by Desmond that it might have been by Grant.[49] Although Grant is certainly a likely candidate, there is no concrete information that would allow authorship to be confidently assigned, so I will regard it here simply as evidence of the kind of articles that could be published in the *Journal* under Jameson's editorship in the 1820s. That it was not by Jameson, at least, is made almost certain by the references to the important role of volcanism and 'the original igneous state of the earth', which would be incompatible with his Wernerian views on the history of the globe.[50] This would not, however, necessarily rule out Boué, who was not as zealous a Wernerian as Jameson.[51] The article opens with a reference to the important role of

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[46] Anon, 'Observations on the nature and importance of geology', *Edinburgh New Philosophical Journal* 1 (1826), p.298
[47] Georges Cuvier, *Recherches sur les Ossements Fossiles de Quadrupèdes* (Paris, 1812), p.80
[48] Anon, 'Nature and importance of geology', p.299
[49] Desmond, *Politics of Evolution*, p.446
[50] Anon, 'Of the changes which life has experienced on the globe', p.299
[51] In an appendix to his autobiography he admitted that in 1820 he was 'extremely plutonist in comparison to my master Professor Jameson'. Ami Boué, Catalogue des œuvres, travaux, mémoires et notices du Dr Ami Boué', *Autobiographie pour mes amis* (Vienna, 1876), p.II

fossils as evidence of 'the history and successive changes of the various races that existed before the present'.[52] The author then goes on to establish two types of causes at work in the natural world. The first and most important act gradually but inexorably: 'The differences which vegetables and animals exhibit at the present day, according to the various climates or situations in which they occur, have been gradually established under the predominating influence of a small number of natural causes, and constitute at length the order of distribution which life now presents at the surface of the earth.'[53] He then proceeds to establish the nature of these causes:

> These gradual variations in the temperature, the lowering of the general level of the seas, the equally successive and gradual diminution of the energy of volcanic phenomena arising from the original igneous state of the earth, as well as the strength and power of atmospheric phenomena, and of the tides – such were the regular, general, and continued natural causes of the modifications which life has undergone …[54]

The author then calls the fossil record as a witness to 'the successive and gradual change which we have pointed out.'[55] The second, and less significant type of cause to which the author then turns consists of 'the irregular, and more or less violent and perturbing secondary causes of the partial vicissitudes experienced by animal and vegetable life.'[56] The catastrophes of Cuvier are therefore consigned to a secondary role and their impact on the history of life minimised. These secondary causes appear to consist largely of local floods and changes in sea level. In its model of double causation this is reminiscent of Lamarck, whose theory included both a continuously acting innate tendency towards progressive change and a secondary mechanism that depended on the effects of unpredictable environmental changes, which disrupted the simple pattern of development that would otherwise have

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[52] Anon, 'On the changes which life has experienced on the globe' *Edinburgh New Philosophical Journal* 3 (1827), p.298
[53] Ibid., pp.298–9
[54] Ibid., p.299
[55] Ibid., p.300
[56] Ibid., pp.299–300

prevailed. He differed radically with Lamarck, however, over the nature of the primary cause of transmutation, which he attributed to the effect of environmental change rather than an innate tendency of living things to become more perfect even in a constant environment. Finally, the author expresses his overwhelming confidence in the correctness of his theory and appeals to its compatibility with natural law as confirmation: 'Our theory, which is founded on all the facts that have been established, cannot but prevail over the systems hitherto established, for it is in harmony with the natural laws of order and permanency which rule the universe'.[57]

In addition to papers that provided a sweeping overview of the history of life, in 1828 the *Journal* also published an account of experiments conducted by Bory de Vincent on spontaneous generation in its 'Scientific intelligence' section. After an account of the experiments in which Bory claimed to have produced life from inorganic matter, the reader is warned that 'many philosophers will probably refuse to admit the consequences, which the author would draw from these facts, for attributing to matter a general disposition to become organised, which would be independent of the ordinary mode of generation.'[58] Despite this note of caution from the editor, readers would have been free to make up their own minds about the results of Bory's experiments based on this account.

Aside from the anonymous articles, whose authorship remains doubtful, the remaining two transformist papers in Jameson's *Journal* were written by, and attributed to, Robert Grant. Grant published a series of sixteen papers between 1825 and 1827 in the *Edinburgh Philosophical Journal* and its successor the *Edinburgh New Philosophical Journal*. These papers mostly dealt with aspects of the biology of invertebrate animals from the Firth of Forth. Two of them contain explicitly transformist themes. It is in a paper published in 1826 'On the structure and nature of the Spongilla friabilis' that we find the first statement in print of Grant's

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[57] Ibid., pp.300–1

[58] Anon, 'On the tendency of matter to become organized', *Edinburgh New Philosophical Journal* 4 (1828), p.195–6

transformist views. Towards the end of the article, he speculated regarding the relationship between the freshwater sponge *Spongilla* and the more complex marine sponges:

> From this greater simplicity of structure and internal texture, we are forced to consider it as more ancient than marine sponges, and most probably their original parent; and, as its descendants have greatly improved their organization, during many changes that have taken place in the composition of the ocean, while the spongilla, living constantly in the same unaltered medium, has retained its primitive simplicity, it is highly probable that the vast abyss, in which the spongilla originated and left its progeny, was fresh, and has gradually become saline, by the materials brought to it by rivers, like the salt lakes of Persia and Siberia.[59]

Grant here gives a concrete example of the principle expounded by Geoffroy in his 'Organisation des gavials' that when the 'physical and chemical agents' to which an organism is exposed remain the same, so does the development of the organism, but when conditions change, the development of the organism exposed to these new conditions will be modified by them, provided the change is not so great as to kill it.[60] Grant then goes on to give his evidence for the alleged primitive character of this freshwater sponge:

> The great looseness and softness of its texture, and the width and defenceless condition of its openings, which now render the spongilla a safe retreat, and a convenient magazine of food for myriads of animalcules and aquatic insects, and a fit receptacle for ova, obscurely indicate the unpeopled state of the waters of the globe, and consequent absence of these numerous assailants, at the period of the first formation of this zoophyte; and its

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[59] Grant, 'Structure and nature of the Spongilla friabilis', pp.283–4

[60] Geoffroy, 'Organisation des gavials', pp.151–2. The explanation given above is a simplified paraphrase of the rather convoluted French original, which reads: 'Que les décompositions animals, le reformations et les nouvelles compositions se passent dans un même milieu et sous l'action des même agens physique et chimiques, les mutations se reproduisent de la même manière; d'où, à chaque metamorphose, c'est à dire, dans chaque âge, les êtres places sous les influences restent des repetitions exacts les uns des autres. Mais que, tout au contraire, il en soit autrement: des nouvelles ordonées, si elles interviennent sans ropre l'action vitale, font varier nécessairement les êtres qui en ressentent les effets; chaque fois, c'en est une consequence toute naturelle, dans le degree de leur puissance modificatrice.'

> aptness for secreting silica, and the abundance of that earth in its skeleton, show the period of its creation to have been nearly synchronous with that of the siliceous or primitive rocks.[61]

Later the same year Grant repeated his views on the evolution of sponges in a paper on the structure of silicious sponges published in the first number of the *Edinburgh New Philosophical Journal*. Here Grant suggested a family tree of sponges based on the form of the spicula which make up the skeletons of many species. He traced the development of the spicula from the simple forms found in freshwater sponges through three stages of increasing complexity, first to forms where 'the unnecessary and probably hurtful embedded point has been removed' and finally to the most complex jointed spiculum.[62] Grant relates these changes directly to function, as he considered that the more advanced forms were better suited for defending the sponge against predators, as 'at the time of its formation, animalicules of larger magnitude swarmed in the heated ocean'.[63] Grant goes on to make an explicit link between these most primitive of animals and inorganic chemical processes. He noted that:

> The appearance of many of their crystalline silicious pointed specula is the same with that of the slender hexahedral acuminated prisms which silica naturally assumes in the crystallized state; and the silicious crystals formed by nature contain cavities and fluids like those formed by organic life. The laws, therefore, which regulate the forms of the simplest silicious spicula composing the skeleton of the marine sponge, do not appear to differ much from those which regulate the forms of brute matter.[64]

This vision of the organic grading into the inorganic as a function of increasing organisation and of living things being subject to the same laws of nature as regulated non-living matter represented a powerful challenge to the vitalist

[61] Grant, 'Structure and nature of the Spongilla friabilis', p.284
[62] Grant, 'Observations on the structure of some silicious sponges', p.350
[63] Ibid., p.350
[64] Ibid., p.351

opinions held by many of Grant's contemporaries, including Grant's friend and patron John Fleming.

A third transformist paper by Grant, this time on the reproduction of the sponge *Lobularia digitata*, was published in David Brewster's *Edinburgh Journal of Science* in 1828. In this paper Grant gave an insight into his views on the fundamental constitution of living things and its relation to the transmutation of species. He first noted that the ova of the sponge were transformed from 'moving, irritable, and free condition of animalcules, to that of fixed and almost inert zoophytes'.[65] He went on to note that freshwater algae (Confervae) had been observed to both resolve themselves into animalcules and that these animalcules could then reunite to reconstitute the plants. He then expressed the belief that 'Mosses and Equiseta are found to originate from confervae, … and all the land confervae with radicles appear to pass into the state of more perfect plants.'[66] The implication is that there is a gradual development within the plant kingdom, and by extension in the animal kingdom too, from animalcules through simple forms to the most perfect types. The animalcules therefore represent the starting point for the development of higher forms and the basic units of life from which higher forms are constructed. Sloane has taken this paper, together with Grant's statement in his *Introductory Lecture* at the University of London that the 'Animal and Vegetable Kingdoms are so intimately blended at their origins, that Naturalists are at present divided in opinion as to the kingdom to which many well-known substances belong', as evidence that Grant went as far as to postulate a common origin for plants and animals.[67] If this were the case, it would put him entirely at odds with Lamarck on the common origin of plants and animals, for according to Lamarck, who wrote of animals and

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[65] Robert E. Grant, 'Observations on the generation of the Lobularia digitata, Lam. (Alcyonium lobatum, Pall.)', *Edinburgh Journal of Science* 8 (1828), p.110
[66] Ibid., p.110
[67] P.R. Sloan, 'Darwin's invertebrate program, 1826–1836: Preconditions for transformism', in D. Kohn, *The Darwinian Heritage* (Princeton, 1985), pp.83–4; Robert E. Grant, *Introductory Essay on the Study of the Animal Kingdom: Being an Introductory Lecture Delivered in the University of London, on the 23rd of October, 1828* (London, 1829), p.20

plants in his *Histoire Naturelle des Animaux sans Vertèbres* that 'the two branches of which I have just spoken are in reality separated from each other at their base, and a positive characteristic that relates to the chemical constitution of the bodies on which nature has worked makes an eminent distinction between the beings which are embraced by one of its branches and those which belong to the other .'[68] Sloan has suggested that Grant's ideas were derived from Friedrich Tiedemann, who wrote 'One might even be almost tempted to believe that, in certain circumstances, the most simple vegetable and animal forms may pass from one to the other. Confervae are resolved into infusoria, and infusoria produce confervae by their union.'[69] The book from which this quotation comes could not be the source of Grant's ideas, as the first edition was not published in Germany until 1830, but Grant may have received these ideas through other channels.[70] Sloan has argued that Grant's use of the term 'zoophyte' to designate sponges, a term abandoned by Lamarck because he rejected the idea of intermediate forms between plant and animals, indicates that Grant saw sponges as intermediate plant-animals. However, Grant consistently referred to sponges as 'animals' in his papers on them in the *Edinburgh Philosophical* and the *Edinburgh New Philosophical Journals* in the 1820s and nowhere suggested that they should be treated as intermediate forms between the two kingdoms. It seems more likely that Grant considered the link between plants and animals to exist at the level of primordial animalcules rather than more complex organisms. His use of the term 'zoophyte' may simply have been a matter of convention.

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[68] Lamarck, *Animaux sans Vertèbres*, vol. 1, p.84. The original French text reads: 'chacune des branches dont je viens de parler se trouve réellement séparée de l'autre à sa base; et qu'un character positif, que tient à la nature chimique des corps sur lesquels la nature a opéré, fournit une distinction éminente entre les êtres qu'embrasse l'une de ces branches, et ceux que appartiennent à l'autre'.

[69] Quoted in Sloan, 'Darwin's invertebrate program', p.78; from Frederick Tiedemann, *A Systematic Treatise on Comparative Physiology, Introductory to the Physiology of Man* (trans J.M. Gully and J.H. Lane) (London, 1834)

[70] Friedrich Tiedemann, *Physiologie des Menschen* (Darmstadt, 1830)

What is undeniable is that Grant saw animalcules as the origin of all life on earth. As he was to say in his lectures as professor of comparative anatomy at the newly founded University of London, which were subsequently published in *The Lancet* in 1833–4:

> When we speak of animals low in the scale, it is equivalent to our speaking of animal forms that have existed in the primitive conditions of this planet; for everything shows, that this kingdom itself has had a development from the most simple forms, and that in the first condition very likely nothing existed but myriads of animalcules swimming in the heated ocean that encompassed this cooling planet. It matters not whether we fancy that those now existing are the same forms changed by circumstances, or that there have been at every successive instant new creations; naturalists differ on that speculation; – almost an idle speculation, where we can only arrive at a probability.[71]

It is noteworthy here that Grant seems somewhat reticent about transformism, making it clear to his students that it was just 'speculation'. It is also interesting to note that in his published 'General view of the characters and the distribution of extinct animals' (1838) he chose to point out that 'the known order of distribution of animal forms, in the strata of the earth, is in perfect accordance with the ordinary laws of animal development, and with the order of creation described in Holy Writ.'[72] By the time these texts were written, Grant was a professor at the new University of London, and it is perhaps feasible that Grant felt he had to hedge his bets to a greater extent in the more hostile atmosphere of London in the 1830s than was necessary in the more tolerant Edinburgh of the previous decade.[73]

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[71] Robert Grant, 'University of London lectures on comparative anatomy and animal physiology: Lecture VI. On the organs of support of acephala and echinoderma', *The Lancet* 1 (1833–4), p.276

[72] Robert E. Grant, 'General view of the characters and the distribution of extinct animals', *British Annual, and Epitome of the Progress of Science for 1839* (ed. Robert D Thomson) (London, 1838), p.281

[73] For a compelling account of the opposition faced by Grant and others who shared his view in London in this period, see Desmond, *The Politics of Evolution*

Grant's last article in any of the Edinburgh journals was a piece in the *Edinburgh New Philosophical Journal* in 1834, and his final contribution to the *Memoirs of the Wernerian Society* was in 1832, so it seems that his association with Edinburgh natural history circles may have gradually faded into the background in the course of his first decade in London. Nevertheless, the evidence from his teaching and writing during his early years in London can still give a useful insight into his earlier opinions, assuming that his views did not radically change on leaving Edinburgh. On 23 October 1828 he gave an introductory lecture that was subsequently published. This document contains no clear and unambiguous references to transformism. The closest Grant comes is when he asserts 'that animal life originated and was developed in the bosom of the deep.'[74] He does, however, make explicit reference to spontaneous generation. He notes that 'From numerous experiments, Naturalists have been led to believe that the simplest organized bodies, as *Monads* and *Globulinae*, originate spontaneously from matter in a fluid state, and that these simple bodies, of spontaneous origin, are the same with the gelatinous globules which compose the soft parts of Animals and Plants.'[75] This statement relates very closely to the comments he made on the same subject in his *Edinburgh Journal of Science* paper of 1828 noted above.

In general, the views expressed in Grant's introductory lecture seem often to be more in tune with the theories of Geoffroy, or even Cuvier, than Lamarck. For example, he clearly believed that many species had become extinct over the course of geological time, or as he puts it 'Numberless species, and even entire *genera* and tribes of animals, the links which once connected the existing races, have long since begun and finished their career'.[76] By contrast, Lamarck believed that the species found in fossil form had either been transformed into the ones we now find inhabiting the earth, or had simply yet to be discovered in living form. Grant came very close to the catastrophism of Cuvier when he remarks that 'By thus pointing

[74] Grant, *An Essay on the Study of the Animal Kingdom*, p.17
[75] Ibid., p.18
[76] Ibid., p.12

out the extensive and terrible catastrophes to which the Animal Kingdom has often been subjected, we are enabled to perceive a cause of the many apparent interruptions in the chain of existing species.'[77] Once again, this statement is quite at odds with the gradualism apparent in Lamarck's vision of the history of life. It is clear that his transformism was strongly influenced by Geoffroy, especially in the role he gave to directional environmental change in bringing about transmutation, although his use of catastrophes as agents of change was a radical departure from the theories of either Lamarck or Geoffroy.

Grant made explicit reference to Serres' and Geoffroy's recapitulation theory early in his *Introductory Lecture*, but without linking it to an evolutionary succession. Instead, he simply noted that 'by comparing the human brain in the earliest stages of development, with the permanent forms of that organ in quadrupeds, birds, reptiles, and fishes, the most singular resemblances have been discovered, which throw new light on the gradual development of that organ in the most perfect animals, and on its remarkable structure in the inferior classes.'[78] This debt to Geoffroy is even more apparent elsewhere in an anonymous set of student's lecture notes from the session 1833/4 that survive from Grant's years in London. Here he went further in that he explicitly linked foetal development to the appearance of new forms over geological time:

> the development of every organ of the human body can be traced thro' all its successive stages in the great body of the animal kingdom & the form w[hi]ch an organ presents in each of the lower classes corresponds with its condition at some period of the human embryo. But the researches of the Com[parati]ve Anatomists are not confined to existing races of animals. In the remains of animals entombed in the ancient strata of the earth, he is enabled to trace phases of organic development on the surface of our planet,

[77] Ibid., p.17
[78] Grant, *An Essay on the Study of the Animal Kingdom*, p.3

> w[hi]ch have long preceded the existing forms, & have been distinct ant[erio]r to the existence of our race, & of all the vertebrate tribes.[79]

A very significant proportion of Grant's lectures on comparative anatomy at the University of London seem to have been taken up with an exposition and defence of Geoffroy's theory of unity of plan. In the notes from the 1833/34 session, he followed Geoffroy in finding 'a unity of plan in the organization of the whole animal kingdom.'[80] This implies a radical rejection of Cuvier's four *embranchements*, or fundamental divisions of animal life. He went to particular pains to establish that the cephalopods provided a link between the invertebrates and the vertebrates, providing a host of pieces of anatomical evidence in support of his claim; for example: 'As we find in the class of fishes, remains of the ext[erna]l shells in the form of calcareous scales or plates or solid spines, so we find in the cephalopods the soft cartilaginous rudiments of the vertebral column, which is met with in the myxene & lampreys & other of the lowest cartilaginous fishes.'[81] As we saw in chapter 2, this very question was the bone of contention between Geoffroy and Cuvier in their famous debate at the Académie des Sciences in Paris in 1830. According to the printed syllabus for Grant's 1830 lecture course in 'Comparative Anatomy in Zoology', the Zoology course ended with a section on 'Relations between the Extinct and Existing species of animals. Revolutions in the Animal Kingdom indicated by Fossil Animals.' [82] It would be interesting to know what the content of this final lecture was, but sadly no notes from this part of the course seem to have survived.

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[79] Robert Grant, Notes on 'Lectures on Comparative Anatomy Delivered in the University of London by Robert E Grant M.D. F.R.S. &c. Session 1833 1834' (anonymous student) (UCL Library, MS ADD 38 (box 19)), f.3

[80] Ibid., f.2

[81] Ibid., f.156

[82] Robert Grant, 'Comparative Anatomy and Zoology. Professor, Robert E. Grant, M.D. F.L.S., F.R.S.E.' (London, 1830), printed syllabus bound in with student's notes on 'Lectures in Comparative Anatomy' (UCL Library, MS ADD 1 (box1)), f.6

A figure whose writings on transformism bear striking parallels to Grant's model of the transmutation of species was his fellow extra-mural lecturer John Fletcher. As we have seen in chapter 4, Fletcher was not himself a transformist, but he did discuss transformism at considerable length in his *Rudiments of Physiology* (1835–7), which was based closely on the syllabus of his extra-mural lecture course. Fletcher cited Grant's *Lectures of Comparative Anatomy* (1833–4) frequently throughout his book, and was clearly thoroughly familiar with his ideas. Like Grant, Fletcher believed that the simplest types of animals and plants converge to a common fundamental form, and that the most simple animals and plants could even be converted into one another:

> it is not the highest, but the lowest tribes of plants which are most intimately related to the lowest tribes of animals – both being, in fact, only one remove from minerals, and perhaps, if not identical with each other, at least mutually convertible; and it is from this point, or the common foundation of both, that Nature seems to arrive by two different roads, till she arrives, by the one at the highest point of the vegetable, and by the other at the highest point of the animal kingdom.[83]

In this Fletcher is in perfect agreement with Grant's article published in the *Edinburgh Journal of Science* in 1828 quoted above. He then went on to present a model of transformism strikingly similar to Grant's in the following passage from the same chapter:

> It has been conjectured that, in the infancy of the organic kingdom of nature, and long after the establishment of the inorganic, none but the simplest possible tribes of plants and animals, as the fungi and polypi, existed. Many of these, it is supposed, continued to be propagated by either simple division or germs; while some of them on the contrary, under the influence of different external circumstances connected with the changes to which the globe itself was gradually subjected, underwent, on the progressive supply with their aliment of fresh materials, in the forms of monades or organic molecules – the supposed nature of which will be elsewhere explained – a greater development than was consistent with the retention of their original

[83] Fletcher, *Rudiments of Physiology*, pp.7–8

> characters, and hence resulted perhaps some higher tribes of acotyledonous plants, and among animals, the mollusca and articulate.[84]

Fletcher then extends this account of the history of life from molluscs and articulates as far as the human species before noting that the fossil record, in which increasingly higher forms had gradually appeared in the course of the geological record, had been presented as evidence for the reality of the transmutation of species by transformist thinkers. This account of the history of life, driven by directional change in the environment, is easily comparable with the theories of Geoffroy and Grant. Fletcher, however, while not denying the reality of the progressive model of the history of life as revealed by the fossil record, did not believe that a transformist interpretation of this phenomenon was plausible. He did not consider that the appearance of new forms over time in the fossil record afforded 'any proof of their mutual convertibility, or indicate any thing more than that the character of its inhabitants has, at different times, varied with that of the globe.'[85] The evidence from Fletcher's books shows clearly that Geoffroy's model of transformism was widely known and discussed in Edinburgh's extra-mural medical schools. Although Fletcher finally rejected the transmutation of species, the model of transformism that he presented in his *Rudiments of Physiology* was essentially the same as that espoused by Grant.

The year before Grant left for London another figure who would be inspired by the theories of Lamarck and Geoffroy to develop his own ideas on the transmutation of species arrived in Edinburgh to study medicine. His name was Henry H. Cheek. As for Grant, Cheek's writings on transformism present a model that was much closer to that of Geoffroy than to Lamarck. Like Geoffroy, he suggested that the environment, while not causing adaptive change directly, did exert a selective pressure on variant forms that arose, eliminating those forms which are ill-adapted

[84] Ibid., pp.12–13
[85] Ibid., p.16

to the environment in which they found themselves. In the dissertation he read to the Royal Medical Society of Edinburgh in January 1830 he asserted that

> the climate does not cause the change – but that there is an ultimate Zoological law, that structures have a tendency to change for adaption to new functions – that the indigenous races are adapted to the climates in which they are found – and that, if a race be removed to a new region it will either become adapted to the new functions required, by the powers of organization, or that it will propagate a sickly & imbecile offspring, and ultimately perish.[86]

The transmutations themselves, on which the selective action of the environment can operate, occur through the action of a 'Zoological law', which appears to manifest itself as an innate tendency to produce new variations in living things. The paragraph quoted from Cheek's essay above is followed by this enigmatic passage:

> The question as to the unity of origin is not necessarily connected with the inquiry as to identity of species. Founded on historical & traditional considerations, the supposition of a single origin for every species does not partake of the nature of a fact to be admitted in Zoology. I believe it to be impossible, and am prepared with my proofs. [87]

Taking 'single' to mean 'separate', this would seem to be a strong affirmation of Cheek's belief that all species have a common origin, and that the contrary opinion was only the result of 'historical and traditional' beliefs obscuring the truth. However, taking 'every species' to mean 'all species', he would seem to be saying the opposite. Taken in the context of what has gone before, the first interpretation appears the more likely, although Cheek seems to have chosen to express himself in a way that is curiously opaque and ambiguous. Of the proofs he had prepared, he sadly had no more to say. Cheek then ended the dissertation on a curiously disconsolate note, remarking 'I am so much dissatisfied at the mode in which I am obliged to treat this problem which I have selected from a transcendental

[86] Henry Cheek, 'On the varieties of the human race', The Royal Medical Society, Dissertations 91 (1829–30) (Library of the Royal Medical Society), p.301
[87] Cheek, 'On the varieties of the human race', p.307

philosophy that it will be better now to terminate.'[88] Who or what had obliged him to treat the problem in a way that he seemed to find so unsatisfying is unfortunately left to the imagination of the reader. Cheek's dissertation raises as many questions as it answers. What we are not left in doubt about, is that Cheek was a convinced transformist.

In an article on the dugong in the December 1829 issue of the *Edinburgh Journal of Natural and Geographical Sciences*, Cheek appeared to combine a developmental vision of the history of life with the concept of unity of form associated with the philosophical anatomy of Geoffroy. In this article he noted that dugongs are 'nothing else than terrestrial mammalia, whose internal organs are concealed under the figure of a fish.' He goes on to suggest that:

> Speculation immediately suggests the geological fact, that fishes existed prior to the creation of the mammalia; and that the Omnipotent has passed by slow gradations from one series of organization to another; that the type or model on which all vertebrate animals are formed is essentially the same.[89]

As we have seen above, the fact that nature had brought forth species progressively over time in an ascending scale of perfection did not necessarily imply the transmutation of species, but was equally compatible with multiple creations. However, the fact that Cheek commented on the slow rate at which the series of organisation pass into each other strongly implies that it was a process of transmutation that he has in mind. It would have made little sense to talk about the speed at which each series had passed into the next if each had been an entirely separate creation.

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88 Ibid., p.307

89 [Henry Cheek], 'On the Natural History of the Dugong, (*Halicore Indicus*, Desm.) – the Mermaid of Early Writers, and particularly on the differences which occur in its Dental Characters', *Edinburgh Journal of Natural and Geographical Science* 1 (December 1829), p.162

Another reference to unity of type is found in the March 1831 issue in an anonymous editorial in Cheek's journal, almost certainly by Cheek himself, on the identity of the vascular arches of terrestrial vertebrates with the branchial arches of fish. This article appeared in a section of the journal entitled 'Zoological Collections'. Cheek noted that from the evidence of the developmental identity of these structures in fish and terrestrial vertebrates

> the transcendental anatomists infer, that the vertebrate (if not the invertebrated) animals are constructed according to the same *type* or plan; and that the higher animals, before arriving at their ultimate degree of development, successively run through stages in which their structure is similar to that of animals of less complex organization.[90]

It was, however, quickly pointed out that this did not mean that the foetus of a human being actually was a fish, a reptile or a bird at any stage in its development, as not all the organs passed through the same stage of development at the same time. The article went on to refer to Geoffroy 's 1829 transformist article from the *Mémoires du Muséum d'Histoire Naturelle*, noting that from 'this gradual process of formation we can understand the production of monsters, some of which have been shown by [Geoffroy] St Hilaire to be caused by stoppage of the development of some of their parts'.[91] Based on the evidence of his articles in the *Edinburgh Journal of Natural and Geographical Science*, Cheek, like his older contemporaries in Edinburgh medical circles, Grant and Knox, was almost certainly a disciple of Geoffroy, subscribing to both his theories of unity of plan and of transformism. It may well have been through either Knox or Grant, the latter a fellow member of the Plinian Society, that Cheek was first exposed to these ideas.

In the April 1830 issue of Cheek's journal an article entitled 'Suggestions on the relation between Organized Bodies, and the Conditions of their Existence' appeared in a section of the journal called 'Natural-Historical Collections', which was

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[90] [Cheek], 'On the Existence of Vascular Arches, p.235

[91] Ibid., p.235.

composed of miscellaneous short articles on subjects of natural-historical interest. Like most of the other pieces in this section, the article was anonymous, but was almost certainly by Cheek, given its similarity in tone and language to his Royal Medical Society essay and other articles from the journal that he published under his own name. The 'conditions of existence' mentioned in the title were first defined as 'the external physical agents with which the organized body is in necessary relation, and upon which the integrity and action of its functions depends'.[92] 'Conditions of existence' is a term often associated with Cuvier, for whom it was synonymous with 'final causes'. [93] The term was also used by Geoffroy, but for him the conditions of existence had become an efficient cause. [94] It is most likely from the latter source that Cheek borrowed it. The article goes on to explain the relationship between living things and their conditions of existence in the following seven propositions, which merit quotation in full:

> 1. The development of the process of organization, – a power imposed by the Deity upon matter, – depends upon the conditions of existence.
> 2. The perfection of organized bodies, or the number and complexity of organs, has a direct ratio with the number of the conditions of existence.
> 3. All organized bodies possess the power of varying the development of the organs, by addition or subtraction of parts, as changes in the conditions of existence occur.
> It is easy to conceive that an organized body can assimilate elements in the form of a new organ, as new functions are required, when we recollect that it is constantly exercising a power of converting inorganic matter into the living emblem of its original form.
> 4. The characters of organized bodies will be permanent during the continuation of the same conditions of existence which led to their development, and no longer.
> 5. The more numerous the conditions of existence, the less liable the characters of the organized body to change, and *vice versa*.

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92 [Henry Cheek], 'Suggestions on the relation between Organized Bodies, and the Conditions of their Existence', *Edinburgh Journal of Natural and Geographical Science* 2 (April 1830), p.65

93 See, for example, Georges Cuvier, *Le Règne Animal Distribué d'après son Organisation*, vol. 1 (Paris, 1817), p.6

94 See, for example, Étienne Geoffroy Saint-Hilaire, 'Anencéphales humains', *Mémoires du Muséum d'Histoire Naturelle* 12 (1825), p.267

6. It has been observed that the older formations of the earth's crust, generally speaking, the less perfect the organic remains they contain. This progressive increase of perfection of organization, would lead us to expect, from the foregoing principle, that, with the advancing age of the earth, the conditions have increased in number; and this seems to be a fact.
7. Adaptation of the law by which organized bodies change with the variation of the conditions of existence; and separation of the functions of relation, and concentration of the vital functions, seems to be the mode of perfection. [95]

Although some of this seems rather obscure, it would appear undeniably to be an unambiguous theoretical statement of the principle that the development of the organisation of living beings and the increasing perfection of living forms over geological time were directly connected to environmental change, in a manner broadly in harmony with the theories of Geoffroy. It is worth noting that the idea expressed in the fourth point, which states that living things will remain the same as long as their external environment remains unchanged, is identical to a concept developed in Geoffroy's transformist article on gavials, published in 1825, and may be derived directly or indirectly from that source.[96] It is furthermore suggested that new organs appear, or are lost, in response to changes in the environment. This is a very similar concept to the principle of the development or disappearance of organs through use or disuse that is central to Lamarck's transformist thought. Lamarck had written in his *Philosophie Zoologique* that 'the failure to use an organ, become constant because of the habits which have been adopted, gradually reduces the organs, and finishes by making it disappear'.[97] Conversely, he wrote that 'the frequent use of an organ which has become constant through habit, augments the faculties of that organ'.[98] These changes are then transmitted to offspring. Here as

[95] [Cheek], 'Suggestions on the relation between Organized Bodies', p.65
[96] Étienne Geoffroy Saint-Hilaire, 'Recherches sur l'organisation des gavials', pp.97–155
[97] 'Le défaut d'emploi d'un organe, devenu constant par les habitudes qu'on a prises, appauvrit graduellement cet organe, et fini par le faire disparaître'. Lamarck, *Philosophie Zoologique*, p.240
[98] 'L'emploi fréquent d'un organe devenu constant par les habitudes, augmente les facultés de cet organe'. Ibid., p.248

elsewhere, we see Cheek drawing on the work of contemporary transformist thinkers to produce his own synthesis.

We have seen how different figures in Edinburgh natural history circles envisaged a progressive history of life and the forces which they imagined drove that progress. We have also seen how they mapped out the broad contours of the history of life on earth using both the fossil record and the evidence of comparative anatomy, drawing on the work of important transformist thinkers such as Lamarck and Geoffroy. In the next section I will examine in more detail how they saw the nature of biological species and the mechanisms they proposed for the origins of new species and varieties.

The origin of species and varieties

Although, as we have seen in the last chapter, Walker held that species were fixed for all eternity, the same was not necessarily true for varieties of plants and animals, which differed from other varieties in only trifling ways. Giving parsley as an example, he stated in his lecture course in 1790 that 'The curld leave parsly [sic] is too only a variety of the common. Nature is slow in producing these changes, and requires time and circumstances to effect them.'[99] The same is true for animal varieties, and in the following passage from his 1797 lectures he gave a fuller explanation of the mechanism of variation: 'The Varieties of Animals are of various Forms, but they are also capable of propagating their like, as is clearly manifested in the numerous Varieties of the Dog Kind. Concerning the Causes of the Varieties in Plants and Animals, we may observe, that they generally arise from too sparing, or too luxurious Nourishment.'[100] Here Walker would seem to be suggesting that new varieties arise through the influence of environmental factors, here the abundance and quality of the food available. In his 1790 lectures he stated the case for the importance of environmental factors even more clearly: 'The Varieties in the Animal

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[99] John Walker, Lecture notes (1790), vol. 4, ff.164–5
[100] Walker, Lecture notes (1797), vol. 9, ff.135–6

Kingdom proceed no doubt from different Climates'. [101]According to Walker's lectures in 1797, once varieties are established they will, like species, breed true. While species remain locked in place in the great chain of being, a limited form of transmutation is allowed between their varieties.

Walker would doubtless have been aware of Buffon's famous definition of a species from the second volume of his *Histoire Naturelle* (1749), given here in Smellie's translation of 1785:

> Those may be regarded as of the same species which, by copulation, uniformly produce and perpetuate beings every way similar to their parents; and those which, by the same means, either produce nothing, or dissimilar beings, may be considered as of different species. A fox, for example, will be of a different species from a dog, if nothing results from the intercourse of a male and a female of these two animals; or if the result be a dissimilar creature, a kind of mule, as this mule cannot multiply, it well be a sufficient demonstration that the fox and the dog are different species of animals.[102]

Nevertheless, in his lectures for 1790 Walker fell back on an older formulation, that 'The word Species is not applied to plants and animals, because they can propagate their like, but it is more extensively applicable in philosophy to all Divisions whether of Organized or inorganized bodies and even the metaphysical arguments.'[103] Thus Walker appears to have explicitly rejected Buffon's definition, at least in 1790. However, by 1797, when the student David Pollock took down a set of notes of Walker's lectures, he seems to have come to accept Buffon's opinion. Here we find the following formulation, almost identical to that of Buffon: 'where two Animals produce a Progeny that is fertile and unalterable, these are accounted a Species.' [104]

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[101] Walker, Lecture notes (1790), vol. 4, f.166
[102] Buffon, *Natural History*, vol. 2, p.10
[103] Walker, Lecture notes (1790), vol. 4, ff.163–4
[104] Walker, Lecture notes (1797), vol. 9, f.138

Walker was not alone in Edinburgh in thinking along these lines in the 1790s. In an unpublished treatise on agriculture, written not long before his death in 1797, James Hutton speculated that the influence of soil and climate on a plant could act in 'the course of time to form variety without changing the species.'[105] Hutton went on to conclude that, over time, small modifications brought about by the environment could add to up to significant change in a living thing: 'If we shall allow the effect which I have been ascribing to the influence of soil and climate in vegetation, as making a small and imperceptible change in the organical constitution of the plants then that change, which may be imperceptible in the form and qualities of the continued reproduction of the plant may become considerable in the constitution of the procreated seed'.[106] In a remarkable passage from later in his treatise on agriculture, which merits quotation at length, Hutton seems to have prefigured the idea of natural selection, although of course only with reference to varieties, not to species of animals:

> To see this beautiful system of animal life (which is also applicable to the vegetables) we are to consider, that in the indefinite variation of the breed the form best adapted to the exercise of those instinctive arts, by which the species is to live, will be most certainly continued in the propagation of this animal, and will be always tending more and more to perfect itself by the natural variation which is continually taking place. Thus, for example where dogs are to live by the swiftness of their feet and the sharpness of their sight, the form best adapted to that end will be the most certain of remaining, while those forms that are least adapted to this manner of chace [sic] will be the first to perish; and, the same will hold with regard to all the other forms and faculties of the species, but which the instinctive arts of procuring its means of substance may be pursued.[107]

As with his views on the general shape of the history of life, there was a significant discontinuity between Jameson's ideas on the nature of species and those of Walker. There is some evidence from Jameson's unpublished papers that he had been

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[105] James Hutton, unpublished treatise on agriculture (*c.*1795), vol. 1 (National Library of Scotland, Ms.23165), f.320
[106] Ibid., f.407
[107] Hutton, Treatise on agriculture, vol. 2, f.7

thinking about the transmutation of species from relatively early in his career. Among his papers there is an incomplete manuscript containing a lengthy discussion of transformism in Jameson's handwriting. Unfortunately this is undated, but the paper it is written on bears an 1802 watermark, so it is likely to be early. The content also suggest an early date, as it harks back to eighteenth-century theories of transformism. A reference to the discovery of more than 20 species of extinct quadrupeds by Cuvier suggests that it is unlikely to be much earlier than 1812, when Cuvier's *Recherches sur les Ossemens Fossiles de Quadrupeds* was published. Even if Jameson had been aware of Cuvier's work a little earlier than this, a date around 1812 still seems likely. The number and nature of the many amendments make it highly unlikely that he was simply copying from an English-language source, although the possibility cannot be entirely ruled out that he was translating from an unidentified foreign-language publication. The manuscript first discusses the process by which varieties become species over time:

> the different ~~species~~ kinds of Colewort which are now as varieties, may in process of time become fixed species – the different varieties of Dog as the Bull dog, Spaniel &c if kept separated from each other for centuries would form distinct species – that would not intermix with each other – like the Fox & Wolf which appear to have been formed from the Dog species in this manner[.][108]

According to the manuscript, the transmutation that produced new species would be brought about by environmental factors, which would then be transmitted to offspring: 'Every peculiarity of climate, of nourishment, of generation, even many accidental mutilations, may give rise to these differences & thus form new varieties, which may pass into new[?] & fixed species after a long series of years.'[109] This is not the only mechanism by which new species could come into being, as the 'number of species in the animal & vegetable kingdoms [illegible word] are formed not only by the transition of varieties into species, but also by the intermixture of different

[108] [Robert Jameson], untitled manuscript on the transmutation of species (Jameson papers, University of Edinburgh Library, Gen.125), ff.1–2
[109] Ibid., f.4

species.'[110] This appears to be a reference to the theory of Linnaeus that new species of plants could be generated through hybridisation. Jameson went on to speculate that the extinction of species left gaps that would have to be filled by new species: 'it would seem necessary that new species should be created because other species die out – as soon as others are formed to take their place in the general oeconomy [illegible word] of nature.'[111] If this were indeed an original manuscript by Jameson, which seems highly likely, it would be very strong evidence indeed for his wholehearted acceptance of transformism at a relatively early date.

One figure whose transformist credentials are beyond doubt is Henry H. Cheek. As we have seen above, in a dissertation read before the Royal Medical Society on 29 January 1830 Cheek made a notable contribution to the debate on the origins of races to be found in the Society's dissertation books. Like most of his contemporaries, Cheek supported the monogenist view, but he went well beyond simply proposing a common origin for the varieties of the human race. He questioned the distinction between species and varieties, and the very existence in nature of species as conventionally understood. He went as far as to propose a common origin for all species. First he tackled the species concept itself, admitting that

> I have not met with an author who can distinguish the species from the permanent variety. And Buffon probably was not so widely inaccurate, when he said "qu'il n y a pas d'espece dans la Nature", for if an animal be liable to the casual production of varieties, whose new characters are transferable to its offspring, these definitions of species must either be declared to be inexact, or the nonexistence of species must be admitted.[112]

As well as reflecting the views of Buffon, this also accords with Lamarck's opinion on the unreality of species. According to this view, all classification was therefore an

[110] Ibid., f.5
[111] Ibid., f.6
[112] Cheek, 'On the varieties of the human race', p.301

artificial creation of natural historians, which were useful only from the perspective of one particular privileged moment in the history of the earth.

Cheek then addressed the question of the distinction between species and varieties. First he noted that the conventional distinction was based on 'the origin of the difference'. If we imagine we can find a natural cause for the difference between two types of living thing – presumably Cheek was thinking principally of the effect of the environment here – we name them varieties, if not, we must assume that the difference has existed *ab initio*, and we name them species. As Cheek himself put it: 'If we fancy we can devise a probable cause for a particular diversity, we name it a variety. If mystery overpowers our subtlety, we name it species.'[113] Having cast doubt on the distinction between species and varieties, Cheek then turned to the question of how both arose. He summarised the various possibilities as he saw them in this remarkable passage:

> All the varieties, as well as all species, may have been transmitted from a single pair, by spontaneous changes; or some physical alterations in the constitution or revolutions of the globe, and its atmosphere may have produced them from a specific type, as some similar changes may have caused the differences of species, whilst previously their several characters were associated under a genus; or lastly the characteristics of permanent varieties, or rather of species may have appeared at once over all those portions of the globe, were the necessary conditions of existence assembled.[114]

Here Cheek gives us three possible theories for the origin of species.[115] Firstly, he considered the possibility that 'spontaneous changes' may be responsible for the diversity of life. This implies that living things have an innate tendency to change their forms over time. Secondly, he suggested that species may have been moulded

[113] Ibid., p.302
[114] Ibid., p.306
[115] Although Cheek doubted the existence of species in nature he continued to use the term in his writings, presumably for want of a suitable alternative. I have followed his example in this paper when discussing his theories.

directly by changes in their environments, so as to generate new species as their conditions of life change. Thirdly, Cheek considered the possibility that species and varieties had come into being simultaneously wherever the environment was suitable for them. The first model is close to Lamarckian transformism, in which the principle mechanism for the transmutation of species was an innate tendency to increasing perfection, although Lamarck did recognise that changes resulting from environmental pressures could complicate this simple progressive pattern.[116] The second is closer to the theories of Geoffroy, who proposed that directional change in the environment, in this case in the composition of the atmosphere, led to progressive changes in living things. The third theory is essentially the model proposed by Buffon in his *Époques de la Nature* (1778), where he suggested that as the earth cooled over time, new species arose which were adapted to the cooler conditions, while warmth-loving species became extinct. Although Buffon gave no indication of how he thought this came about, he clearly was not proposing the transmutation of species, but rather the appearance *de novo* of entirely new species. Curiously, Cheek then appeared to dismiss the whole question as a 'mere barren waste of speculation' before giving his own opinion that transmutation came about through the action of a 'Zoological law'.[117] Was this an attempt to deflect criticism from his own opinions by denying the validity of any such theorising? It is impossible to say for certain.

The subject of the inheritance of acquired characteristics was raised by Cheek in a communication read to the Plinian Society on 5 April 1831. This was his last communication to the Society, although his last recorded attendance was in fact not until 3 April 1832. In it he recounted a case where a 'pointer bitch had had her tail mutilated in a trap or by some such accident, in a litter of three pups she produced shortly after they all had tails with a less number of caudal vertebrae than the

[116] Lamarck, *Philosophie Zoologique*, pp.218–19
[117] Cheek, 'On the varieties of the human race', p.306

species in general have.'[118] This is a clear example of the inheritance of acquired characteristics, implicated by both Lamarck and Erasmus Darwin as an important mechanism in the formation of new species. The same subject was raised again in two articles published in March and May 1831 in the *Edinburgh Journal of Natural and Geographical Science*, one probably by Cheek, the other by B.S. Shuttleworth, in which the transmission of mutilations suffered by animals to the next generation was discussed in the context of the theories of Johann Friedrich Meckel (1781–1833) and Johann Friedrich Blumenbach (1752–1840).[119] Cheek was, however, sceptical that a crude modification of an individual animal caused by a single, accidental event, such as the loss of a tail, could be transmitted from generation to generation:

> We have, in a former number, stated it to be our opinion, that the characters of an organism are permanent during the operation of the circumstances, internal or external, which produce them, *and no longer*. Whence, the original deficiency of caudal vertebrae being the result of mutilation, a new operation would be required in every successive generation, to continue the character (if it may be so called) in the race.[120]

It can be inferred from the above that Cheek saw the inheritance of acquired characteristics as unnecessary to explain the transmutation of species, and that the increasing complexity of living things over time was both driven and maintained directly by the increasing complexity of the conditions of existence. Here again he deviated from the Lamarckian model of transformism, and is closer to Geoffroy.

We have seen thus far how an essentially progressive vision of the history of the earth was generally linked to a progressive history of life by both Wernerian

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[118] Plinian Society Minutes, vol. 2, ff.90 verso – 91 recto

[119] Anon, 'Query on the Hereditary Transmission of Accidental Characters', *Edinburgh Journal of Natural and Geographical Science* 3 (March 1831), p.173; and B.S. Shuttleworth, 'Hereditary Transmission of Accidental Characters', *Edinburgh Journal of Natural and Geographical Science* 3 (May 1831), p.301

[120] [Henry Cheek], editorial comment on 'Query on the Hereditary Transmission of Accidental Characters', *Edinburgh Journal of Natural and Geographical Science* 3 (March 1831), p.173

geologists and transformist thinkers in Edinburgh. Most transformists envisaged the origin of new species occurring within such a progressive framework. However, not all transformists of the Edinburgh school agreed with this picture of universal progress. I will now turn to two thinkers who developed original ideas regarding the origins of species, but explicitly denied the connection between their conceptions of transformism and the idea of progress in the natural world, Robert Knox and Hewett Cottrell Watson.

Transmutation without progress: Robert Knox and Hewett Cottrell Watson

We have seen in chapter 4 that Robert Knox was a vociferous opponent of progressivist visions of the history of life, and was deeply critical of Lamarckian transformism on those grounds. This did not mean, however, that he believed in a static model of the history of life on earth. 'That Knox held to a theory of organic descent is beyond question' was Evelleen Richards' conclusion, based on a detailed analysis of his later works.[121] She has also sought to demonstrate that his theoretical views were both strikingly original and radically different from those of either Lamarck or Geoffroy. Unfortunately Knox wrote practically nothing on the subject of transformism and the relationship between species and genera during his years in Edinburgh, but only much later, when he was resident in London in the 1850s. Although, of course, it is not possible to extrapolate with any certainty from Knox's theories of the 1850s back into the 1820s, it does seem worth exploring some of the evidence that can be mustered in defence of Richards' interpretation.

In Knox's later works, he presented a theory of the relationship between the species and the genus that may have led to some confusion over his views on transformism. A keen fisherman, Knox gave the clearest exposition of his theory using the example of the salmon. For Knox, the young salmon was a 'generic animal', displaying the characteristics of all species of the genus. In the words of an article from *The Lancet*

[121] Richards, 'The "Moral Anatomy" of Robert Knox, p.399

in 1855: 'In the young of the true salmon, I found the specific characters of all the sub-families of the genus present; that is, red spots, dark spots of several kinds, silvery scales, proportions, and a dentition identical. The young fish before me was, in fact, a generic animal, including within it the specific characters of all the species composing the natural family.'[122] The young fish then differentiate, losing some of the 'generic' characteristics and becoming members of one or another species. In an article for *The Zoologist* in the same year, he develops the same idea further:

> Thus the young animal, at a certain stage of its growth, is the type not of the species to which it belongs by hereditary descent, but represents the generic type, transcendental, and requiring for its full development or embodiment in all its material, that is, specific forms, countless millions of years; for as the young, that is, the generic animal, includes many species, perhaps all which the natural family can assume in time and space, so as species die out, others appear, new to the world as species, but not generically.[123]

It is with this definition of a species in mind that we must read his pronouncement that 'I adhere to the same view – namely the inconvertibility of species into each other by any physical laws now in operation'.[124] Under Knox's definition of a species, this need not contradict his assertion that 'I believe all animals to be descended from primitive forms of life'.[125] Indeed he goes as far as to say that 'In time there is probably no such thing as species.'[126] For Knox, the fundamental unit of nature was not the species, but the genus, so when he speaks of the impossibility of the transmutation of *species*, this does not contradict his open avowals of transformism elsewhere.

In Knox's theory the process of differentiation of the generic animal to give the species is distinct from, but preceded by, its ascent through all consecutive degrees

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[122] Knox, 'The philosophy of zoology', p.627
[123] Robert Knox, 'Enquiries into the philosophy of zoology. Part I. – On the dentition of Salmonidae', *The Zoologist: A Popular Miscellany of Natural History* 13 (1855), pp.4789–90
[124] Knox, 'Philosophy of zoology', p.626
[125] Knox, *Great Anatomists and Great Artists*, p.109
[126] Knox, *Races of Men*, p.36

of organic complexity during foetal development, as proposed by Serres and Geoffroy: 'from the moment of conception or of independence, that living point, that embryo, passes through a succession of *forms*, shadowing forth the organic world as it now exists, from the highest to the lowest; shadowing forth the organic world as it existed from the dawn of creation to the present day – that is proved by geology'.[127] It is worth noting here that Knox assumed that the succession of forms in order of complexity would also recapitulate the history of life through geological time as evidenced by the fossil record. This did not mean, however, that he saw the history of life as progressive; as we have seen in chapter 4, Knox poured scorn on humanity's presumption that they were the pinnacle of creation, towards which the history of life had been leading, pointing out that the fishes that inhabited the primordial oceans would, had they been able to speak, probably have said the same of themselves.[128]

Another Edinburgh graduate whose ideas on the relationship between varieties, species and genera bear comparison with those of Knox, as well as harking back to the ideas of Linnaeus on hybridisation, was the botanist Hugh Cottrell Watson (1804–81). Watson studied medicine at the University of Edinburgh between 1828 and 1832, but left without graduating. During his time in Edinburgh, Watson got to know the leading phrenologist George Combe, and developed a deep interest in phrenology. After leaving Edinburgh he maintained close links with the city both through his friendship with Combe and his continuing association with botanical circles in the city. In 1836 he helped to found the Botanical Society of Edinburgh. In his monograph on Watson, Frank N. Egerton has claimed Watson as a transformist, although only after he left Edinburgh.[129] In his foreword to Egerton's book, David L. Hull states that 'Watson had accepted the transformation of species from at least

[127] Ibid., p.421
[128] Robert Knox, 'Introduction to Enquiries into the Philosophy of Zoology', *The Lancet* 7: 1 (1855), p.369
[129] Frank N. Egerton, *Hewett Cottrell Watson: Victorian plant ecologist and evolutionist* (Aldershot, 2003), p.148

1834.'[130] I would argue that Watson's surviving correspondence and published work suggest that he was certainly never a transformist of the same stamp as, for example, Robert Grant. Like Knox, he was highly sceptical, if not actively hostile, to the idea of progress in the natural world. In an outline of the 'Progress of the Earth's Changes' he sent to George Combe in December 1836, he wrote of the fossil evidence for the history of life that 'I think the evidence shows oscillation to & fro, without any onward or backward course in continuity.'[131] His transformism, then, if it can be described as such, was of an unusual uniformitarian variety. This did not mean, however, that he believed that new species did not come into existence over time. It is to his ideas on the origins of new species and the relationship between varieties, species and genera that I now turn.

In his *Outlines of the Geographical Distribution of British Plants*, published in Edinburgh in 1832, Watson discussed at some length the various opinions held by botanists on the origins of species of plant in the following passage:

> Investigations concerning the original creation of plants, in the present state of human knowledge, might be deemed by many at best an idle waste of time; and even inquiries into the means by which some occupy their present situations, except in some, and these comparatively but few particular instances, may truly seem a speculation not much more profitable in itself, or likely of ultimate success. This inquiry, nevertheless, has occupied the attention of several excellent botanists, and has lead [sic] to considerable diversity of opinion amongst them; – one party imagining all plants to have originated in some central point from which they have been gradually spread over the earth's surface; others conceiving that several of such centres must have existed; and a third party believing species for the most part to have originated where they now appear as natural and untransported products of the soil and climate. Some again suppose, that at first only *genera* existed, *species* arising from generic admixture; while others maintain that all vegetable forms are modifications of each other, or the result of certain concurrence of molecules dispersed through matter; hence liable to be

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[130] David L. Hull, 'Foreword', in Egerton, *Hewett Cottrell Watson*, p.xix

[131] Hugh Cottrell Watson to George Combe, 14 December 1836 (Research collections, Edinburgh University Library Ms 7241), f.161 recto

> produced in any situation where the necessary conditions of their existence occur. [132]

First Watson issued a caveat warning of the doubtful value of speculation, before reviewing the various explanations for the distribution of plants that were current. He then turned his attention to the various theories offering to explain how new species may have arisen. He considered three possibilities: firstly, Linnaeus' model of the formation of new species through hybridisation; secondly, the transmutation of existing species; and thirdly, the model elaborated by Buffon in his *Époques de la nature*, in which new species arise spontaneously from 'organic molecules' wherever conditions are suitable for them.

Like Buffon, Lamarck, Knox and Cheek, Watson seems to have doubted that species had any real existence in nature, instead being merely a product of the human need to reduce the world to order. For Watson species were fluid entities, which were in a constant state of flux. As he wrote in a letter to Nathaniel Winch in October 1834, '*Species* in any sense or degree I look on as human divisions, not as the creations of nature. The changes, provided by geologic evidence, to have occurred in organic forms, and those now effecting by climate, elevation, cross-breeding, &c. &c. strongly discountenance the idea of absolute and permanent distinctions.'[133] In a work entitled *An Examination of Mr. Scott's Attack upon Mr. Combe's 'Constitution of Man'* in which Watson set out to defend the ideas of his friend and fellow phrenologist George Combe from the attacks of his Evangelical critic, William Scott, he suggested that there were no fixed boundaries between species:

> we find varieties [of plants] produced, and regularly continued by descent, having greater differences between themselves, than are seen between other races generally supposed to be distinct species. So much do our gardens now abound with intermediate varieties or transition-species, so generally is one

[132] Hewett Cottrell Watson, *Outlines of the Geographical Distribution of British Plants; Belonging to the Division of Vasculares or Cotyledones* (Edinburgh, 1832), pp.2–3
[133] Hewett Cottrell Watson to Nathaniel Winch, 7 October 1834, quoted in Egerton, *Hewett Cottrell Watson*, p.148

> kind run into another, that the united skills of all the botanists in the world would fail to distinguish them.[134]

He backed up his argument with evidence drawn from the production of breeds of domestic animals and plants, although, at least as regards higher animals, he was less than sure that the evidence was conclusive, but only admitted that 'it "tends" to show a possibility of change and progression'.[135] In response to Scott's use of Cuvier's argument that historical records and the discoveries of archaeology show no discernible change in species, Watson gives the same reply as Lamarck and Geoffroy, pointing out that the two or three thousand years quoted by Cuvier represents 'a space of time which shrinks to a mere point, if compared with the eras of geologists'.[136] Watson's conception of the history of life, then, is one in which there is constant flux and change, but no clear sign of progressive development.

Conclusion

We have seen how Robert Jameson embraced a progressive model of the history of the earth and the history of the life right from his first days as professor of natural history at Edinburgh. The theory of the earth he inherited from his master Werner provided the stage on which a progressive story of life could unfold. This story was given added credibility by the increasingly detailed picture of the fossil record that was emerging at the beginning of the nineteenth century. As early as 1804 Jameson had identified the directional, progressive change apparent in the fossil record as a problem that had to be addressed. We know that at that time Jameson was already asking himself the question 'Were all animals and plants originally created as we at present find them, or have they by degrees assumed the specific forms they now possess?'. There is considerable evidence to suggest that his sympathies came to lie with those who favoured the second possibility. We have seen that under his

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[134] Hewitt C. Watson, *An Examination of Mr. Scott's Attack upon Mr. Combe's 'Constitution of Man'* (London, 1836), p.27
[135] Ibid., p.26
[136] Ibid., p.25

editorship the *Edinburgh New Philosophical Journal* became a vehicle for the diffusion of transformist ideas in a series of articles, at least some of which were probably written anonymously by Jameson himself, or at least by some of his former students. There is also significant evidence from Jameson's unpublished papers that he embraced a transformist explanation of the origin of new species.

One thing that most Edinburgh transformists had in common was their emphasis on the role of a changing environment as the motor for directional change in the history of life. Both Jameson and Grant explicitly cited the declining temperature of the earth as the prime agent of change. Cheek also identified changes in the 'conditions of existence' as the motor of transmutation. In this they were in broad harmony with the theories of Buffon and Geoffroy, but radically opposed to the transformism of Lamarck, whose geology was entirely uniformitarian. Lamarck's innate tendency towards increasing perfection generally gave way to directional environmental change as the driving force of transformism. Jameson, Grant and Knox also openly rejected Lamarck's belief that species never became extinct in nature. Far from being 'Edinburgh Lamarckians', it would seem that the Edinburgh transformists rejected many of the central tenets of Lamarckian transformism. In general, they were closer to those of Geoffroy, whose ideas on 'unity of plan' were revolutionising comparative anatomy in the late 1820s and early 1830s. We know that Grant and Knox were lifelong disciples of Geoffroy, while Cheek was also heavily influenced by him. Even Jameson gave a paper on Geoffroy's theories to the Wernerian Society in 1829. It seems more than likely that Jameson's background in Wernerian geology would have left him predisposed to Geoffroy's environment-driven version of transformism.

Like Buffon and Lamarck, Cheek, Knox and Watson questioned the existence in nature of species, although they each had their own distinctive ideas about the nature of kinds of animals and plants. Cheek believed that there was no real distinction between species and varieties, but rather both were part of a dynamic continuum. Watson, like Cheek, believed that species formed a continuum in a constant state of flux. Knox, on the other hand, had a unique conception of the relationship between species and genera. For him only the genera had real existence in nature, species were just transient forms which arose from the parent genera in different places and times. Knox's stated belief that 'all animals to be descended from primitive forms of life' would therefore seem to suggest that genera rather than species were the ultimate products of a historical process of transmutation for Knox.

In this chapter I have tried to show how a loose group of transformist natural historians in Edinburgh in the first few decades of the nineteenth century took ideas from earlier transformist thinkers and used them as a basis to develop their own distinctive theories. All one way or another rejected the static view of species as entities that were stable over geological time, and in so doing radically questioned the existence of species as a category in nature. Most of these individuals seem to have formulated their ideas largely in the 1820s and early 1830s, although in the cases of Knox and Watson our evidence comes largely from later writings, and Jameson may have been thinking along these lines rather earlier. Grant, Watson and Knox left Edinburgh in 1827, 1832 and 1842 respectively. Grant and Knox moved to London, where they both became increasingly marginalised figures. Cheek died tragically young in 1833, only a year after graduating from the University of Edinburgh. Jameson and the *Edinburgh New Philosophical Journal* seem to have had less to say about the grand scheme of the history of life from the mid-1830s onwards. Then suddenly, in 1844, a book was published by an Edinburgh author that brought transformism dramatically back into the limelight. That book was Robert Chambers' anonymously published *Vestiges of the Natural History of Creation*. In the next chapter, I will attempt to see what, if any, connection can be discerned between the development hypothesis elaborated by Chambers in the early 1840s and the theories of the Edinburgh transformists of earlier decades.

Chapter 6: Physiology, phrenology and *Vestiges*

Introduction

The publication of *Vestiges of the Natural History of Creation* in October 1844 radically changed the terms of the debate on transformism in Britain. This book presented a distinctive theory of transformism set within an all-encompassing vision of universal progress. Its anonymous author was Robert Chambers (1802–71), the well-known Edinburgh journalist and publisher. It was an extraordinary publishing success. The first two editions of 750 and 1000 copies respectively sold faster than the author or publisher had dared to hope, and the third edition of 1,500 copies sold out on the day of publication.[1] The book was soon at the centre of a storm of controversy. In Scotland, a group of members of the Evangelical Party of the Church of Scotland with scientific interests were particularly outspoken in their criticism. Two of the most important Evangelical scientists of age, the natural philosopher David Brewster and the geologist Hugh Miller (1802–56), directly attacked the arguments of *Vestiges* and its sequel *Explanations* (1845) in print; Brewster wrote blistering reviews of both for the *North British Review* and Miller produced a book-length refutation entitled *Foot-prints of the Creator* (1849). Brewster blasted Chambers' anonymous magnum opus in his review of August 1845 with these words: 'Prophetic of infidel times, and indicating the unsoundness of our general education, "The Vestiges of the Natural History of Creation," has started into public favour with a fair chance of poisoning the fountains of science, and sapping the foundations of religion'.[2] In *Foot-prints of the Creator* Miller issued the following call to arms against the pernicious doctrines contained in *Vestiges*:

> The evangelistic church cannot, in consistency with their character, or with a due regard to the interests of their people, slight or overlook a form of error at once exceedingly plausible and consummately dangerous, and which is telling so widely in society, that one can scarce travel by railway or in a

[1] Secord, *Victorian Sensation*, p.130

[2] [Brewster], Review of [Chambers], *Vestiges*, p.471

> steam-boat, or encounter a group of intelligent mechanics, without finding decided traces of its ravages.[3]

I am not going to discuss at length the reception of *Vestiges*, as James Secord has already given a detailed account of it in his masterful *Victorian Sensation* (2000). Rather, in this chapter I will be asking whether any connection can be discerned between the Edinburgh transformists of the 1820s and early 1830s whose ideas I have been exploring in earlier chapters and the appearance of Chambers' sensational transformist work in 1844. Was it entirely coincidental that the author of *Vestiges* hailed from Edinburgh, or is there some link to be discerned between the transformist speculations of Edinburgh natural historians and medical men in earlier decades and the spectacular emergence of transformist ideas into the public sphere with the publication of *Vestiges*?

Robert Chambers and his elder brother William were born in Peebles in the early years of the nineteenth century, the sons of a small-scale cotton manufacturer who put out work to a network of handloom weavers. The arrival of the power loom and some unwise loans made to French prisoners of war, which were never repaid, ruined the fortunes of the family, who subsequently moved to Edinburgh in 1813. Both Robert and William went into bookselling, at first on a very modest scale, but they soon branched out into publishing their own books. William was an astute businessman, and W. & R. Chambers, founded in 1832, soon became a major Scottish publishing company, producing such innovative publications as *Chambers Encyclopaedia* and *Chambers Edinburgh Journal*. In the early days, Robert wrote a large proportion of their publications himself. He and his brother made an excellent team, William taking care of the running of the business, while Robert dealt more with the literary and journalistic side of things. Robert had originally been principally interested in Scottish history and traditions. Among his earliest works were *Illustrations of the Author of Waverley* (1822), *Traditions of Edinburgh* (1824) and *Rhymes*

[3] Miller, *Foot-Prints of the Creator*, p.19

of Scotland (1826). However, as his position in society became more secure in the 1830s, he turned his attention increasingly towards natural history. It was this new interest that was ultimately to lead him towards the formulation of his 'development hypothesis', which found expression in *Vestiges*.

Although the whole of *Vestiges* preaches the doctrine of universal progress by means of the development hypothesis, only chapters 12 to 14 of the 19 that constituted the first edition actually addressed transformism directly. The first two chapters deal with the formation of the earth and the other bodies of the solar system in accordance with the nebular hypothesis recently popularised by the University of Glasgow's professor of astronomy, John Pringle Nichol (1804–59), in his *Views of the Architecture of the Heavens in a Series of Letters to a Lady* (1837).[4] The next nine present a potted history of the earth and of life which is progressive but not specifically transformist; it would in itself have been familiar and entirely acceptable to such grandees of the geological establishment as William Buckland and Adam Sedgwick. After this come the three chapters in which Chambers gave an exposition of his transformist theories. He then devoted a chapter each to William Sharp MacLeay's eccentric quinary system of taxonomy, a progressivist interpretation of human history, the mental powers of animals, some vaguely natural-theological musings on the purposes of the animal creation and finally a conclusory note. I will therefore be concentrating my attention on the content of the three chapters that deal directly with transformism. When I refer to *Vestiges*, it is to be understood that I mean the first edition of 1844. Chambers made significant changes to later editions in order to meet the objections of his critics, but these are not directly relevant to my argument here.

Despite the marked evolution of the book from one edition to the next, the essential core of his argument remained the same. Chambers believed that life had first come

[4] John Pringle Nichol, in his popular *Views of the Architecture of the Heavens in a Series of Letters to a Lady* (Edinburgh, 1837)

into being through spontaneous generation, and continued to do so to the present day. His model of the history of life, like Lamarck's, was one that depicted numerous parallel lineages all at different phases of development. Chambers saw in this the working out of a set of natural laws established by the deity. For him the spontaneous generation of life was analogous to the law-governed formation of the globe from a cloud of 'nebulous matter', as proposed in the nebular hypothesis popularised by Nichol; 'one set of laws produced all orbs and their motions and geognostic arrangements, so one set of laws overspread them all with life.'[5] To explain the development of increasingly advanced forms of life, Chambers developed an analogy based on the normal process of gestation. He suggested that every species followed a common developmental path during foetal development up to a given point, at which the species diverged and followed its own path to the adult form. To support his argument, he drew on comparative anatomy, pressing into service the well-worn example of the vertebrate limb, to demonstrate the common basic design that underlay the body plans of all animal species. All that was required for a new, more advanced, species to arise, was for that common path of foetal development to be extended before branching off occured: 'To protract the *straightforward part of the gestation over a small space* – and from species to species the space would be small indeed – is all that is necessary.'[6] Whether the development of the foetus is extended further than normal or cut short depended on the environmental conditions to which the mother was exposed. In the following passage, Chambers used the example of congenital malformations of the human heart to illustrate the effect of an impoverished environment on living things before showing how the same process in reverse could lead to progress from a lower to a higher form:

> A human foetus is often left with one of the most important parts of its frame imperfectly developed: the heart, for instance, goes no farther than the three chambered form, so that it is the heart of a reptile. There are even instances

[5] [Chambers], *Vestiges*, p.164
[6] Ibid., p.213

> of this organ being left in the two-chambered or fish form. Such defects are the result of nothing more than a failure of the power of development in the system of the mother, occasioned by weak health or misery. Here we have apparently a realization of the converse of those conditions which carry on species to species, so far, at least, as one organ is concerned. Seeing a complete specific regression in this one point, how easy it is to imagine an access of favourable conditions sufficient to reverse the phenomenon, and make a fish mother develop a reptile heart, or a reptile mother develop a mammal one.[7]

In the next section of this chapter I will take a look at what Chambers' knew about earlier transformist thinkers and what his sources of information on them and their theories might have been. Since Chambers included a critique of Lamarckian transformism in his book, I will pay particular attention to the possible sources of his information on Lamarck's theories. As Chambers made significant use of works on embryology and comparative anatomy in developing his theory, and many of the authors whose works he consulted were graduates of the University of Edinburgh's medical school, I will then go on to explore possible connections between the transcendental anatomy that was so influential in Edinburgh medical circles in the 1820s and 30s and Chambers' theories; as we have seen, espousal of the transcendental anatomy of Geoffroy in Edinburgh was often accompanied by a sympathetic attitude towards his transformist speculations. Finally, I will explore the influence of the leading Edinburgh phrenologist and friend of Chambers George Combe (1788–1858) and his influential and popular book the *Constitution of Man* (1828 and numerous subsequent editions), which is everywhere apparent in *Vestiges*. I will also be asking whether the world of phrenology can provide a link between Chambers' development hypothesis and the transformism of earlier decades in Edinburgh. Combe's progressivism and emphasis on natural law would certainly seem to suggest an essential philosophical kinship with some transformist doctrines, if nothing more. It should then be possible to reach some tentative conclusions regarding whether the re-emergence of transformism in the mid-1840s in

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[7] Ibid., pp.218–19

Edinburgh, the home of a notable group of transformists in earlier decades, was coincidental, or whether there was indeed some direct or indirect connection.

Robert Chambers and early nineteenth-century transformism

Before turning to look the question of where Chambers found the building blocks for his theory, I will first attempt to establish how much he know about the theories of earlier transformists and whether these ideas were transmitted to him directly or through writings of secondary sources in Edinburgh or beyond. From the evidence available it seems unlikely that he had himself read the works of Geoffroy or Lamarck, or any of the other continental sources of the ideas he used. As Secord has established, Chambers' 'awareness of continental authors, such as E. Geoffroy St Hilaire, Lamarck, E. Serres, Friedrich Tiedemann and Karl Ernst von Baer, seems to have been entirely at second hand.'[8] In order to establish what Chambers did in fact know about earlier transformist theories and from which sources he gained this knowledge, I will first examine the evidence of *Vestiges*, before turning to Chambers' earlier writings.

Chambers' theory as expressed in *Vestiges* was radically different from that of either Lamarck or Geoffroy. Unlike Geoffroy's transformism, it assumed that transmutation occurred along a fixed developmental track, rather than depending on the influence of a changing environment. Although both theories drew on embryology, Geoffroy saw the production of 'monsters' resulting from deviations from the normal developmental pathway as crucial, while Chambers considered that the production of new forms came about through a prolongation of normal foetal development. His theory had somewhat more in common with Lamarckism, as both assumed an inbuilt tendency towards increasing perfection in living things. However, Lamarck's theory did not depend on embryology and relied rather on changes to the body of the adult form being transmitted from generation to

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[8] Secord, 'Beyond the veil', pp.179–80

generation. Chambers also rejected absolutely the second strand of Lamarck's theory, which suggested that new habits derived from new needs could modify the bodies of living things, and that these changes could be transmitted to offspring. Indeed, from the evidence of *Vestiges* it would be easy to believe that this was the only part of Lamarck's theory he was aware of, and even then he seems to have viewed it through the distorting lens of the writings of Lamarck's earlier critics.

That Chambers knew something about Lamarck's theory is undeniable. He devoted a sizable passage in his chapter on his 'hypothesis of the development of the vegetable and animal kingdoms' to a critique of Lamarckian transformism. However, the reader who expected a sympathetic account of Lamarck's theory would be sorely disappointed. When we read Chambers' account of Lamarck's theory, we find the same contorted caricature as can be found in the writings of Julien-Joseph Virey, Georges Cuvier, John Fleming and James Duncan that were examined in earlier chapters. The image presented is familiar from the writings of many of Lamarck's more hostile critics. Chambers presented Lamarck's theory as implying that animals willed themselves to evolve in order to meet immediate physical needs. According to Chambers, in Lamarck's theory 'one being advanced in the course of generations to another, in consequence merely of its experience of wants calling for the exercise of its faculties in a particular direction, by which exercise new developments of organs took place, ending in variations sufficient to constitute a new species.'[9] Based on this skewed account, Chambers concluded that Lamarck's theory was 'obviously so inadequate to account for the rise of the organic kingdoms, that we can only place it with pity among the follies of the wise.'[10] In the circumstances, it seems highly unlikely that Chambers had read any of Lamarck's works, and more than likely that he had gathered his information from one or more of his critics. However, he does not cite any sources for these opinions in *Vestiges*.

[9] [Chambers], *Vestiges*, p.230
[10] Ibid., p.231

One plausible answer to the question of where Chambers got his information on Lamarck from emerges from the pages of *Chambers's Edinburgh Journal* for 26 September 1835, in the form of an article entitled 'Popular information on science: Transmutation of species'. As Secord has noted, the articles in the *Journal* were 'unsigned and at least some of them may not be by Chambers, but they had to pass his close editorial scrutiny.'[11] Even if Chambers did not write this article, which it is very likely he did, he could hardly have avoided reading it. This article contains a number of references to Charles Lyell's critique of Lamarck, which takes up a large part of volume 2 of Lyell's *Principles of Geology*. Chambers included a lengthy quotation from this work, in which Lyell devoted considerable space to an extremely thorough refutation of transformism.[12] Indeed, the article is largely a summary of the opinions on transformism given in Lyell's book. The picture of Lamarck's theory presented here therefore bears little relation to the one propounded by Chambers in *Vestiges*, because Lyell's critique was radically different, and much more sophisticated. Far from simply repeating Cuvier's assertion that Lamarck had believed that 'it is the desire of flying that has converted the arms of all birds into wings',[13] Lyell gave an essentially fair and accurate account of Lamarck's theory.[14] He seems to have based his analysis largely on a careful reading of the *Philosophie Zoologique*. Despite the obvious differences, Chambers may still have had Lyell's account in mind when he wrote his critique of Lamarck in *Vestiges*; it may not be a coincidence, for example, that both chose the example of the development of webbed feet in aquatic creatures to support their rather different arguments.[15] Nonetheless, familiar as he may have been with Lyell's critique of Lamarck, this cannot be the source of his belief that Lamarck considered that the 'wants' of animals drove the transmutation of species, as this distortion of

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[11] Secord, 'Beyond the veil', p.177
[12] [Robert Chambers], 'Popular information on science: Transmutation of species', *Chambers Edinburgh Journal*, vol. 4, No. 191 (Saturday, 26 September 1835), p.274. The quote comes from Charles Lyell, *Principles of Geology* (London, 1834), 3rd ed., vol. 2, pp.402–3
[13] Cuvier, 'Memoir of M. de Lamarck', p.14
[14] See, for example, Lyell, *Principles of Geology*, 3rd ed., vol. 2, pp.331–2
[15] In Lyell, *Principles of Geology*, p.333 and [Chambers], *Vestiges*, pp.230–1

his theory is not to be found in the *Principles of Geology*. These ideas may plausibly have come from Fletcher, who we know discussed and rejected Lamarck's theory in his *Rudiments of Physiology*, as Fletcher criticised Lamarck on similar grounds, as we have seen in chapter 4. Fleming's critique was, however, rather more nuanced than the one found in *Vestiges*. The translation of Cuvier's 'Memoir' of Lamarck, which appeared in the pages of the *Edinburgh New Philosophical Review* in 1836, or Fleming's *Philosophy of Zoology* (1822) are also both plausible sources, but it is impossible to determine this with any degree of certainty. This particular caricature of Lamarck's theory seems to have had sufficient currency in natural history circles in the 1830s that there were probably any number of possible sources.

The 1835 article on the transmutation of species was not the only piece from the *Chambers's Edinburgh Journal* that dealt with transformism. In November 1832 there appeared an article entitled 'Natural history: Animals with a backbone', in which the transformism of Erasmus Darwin was discussed. The article noted Darwin's belief in spontaneous generation, and that all life had progressed from very simple primordial forms. However, the only one of the several mechanisms proposed by Darwin that is mentioned is the production of new species through hybridisation. Chambers, if he was indeed the author, noted that hybridisation could never give rise to fertile offspring, otherwise 'the world would become a scene of hideous monstrosity'.[16] Darwin's theories were in general dismissed as 'baseless doctrines'. According to Chambers 'views like these can never be entertained by healthy minds, and it required little reflection to dispel such absurd theories.'[17] In 1840 Darwin made another appearance in the *Journal*, this time in an article on 'The life and poetry of Darwin'. Here again, Darwin's theories were again rejected as 'too hypothetical, and even fantastical, to stand the test of sober and close

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[16] [Robert Chambers], 'Natural history: Animals with a backbone', *Chambers Edinburgh Journal*, vol. 1, No. 43 (Saturday, 24 November 1832), p.338
[17] Ibid., p.138

examination'.[18] The discussion of spontaneous generation in this article, may, however, have brought the idea to the attention of Chambers, in whose later theories it would take an important place.

Other articles in Chambers' *Journal* dealt with other theories that were to become important components of the theory of transformism advocated in *Vestiges*. In an article on 'Natural history: Monkeys, apes, and orang-outangs', published in December 1832, the reader was introduced to the doctrine of unity of plan in comparative anatomy.[19] In October 1837, in an article on 'Popular information on science: Third ages of animal life', it was proposed that 'the system of organic being was progressive, and graduated regularly onwards from the simplest forms up to those of a higher and more complex character.'[20] The prevalence of articles of this nature in the *Journal* prompted Secord to suggest that 'a careful reader of the *Journal* would have been familiar with much of *Vestiges* before it appeared.'[21] It does seem more than likely that a significant proportion of Chambers' ideas were ultimately derived from the extremely diverse body of material he digested through his role as author and editor for the *Chambers's Edinburgh Journal*. There is, however, nothing to suggest that his knowledge of earlier transformist thinkers was transmitted to him directly from the writings of any of the Edinburgh transformists we have examined in earlier chapters, or through any personal acquaintance with them or their ideas. There is certainly no evidence from his surviving letters held at the National Library of Scotland or from published sources that he personally knew or corresponded with any of the transformist figures discussed in earlier chapters. It does seem, however, that some of the central elements from which Chambers constructed his model of transformism do derive from published sources close to the medical

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[18] [Robert Chambers], 'The life and poetry of Darwin', *Chambers Edinburgh Journal*, vol. 8, No.394 (Saturday, 17 August 1839), p.251

[19] [Robert Chambers], 'Natural history: Monkeys, apes, and orang-outangs' *Chambers Edinburgh Journal*, vol. 1, No. 46 (Saturday, 15 December 1832), p.362

[20] [Robert Chambers], 'Popular information on science: Third ages of animal life', *Chambers Edinburgh Journal*, vol. 6, No. 298 (Saturday, 14 October 1837), pp.298–9

[21] Secord, 'Beyond the veil', p.175

transformists of the Edinburgh of the 1820s and 1830s, and it is to these that I turn in the next section.

Comparative anatomy, embryology and the transmutation of species

As we have seen above, Chambers' theory depended to a considerable extent on ideas derived from comparative anatomy and embryology. What were the sources of these ideas and concepts that he wove into his theory? Fortunately for the historian, Chambers cited a great many of his sources in the text of his book. It is therefore easy to establish that for the underpinnings of his theory in comparative anatomy and embryology he drew on the work of seven medical men who wrote on physiology and anatomy: William Benjamin Carpenter, Robert Bentley Todd, Martin Barry, Leonard Horner, Allen Thompson, Percival Barton Lord and John Fletcher. Of these seven, all but Todd, had studied medicine at the University of Edinburgh in the first four decades of the nineteenth century. While none of these figures advocated transformism, Chambers drew heavily on aspects of their work in developing his own transformist theory. As Secord has shown, Chambers relied mainly on the work of three of these Edinburgh graduates, Percival Barton Lord (1808–40, studied at Edinburgh 1832–34), John Fletcher (graduated 1816) and William Benjamin Carpenter (1813–85, graduated 1839).

The concept of unity of plan, promulgated in Edinburgh by the disciples of Geoffroy in the late 1820s and 1830s, was much in evidence in *Vestiges*. In his chapter on the 'hypothesis of the development of the vegetable and animal kingdoms', Chambers stated that

> While the external forms of these various animals are so different, it is very remarkable that the whole are, after all, variations of a fundamental plan, which can be traced as a basis throughout the whole, the variations being mere modifications of the plan to suit the particular conditions in which each particular animal has been designed to live.[22]

[22] [Chambers], *Vestiges*, p.182

Chambers does not cite any authority for these ideas, aside from a fleeting reference to Louis-Jean-Marie Daubenton's observations on the constancy in the number of neck vertebrae in mammals. However, he would have readily found these ideas in Fletcher's *Rudiments of Physiology* (1835–7), which we know from citations elsewhere in *Vestiges* he had read, where an exposition of unity of plan takes up most of section II of part 1 of the book. There Fletcher, who, as we have seen in chapter 4, was an enthusiastic disciple of Geoffroy, stated confidently that 'however different may seem, both in their anatomical and physiological relations, the organs of the higher and those of the lower tribes, if not of plants, certainly of animals, they are both essentially the same'.[23]

Embryology also played a central role in Chambers' theory of the transmutation of species, as it had done in Geoffroy's. However, there the similarity ends. For Geoffroy it was a disruption of the normal process of foetal development that led to the production of new species. For Chambers the appearance of a new species was simply an extension of the normal process of gestation. As he put it in *Vestiges*, 'the production of new forms, as shewn in the pages of the geological record, has never been anything more than a new stage of progress in gestation'.[24] For Geoffroy the physiological stress caused to organisms by a degrading environment during foetal development spurred transmutation forward, while for Chambers the opposite was true; a hostile environment could only lead to degradation, while favourable conditions were necessary for progress. Chambers drew on recapitulation theory as evidence for his theory. This theory, which is sometimes referred to as the 'Meckel-Serres Law', after Johann Friedrich Meckel (1781–1833) and Étienne Serres (1786–1868), two of its early proponents, had been championed and popularised by Geoffroy. Chambers noted that 'each animal passes, in the course of its germinal

[23] Fletcher, *Rudiments of Physiology*, p.36
[24] [Chambers], *Vestiges*, pp.222–3

history, through a series of changes resembling the *permanent forms* of the various orders of animals inferior to it in the scale.'[25]

Chambers quoted at length from Lord's *Popular Physiology* to demonstrate that the brain of the human foetus passes through the forms of the brains of all the lower animals, in order of perfection of organisation, before arriving at the adult human form.[26] Lord did not interpret this in a transformist manner, but rather stated that 'man, considered merely as an animal, is, by his organization, superior to every other being; and that, in the growth of a single individual, Nature exhausts, as it were, the structure of all other animals before she arrive at this her *chef-d'oeuvre*.'[27]Later in the same chapter he cited Fletcher's *Rudiments of Physiology* to much the same effect as Lord. He quoted Fletcher as noting that 'as the brain of every tribe of animals appears to pass through the types of all those bellow it, so the brain of man passes through the types of every tribe in the creation.'[28] Fletcher's book seems to contain the germs of a great many of the ideas which appear in *Vestiges*. His implicit link between unity of plan and the development of the individual organism, such that 'the splendid human organism itself consists merely of the same organs, regarded fundamentally, as exist in the polype, the differences consisting chiefly in their different degrees of elaboration' was almost certainly of great significance in the development of Chambers' ideas.[29] Chambers himself acknowledged that the key concept that an extension of the period of gestation leads to the production of a higher form of animal had been 'suggested to me by, in consequence of seeing the scale of animated nature in Dr Fletchers Rudiments of Physiology' .[30] In fact the idea is even more clearly expressed in the following

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[25] [Chambers], *Vestiges*, p.198
[26] Ibid., pp.200–1
[27] Percival B. Lord, *Popular Physiology; Being a Familiar Explanation of the Most Interesting Facts Connected with the Structure and Function of Animals and Particularly Man* (London, 1839), p.296
[28] [Chambers], *Vestiges*, p.224. The quote comes from Fletcher, *Rudiments of Physiology*, pp.60–1
[29] Fletcher, *Rudiments of Physiology*, pp.36–7
[30] [Chambers], *Vestiges*, p.223

passage from Fletcher's book, where he stated 'that all tribes of animated nature, with respect to their several organs, start, as it were together, that the germ of each of their organs is in all the same, and that they subsequently differ from each other only or chiefly in their arriving at their appointed goal sooner or later'.[31]

We know that Fletcher was emphatically not a transformist, but he did discuss transformism at length in *Rudiments of Physiology*, as we have seen in chapter 5. What is more, he linked transformism explicitly with the theory of recapitulation in a way that is strikingly similar to Chambers' theory, although while Fletcher concluded in a footnote that '[t]he system then which would establish that 'men and toads' differ only in their greater or lesser development is not tenable', the idea that progress along a single developmental track for all animals was the key to explaining the progressive history of life became the backbone of Chambers' transformism.[32] With some modifications derived principally from Carpenter, the exposition of the concept of unity of plan and the theory of recapitulation found in Fletcher's book would seem to be largely sufficient to explain the genesis of the central core of Chambers' theory of transformism.

Although Chambers does not credit Carpenter with the idea, Secord has suggested that it was through his writings that Chambers became aware of the theories of Karl Ernst von Baer (1792–1876), according to whom the foetus did not resemble the adult forms of lower animals, but rather their forms at a particular moment in foetal development. Foetal development therefore became a process of differentiation. Von Baer's theory had, in fact, been introduced to the English-speaking world by Martin Barry (1802–55), who had graduated from Edinburgh in 1833.[33] (Chambers also cites Barry in *Vestiges*, although not in this context.) We also know from the

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[31] Fletcher, *Rudiments of Physiology*, p.61
[32] Ibid., p.16
[33] Dov Ospovat, 'The Influence of Karl Ernst von Baer's Embryology, 1828-1859: A reappraisal in light of Richard Owen's and William B. Carpenter's "Palaeontological Application of "Von Baer's Law"', *Journal of the History of Biology* 9: 1 (1976), pp. 8–10

correspondence between Chambers and Alexander Ireland, who acted as his intermediary with the publisher to maintain his anonymity, that Carpenter acted as a paid consultant on later editions of *Vestiges*, although not the first one.[34] The version of the theory derived from Carpenter is represented in the diagram from *Vestiges* shown in figure 1 below, which, as Dov Ospovat has pointed out, is a slightly modified version of a diagram which appears in Carpenter's *Principles of General and Comparative Physiology* .[35] In this figure F, R, B and M represent the adult forms of fish, reptiles, birds and mammal respectively. All follow a common pathway of foetal development as far as A, where F diverges while F, M, R and M continue on the same track until C, where R diverges, and so on. For Chambers, all that was necessary for a new species to arise was for the foetus to remain on the common developmental path for longer than normal before diverging.

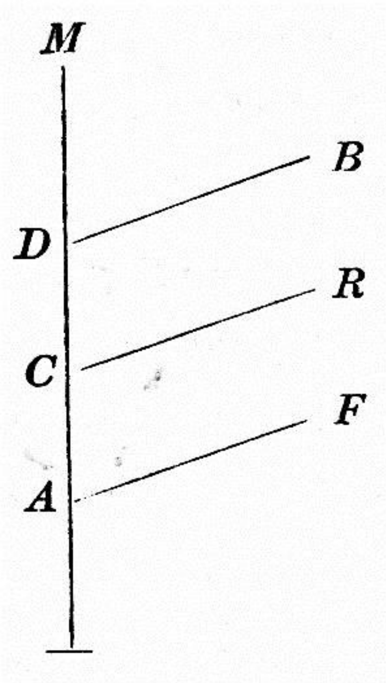

Figure 4 Diagram showing the process of differentiation during development, from Robert Chambers' *Vestiges of the Natural History of Creation*.[36]

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34 See, for example, Robert Chambers to Alexander Ireland, 29 March 1845 (National Library of Scotland, Dep. 341/113), ff.168 verso – 169 recto; and Robert Chambers to Alexander Ireland, [day and month unknown] 1847 (National Library of Scotland, Dep. 341/110), f.26 recto

35 Ibid., p.13. For the original of the diagram see William Benjamin Carpenter, *Principles of General and Comparative Physiology*, 2nd ed. (1841), p. 197

36 [Chambers], *Vestiges*, p.212

As Secord has pointed out, Von Baer's model jarred somewhat with the model derived from Lord and Fletcher propounded by Chambers elsewhere in the same chapter: 'The first edition thus seemed contradictory to many readers, and within a few weeks Chambers altered his discussion of recapitulation to bring it closer to Von Baer.'[37] It is possible that Chambers' debt to Carpenter goes deeper than this, however. In a chapter 'On the evidences of design presented by the structure of organised beings' in his *Principles of General and Comparative Physiology* we find Carpenter speculating that 'the same Almighty *fiat* which created matter out of nothing, impressed upon it one simple law' which would not only establish the cosmic order of the stars and planets, but 'people all these worlds with living beings of endless diversity of nature, providing for their support, their happiness, their mutual reliance, ordaining their constant decay and succession, not merely as individuals but as races, and adapting them in every minute particular to their dwelling.'[38] The resemblance of Carpenter's 'one simple law' to Chamber's universal law of progress is striking, and the doctrine of the primacy of natural law that he inherited from Combe may well have been reinforced by his reading of Carpenter.

We have seen how Chambers' unique brand of transformism drew on ideas on comparative anatomy and embryology that had also helped to inspire and support Geoffroy's rather different theory. However, as Secord has shown, Chambers only knew these ideas at one remove from the original sources. There is no suggestion he had read, for example, Serres, Meckel or Geoffroy. Many of the authors of the physiology books that Chambers had read did not directly discuss transformism, with the notable exception of Fletcher. While Fletcher rejected the transmutation of species, in his exposition of it he made a crucial connection between transformism, unity of plan and recapitulation theory that is strikingly reminiscent of Chambers' theory of the 'universal gestation of nature'. It seems more than likely that the ideas that Fletcher presented and linked together, with some modification based on his

[37] Secord, 'Beyond the veil', p.182
[38] Carpenter, *Principles of General and Comparative Physiology*, pp.562–3

reading of Carpenter, were the principal materials Chambers used to formulate the core of his theory. These influences provide a tangible if indirect link with the Geoffroyan transformism of Grant, Cheek and Knox. Like Chambers' rather different theory, their models of the transmutation of species also emerged in close association with the new transcendental anatomy championed by Geoffroy that exerted a powerful influence in Edinburgh medical circles in the 1820s and 1830s.

Phrenology, natural law and progress

There was one further important influence on Chambers, without which it is impossible to understand the roots of his theory or his motivation for developing it. This influence was also close at hand in Edinburgh, and was transmitted to Chambers through the work of his friend, the phrenologist George Combe. Chambers appears to have become acquainted with Combe and his philosophy in the mid-1830s. In a letter to Combe in December 1833, he described himself as 'not myself altogether ignorant of phrenology, or altogether a sceptic'. [39] He first wrote about the subject in an article for the *Chambers Edinburgh Journal* of 4 January 1834 entitled 'Is ignorance bliss?', in which he warmly recommended the *Constitution of Man* to his readers.[40] Before publishing the article he sent a proof to Combe, who wrote back to say:

> Considering that you are unacquainted with Phrenology, you have caught a good deal of the spirit of the book, but have not penetrated fully the principle of it, and no one can do this, or be competent to judge of its real importance, who has not the conviction from observation of Phrenology being true.[41]

[39] Robert Chambers to George Combe, 14 December 1833, Chambers papers (National Library of Scotland, Ms.7330), f.47

[40] [Robert Chambers], 'Is ignorance bliss?', *Chambers Edinburgh Journal* 2: 49 (4 January 1834), pp.385–6

[41] George Combe to Robert Chambers, 13 December 1833, Chambers papers (National Library of Scotland, Ms.7386), f.64

By late 1835, Chambers had set aside his uncertainty about phrenology. In a letter to Combe dated 25 December of that year, he admitted that he had

> in particular been impressed with the truth of the metaphysical department of the science, and with your singularly excellent work, the Essay on the Constitution of Man, that, in writing upon human nature, I cannot now do otherwise that [sic] employ this philosophy both as a system of mind and of morals.[42]

In the same letter, however, he expressed caution regarding openly broadcasting his adherence to phrenology due to the controversy surrounding the subject. Instead, he thought it better to introduce phrenological principles to his readers by stealth rather than openly declaring their source. Combe did not agree, and wrote to him twice in November 1835 on the subject, on the second occasion saying:

> I am not disposed to dispute that the philosophy of mind developed by Phrenology is, as you say, "the point of the wedge which is yet to rend asunder the mass of philosophical heathenism"; & I am willing to see it driven by all hands who are disposed to give it a blow. Bet agreeing with you in this estimate of it, I am anxious that it should be known, when used, to be the Phrenological wedge.[43]

Chambers, however, was not to be persuaded, and continued to refer to phrenology cautiously and infrequently in his published work, a policy which he also extended to the anonymously published *Vestiges*, although, as we will see, it was steeped in phrenological ideas and Combe's doctrine of universal progress was at the very heart of the work.

Combe was the most influential phrenologist in Britain in the middle decades of the nineteenth century. He had been a successful Edinburgh lawyer until he retired to devote himself entirely to phrenology in 1833. An early convert to this would-be

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[42] Robert Chambers to George Combe, 25 November 1835, Chambers papers (National Library of Scotland, Ms.7234), f.140
[43] George Combe to Robert Chambers, 26 November 1835, Combe papers (National Library of Scotland, Ms.7386), f.423

science, his first contact with the subject was through an extremely negative critique of phrenology written by John Gordon he had read in the June 1815 number of the *Edinburgh Review*. At the time, this convinced him that the doctrines described were 'contemptibly absurd'.[44] However, the following year Combe attended the dissection of a brain by Johann Casper Spurzheim (1776–1832), the great German populariser of phrenology, at the house of a friend. After attending a course of lectures by Spurzheim, he 'arrived at complete conviction of the truth of Phrenology.'[45] His growing interest in the subject led him to found the Edinburgh Phrenological Society in 1820. The fundamental principle behind phrenology was the presupposition that the brain is composed of a number of separate organs, each of which corresponds to a faculty of the human mind, such as 'destructiveness', 'secretiveness' or 'veneration'. According to phrenologists, the shape of the skull faithfully mirrors the form of the brain within in such a way that the development of these different organs can be read by a skilled practitioner from the shape of the skull. In the *Elements of Phrenology* Combe neatly summarised the essentials of his doctrine as follows: 'the brain is the material instrument by means of which the mind acts, and is acted upon; and it is a congeries of organs.'[46] He published a number of works on phrenology, including *Essays on Phrenology* (1819) and *A System of Phrenology* (1824), but it was *The Constitution of Man* published in 1828 that was to be his most influential and best-selling work.

However, as we will see below, Combe's *Constitution* was much more than just an exposition of the doctrine of the phrenological organs, which only accounts for a relatively small proportion of the work. For Combe's vision of human nature was not a static one, but a story of progress and development. One of the central tenets of the *Constitution of Man*, an essential part of what Chambers described as the 'metaphysical department of the science', was the doctrine of universal progress and the perfectibility of human nature. Combe not only found the idea of progress

[44] George Combe, *A System of Phrenology*, 3rd ed., (Edinburgh, 1830), p.iii
[45] Ibid., p.iv
[46] George Combe, *Elements of Phrenology*, 3rd ed., (Edinburgh, 1828), p.23

built into the constitution of man, he also found analogies for the progress of society in the progressive constitution of the world. In particular, he considered his views of human nature in perfect harmony with the progressive story of the history of the globe presented by contemporary geology. As he wrote in the 'introductory remarks' to the 'people's edition' of *Constitution*:

> The constitution of the world … appears to be arranged in all its departments on the principle of slow and progressive improvement. Physical nature itself has undergone many revolutions, and apparently has constantly advanced. Geology seems to show a distinct preparation of it for successive orders of living beings, rising higher and higher in the scale of intelligence and organisation, until man appeared.[47]

Combe made the sources of his ideas explicit in a footnote. The principle authority he relied on for the geological history of the earth was Humphrey Davy, from whose *Consolations in Travel, or, the Last Days of a Philosopher* (1831) he quoted from at length. However, he also made reference to more conventional works by the geologists Henry de La Beche and Adam Sedgwick, specifically *A Geological Manual* (1831) by the former and *Discourse on the Studies of the University of Cambridge* (1833) by the latter. Combe seems to have been relatively well informed of the latest developments and debates in geology. He was also to some extent aware of the issue of the transmutation of species, as he noted that in the first volume of Charles Lyell's *Principles of Geology*, first published in 1830, the author 'controverts the doctrine of a progressive development of plants and animals'.[48]

Combe nonetheless seems not to have been entirely confident of his own expertise in this area; we know from a letter from Hewitt Cottrell Watson to Combe dated 14 September 1836 that Combe had written to Watson to ask for his opinion on the progressive vision of the history of the earth and of life presented in the 'introductory remarks' to the fourth edition of *Constitution*. In his reply Watson

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[47] Combe, *The Constitution of Man*, 4th ed., p.2
[48] Ibid., p.2

advised Combe to 'retain the passage, but intimate that it is merely a remote analogy, on which you lay no particular stress, & whether correct or not so, of no essential importance to your own views touching on the progress of mankind.'[49] He appended to his letter his own account of the 'Progress of the Earth's Changes', significantly more cautiously worded than the passage that Combe had quoted from Davy, in which Watson suggested that the absence of fossils of higher forms of life from older rocks may simply be due to the incompleteness of the fossil record and the relative scarcity of species and individuals of higher forms of life. In the end Watson concluded that 'the evidence shows oscillation to & fro, without any onward or backward course in continuity.'[50] Despite Watson's misgivings, subsequent editions of *Constitution* retained the passages from Davy. It would seem that the analogy between the progressive nature of the constitution of the globe and of the constitution of man were of too much importance to Combe for him to quietly drop the offending passage or amend it in line with Watson's suggestions.

In addition to the principle of universal progress, Combe also emphasised the centrality of natural law. He expressed this in a letter to Robert Chambers in December 1833, where he stated that 'The leading principles of the 'Constitution of Man', to which I attach value, & which seem to myself to be original, are, the separate existence & operation of each natural law; the necessity of obeying <u>all</u> of them; & the evident adaptation of all to the moral & intellectual advancement of the race.'[51] The natural laws could be divided into three categories: the physical, organic and moral or intellectual laws. Following some of the examples given by Combe, the physical laws are those which determine that an unsound ship will sink, while a sound one will float; the organic laws determine that an individual who has a healthy diet and takes exercise will be healthy, while one who has a bad diet and

[49] Hewitt Cottrell Watson to George Combe, 14 September 1836, Correspondence of George Combe, National Library of Scotland, Ms.7241, f.160 recto
[50] Ibid, f.161 verso
[51] George Combe to Robert Chambers, 15 December 1835, Robert Chambers, letters of noted persons, 1833–38 (National Library of Scotland, Dep.341/91), f.13

does insufficient exercise will become ill; and the moral laws ensure that a person who 'obeys the precepts of Christianity, will enjoy within himself a fountain of *moral and intellectual happiness.*'[52] These natural laws had been instituted by the deity for the governance of the world. Constant divine intervention was therefore not necessary to realise God's purposes on earth. This idea was to be absolutely central to Chambers' argument in *Vestiges*, where he stated that:

> To a reasonable mind the Divine attributes must appear, not diminished or reduced in any way, by supposing a creation by law, but infinitely exalted. It is the narrowest of all views of the Deity, and characteristic of a humble class of intellects, to suppose him acting constantly in particular ways for particular occasions.[53]

Combe's progressivism, his faith in natural law and his association with Watson might be seen as evidence that Combe too was a transformist, and therefore could have been provided a link between earlier transformist thinkers and Chambers. However, as Secord has noted, transformism 'had few if any supporters in Edinburgh phrenological circles.'[54] There is no evidence that Combe or any other major Edinburgh phrenologist before Chambers himself took any interest in transformism, and the *Edinburgh Journal of Phrenology* contains no articles touching on the subject between its foundation in 1823 and Combe's review of *Vestiges* in 1845.[55] Nor does the subject appear in the minute book of the Edinburgh Phrenological Society.[56] Although Combe's review of 1845 was full of praise for *Vestiges*, he concluded that:

> Unquestionably the hypothesis of the author is far from being *proved* to be true; but he has invested the principle of evolution or development of animal

[52] Combe, *The Constitution of Man*, 4th ed., p.6
[53] [Chambers}, *Vestiges*, p.156
[54] Secord, 'Beyond the veil', p.182
[55] [George Combe], Review of *Vestiges of the Natural History of Creation*, *Phrenological Journal and Miscellany* 18 (1845), pp.69–79
[56] Minute Book of the Phrenological Society, vol. I, to 2nd September 1841 (Edinburgh University Library, Gen.608/2)

> existence with so much importance and plausibility, that we venture to predict that it will henceforth command much more of the serious attention of philosophers than it has hitherto done; and that this work will lead either to its refutation, or to its establishment on sufficient evidence, and thus equally confer a benefit on science.[57]

These cautious words do not seem like those of a convinced transformist. Nonetheless, it could be argued that Combe was simply avoiding controversy. More convincing evidence comes from a letter Combe wrote to the anonymous author via his publisher in October 1844 in response to the complimentary copy he had received, not realising that the author was in fact his friend Chambers. In this letter Combe had no reason to feign indifference to transformism, as he might have had in a published review. As in his review, the letter is full of admiration for the work, and he thanked the author 'for much pleasure and instruction'. However, he concluded in very much the same vein as in his review that the author's 'leading idea is not yet proved; it was impossible for you to do so in the present stage of science & of man's experience; but you have invested it with probability, and brought so strong an array of phenomena to support it'.[58] When the third edition of *Vestiges* was published, Combe again received a complimentary copy. Again he wrote to thank the anonymous author, this time at greater length.[59] As in his previous letter, Combe largely skated over the subject of the transmutation of species and devoted a substantial part of the letter to a detailed critique of the section of the book dealing with the development of human society, a subject clearly closer to Combe's heart. It can only be concluded that, while Combe does not seem to have been shocked or scandalised by the transformist doctrines expounded in *Vestiges*, neither were they of great interest to him. While he was prepared to countenance the possibility that Chambers' development hypothesis might have had merit, he certainly did not consider that the case for it had been proved.

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[57] [Combe], Review of *Vestiges*, p.78

[58] George Combe to [Robert Chambers], 30 October 1844, Combe papers (National Library of Scotland, Ms.7388), f.780

[59] George Combe to [Robert Chambers], 1 March 1845, Combe papers (National Library of Scotland, Ms.7390), ff.66–71

There is also strong evidence from one of Combe's staunchest critics that Combe was no transformist. In 1836, spurred on by the success of the People's Edition of *Constitution* published the previous year, William Scott (1782–1841) published an attack on Combe's ideas entitled *The Harmony of Phrenology with Scripture: Shewn in a Refutation of the Philosophical Errors Contained in Mr Combe's 'Constitution of Man'*. Scott had himself been a leading phrenologist in Edinburgh and was a former president of the Edinburgh Phrenological Society. He knew Combe well, but had broken with him and the Society over Combe's doctrine of the natural laws and advocacy of universal progress. In his book, Scott dealt harshly with Combe, describing his theories as 'a low and grovelling system.'[60] He reserved particular scorn for Combe's progressivism, stating that:

> We have, therefore, every kind of evidence, positive and negative, for asserting, that neither in the vegetable nor in the animal creation is there any such thing as a natural state of progression; and that no race or species of either has ever, as a species, improved itself, or shewn any symptom of 'possessing within itself the elements of improvement.'[61]

However, one thing Scott never accused Combe of was advocating transformism. Rather he noted that Combe 'distinctly and correctly states, that each new race of plants or animals was the result of a separate act of creation; and he states, moreover, in the very outset of his work, the general fact, that every creature, and every physical object, "has received its own *definite constitution*."'[62] Scott knew Combe and his opinions well, and was under no obligation to spare him if he had evidence that he held views that could be used against him. It therefore seems more than likely that we can take Scott at his word when he declared Combe innocent of advocating the transformation of species.

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[60] Scott, *Harmony of Phrenology with Scripture*, p.325
[61] Ibid., pp.16–17
[62] Ibid., p.12

While Combe may not have been a transformist, or indeed known much about transformist theories, his work still had a profound influence on Chambers. As noted above, for two of the central ideas embodied in *Vestiges* Chambers clearly owes a profound debt to Combe: the reality of progressive change and the central importance of natural law. One of the most important ideas found in Combe's work was his assertion of the power of the environment to effect positive or negative change on animals or people. In *Constitution* Combe suggested that social reform and a reduction in working hours could produce

> improvement in the organic, moral, and intellectual capabilities of the race; for the active moral and intellectual organs in the parents would tend to increase the volume of those in their offspring – so that each generation would start not only with greater stores of acquired knowledge than those which its predecessors possessed, but with higher natural capabilities of turning them to account.[63]

There is clearly a close analogy between the picture of human progress through the positive influence of the social environment envisaged here and the progressive development of increasingly advanced forms of animal life through the action of a favourable environment on the mother proposed by Chambers. We have seen above that natural laws played a crucial role as secondary causes underlying the universal progress that Chambers observed in nature. For Combe too, 'every mode of action, which is said to take place according to a natural law, is inherent in the constitution of the substance of being'.[64] It can be concluded that, while Combe himself was not an advocate of transformism, of which it seems he knew little, his ideas of progress through natural law had a profound influence on Chambers and are in evidence throughout *Vestiges*. Both he and many of the transformist thinkers we have examined believed passionately in the reality of universal progress and the supremacy of natural law, beliefs with their roots in late-Enlightenment optimism. In this they do have a deep philosophical kinship, even if no more direct link can be

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[63] Combe, *Constitution of Man*, 4th ed., p.61
[64] Ibid., p.8

established between them. He may not have been a source of the theory of transformism proposed by Chambers, or even any of its elements, but his writings held out the prospect of continuous, law-governed progress of the kind that also made transformism such a compelling idea for Chambers and for many of his readers.

Conclusion

In this penultimate chapter I have attempted to explore the sources of the transformist ideas expressed in Chambers' *Vestiges*. There is no denying that Combe's advocacy of universal progress provided the central inspiration for Chambers' work. The overarching emphasis on progress through natural law is clearly derived from this source. It would indeed be difficult to imagine that *Vestiges* would have been written at all without the influence of Combe's optimistic progressivism. In addition, it seems likely that Chambers also owed his emphasis on the effects of the environment to Combe. Combe saw a favourable environment for people as a prerequisite for the fullest development of the individual as well as for social progress, while Chambers transferred this idea to the whole of the living world through the power of a favourable environment to prolong gestation, allowing new, more advanced forms to appear.

As regards Chamber's knowledge of the works of earlier transformists, the evidence of *Vestiges* indicates at best a limited and superficial knowledge of older transformist theories. Chambers repeated a tired and erroneous misinterpretation of Lamarck's theories, even although he seems to have been familiar with Lyell's much more fair and accurate appraisal of Lamarckism. This raises the question of why Chambers repeated the more hostile account of Lamarck's theories when he had access to the more sympathetic one provided by Lyell. Perhaps he simply wanted to differentiate his theory from Lamarck's, which clearly it has more in common with than Chambers might have wanted to admit, but it is impossible to know for certain.

There is no evidence that Chambers had any knowledge of Geoffroy's transformist speculation, with which Chambers' theory in any case has much less in common. This is not to say, however, that Chambers owes nothing to Geoffroy. His work is steeped in ideas derived from the works of Edinburgh-educated medical writers who were profoundly influenced by Geoffroy and other continental comparative anatomists and embryologists. Almost every element of his theory of transformism can be found in the Edinburgh extra-mural lecturer John Fletcher's *Rudiments of Physiology*, including the idea of unity of plan, the recapitulation theory of foetal development and the crucial connection between increased length of gestation and the development of more advanced forms. Coupled with the concept, derived from Carpenter, that all animals diverged off from a common developmental track during foetal development, this formed the backbone of Chamber's theory of 'the great gestation of nature.' In this sense there was a very real link between the intellectual ferment in which transformist ideas had prospered in the Edinburgh of the 1820s and early 1830s and the eruption of transformism into the public sphere in the form of *Vestiges* in 1844.

In summary, the overarching natural-theological underpinnings of Chambers' development hypothesis, in which immutable natural laws established by the deity ensure universal progress, seems to have been borrowed wholesale from Combe's *Constitution*. On the other hand, the details of his theory of 'the universal gestation of nature' can practically all be found in Fletcher's *Rudiments of Physiology*, with some important modifications based on Von Baer's embryology, almost certainly derived from Carpenter's *Principles of General and Comparative Physiology*. While it is not impossible that Chambers drew some of these ideas from other sources, it is striking that his entire theory could well have been derived from his reading of these three sources without leaving many obvious gaps. Although Chambers was broadly aware of some earlier transformist theories, notably that of Lamarck, his knowledge of their details seems to have been at best sketchy. While they may have sown the seeds of the possibility of the transmutation of species in his mind, he does not seem to have relied on them to any significant extent when developing his own theory. Although Fletcher rejected transformism himself, his book did hint at transformist interpretations of the evidence of comparative anatomy and embryology. He presented these in such a way that Chambers would have had little work to do to tailor these ideas to transformist ends, in line with his belief in the principle of universal progress he shared with Combe. Fletcher therefore provides a very real link between the ferment of new ideas that supported transformist interpretations of the natural world in Edinburgh in earlier decades and Chambers' transformist magnum opus. Although he may have come to his theory of transformism in an indirect way, it would almost certainly not have been possible to construct it at all if it had not been for the flowering of interest in new ideas in medical and the natural history circles in the relatively tolerant atmosphere of the Edinburgh of the 1820s and early 1830s.

Chapter 7: Conclusion

We have seen that both James Secord and Pietro Corsi have suggested that transformist ideas were better known and more widely accepted in the early decades of the nineteenth century than has often been acknowledged.[1] I believe that the research that has been presented in this study strongly confirms their belief in the case of Edinburgh. It seems that an interest in the transmutation of species and even wholehearted acceptance of transformist theories were not confined to a few radicals on the margins of the Edinburgh medical establishment, as has sometimes been suggested.[2] Even the professor of natural history at Edinburgh, Robert Jameson, very much a figure of the academic establishment, was prepared to countenance the possibility of the transmutation of species, and did much to promote the spread of transformist ideas both though his patronage of leading advocates of transformism, must notably Robert Grant, and through his editorship

[1] See, in particular, Secord, 'Edinburgh Lamarckians'; and Corsi, 'Before Darwin'
[2] See, for example, Desmond, 'Robert E Grant'

of the *Edinburgh New Philosophical Journal*, which published a significant number of transformist papers in the late 1820s and early 1830s. Although his attitude towards transformism hardened in later decades, even John Fleming, a leading figure of the Evangelical Party in the Church of Scotland, appears to have been surprisingly open to Lamarck's theories in his writings of the 1820s, and this openness seems to have been typical for Edinburgh natural historians of the period, even for those who did not themselves accept the reality of the transmutation of species.

At the centre of a patronage network which connected almost all of the principal figures discussed in this study was the somewhat enigmatic figure of Robert Jameson. As the professor of natural history and keeper of the University's museum, he wielded significant power and influence in the Edinburgh natural history community. The sometimes arbitrary way in which Jameson granted or denied access to the museum and its collections earned him a stern rebuke in the report of the Scottish Universities Commission of 1826, who considered that 'It is difficult to conceive a more injurious or arbitrary power of controul than that which the Professor claims a right to exercise, and which may obviously be made to operate very oppressively and very unequally.'[3] It is clear that access to the valuable collections depended very much on the whim of the professor, who thought nothing of denying it to those to whom he took a dislike. Nonetheless, he was generous to his friends with access to specimens. Robert Knox, for example, had cause to thank Jameson in print for giving him access to specimens on at least five occasions between 1824 and 1826.[4] It was presumably of such favours that Patrick Neill, the secretary of the Wernerian Society, was thinking when he wrote of Knox that it was

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[3] Scottish Universities Commission (1826), *General Report of the Commissioners Appointed to Visit the Universities and Colleges of Scotland (*October 1830), p.98

[4] See Knox, 'An account of the *Foramen centrale*'; Knox, 'Inquiry into the origin and characteristic differences of the native races'; Robert Knox, 'On the Wombat of Flinders', *Edinburgh New Philosophical Journal* 1 (1826), pp. 104–12; and Knox, 'Observations on the duck-billed animal'

'in the University Museum, and under the auspices of Professor Jameson, that he first had an opportunity to distinguish himself'.[5]

Jameson's status as founder and life president of the Wernerian Natural History Society also gave him direct contact with almost all of the leading figures in natural history in Scotland. However, the role that perhaps furthest extended Jameson's influence was his editorship of the *Edinburgh New Philosophical Journal*, one of the most important British scientific journals of the period, which published papers by scholars from across Europe and beyond, as well as by luminaries of the Edinburgh natural history scene. 'Jameson's journal', as it was often referred to by contemporaries, also played an important role in the dissemination of transformist ideas, as it published a stream of articles on the transmutation of species in the 1820s. In particular, it provided a valuable forum for Grant to publish his transformist theories. Jameson also published a number of anonymous transformist articles, for at least one or two of which there is some evidence he may have written them himself. He published a significant number of papers by former students, including the transformist geologist Ami Boué, who is also a likely candidate for the authorship of one or more anonymous transformist articles. But Jameson's journal was not the only conduit for transformist ideas in Edinburgh. As long ago as 1978 Pietro Corsi had pointed out that Fleming's relatively nuanced account of Lamarck's theories in his *Philosophy of Zoology* (1822) may well have introduced many natural historians to his transformist speculations ten years before Charles Lyell's more famous intervention in the debate in the second volume of his *Principles of Geology* (1832).[6] For many English-speaking readers Fleming's book and the papers published in the *Edinburgh New Philosophical Journal* were likely to have been their first introduction to continental transformist theories.[7]

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[5] Neill, *Supplement to an Address to the Wernerian Natural History Society*, p.26

[6] Corsi, 'The importance of French transformist ideas', pp.222–4

[7] An even earlier Scottish source for the transmission of transformist ideas was an 1811 review by Lockhart Muir, the professor of natural history at Glasgow; see [Lockhart

Robert Grant is the one figure for whom we have unequivocal evidence in print from the mid-1820s onwards that he openly advocated transformism, primarily from the pages of Jameson's journal. If transformism was a creed only espoused by radicals on the margins of the medical world at this time, we might expect Grant to have been a marginalised figure in Edinburgh. This was clearly not the case. He appears to have been almost universally respected and admired, even by those natural historians who certainly did not share his belief in the transmutation of species. Overwhelming evidence for this is provided by the veritable constellation of luminaries from medical and natural history circles in Edinburgh, including Sir David Brewster, John Fleming, Alexander Munro, secundus and Robert Jameson, who supported Grant's successful application for the post of professor of comparative anatomy at University College London in 1827.[8] This was not the only occasion on which Jameson used his influence to advance the career of his former student; in 1820, or example, he also recommended Grant for membership of the Linnean Society of London.[9] Fleming and Brewster were both leading members of the Evangelical Party of the Church of Scotland, and no friends to transformism, as was to be made abundantly clear in their reactions to the publication of *Vestiges of the Natural History of Creation* in 1844. However, they both seem to have maintained perfectly cordial relations with Grant in the 1820s. Brewster not only supported Grant's application for the chair at University College London, but he also published a series of papers by Grant in the *Edinburgh Journal of Science*, including one of a decidedly transformist tendency in 1828.[10] Grant seems to have been a particular friend of Fleming, who even did him the honour of naming a newly discovered species of sponge *Grantia*.[11]

Muirhead], Review of Lamarck's *Philosophie Zoologique*, *Monthly Review* 55 (August 1811), pp.473–84.

[8] [Wakley], 'Biographical Sketch of Robert Edmond Grant', p.690

[9] Ibid., p.690

[10] Grant, 'Observations on the generation of the Lobularia digitata'

[11] Fleming, *History of British Animals*, p.524

The existence of an extensive network of friendship and patronage among Edinburgh's natural history community does not mean that there was never any conflict. Although he held a place at the centre of Edinburgh natural history circles, Jameson was a difficult character and managed to fall out with almost everybody around him at one time or another. He had, for example, shared the role of editor of the *Edinburgh Philosophical Journal* with David Brewster until 1824, but Brewster found Jameson impossible to work with and left to found his own *Edinburgh Journal of Science*.[12] As a medical student at the University, Henry Cheek was so alienated by Jameson's teaching style and management of the University museum and the Wernerian Society that he conducted a veritable campaign against him through the pages of the *Edinburgh Journal of Natural and Geographical Science* in the early 1830s. Robert Knox, who helped Cheek edit his journal, also seems to have been sympathetic to his criticisms of Jameson, despite the favour he had received from the professor in the past. Patrick Neill went as far as to accuse Knox of ingratitude for taking Cheek's part.[13] There is, however, no indication that these episodes of conflict were in any sense political, or indeed were over scientific or philosophical differences, but were rather a result of Jameson's character and conduct. Uniquely, Fleming, whose relations with Jameson seem to also have become embittered towards the end of his life, did attack Jameson as a transformist, but only in a book published posthumously in 1859, after Jameson too was dead, and long after transformism had been rendered deeply controversial by the publication of *Vestiges* in 1844.[14]

Adrian Desmond has painted a convincing picture of a situation where transformism and transcendental anatomy in London were strongly associated with a radical faction in medical circles, which after 1827 included Grant, who found

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[12] For an account of the breakdown of Jameson's editorial relationship with Brewster, see W.H. Brock, 'Brewster as a scientific journalist', in A.D. Morrison Lowe and J.R.R. Christie (eds), *'Martyr of Science': Sir David Brewster 178 –1868* (Edinburgh, 1984), pp.37–41

[13] Neill, *Supplement to an Address to the Wernerian Natural History Society*, p.20

[14] Fleming, *Lithology of Edinburgh*, p.15

themselves in conflict with a medical establishment fiercely protective of its privileges. However, the evidence presented in this study suggests that the Edinburgh context was quite different. In place of the hostile political camps of the London medical scene depicted by Desmond, what emerges from the surviving evidence from Edinburgh in the early decades of the nineteenth century is a surprisingly inclusive network of personal friendships and patronage extending across the extra-mural medical schools, the university and wider natural history circles in the city. When conflict occurred, which, as we have seen, it sometimes did, it was generally between individuals and for personal reasons, and cannot easily be interpreted as ideological conflict between representatives of entrenched political camps or interest groups. Personal power struggles broke out from time to time between individuals, for example, over editorial control of a journal, or access to the University's museum, rather than between groups of actors representing identifiable social or political interests. Jameson seems to have generally been at the centre of these conflicts, but no clear pattern emerges regarding his antagonists. Brewster, for example, was an Evangelical and no friend of transformism, while Cheek was a rebellious student who openly avowed his belief in the transmutation of species. Each had his own reasons for challenging the authority of Jameson, which stemmed from the perceived unreasonableness of the professor's behaviour. That Jameson's behaviour could at times be arbitrary and unreasonable and that he was overly jealous of his prerogatives is amply attested by the report of the 1826 Scottish Universities Commission on the University. But there is no evidence that transformism and its supposed political or social correlates were ever the issue.

While, as Corsi admits, transformist theories clearly could provoke 'anxiety in moderate and conservative intellectual and scientific circles' across Europe before 1844, it is not clear to what extent it is possible to map the social and political tensions and conflicts of the period onto these scientific debates.[15] It sometimes seems too easy to assign transformism as the province of political and scientific

[15] Corsi, 'Lamarck, "From Myth to History"', p. 16

radicals on the fringes of respectable society. However, the evidence presented in this study of the reception of transformism in Edinburgh strongly suggests that this neat equation is by no means reflected in the picture that emerges from a detailed analysis of contemporary sources. There is little solid evidence that many people prior to the publication of *Vestiges* were drawing political or social morals from the transmutation of species or claiming that transformism was a threat to the political and social order. The only concerted ideological opposition seems to have come from the evangelicals, who saw transformism as incompatible with some of the most important doctrines of their faith. Even the evangelicals were relatively restrained in their criticisms of Lamarck in the 1820s, and appear to have treated his theories with respect, although they generally ended up rejecting them as unduly speculative.

Evangelical hostility towards transformism in Edinburgh from the mid-1830s onwards, usually aimed at the theories of Lamarck, was represented by a group of religiously minded natural historians which included John Stark, Hugh Miller and James Duncan. Even the Evangelical minister John Fleming, who in the early 1820s displayed a surprisingly open attitude towards Lamarck's theories, became increasingly critical of them in his later works. As we have seen in chapter 4, attacks from this quarter became more marked during the 1830s and 1840s, as the Evangelical Party in the Church of Scotland in general became more strident and assertive in the run up to the Disruption of 1843. As the Disruption approached science seems to have become another important battle ground between Evangelicals and Moderates within the Church of Scotland.[16] In a book published six years after the Disruption Hugh Miller lumped together Chambers' 'development hypothesis' with the Moderate natural-theological tradition; in his opinion both made Christianity seem 'an idle and unsightly excrescence on a code

[16] For a fascinating analysis of Evangelical attitudes to science in the run up to the Disruption, see Baxter, 'Deism and development'

of morals that would be perfect were it away.'[17] Two particular aspects of transformism made it unpalatable to the Evangelicals. Firstly, it seemed to deny God's direct, supernatural intervention in the history of the earth, and hence to set limits on God's power. The ideas of a universe governed by natural law that left God without a role in the unfolding of his own creation was unthinkable to them. Secondly, the idea of a continuously progressing cosmos was anathema to evangelicals who considered that the Fall had rendered both humanity and nature essentially corrupt and degenerate. They preferred to see the history of the world as a story of decline from original perfection rather than of progress. It is therefore hardly surprising that those Scottish evangelicals who took an interest in natural history formed the main opponents of transformism in this period. However, this does not mean that they were closed-minded scientific reactionaries; some evangelical scientists, such as Hugh Miller and David Brewster, did ground-breaking work in their fields, and from their writings it is clear that they saw themselves as champions of correct scientific practice as much as of true religion.

From the evidence examined in this study a strong connection emerges between progressivist models of earth history and transformist models of the history of life. This is very evident both in the writings of Jameson and in many of the articles published in the *Edinburgh New Philosophical Journal*. In particular, the Neptunist theories of Abraham Gottlob Werner were extremely influential in Edinburgh in the early decades of the nineteenth century, in large part due to the efforts of Jameson, Werner's most important British disciple. In addition, other directional models of earth history, such as the one proposed in Buffon's *Epoques de la Nature* (1778), were widely known and influential. The gradual evolution of the earth provided a powerful and compelling analogy, as well as a potential mechanism, for the progressive development of life. As Corsi has convincingly demonstrated, transformist ideas were much more prevalent across Europe than has been acknowledged by a tradition of historiography that has put Darwin centre-stage and

[17] Miller, *Foot-Prints of the Creator*, p.17

viewed older transformists as at best precursors and at worst irrelevant dreamers.[18] In the process, the strong connection between directional, Neptunist theories of the history of the earth and transformism which emerges so strongly from the Edinburgh sources seems largely to have been missed. It appears that to be 'simultaneously a neputunist, a gradualist, and a transmutationist' may not have represented as much of a contradiction as James Secord thought in his 1991 paper on the 'Edinburgh Lamarckians'.[19]

There are, however, notable exceptions to the progressive nature of transformist thinking among Edinburgh naturalists. The ideas of Robert Knox and Hewett Cottrell Watson in particular are harder to fit into any coherent pattern. Knox was strongly opposed to the idea that the history of life was progressive, while Watson seems to have also believed that transmutation of species did not take place in a directional way, but rather that it was more akin to variation around a mean. Their radical rejection of progressivism sets them apart from most other transformist thinkers of the time. If nothing else, the extreme variants of transformism espoused by Knox and Watson demonstrate the sheer diversity of ideas that emerged from Edinburgh in the early decades of the century.

The early nineteenth-century transformist thinker who has perhaps received the most attention from historians of science is Jean-Baptiste Lamarck, and he was certainly an influential figure in Edinburgh. Particularly after the publication of his *Histoire Naturelle des Animaux sans Vertèbres* (1815–22), Lamarck's work seems to have been widely known, and he was lauded as one of the most important and influential zoologists of his age. His transformist theories received more mixed reviews, although he clearly made some important converts in Edinburgh. Although Lamarck's theories were well known and influential, it was the theories of Geoffroy that seem to have had the largest influence on the Edinburgh

[18] See, in particular, Corsi, 'Before Darwin'
[19] Secord, 'Edinburgh Lamarckians', p.18

transformists. This may have been in part due to the incompatibility between Lamarck's uniformitarian geological views and the progressive vision of earth history prevalent in Edinburgh at the time. Geoffroy's version of transformism was ultimately driven by change in the environment, which accorded better with prevalent idea of an earth that was subject to directional change over geological time, whether this was seen as a result of global cooling, changes in the composition of the atmosphere or the gradual retreat of a universal ocean. We know, for example, that in April 1829 Jameson, the most important Neptunist geologist in the country, gave a paper to the Wernerian Society on Geoffroy's transformist theories, although sadly no account of what he said survives.[20] Grant and Cheek also seem to have been primarily disciples of Geoffroy when it came to formulating their own ideas on the transmutation of species. Lamarck's theories, consonant with his own uniformitarian beliefs in geology, relied instead on an innate tendency to progressive development. Ironically, this model of transformism was to be revived by Robert Chambers, who rejected Lamarck's theories, although his own version of transformism was also clearly based on an inherent tendency towards evolutionary progress.[21] Perhaps Chambers did not want to associate himself with Lamarck, who by 1844 had been the subject of a number of sustained critiques by leading geologists, including Charles Lyell and Hugh Miller, or perhaps he simply wanted to emphasise the originality of his own theory by distancing himself from Lamarck.

As we have seen, there is much evidence for the strength of continental influences in Edinburgh in the early decades of the nineteenth century. Many of the key figures of the time had studied on the continent; for example, Jameson had been a student of Werner in Freiberg and Knox and Grant had attended the lectures of Geoffroy in Paris. Geoffroy's transcendental anatomy had made many converts in Edinburgh medical circles, although some, like John Fletcher, stopped short of adopting his views on transformism. Adrian Desmond has shown how Edinburgh's extra-mural

[20] Minutes of the Wernerian Society, vol. 1, f.297
[21] [Chambers], *Vestiges*, p.230

medical schools played a vital role in disseminating Geoffroy's theory throughout Great Britain the late 1820s and 1830s. They produced a new generation of Edinburgh-educated anatomists and physiologists imbued with the latest continental theories. It is no surprise that the works of some of these Edinburgh-trained medical writers were the source to which Robert Chambers principally turned when developing his own theory of transformism for *Vestiges of the Natural History of Creation*. The relatively open and tolerant intellectual climate that seems to have existed in Edinburgh in the early decades of the nineteenth century allowed a wide variety of innovative ideas to take root and compete. Among the new ideas that emerged in this period were the phrenological theories of George Combe. Combe's emphasis on progress through the operation of natural law also had a powerful influence on Chambers' work, and provided the core around which the whole of his theory of universal development was constructed.

While I am discussing the significance of the Edinburgh transformists for developments in later decades, it would hardly be possible to end this study without asking what its findings might tell us about their role in the development of Charles Darwin's theory of evolution. It was implied by Darwin in his autobiography, written many decades later, that any transformist ideas he had encountered in Edinburgh had had no influence on him. Darwin may have chosen to distance himself from the transformist theories he was almost certainly exposed to in Edinburgh because he was anxious to demonstrate his rigorous adherence to the inductive method that dominated scientific discourse in the nineteenth century. To be seen to be in conformity with inductive principles, his theory had to seem to emerge directly from the data he had gathered rather than growing out of the theories of earlier, more speculative thinkers. Darwin was certainly anxious to be seen to have conformed to correct methodology, later writing of his research methods in the period after 1837 that 'I worked on true Baconian principles, and without any theory collected facts on a wholesale scale'.[22] He certainly showed little enthusiasm for giving any credit his transformist predecessors, whose theories were widely regarded as wild speculation by many of the figures whose approval Darwin would have been most anxious to earn for his theory. The furore that surrounded the publication of *Vestiges of the Natural History of Creation* might have made a denial of any connection with older transmutationist theories seem even more imperative. It was only for the third edition of *Origin of Species* in 1861 that he decided to acknowledge some of his predecessors by adding 'An historical sketch of the recent progress of opinion on *The Origin of Species*'.[23] Grant does feature in the list of earlier transformists included in this sketch; he could hardly be ignored, as he was still alive and lecturing at University College London at the time. Nevertheless, he merited only a brief paragraph that made no mention that he and Darwin had even ever known each other personally. In his 'Recollections', published in 1876, Darwin claimed that Grant had had no influence on his ideas, writing that when Grant 'burst forth in admiration of Lamarck', he 'listened in silent astonishment, and as far as I can judge, without any effect on my mind.'[24]

By his own admission we know that Grant had first been introduced to transformism through reading the works of Erasmus Darwin.[25] It would seem astonishing if he had never mentioned the important influence that Erasmus Darwin had had on his ideas during the time he spent with his grandson on collecting trips along the Firth of Forth. But this is never mentioned by Darwin. Nor does he mention hearing any other talk about transformism during his time in Edinburgh, except for on the one occasion mentioned above. But we now know that Edinburgh was abuzz with a veritable ferment of new and unconventional ideas on the natural world while Darwin was a student there, and that transformism was

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[22] Charles Darwin, 'My several publications', in Darwin, *Autobiographies*, p.72
[23] Charles Darwin, *The Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, 3rd ed. (London,1861), p.xiv
[24] Darwin, 'Recollections of the development of my mind and character', p.24
[25] Grant, *Tabular View*

certainly not least among them. We also know that a significant number of individuals who must have been known to Darwin were active advocates of the transmutation of species. In 1826–7 Darwin attended the same university lectures and Plinian Society meetings, and mixed in the same student circles as Henry Cheek, another avowed transformist. The likely influence of continental ideas on Darwin during his Edinburgh years is also consonant with the clearly Lamarckian flavour of Darwin's thought at the time he was writing his early evolutionary notebooks in the late 1830s, as established by Dov Ospovat.[26] When Darwin's statements are viewed with a broader appreciation of the prevalence of transformist ideas in the Edinburgh of the 1820s, it seems more than likely that he was being somewhat disingenuous when he claimed that his theory of evolution owed nothing to the ideas and individuals he encountered during his time in the city.

Much time and energy has been devoted by historians of science over the years in tracking down the precursors of Darwin and assessing their influence on him, and a vast amount of light has thus been shed on the roots of his theories. In this study I have generally tried to avoid taking this approach, partly because so many eminent scholars have travelled that path before, but also in the belief that when studying the transformists of the earlier nineteenth century it may be more profitable to try to understand them on their own terms and in their own context, temporarily setting aside any question of their later influence. If we can resist the temptation to see early nineteenth century transformists primarily as precursors of Darwin, but rather view them as heirs to a tradition of progressive theorising on the history of the earth and of life that reaches far back into the eighteenth century and was irrigated from the wellsprings of Enlightenment optimism, we may ultimately gain a clearer understanding of their true significance. Pietro Corsi's pioneering work has begun to demonstrate to what extent transformist ideas were not only the province of shadowy figures on the fringes of science and society, but formed one of the

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[26] Dov Ospovat, *The Development of Darwin's Theory: Natural History, Natural Theology and Natural Selection 1838–1859* (Cambridge, 1981), p.53

competing strands of mainstream thought in France and on the broader European stage. I feel that the picture that emerges of the place of transformism in the intellectual life of Edinburgh in the early decades of the nineteenth century revealed by my research fully supports Corsi's conclusions regarding the situation in the wider European context. As Corsi himself has written about Lamarck, although the same could surely be said for the Edinburgh transformists, the assumption that he, or they, were 'isolated because of the religious and philosophical implications and consequences of his transformist doctrines has no foundation, and can be maintained only by ignoring the actual state of affairs in contemporary French and European scientific, political and cultural life.'[27]

[27] Corsi, 'Idola Tribus', p.28

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Appendix A: The natural history syllabus for geology, botany and zoology at the University of Edinburgh, 1826–*c*.1830

From the amendments in Jameson’s hand it can be confidently determined that *c.*1830a is earlier than *c.*1830b, although given that the latter is clearly based closely on the former, it is unlikely that a long period of time intervened between them. Corrections in Robert Jameson’s handwriting in the second two syllabuses are indicated as follows: deletions are struck through; insertions are underlined.

1826	***c.*1830a**	***c.*1830b**
IV. GEOLOGY. This branch of Natural History treats of the Structure and Composition of the solid mass of the Earth, and also considers its mode of Formation. The general Cosmical Properties of the Globe, its connection with the Planetary System, and that of the Universe, are also subjects of interesting discussion in my Geology Lectures:– 1. Account of the Physiogomy of the Earth, including discriptions[sic] of High Lands and Low Lands, Plains, Groups of Mountains, Chains of Mountains, and Single Mountains – of the different kinds of Valleys, and of the Inequalities of the Submarine Land. 2. Account of the different kinds of Structure observable in the sold mass of the Earth. 3. On the Materials of which Mountain Rocks are composed. 4. Description of the different classes of Mountain Rocks, beginning with the deepest seated or oldest, and terminating with the uppermost or newest.	IV GEOLOGY. This branch of Natural History treats of the structure and composition of the solid mass of the Earth, and also considers its modes of formation. The general cosmical properties of the Globe, its connexion with the Planetary System and that of the Universe, are also considered. The following is the order of the lectures: – 1. ~~On the~~ Figure, Density, Magnitude, Temperature, Electricity, and Magnetism of the Earth. 2. ~~Account of t~~The Physiogomy of the Earth, including descriptions of High Lands and Low Lands, Plains, including Landes, Steppes, Deserts, and Oases; Groups of Mountains, Chains of Mountains, and Single Mountains, of the different kinds of Valleys, of Caves and Caverns, and of the inequalities of the submarine land. 3. ~~Account of the d~~Different kinds of Structure observable in the solid mass of the Earth; Uses of the Compass and Quadrant explained.	IV GEOLOGY. This branch of Natural History treats of the structure and composition of the solid mass of the Earth, and also considers its modes of formation. The general cosmical properties of the Globe, its connexion with the Planetary System and that of the Universe, are also considered. The following is the order of the lectures: – Cosmical properties of the Earth. – 1. Figure; 2. Density; 3. Magnitude; 4. Temperature; 5. Electricity; 6. Magnetism. 2. Physiogomy of the Earth, including descriptions of High Lands and Low Lands; Plains, including Landes, Steppes, Deserts, and Oases; Mountains, including Single Mountains, Chains of Mountains, and Groups of Chains of Mountains; Valleys; Caves, Caverns; inequalities of the Submarine land. 3. Structures observable in the solid mass of the Earth; Uses of the Compass and Quadrant explained. 4. Materials of which mountains are composed.

1826	***c*.1830a**	***c*.1830b**
5 Description of the Species of Mountain Rocks, their various natural relations, and their use in the economy of nature and to mankind. 6. On Veins, and as connected with this subject; details regarding the distribution of Metalliferous Minerals. 7. The Phenomena and Effects of Volcanoes. 8. Descriptions and Arrangement of Soils, or those loose superficial matters that cover the solid strata, and in which plants grow, and many animals live. 9. On the Fossil Organic Remains, especially their Geognostical Distribution in the Crust of the Earth, and the connection by their distribution with the state of the Earth during different periods of its formation. 10. On the Shape, Magnitude, Heat, Electricity and Magnetism of the Earth. 11. On the Formation of Mountains, Valleys and Plains, in reference to the various Phenomena exhibited by the Earth's Physiogomy. 12. Theory of the Earth, as deduced from the facts and views in the previous part of the Course. 13. On the Deluge and the Age of the World. 14. On the Earth, as a Member of the Planetary System – Comparison of its Form, Magnitude, Surface, Light, Atmosphere, and Changes, with	4. ~~On the m~~Materials of which mountains are composed. 5. Account of Quartz, Felspar, Mica, Hornblende, and Limestone, the minerals of which the greater part of the Earth is composed. 6. General Account of different classes of Rocks, viz. Primitive, Transition, *Silurian*, Secondary, Tertiary, ~~Volcanic,~~ Alluvial. 7. ~~On~~ Fossil Organic Remains, their systematic arrangement and Description. Geognostical Distribution in the Crust of the Earth, and that distribution as connected with the State of the Earth during the different periods of formation. 8. Particular Account of the different Neptunian, Plutonian & Volcanic Rock formations, their importance in the economy of Nature, and to Mankind.9. ~~On~~ Mineral Veins; and, as connected with them, details in regard to the Distribution of Metalliferous Minerals. 10. ~~The~~ Phenomena, Effects, and Theory of Volcanoes and Earthquakes. 11. ~~The~~ Formation of Mountains, Valleys, Plains, and Caves. 12. Theory of the Earth, as deduced from the facts and view previously detailed. 13. Deluges, and *The Deluge* explained. 14. Description and Arrangement of Soils; or	5. Account of Quartz, Felspar, Mica, Hornblende, and Limestone, the minerals of which the greater part of the Earth is composed. 6. General Account of different classes of Rocks, viz. Primitive, Transition, *Silurian*, Secondary, Tertiary, Alluvial. 7. Fossil Organic Remains, their systematic Arrangement, and Description. Geognostical Distribution in the Crust of the Earth, and that distribution as connected with the State of the Earth during the different periods of formation. 8. Particular Account of the different Neptunian, Plutonian, and Volcanic Rock formations, their importance in the economy of Nature, and to Mankind. 9. Mineral Veins; and, as connected with them, details in regard to the Distribution of Metalliferous Minerals. 10. Phenomena, Effects, and Theory of Volcanoes and Earthquakes. 11. Formation of Mountains, Valleys, Plains, and Caves. 12. Deluges, considered. 13. Theory of the Earth, as deduced from the facts and view previously detailed. 14. Description and Arrangement of Soils; or those loose superficial matters that cover the

1826	***c*.1830a**	***c*.1830b**
those which have been observed in other parts of the Planetary System, especially the Moon and Sun. 15. On the Planetary System, as forming a part of the Milky Way – of the Connection of the Via Lactea, with other similarly constructed parts of the Sideral System, and of the connection of these with the grand framework of the Universe. *a*. On the Geognostical Structure of Scotland, England, and Ireland – *b*. Mode of conducting Mineral Surveys, and of constructing Geognostical Sections and Maps.	those loose superficial matters that cover the sold strata, and in which plants grow and many live. 15. ~~On t~~The Connexion of Geology with Agriculture and Planting. 16. Account of the Planetary System. 17. ~~On t~~The Earth as a member of the Planetary System, comparison of its Form, Magnitude, Surface, Light, Atmosphere, and Changes, with those which have been observed in other parts of the Planetary System, especially in the Moon and Sun. 18. ~~On the~~ Fixed Stars, as seen by the naked eye and telescope; and on the various groupings and arrangements of these, constituting the Grand System of the Universe. **a*. ~~On the~~ Geognostical Structure of Scotland, England, and Ireland. *b*. Modes employed in searching for useful minerals. *c*. Mode of conducting Mineral Surveys, of constructing Geognostical Sections and Maps, and of modelling Mountains, Hills, and Plains.	sold strata, and ~~in~~on which plants grow and many live. 15. The Connexion of Geology with Agriculture, Planting, and the Characters and Distribution of Diseases. 16. Account of the Planetary System. 17. Comparison of its Form, Magnitude, Weight, Surface, Light and Atmosphere, of the Sun, Moon, and other members of our Planetary System, with those of the Earth. 18. Fixed Stars, as seen by the naked eye and the telescope; and on the various groupings and arrangements of these, constituting the Grand System of the Universe. **a*. Geognostical Structure of Scotland, England, and Ireland. *b*. Modes employed in searching for useful minerals. *c*. Mode of conducting Mineral Surveys, of constructing Geognostical Sections and Maps, and of modelling Mountains, Hills, and Plains.
V. BOTANY In the view of Botany given in this Course, the attention is principally directed to those general details and views which are connected with and illustrative of the other departments of Natural History. It is treated in the following	V. BOTANY. In the view of Botany given in these Lectures the attention is principally directed to those general details and views which are connected with, and illustrative of, the other departments of Natural History. It is treated in the following	V. BOTANY. In the view of Botany given in these Lectures the attention is principally directed to those general details and views which are connected with, and illustrative of, the other departments of Natural History. It is treated in the following

1826	***c*.1830a**	***c*.1830b**
order.– 1. General Account of the Structure and Physiology of Plants. 2. On the Physical and Geographical Distribution of Plants over the Globe. 3. On the Fossil Plants met with in rock formations of different descriptions. 4. Comparison of the present Distribution of Plants, with that exhibited in the Fossil Plants. 5. Deductions illustrative of Gradual Change in the Heat of the Earth, and of Alteration in Climate, as disclosed by the facts in the Physical and Geographical Distribution of Fossil and Living Plants. 6. On the Connection of the Geography of Plants with the Political and Moral History of Man. 7. On the influence which the Phenomena of Vegetation exercises on the Taste and Imagination of Nations. 8. On those grand general relations of the Vegetable Kingdom, which stand in connection with the Animal and Mineral Kingdoms.	order: 1. General account of the Structure and Physiology of Plants. 2. ~~On the~~ Physical and Geographical Distribution of Plants over the Globe. 3. ~~On the~~ Fossil Plants met with in Rock Formations of different descriptions. 4. Comparison of the present Distribution of Plants with that exhibited by Fossil Plants. 5. Observations illustrative of the changes in the climate of the Earth, as disclosed by the Physical and Geographical Distribution of Living and Fossil plants. 6. ~~The~~ Natural History of Coal illustrated by ~~reference to~~ the phenomena exhibited by ~~of~~ Fossil Plants. 7. ~~On the~~ Connexion of the Geography of Plants with the Political and Moral History of Man. 8. ~~On the~~ Influence which the Phenomena of Vegetation exercise on the Taste and Imagination of Nations. 9. ~~On those grand g~~General relations of the Vegetable ~~Kingdom, which stand in connexion~~ with the Animal and Mineral Kingdoms.	order: 1. General account of the Structure, Physiology and Systematic Arrangement of Plants. 2. Physical and Geographical Distribution of Plants; *a*. History of the Geography of Plants; *b*. Humboldt's investigations in regard to the distribution of Families of Plants; *c*. Changes in Vegetation, according to height above the level of the sea. and distance from the Equator, as exemplified in the Floras of Scandinavia, Iceland, Great Britain, Alps, Pyrenees, Sicily, Caucasus, Himalaya, Andes, Madeira, Canary Isles, Isle of Ascension, &c.; *d*. Phytogeographic distribution of those Plants employed as food or otherwise by man. 3. Distribution of Fossil Plants in Transition, Secondary, Tertiary, and Alluvial Formations. 4. Comparison of the present Distribution of Plants with that exhibited by Fossil Plants. 5. Changes in the climate of the Earth, as disclosed by the Physical and Geographical Distribution of Living and Fossil plants. 6. Natural History of Coal illustrated by the phenomena exhibited by Fossil Plants. 7. Connexion of the Geography of Plants with the Political and Moral History of Man. 8. Influence which the Phenomena of Vegetation exercise on the Taste and

1826	***c.*1830a**	***c.*1830b**
		Imagination of Nations.
VI. ZOOLOGY. This branch of Natural History is considered in the following order. After explaining the systems of arrangement proposed by Zoologists, and stating my own method, I then proceed to treat of the different Classes of Animals, beginning with those which have the most perfect structure, and concluding our Zoological Descriptions and Histories with an account of the lest perfect or most simple animals. As the Human Species stands apart from the other Animals of the Zoological System, we begin with the	VI. ZOOLOGY. This branch of Natural History, which makes us acquainted with the various Properties and Relations of Animals, is considered in the following order. After explaining the systems of Arrangement proposed by zoologists, we consider the Natural History of the different Classes of Animals, beginning with those which have the most perfect structure, and concluding our zoological descriptions and histories with an account of the least perfect or more simple animals. As the human species stands apart from all the other animals of the zoological system, we begin with the Natural History of	VI. ZOOLOGY. This branch of Natural History, which makes us acquainted with the various Properties and Relations of Animals, is considered in the following order. As the human species stands apart from all the other animals of the zoological system, we begin with the Natural History of
Natural History of Man. 1. General view of the Structure of Man. 2. Physiological relations of Man. 3. Man traced through his first period of existence, or his Monadal State, to the Period of his Birth. 4. Characters by which Man is distinguished from the lower animals. 5. Division into Male and Female – the general and particular characters of each. 6. There is but one species of Man.	MAN. 1. General view of the Structure of Man. 2. Physiological relations of Man. 3. Man traced through his first period of existence, or from the monadal state, to the period of his birth. 4. Characters by which Man is distinguished from the lower animals. 5. Division into Male and Female, the general and particular characters of each.	MAN. 1. General view of the Structure of Man. 2. Physiological relations of Man. 3. Man traced through his first period of existence, or from the monadal state, to the period of his birth. 4. Characters by which Man is distinguished from the lower animals. 5. Division into Male and Female, the general and particular characters of each.

1826	***c.*1830a**	***c.*1830b**
7. The Species of Man is divided into Races, Sub-races, Kinds, Families and Varieties. These defined and described. 8. Man considered as to Colour, Stature, Size, Strength, Longevity. 9. Man traced from the period of his birth, through the different stages of his second existence, until his career terminates in this Planet. 10. Geographical Distribution of Man. 11. Physical Distribution of Man. 12. Population of the Globe. 13. Age of Man – 1. Historically considered – 2. Geologically considered.	6. There is but one species of Man. 7. The species Man is divided into Races, Subraces, Kinds, Families and Varieties. These defined and described. 8. Man considered as to Colour, Stature, Size, Strength, Longevity. 9. Man traced from the period of his birth, through the different stages of his second existence, until his career terminates in this planet. 10. Geographical Distribution of Man. 11. Physical Distribution of Man. 12. Population of the Globe. 13. Age of Man. – 1. Historically considered. 2. Geologically considered.	6. There is but one species of Man. 7. The species Man is divided into Races, Subraces, Kinds, Families and Varieties. These defined and described. 8. Man considered as to Colour, Stature, Size, Strength, Longevity. 9. Man traced from the period of his birth, through the different stages of his second existence, until his career terminates in this planet. 10. Geographical Distribution of Man. 11. Physical Distribution of Man. 12. Population of the Globe. 13. Age of Man. – 1. Historically considered. 2. Geologically considered.
Animals divided into Vertebrate and Invertebrate *VERTEBRATA. *Class 1. Mammalia* – Including Quadrupeds and Cetaceous Animals. 1. Osseous System – 2. Muscular System – 3. Circulating System – 4. Respiratory System – 5. Digestive System – 6. Urinary System – 7. Generative System – 8. Organs of the Senses – 9. Nervous System – 10. Cutaneous System, its varieties and kinds – 11. Organs of Locomotion – 12. Generation – 13. Hybrids – 14. Hybernation – 15. Longevity – 16. Number – 17. Uses in the Economy of	ANIMAL KINGDOM *Divided into* VERTEBRATE *and* INVERTEBRATE *VERTEBRATE, *provided with an Internal Skeleton.* FIRST SUB-KINGDOM. VERTEBRATA. *Characters of this Sub-Kingdom enumerated.*	ANIMAL KINGDOM *Divided into Sub-Kingdoms.* FIRST SUB-KINGDOM. VERTEBRATA or ENCEPHALATA (*Osteozoa. Myelencephala.*) *Characters of this Sub-Kingdom enumerated.* CLASS I. MAMMALIA. *Including Quadrupeds and Cetaceous Animals.* 1. Osseous System; 2. Muscular System; 3.

1826	*c.*1830a	*c.*1830b
Nature – 18. Migrations – 19. Geographical and Physical Description – 20. Domestication – 21. Dietetical Uses – 22. Diseases. Account of the Characters used in the description, arrangement, and determination of Species. Then follows an account of the Orders, Genera, and Species, as for as is necessary for the Student. Description and History of the different Species of Fossil Mammalia.	CLASS I. MAMMALIA. *Including Quadrupeds and Cetaceous Animals.* 1. Osseous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Urinary System; 7. Generative System; 8. Organs of the Senses; 9. Nervous System; 10. Cutaneous System, its varieties and kinds; 11. Organs of Locomotion; 12. Generation; 13. Hybrids; 14. Hybernation; 15. Longevity; 16. Number; 17. Migrations; 18. Uses in the economy of Nature; 19. Geographical and Physical Distribution; 20. Domestication; 21. Dietetical Uses; 22. Diseases. * Account of the characters used in the Description, Arrangement, and Determination of Species. ** Account of the principal Orders, Genera, and Species. *** Description and History of the different genera and species of Fossil Mammalia.	Circulating System; 4. Respiratory System; 5. Digestive System; 6. Urinary System; 7. Generative System; 8. Organs of the Senses; 9. Nervous System; 10. Cutaneous System; its varieties and kinds; 11. Organs of Locomotion; 12. Generation; 13. Hybrids; 14. Hybernation; 15. Longevity; 16. Number; 17. Migrations; 18. Uses in the economy of Nature; 19. Geographical and Physical Distribution; 20. Domestication; 21. Dietetical Uses; 22. Diseases. * Characters used in the Description, Arrangement, and Determination of Species. ** Account of the principal Genera and Species. *** Fossil Mammalia.
Class 2. Aves. – 1. Osseous System – 2. Muscular System – 3. Circulating System – 4. Respiratory System – 5. Digestive System – 6. Urinary System – 7. Generative System – 8. Organs of the Senses – 9. Nervous System – 10. Cutaneous System – 11. Organs of Locomotion – 12. Generation – 13. Hybrids – 14. Hybernation – 15. Longevity – 16. Number –	CLASS II. AVES. 1. Osseous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Urinary System; 7. Generative System; 8. Organs of the Senses; 9. Nervous System; 10. Cutaneous System, its varieties and kinds; 11. Organs of Locomotion; 12. Generation; 13. Hybrids; 14. Nidification;	CLASS II. AVES. 1. Osseous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Urinary System; 7. Generative System; 8. Organs of the Senses; 9. Nervous System; 10. Cutaneous System, its varieties and kinds; 11. Organs of Locomotion; 12. Generation; 13. Hybrids; 14. Nidification;

1826	*c.*1830a	*c.*1830b
17. Nidification – 18. Pairing – 19. Geographical and Physical Distribution – 20. Uses in the Economy of Nature – 21. Domestication – 22. Dietetical Uses – 23. Diseases. I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. III. Description and History of the Genera and Species of Fossil Birds.	15. Pairing; 16. Migration; 17. Longevity; 18. Number; 19. Geographical and Physical Distribution; 20. Uses in the economy of Nature. * ~~Account of the~~ characters used in the Description, Arrangement, and Determination of Species. ** Account of the principal Orders, Genera, and Species, as far as is necessary for the student. *** Description and History of the different genera and species of Fossil Birds.	15. Pairing; 16. Migration; 17. Longevity; 18. Number; 19. Geographical and Physical Distribution; 20. Uses in the economy of Nature; 21. Domestication; 22. Dietetical Uses; 23. Diseases. * Characters used in the Description, Arrangement, and Determination of Species. ** Account of the principal Orders, Genera, and Species, as far as is necessary for the student. *** Fossil Birds.
	CLASS III. REPTILIA. 1. Osseous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Urinary System; 7. Generative System; 8. Organs of the Senses; 9. Nervous System; 10. Cutaneous System; 11. Organs of Locomotion; 12. Generation; 13. Hybernation; 14. Longevity; 15. Number; 16. Geographical and Physical Distribution;17. Dietetical Uses; 18. Uses in the economy of Nature; 19. 20. Domestication; 21. 22. Diseases. * ~~Account of the~~ characters used in the Description, Arrangement, and Determination of Species. ** Account of the principal Orders, Genera, and Species.	CLASS III. REPTILIA. 1. Osseous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Urinary System; 7. Generative System; 8. Organs of the Senses; 9. Nervous System; 10. Cutaneous System; 11. Organs of Locomotion; 12. Generation; 13. Hybernation; 14. Longevity; 15. Number; 16. Geographical and Physical Distribution; 17. Dietetical Uses; 18. Uses in the economy of Nature. * Characters used in the Description, Arrangement, and Determination of Species. ** Principal Genera and Species. *** Fossil Reptilia.

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	*** Description and History of the different genera and species of Fossil Amphibia.	
Class 3. Amphibia. – 1. Osseous System – 2. Muscular System – 3. Circulating System – 4. Respiratory System – 5. Digestive System – 6. Urinary System – 7. Generative System – 8. Organs of the Senses – 9. Nervous System – 10. Cutaneous System – 11. Organs of Locomotion – 12. Generation – 13. Hybernation – 14. Longevity – 15. Number – 16. Geographical and Physical Distribution – 17. Dietetical Uses – 18. Use in the Economy of Nature. I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. III. Description and History of the Genera and Species of Fossil Amphibia.		CLASS IV. AMPHIBIA. 1. Osseous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Urinary System; 7. Generative System; 8. Organs of the Senses; 9. Nervous System; 10. Cutaneous System; 11. Organs of Locomotion; 12. Generation; 13. Hybernation; 14. Geographical and Physical Distribution; 15 Uses in the economy of Nature. * Characters used in the Description, Arrangement, and Determination of Species. ** Principal Genera and Species. *** Fossil Amphibia.
Class 4. Pisces. – 1. Osseous System – 2. Muscular System – 3. Circulating System – 4. Respiratory System – 5. Digestive System – 6. Urinary System – 7. Generative System – 8. Organs of the Senses – 9. Nervous System – 10. Cutaneous System, its kinds and varieties – 11. Organs of Locomotion – 12. Organs of Reproduction – 13. Electrical Organs – 14. Longevity – 15. Number – 16. Physical and	CLASS IV. PISCES. 1. Osseous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Urinary System; 7. Generative System; 8. Organs of the Senses; 9. Nervous System; 10. Cutaneous System, its kinds and varieties; 11. Organs of Locomotion; 12. Organs of Reproduction; 13. Electrical Organs; 14. Longevity; 15. Number; 16.	CLASS V. PISCES. 1. Osseous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Urinary System; 7. Generative System; 8. Organs of the Senses; 9. Nervous System; 10. Cutaneous System, its kinds and varieties; 11. Organs of Locomotion; 12. Organs of Reproduction; 13. Electrical Organs; 14. Longevity; 15. Number; 16.

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Geographical Distribution – 17. Migrations – 18. Uses in the Economy of Nature – 19. Domestication – 20. Dietetical Uses – 21. Diseases. I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. III. Description and History of Fossil Fishes.	Migrations; 17. Geographical and Physical Distribution;17. Uses in the economy of Nature; 19. Domestication; 20. Dietetical Uses; 21. Diseases. * ~~Account of the~~ characters used in the Description, Arrangement, and Determination of Species. ** Account of the principal Orders, Genera, and Species. *** Description and History of the different genera and species of Fossil Fishes, according to the system of Agassiz.	Migration; 17. Physical and Geographical Distribution;17. Uses in the economy of Nature; 19. Domestication; 20. Dietetical Uses; 21. Diseases. * Characters used in the Description, Arrangement, and Determination of Species. ** Principal Genera and Species. *** Fossil Fishes, according to the system of Agassiz.
Class I Mollusca. – 1. Cutaneous System, with Account of the mode of Formation of Shells – 2. Muscular System – 3. Circulating System – 4. Digestive System – 5. Respiratory System – 6. Generative System – 7. Organs of the Senses – 8. Nervous System – 9. Organs of Locomotion – 10. Number – 11. Physical and Geographical Distribution – 12. Uses in the Economy of Nature – 13. Domestication – 14. Dietetical Uses. I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student.	INVERTEBRATE, *without an internal Skeleton*. SECOND SUB-KINGDOM. MOLLUSCA. 1. Cutaneous System, with an account of the mode of formation of Shells; 2. Muscular System; 3. Circulating System; 4. Digestive System; 5. Respiratory System; 6. Generative System; 7. Organs of the Senses; 8. Nervous System; 9. Organs of Locomotion; 10. Number; 11. Physical and Geographical Distribution;12. Uses in the economy of Nature; 13. Domestication; 14. Dietetical Uses.	SECOND SUB-KINGDOM. MOLLUSCA or CYCLOGANGLIATA. 1. Cutaneous System, with an account of the mode of formation of Shells; 2. Muscular System; 3. Circulating System; 4. Digestive System; 5. Respiratory System; 6. Generative System; 7. Organs of the Senses; 8. Nervous System; 9. Organs of Locomotion; 10. Number; 11. Physical and Geographical Distribution;12. Uses in the economy of Nature; 13. Domestication; 14. Dietetical Uses.

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III. Description and History of Fossil Mollusca. *II. Insecta.* – 1. Cutaneous System – 2. Muscular System – 3. Circulating System – 4. Respiratory System – 5. Digestive System – 6. Generative System – 7. Organs of the Senses – 8. Nervous System – 9. Organs of Locomotion – 10. Number – 11. Longevity – 12. States of Insects – 13. Societies – 14. Noises – 15. Luminosity – 16. Hybernation – 17. Instincts – 18. Geographical and Physical Distribution – 19. Uses in the Economy of Nature – 20. Domestication – 21. Dietetical Uses – 22. Diseases – 23. Diseases in Man and other Animals, also in Plants, occasioned by Insects. I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. III. Description and History of Fossil Insects. *III. Arachnidae.* – 1. Form and Structure – 2. Functions – 3. Geographical and Physical Distribution – 4. Instincts – 5. Food. I. Account of the Characters used in the description, arrangement, and determination	CLASS I. CEPHALOPODA. Form and Structure; Functions; Geographical and Physical Distribution. Account of the principal Orders, Genera, and Species. The Fossil Species considered. CLASS II. PTEROPODA. Form and Structure; Functions; Geographical and Physical Distribution. Account of the principal Orders, Genera, and Species. The Fossil Species considered. CLASS III. GASTEROPODA. Form and Structure; Functions; Geographical and Physical Distribution. Account of the principal Orders, Genera, and Species. The Fossil Species considered. CLASS IV. ACEPHALA Form and Structure; Functions; Geographical and Physical Distribution. Account of the principal Orders, Genera, and Species. The Fossil Species considered. CLASS V. BRACHIOPODA	CLASS I. CEPHALOPODA. 1. Form and Structure; 2. Functions; 3. Physical and Geographical Distribution. Orders. – 1. Cryptodebranchia; 2. Syphonifera; 3. Foraminifera. * Fossil Cephalopoda. CLASS II. PTEROPODA. 1. Form and Structure; 2. Functions; 3. Physical and Geographical Distribution. Orders. – 1. Gymnosomata; 2. Thecosomata. CLASS III. GASTEROPODA. 1. Form and Structure; 2. Functions; 3. Physical and Geographical Distribution; 4. Dietetical Uses. Orders. – 1. Pulmonata; 2. Tectibranchia; 3. Nudibranchia. * Fossil Gasteropoda. CLASS IV. CONCHIFERA OR ACEPHALA. 1. Form and Structure; 2. Functions; 3. Physical and Geographical Distribution; 4. Dietetical Uses. Orders. – 1. Dimyia; 2. Monomyia. * Fossil Conchifera. CLASS V. TUNICATA 1. Form and Structure; 2. Functions; 3. Physical

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of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. III. Description and History of Fossil Arachnidae. *IV. Crustacea.* – 1. Form and Structure – 2. Functions – 3. Geographical and Physical Distribution – 4. Instincts – 5. Food – 6. Relation of the Sexes – 7. Dietetical Uses. I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. III. Description and History of Fossil Crustacea. *V. Radiaria.* – 1. Form and Structure – 2. Functions – 3. Geographical and Physical Distribution – 4. Instincts – 5. Food. I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. III. Description and History of Fossil Radiaria.	Form and Structure; Functions; Geographical and Physical Distribution. Account of the principal Orders, Genera, and Species. The Fossil Species considered. CLASS VI CIRRHOPODA Form and Structure; Functions; Geographical and Physical Distribution. Account of the principal Orders, Genera, and Species. The Fossil Species considered. THIRD SUB-KINGDOM. ARTICULATA. – *Characters of the Articulata*. CLASS I. ANNELIDA. 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution. * Account of the principal Orders, Genera, and Species. CLASS II. CRUSTACEA. 1. Form and Structure; 2. Functions; 3. Instinct; 4. Geographical and Physical Distribution. * Account of the Characters used in the Description, Arrangement, and Determination of the Species.	and Geographical Distribution. Orders. – 1. Ascidiaria; 2. Salparia. THIRD SUB-KINGDOM. ARTICULATA *(Homogangliata).* CLASS I. CRUSTACEA. 1. Form and Structure; 2. Functions; 3. Physical and Geographical Distribution; Dietetical Uses. * Orders. – 1. Decapoda; 2. Stomapoda; 3. Amphipoda; 4. Laemodipoda; 5. Isopoda; 6. Branchiopoda; 7. Paecilopoda. ** Fossil Crustacea. CLASS II. ARACHNIDA. 1. Form and Structure; 2. Functions; 3. Physical and Geographical Distribution. * Orders. – 1. Pulmonata; 2. Tracheata. ** Fossil Arachnida.

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VI. Annularia. – 1. Form and Structure – 2. Functions – 3. Geographical and Physical Distribution – 4. Instincts – 5. Food. I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. III. Description and History of Fossil Annularia. *VII. Enthelmintha.* – 1. Form and Structure – 2. Functions – 3. Geographical and Physical Distribution – 4. Instincts – 5. Their connection with Diseases in Man and other Animals. I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. *VIII. Protozoa.* – 1. Form and Structure – 2. Functions – 3. Geographical and Physical Distribution – 4. Uses and Importance in the Economy of Nature.	** Account of the principal Orders, Genera, and Species; 5. Dietetical Uses. *** Description and History of Fossil Crustacea. CLASS III. ARACHNIDA. 1. Form and Structure; 2. Functions; 3. Instincts; 4. Food; 5. Geographical and Physical Distribution. * Account of the principal Orders, Genera, and Species. CLASS IV. INSECTA. 1. Cutaneous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Generative System; 7. Organs of the Senses; 8. Nervous System; 9. Organs of Locomotion; 10. Number; 11. Longevity; 12. States; 13. Societies; 14. Noises; 15. Luminosity; 16. Hybernation; 17. Instincts; 18. Geographical and Physical Distribution; 19. Uses in the economy of Nature; 20. Domestication; 21. Dietetical Uses; 22. Diseases; 23. Diseases in Man and other Animals, also in plants, occasioned by insects. * Account of the Characters used in the Description, Arrangement and determination of the Species. ** Description and History of Fossil Insects.	CLASS III. INSECTA. 1. Cutaneous System; 2. Muscular System; 3. Circulating System; 4. Respiratory System; 5. Digestive System; 6. Generative System; 7. Organs of the Senses; 8. Nervous System; 9. Organs of Locomotion; 10. Number; 11. Longevity; 12. States; 13. Societies; 14. Noises; 15. Luminosity; 16. Hybernation; 17. Instincts; 18. Geographical and Physical Distribution; 19. Uses in the economy of Nature; 20. Domestication; 21. Dietetical Uses; 22. Diseases; 23. Diseases in Man and other Animals, also in plants, occasioned by insects. * Orders. – 1. Coleoptera; 2. Dermaptera; 3. Orthoptera; 4. Hemiptera; 5. Neuroptera; 6. Hymenoptera; 7. Lepidoptera; 8. Rhipiptera; 9. Diptera; 10. Syphonaptera; 11. Parasita; 12. Thysanoura. ** Fossil Insecta. CLASS IV. MYRIAPODA. 1. Form and Structure; 2. Functions; 3. Physical and Geographical Distribution. * Orders. – 1. Chilopoda; 2. Chilognatha. ** Fossil Myriapoda ** *Helminthoida or Diploneura* (*Nematoneura*). CLASS V. ANNELIDA.

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I. Account of the Characters used in the description, arrangement, and determination of Species. II. Account of the Orders, Genera, and Species, as for as is necessary for the Student. III. Description and History of Fossil Protozoa.		1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution. * Orders. – 1. Pulmonata; 2. Dorsibranchia; 3. Cephalobranchia; 4. Apnuemata. ** Fossil Annelida. CLASS VI CIRRIPED~~I~~A 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution. * Orders. – 1. Anatafida; 2. Balanida. ** Fossil Cirriped~~i~~a. CLASS VII. ENTOZOA or SUCTORIA. 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution; Connection with Diseases. * Orders. – 1. Epizoa; 2. Nematoidea; 3. Acanthocephala; 4. Trematoda; 5. Cestoidea; 6. Cystica. CLASS VIII. ROTIFERA. 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution. * Orders. – 1. Nuda; 2. Loricata. CLASS IX. BRYOZOA OR CILIBRACHIATA. 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution.

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	FOURTH SUB-KINGDOM. RADIATA OR ZOOPHYTA. – *Characters of Radiata*. CLASS I. ECHINODERMATA. 1. Form and Structure; 2. Functions; 3. Instincts; 4. Food; 5. Geographical Distribution. * Account of the principal Orders, Genera, and Species. ** Description and History of Fossil Echinodermata. CLASS II. INTESTINA OR ENTOZOA. 1. Form and Structure; 2. Functions; 3. Distribution; 4. Food; 5. Their connexions with Diseases in Man and other Animals. * Account of the principal Orders, Genera, and Species. CLASS III. ACEPHALA OR MEDUSARIA. 1. Form and Structure; 2. Functions; 3.	* Orders. – 1. Bowerbankia; 2. Flustra; 3. Cristatella. ** Fossil Cilibranchiata. [p.16] FOURTH SUB-KINGDOM. RADIATA. *Nerves visible and Cycloneurons*. CLASS I. ECHINODERMATA. 1. Form and Structure; 2. Functions; 3. Instincts; 4. Food; 5. Geographical Distribution. * Orders. – 1. Holothurida; 2. Echinida; 3. Asterida; 4. Crinoida. ** Fossil Echinodermata. ~~** *No Nerves discernible.*~~ ~~*Cryptoneura, Acrita, Protzoa, Oozoa.*~~ CLASS II. MALACTINIA OR ACEPHALA. 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution. * Orders. – 1. Palliograda; 2. Physograda; 3. Ciliograda. ** *No Nerves discernible.* *Cryptoneura, Acrita, Protzoa, Oozoa.*

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	Geographical and Physical Distribution; 4. Uses and importance in the Economy of nature. * Account of the principal Orders, Genera, and Species. CLASS IV. POLYPI. 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution; 4. Uses and importance in the Economy of nature. * Account of the principal Orders, Genera, and Species. ** Description and History of Fossil Polypi. CLASS V. INFUSORIA. 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution; 4. *Ehrenberg's* discoveries; 5. Uses and importance in the Economy of nature. * Account of the principal Orders, Genera, and Species.	CLASS III. POLYPI OR POLYPIFERA *(Corallia, Phytozoa, Zoophyta).* 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution; 4. Uses. * Orders. – 1. Corticifera; 2. Lamellifera; 3. Vaginata; 4. Carnosa. ** Fossil Polypina. CLASS IV. POLYGASTRICA OR INFUSORIA. 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution; 4. Uses. * Orders. – 1. Diacaela; 2. Cyclocaela; 3. Anentera. ** Fossil Polygastrica. CLASS V. PORIPHERA (*Amorphozoa*). 1. Form and Structure; 2. Functions; 3. Geographical and Physical Distribution; 4. Uses. * Orders. – 1. Keratosa; 2. Leuconida; 3. Halinida. ** Fossil Poriphera.
PHILOSOPHY OF ZOOLOGY. 1. Origin of the Species of Animals. 2. Their different Modes of Generation, stated with the view of illustrating the Theory of	PHILOSOPHY OF ZOOLOGY 1. Origin of the Species of Animals. 2. Their different modes of generation stated, with the view of illustrating the Theory of	

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Generation in the Animal Kingdom in general. 3. The Growth of Animals. 4. The Decay and Death of Animals. 5. Duration of Animals. 6. Migrations of Animals. 7. Number of Animals. 8. Instinct in general. 9. Hybernation. 10. Distribution of Animals, both physical and geographical, over the surface of the earth, in the waters of the ocean, in the lakes and rivers, and in the air of the atmosphere. 11. The various Revolutions or Changes the Animal World has experienced, from the first creation to the present time. 12. The Connection of the Animal with the Vegetable Kingdom. 13. The Connection of the Animal and Vegetable Kingdoms with the Mineral Kingdom. 14. *Lastly*, The mutual relations that exist amongst all the objects of nature, and those general laws that appear to be common to the whole.	Generation in the animal kingdom in general. 3. ~~The~~ Growth of Animals. 4. ~~The~~ Decay and Death of Animals. 5. Duration of Animals. 6. Migrations of Animals. 7. Number of Animals. 8. Instinct in general. 9. Hybernation. 10. ~~The d~~Distribution of Animals, ~~both~~ Physical and Geographical, over the surface of the Earth, in the Waters of the Ocean, in Lakes and Rivers, and in the air of the Atmosphere. 11. ~~The various~~ Revolutions or Changes which the Animal World has experienced, from the first creation to the present time. 12. ~~The c~~Connexion of the Animal and Vegetable Kingdom. 13. ~~The c~~Connexion of the Animal and Vegetable Kingdom with the Mineral Kingdom. 14. Lastly, The mutual relations that exist amongst all the objects in Nature, and these general laws that appear to be common to the whole.	
CONCLUSION. 1. Instructions and Demonstrations as to the mode of collecting, preserving, transporting, and arranging objects of Natural History.	CONCLUSION. 1. Instructions and Demonstrations as to the mode of Collecting, Preserving, Transporting, and Arranging objects of Natural History.	CONCLUSION. 1. Instructions and Demonstrations as to the mode of Collecting, Preserving, Transporting, and Arranging objects of Natural History; 2.

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2. Collecting of Objects of Natural History strongly recommended. 3. Advantages of Travelling. 4. Books in different branches of Natural History recommended. 5. Plans for future Study of Natural History pointed out and explained.	2. Collecting objects of Natural History strongly recommended. 3. Advantages of Travelling. 4. Books in different branches of Natural History recommended. 5. Plan for future study in Natural History pointed out and explained.	Collecting objects of Natural History strongly recommended; 3. Advantages of Travelling; 4. Books in different branches of Natural History recommended; 5. Plan for future study in Natural History pointed out and explained.

Appendix B: Phrenology, heredity and progress in George Combe's Constitution of Man[1]

This paper is is a draft version of an article to appear in the *British Journal for the History of Science*, volume 48, issue 3. The published version of this article is available online at *http://journals.cambridge.org/action/displayAbstract?fromPage=online&aid=9701251&fileId=S0007087415000278*

Abstract

The *Constitution of Man* by George Combe (1788–1858) was probably the most influential phrenological work of the nineteenth century. It offers not only an exposition of the phrenological theory of the mind, but also presents Combe's vision of universal human progress through the inheritance of acquired mental attributes. In the decades before the publication of Darwin's *Origin of Species* the *Constitution* was probably the single most important vehicle for the dissemination of naturalistic progressivism in the English-speaking world. Although there is a significant literature on the social and cultural context of phrenology, the role of heredity in Combe's thought has been less thoroughly explored, although both John van Wyhe and Victor L. Hilts have linked Combe's views on heredity with the transformist theories of Jean-Baptiste Lamarck. In this paper I examine the origin, nature and significance of his ideas and argue that Combe's hereditarianism was not related to Lamarckian transformism but formed part of a wider discourse on heredity in the early nineteenth century.

Introduction

George Combe (1788–1858) was Britain's leading phrenologist in the first half of the nineteenth century. His writings have been described by Steven Shapin as 'the most important vehicle for the diffusion of naturalistic and materialistic views in early to mid-nineteenth-century Britain'.[1] These ideas were largely presented to the reading public through the medium of Combe's most important work, the *Constitution of Man*, first published in 1828. Over the course of the century it comfortably outsold comparable books of the period, such as Robert Chambers' *Vestiges of the Natural History of Creation* (1844) and Charles Darwin's *Origin of Species* (1859), and was enormously influential; the *Constitution* has been described by John van Wyhe as 'one of the most important books of the second quarter of the nineteenth century'.[2] It was certainly a publishing phenomenon, having sold 100,000 copies in Britain and

[1] Steven Shapin, 'Edinburgh and the diffusion of science in the 1830s', in Ian Inkster and Jack Morrell (eds.) *Metropolis and Province: Science in British Culture, 1780–1850* (London, 1983), p. 158.

[2] John van Wyhe, *Phrenology and the Origins of Victorian Scientific Naturalism* (Aldershot, 2004), p. 109.

around 200,000 copies in America by 1860.[3] It has been estimated that this made it the fourth best-selling book in Britain in the second quarter of the century after the Bible, *Pilgrim's Progress* and *Robinson Crusoe*. [4] As James Secord has pointed out, the book grew and expanded over many editions, and so 'was effectively not a single work, but a serial production'.[5] As for the ideas contained in this book, in his *Life of Richard Cobden* (1881), John Morley considered that its 'principles have now in some shape or form become the accepted commonplaces of all rational persons.'[6] Sales of the first three editions were in fact relatively modest, and they only really took off after the appearance of a cheap 'People's Edition', published in Edinburgh by William and Robert Chambers in 1835. This edition was subsidised by a bequest from William Henderson, a wealthy supporter of phrenology, which allowed the book to be priced at only 1s 6d. Unlike earlier works by Combe, the *Constitution* was not merely a phrenological manual. Much more than this, it presented an all-encompassing vision of human nature and humanity's relationship with the natural world, with the idea of universal progress at its heart.

A discussion of hereditary transmission of mental and physical traits forms an important component of Combe's argument in the *Constitution*. However, relatively few scholars have commented on the importance of these hereditarian doctrines in Combe's work. Three who have addressed this aspect of his thought to some extent are Victor L. Hilts, John C. Waller and John van Wyhe. In general, these scholars have viewed Combe's use of hereditarian ideas in the light either of Lamarckian transformism or of later Darwinian and post-Darwinian evolutionary thought. In a paper on phrenological views of heredity published in 1982, Hilts has suggested that the hereditarian ideas of Combe made him a precursor of the social Darwinists and eugenicists of the late nineteenth and early twentieth centuries. However, he

[3] Ibid., p. 128.
[4] Roger Cooter, *The Cultural Meaning of Popular Science: Phrenology and the Organization of Consent in Nineteenth-Century Britain* (Cambridge, 1984), p. 120.
[5] James A. Secord, *Visions of Science: Books and Readers at the Dawn of the Victorian Age* (Oxford, 2014), p. 194.
[6] John Morley, *The Life of Richard Cobden*, vol. 2 (London, 1881), p. 93.

also noted that Combe's ideas differed from theirs in several crucial respects, including his emphasis on the duty of the individual to obey the laws of heredity and his emphasis on the dangers of in-breeding.[7] In 2001 Waller published a paper that also set out to demonstrate the early nineteenth-century genealogy of social Darwinist and eugenicist ideas and to demonstrate that Francis Galton's role as the 'father' of the eugenics movement had been overstated.[8] To this end he focused on the hereditarian ideas of George Combe and his brother Andrew as evidence for the prevalence of the doctrine of hereditary transmission before 1859. Van Wyhe, in a detailed study of *Phrenology and the Origins of Victorian Scientific Naturalism* (2004), has given some attention to the hereditarian strand in phrenological thought, and in that of Combe in particular, and its wider impact on later nineteenth century ideas. Van Wyhe noted that Combe's 'talk of inheritance, which extended from Man to domesticated animals and their variability, brought to a vast reading audience many of the themes for which Darwin is now better known.'[9] Both van Wyhe and Hilts have drawn parallels between Combe's hereditarianism and Lamarckian transformism. The discourse on heredity has therefore often been seen as merely one facet of a wider discourse on the mutability of species. The idea that traits acquired during the lifetime of a living thing can be transmitted to offspring, an idea now largely associated with the transformism of Jean-Baptiste Lamarck (1744–1829) and his followers, is particularly open to such interpretations. In this paper I will attempt to question the bracketing of Combe's hereditarian ideas, including that of the inheritance of acquired characteristics, with the transmutationist theories with which they are often associated. In order to do this I will look not only at the role of hereditarian ideas in the argument of Combe's book, but also on the sources from which he drew these ideas in order to assess the extent to which these beliefs, so much associated with evolutionary discourses in the minds of modern readers, may

[7] Victor L. Hilts, 'Obeying the laws of hereditary descent: Phrenological views on inheritance and eugenics', *Journal of the History of the Behavioral Sciences* 18 (1982), pp. 62–76.
[8] John C. Waller, 'Ideas of heredity, reproduction and eugenics in Britain, 1800–1875', *Studies in the History and Philosophy of the Biological and Biomedical Sciences* (2001) 32: 3, pp. 457–89.
[9] Van Wyhe, *Phrenology*, p. 122.

have simply represented a common-sense view of inheritance in the context of the 1830s.

Combe the phrenologist

Combe was the most influential phrenologist in Britain in the middle decades of the nineteenth century. He had been a successful Edinburgh lawyer until he retired to devote himself entirely to phrenology after his marriage in 1833. An early convert to this would-be science, his first contact with the subject was through an extremely negative critique of phrenology written by John Gordon he had read in the June 1815 number of the *Edinburgh Review*. At the time, this convinced him that the doctrines described were 'contemptibly absurd'.[10] However, not long after this, in 1816, Combe attended the dissection of a brain by Johann Casper Spurzheim (1776–1832), the great German populariser of the presumptive science, at the house of a friend. After attending a course of lectures by Spurzheim, he 'arrived at complete conviction of the truth of Phrenology.'[11] His growing interest in the subject led him to found the Edinburgh Phrenological Society in 1820. He published a number of works on phrenology, including *Essays on Phrenology* (1819) and *A System of Phrenology* (1824), but it was the *Constitution of Man*, published in 1828, that was to be his most influential and best-selling work.

The fundamental principle behind phrenology was the presupposition that the brain is composed of a number of separate organs, each of which corresponds to a faculty of the human mind. According to phrenologists – although this was vociferously disputed at the time by their critics – the shape of the skull faithfully mirrors the form of the brain within in such a way that the development of these different organs can be read by a skilled practitioner from the shape of the skull. For Combe there were 35 such organs, two more than the 33 proposed by Spurzheim, and significantly more than the 27 suggested by Franz Josef Gall (1758–1828), the

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[10] George Combe, *A System of Phrenology*, 3rd edn (Edinburgh, 1830), p. iii.
[11]Ibid., p. iv.

founder of phrenology. In the *Elements of Phrenology* Combe neatly summarised the essentials of his doctrine as follows: 'the brain is the material instrument by means of which the mind acts, and is acted upon; and it is a congeries of organs.'[12] These organs could be divided into those common to humans and the animals, such as 'amativeness', 'destructiveness' and 'secretiveness', and those proper to humanity alone, such as 'veneration', 'hope' and 'ideality'. These views were at extreme variance with the dominant image of the mind in the early nineteenth century. Followers of the influential Common Sense school of philosophy saw the mind as a mysterious, indivisible monad, the 'thinking principle' of Thomas Reid (1710–97), whose attributes could only be studied through introspection.[13] However, as we will see below, Combe's *Constitution* was much more than just an exposition of the doctrine of the phrenological organs, which only accounts for a relatively small proportion of the work. For Combe's vision of human nature was not a static one, but a story of progress and development.

Phrenology and heredity

As Carlos López-Beltrán has pointed out, 'gathering of convincing cases of hereditary transmission of a wide range of different characteristics, and the progressive closing of alternative avenues of dealing with them (such as their ascription to chance, or their sheer irrelevance), was one of the central themes of the eighteenth-century debates around generation.'[14] By the early decades of the nineteenth century the inheritance of both positive and negative traits of mind and body was therefore well established as a subject of lively debate. According to López-Beltrán, the idea of hereditary transmission 'was adopted in several different fields whose paths coincided towards the third decade of the nineteenth century,

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[12] George Combe, *Elements of Phrenology*, 3rd edn (Edinburgh, 1828), p. 23.
[13] See, for example, Thomas Reid, *An Inquiry into the Human Mind on the Principles of Common Sense* (Edinburgh, 1997), p. 13.
[14] Carlos López-Beltrán, 'Forging heredity: From metaphor to cause, a reification story', *Studies in the History and Philosophy of Science* 25: 2 (1994), p. 219.

when our modern concept of heredity finally acquired its basic structure.'[15] The 1820s were therefore a particularly crucial period for the development of this concept. As Waller has noted, the field of heredity was dominated at this time by three groups: stock breeders and horticulturalists, ethnologists and medical practitioners.[16] The medical men were principally preoccupied by the way hereditary diseases could be transmitted in families. Their main concern therefore was how to avoid the transmission of undesirable traits from generation to generation. The stock breeders and horticulturalists were interested in how both the transmission of desirable qualities could be assured, and the inheritance of undesirable ones avoided. Ethnologists had more of a theoretical interest in the question. For them, the prime interest of heredity lay in its relevance to the question of race. There was much discussion at this period as to whether racial characteristics were fixed for all time or whether they were the result of the different climates in which the different races found themselves. The latter possibility raised the thorny question of why races did not rapidly transform into each other when transplanted to different climates. The attempts of ethnologists to resolve this problem sparked a lively debate on the origins of varieties in the human species and the genealogy of the races. Combe was to draw on the work of all three groups in his writings on heredity.

The first edition of the *Constitution* (1828) contained a section on 'transmission of hereditary qualities', while the fourth, People's Edition, of 1835 in addition included substantial appendices on 'hereditary descent of national peculiarities' and 'hereditary transmission of qualities'. Combe claimed that the 'brain is a portion of our organised system, and, as such, is subject to organic laws, by one of which, as already observed, its form, size, and qualities, are transmitted by hereditary

[15] Carlos López-Beltrán, 'The medical origins of heredity', in Steffan Müller-Wille and Hans Jörg Rheinberger (eds), *Heredity Produced: At the Crossroads of Biology, Politics, and Culture, 1500–1870* (Cambridge, MA, 2007), p. 106.
[16] Waller, 'Ideas of heredity', p. 459.

descent.'[17] In his speculations on the subject of heredity Combe cited as an influence the writings of the noted eighteenth-century Scottish physician and professor of medicine at the University of Edinburgh, John Gregory (1724–73), who wrote a *Comparative View of the State and Faculties of Man with those of the Animal World*, first published in 1765, in which he explored the analogies between humanity and the animal creation. One of the analogies identified by Gregory related to the hereditary transmission of characteristics and the way in which this could be used to improve both domesticated animal species and the human race through selective breeding. He wrote that 'by a proper attention we can preserve and improve the breeds of Horses, Dogs, Cattle, and indeed of all other animals. Yet it is amazing that this Observation was never transferred to the Human Species, where it is equally applicable.'[18] Combe used Gregory's work to demonstrate that it had long been recognised that both mental and physical traits were transmitted from generation to generation among humans just as they were among animals.

In making his case, Combe also made extensive use of the second edition of James Cowles Prichard's *Researches into the Physical History of Mankind* (1826). Prichard (1786–1848) was a medical graduate of the University of Edinburgh who went on to become a physician and ethnologist. He wrote widely on all aspects of human nature and the mind. His ethnological writings were very influential in the mid-nineteenth century, although the religious slant of his works led them to go rapidly out of favour after his death.[19] Partly from religious conviction, Prichard was a strong monogenist, and his *Researches* was originally written as a refutation of Lord Kames' polygenist theories. To this end he explored not only heredity, but also the nature and causes of variations in humans and animals. In the following passage

[17] George Combe, *The Constitution of Man Considered in Relation to External Objects*, 4th edn (Edinburgh, 1835), p. 43.
[18] John Gregory, *Comparative View of the State and Faculties of Man with those of the Animal World*, 7th edn (London, 1777), p. 21.
[19] H. F. Augstein, 'Prichard, James Cowles (1786–1848)', *Oxford Dictionary of National Biography* (Oxford, 2004) [http://www.oxforddnb.com/view/article/22776, accessed 12 Aug 2013].

from the second edition he gave a summary of his views of the hereditary transmission of varieties:

> It appears to be a general fact, that all connate varieties of structure [variations present since birth], or peculiarities which are congenital, or which form a part of the natural constitution impressed on an individual from his birth, or rather from the commencement of his organization, whether they happen to descend to him from a long inheritance, or to spring up for the first time in his own person – for this is perhaps altogether indifferent – are apt to reappear in his offspring.[20]

Although he does not explicitly cite the works of Spurzheim in his discussion of heredity in the *Constitution*, it seems clear that Combe was also influenced by his *A View of the Elementary Principles of Education*, first published in 1821, which was the first phrenological work to deal at length with heredity.[21] In this work Spurzheim stated that 'children participate in the bodily configuration and constitution of their parents, and also in their tendencies to particular manifestations of the mind'.[22] Spurzheim considered that these ideas ought to be put into practice to improve the race, prefiguring many of the arguments of the eugenicists. He claimed that it 'is indeed a pity that the laws of hereditary descent are so much neglected, whilst, by attention to them, not only the condition of single families, but of whole nations, might be improved beyond imagination, in figure, stature, complexion, health, talents and moral feelings.'[23] Many of the measures he suggests, such as discouraging people from having children when they are too young or too old, have direct echoes in the *Constitution*, and it is clear that Spurzheim was a major source for Combe's ideas. On the question of the perfectibility of human nature, Spurzheim

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[20] James Cowles Prichard, *Researches into the Physical History of Mankind*, vol. 2 (London, 1826), p. 536.
[21] Hilts, 'Obeying the laws of hereditary descent', p. 63.
[22] G. Spurzheim, *A View of the Elementary Principles of Education, Founded on the Study of the Nature of Man*, 2nd edn (Boston, MA, 1833), p. 37.
[23] Ibid., p. 46.

admitted that some degree of improvement was possible, but that 'it is not to be understood that man may lose one faculty and acquire another; for the fundamental nature of man being unchangeable, in body as well as in the faculties of the mind, such an event is impossible on earth', allowing only that 'certain powers are capable of attaining greater or lesser activity'.[24] The key to the progress of the race for Spurzheim lay in selective breeding, rather than the improvement of the individual through social engineering. As we will see below, this is an important point of difference with Combe, who made a strong connection between the development of the individual in society and universal progress.

Influenced by Spurzheim, and drawing on the works of writers such as Gregory and Prichard, Combe applied hereditarianism to the mental organs revealed by phrenology. According to the *Constitution*, 'Phrenology reveals the principle on which dispositions and talents are thus hereditary. Mental qualities are determined by the size, form, and constitution of the brain. The brain is a portion of our organised system, and, as such, is subject to the organic laws, by one of which, as already observed, its form, size, and qualities, are transmitted by hereditary descent.'[25] For Combe the form, size and qualities of the phrenological organs were therefore determined by heredity. But, as we will see in the next section, for Combe the character of the individual was determined not simply by the relative size of the organ, but by the relationship between its size and its activity.

Developing the faculties

For Combe, even if the relative size of a phrenological organ was fixed for the lifetime of the individual, its activity could vary according to circumstances and the level of stimulation provided by the social and physical environment. The activity of the organs could clearly be subject to change during the lifetime of the person, and this would be reflected in changes in the person's character. The way in which the

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[24] Ibid., p. 2.
[25] Combe, *Constitution of Man*, p. 43.

faculties of the mind manifested themselves in the character of the individual were therefore dependent to a significant extent on their relations with the external world. Combe considered that the world had been constituted in such a way that humanity was subject to a series of unbending natural laws. He further believed that the constitution of the human mind corresponded directly to the constitution of the world, and hence to the natural laws which governed it. There was therefore an ideal harmony between the world and the mind, which it was humanity's duty to discover and maintain. According to Combe, there was a direct relationship between the development of the brain during the lifetime of the person and the extent to which they obeyed the natural laws. He emphasised the centrality of the concept of natural law to his thought in a letter to Robert Chambers in December 1833, where he stated that 'The leading principles of the 'Constitution of Man', to which I attach value, & which seem to myself to be original, are, the separate existence & operation of each natural law; the necessity of obeying all of them; & the evident adaptation of all to the moral & intellectual advancement of the race.'[26]

The natural laws could be divided into three categories: the physical, organic and moral or intellectual laws. Following some of the examples given by Combe, the physical laws are those which determine that an unsound ship will sink, while a sound one will float; the organic laws determine that an individual who has a healthy diet and takes exercise will be physically sound, while one who has a bad diet and does insufficient exercise will become ill; and the moral laws ensure that a person who 'obeys the precepts of Christianity, will enjoy within himself a fountain of *moral and intellectual happiness*.'[27] According to the arrangement of the *Constitution*, heredity comes under the heading of 'organic law', although it clearly also has a close relation with the moral laws when it regarded the hereditary transmission of the higher, moral faculties of the mind. Obedience to the natural laws would lead to a healthy development of the higher faculties through promoting the activity of the

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[26] George Combe to Robert Chambers, 15 December 1835, Robert Chambers, letters of noted persons, 1833–38, National Library of Scotland, Dep.341/91, 13.
[27] Combe, *Constitution of Man*, p. 6.

corresponding phrenological organs, while disobedience or ignorance of them could conversely be deeply harmful and lead to degeneration. The latter situation was generally brought about by giving the lower, 'animal' faculties free reign. According to Combe, living in harmony with the natural laws had a profound positive influence on the brain. As he put it: 'A perception of the Importance of the natural laws will lead to their observance, and this will be attended with an improved development of brain'.[28] The development of the faculties of the mind, as for any other organs of the body, was stimulated by their use, which was in turn guaranteed by following the dictates of the natural laws. Combe admirably summed up his theory of the human mind in the following passage:

> The best mode of increasing the strength and energy of any organ and function, is to exercise them regularly and judiciously, according to the laws of their constitution. The brain is the organ of the mind; different parts of it manifest different faculties; and the power of manifestation in regard to each is proportionate, *caeteris paribus*, to the size and activity of the organ. The brain partakes of the general qualities of the organised system, and is strengthened by the same means as the other organs.'[29]

George Combe: Social Lamarckian?

We have seen above that Combe believed that the faculties of the mind could be developed during the lifetime of the individual. But Combe also believed that these acquired traits could be inherited by offspring. After considering and rejecting the possibilities that the qualities of the brain are identical to the parent of the same sex, or an amalgam of the qualities of the two parents, Combe settles on the formula that the 'qualities of the child are determined jointly by the constitution of the stock, and by the faculties which predominate in power and activity in the parents at the

[28] Ibid., p. 295.
[29] Ibid., p. 34.

particular time when the organic existence of each child commences.'[30] In his advocacy of the inheritance of acquired characteristics some writers have seen echoes of the transformist theories of Lamarck. When Combe pronounced that 'the children of the individuals who have obeyed the organic, the moral, and the intellectual laws, will not only start from the highest level of their parents in acquired knowledge, but will inherit an enlarged development of the moral and intellectual organs', it is easy to see how such a doctrine could be described as social Lamarckism.[31] Victor L. Hilts has even suggested that Combe had 'resurrected Lamarck'.[32] John van Wyhe has also noted that Combe's 'law of hereditary descent would today be called "Lamarckian inheritance"'.[33] But is there a genuine connection between Combe's views and Lamarckism, or are Combe's theories more likely to have been grounded on ideas of heredity that were commonplace in the early decades of the nineteenth century?

In the first volume of his influential book on the *Histoire Naturelle des Animaux sans Vertèbres* (1815), Lamarck set down the four fundamental laws that underpinned his theory of transformism:

> *First law*: Life, by its own forces, tends continually to increase the volume of every body that possesses it, and extends the dimensions of those parts up to a point determined by itself.
> *Second law*: The production of a new organ in the animal body results from a new need which continues to make itself felt, and a new movement which this need brings about and supports.

[30] Ibid., p. 44.
[31] Ibid., p. 49.
[32] Hilts, 'Obeying the laws of hereditary descent', p. 74.
[33] John van Wyhe, 'George Combe's law of hereditary descent', in *A Cultural History of Heredity II: 18th and 19th Centuries*, Max Planck Institute for the History of Science, Preprint 247 (2003) (www.mpiwg-berlin.mpg.de/Preprints/P247.PDF (Consulted 24 September 2013)), p. 168.

> *Third law*: The development of organs and their force of action are constantly in proportion to the use of these organs.
> *Fourth law*: All that which has been acquired or changed in the organisation of individuals during the course of their lives, is conserved by generation, and transmitted to the new individuals descended from those who underwent the changes.[34]

It is certainly not difficult to draw parallels between Combe's ideas on the development and inheritance of mental faculties and Lamarck's third and fourth laws. However, the fixity of the phrenological organs seems quite counter to the second law, which deals with the development of new organs. In Combe's writings there is no suggestion that the organs that composed the human brain could be gained or lost through use or disuse; there would therefore always be a strict limit on the development that was achievable. On the contrary, all evidence shows 'that every creature and every physical object has received a definite constitution, and been placed in certain relations to other objects.'[35] This applied as much to the organs of the mind as to those of the body. That humanity could perfect itself through its own moral and intellectual exertions was one thing, but for it to be transformed into an entirely new species was quite another, for which there is no evidence in the writings of Combe. In this Combe seems to have followed the lead of Joseph Gall and Johann Spurzheim, the founders of phrenology, who criticised the transformist theories of Lamarck in a book on the *Innate Dispositions of the Soul and the Mind* (1811) in which they asked:

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[34] '*Première loi*: La vie, par ses propres forces, tend continuellement à accroître le volume de tout corps que la possède, et à étendre les dimensions de ces parties, jusqu'à un terme qu'elle amène elle-même. *Deuxième loi*: La production d'un nouvel organe dans un corps animal, résulte d'un nouveau besoin survenu qui continue de se faire sentir, et d'un nouveau mouvement que ce besoin fait naître et entretient. *Troisième loi*: Le développment des organes et leur force d'action sont constamment en raison de l'emploi de ces organes. *Quatrième loi*: Tout ce qui a été acquis, tracé ou changé, dans l'organisation des individus, pendant le cours de leur vie, est conservé par la génération, et transmis aux nouveaux individus qui proviennent de ceux qui ont éprouvé ces changemens.' Jean-Baptiste Lamarck, *Histoire Naturelle des Animaux sans Vertèbres*, 7 vols (Paris, 1815–22), vol.1, pp. 181–2.
[35] Combe, *Constitution of Man*, p. 1.

> Is it not more in conformance with the wisdom of the Creator, which can be recognised in a blade of grass as well as in the arrangement of worlds, that, in the first instant of creation, every being, both inanimate and living, received its own particular properties, and was thus differentiated from all other beings?[36]

I turn now to the important question of how widely Lamarck's transformist theories were known in Edinburgh in the 1820s, at the time Combe was formulating the ideas on heredity that were to appear in his *Constitution*. There is in fact little evidence for any significant interest in Lamarckian transformism in Edinburgh before the publication of his *Histoire Naturelle des Animaux sans Vertèbres* in 1815–22. This is perhaps not surprising, as Pietro Corsi has noted that it was to a considerable extent on the *Histoire Naturelle* that Lamarck's Europe-wide reputation was built in the 1820s.[37] An early reference appears in the preface to Cuvier's *Theory of the Earth* (1815) by Robert Jameson, the professor of natural history at the University of Edinburgh.[38] This, however, only consists of a single sentence, which makes no reference to Lamarck's views on heredity. John Fleming, the Church of Scotland minister and natural historian, wrote a review of the *Histoire Naturelle* for the *Edinburgh Review* in 1820.[39] He also included a critique of Lamarck in volume 1 of his *Philosophy of Zoology* (1822).[40] However, Fleming too did not address the issue of the

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[36] 'N'est-il pas plus conforme à la sagesse du Créateur qui se fait reconnoître dans le brin d'herbe comme dans l'arrangement des mondes, que, dès le premier instant de la création, chaque être, inanimé et vivant, ait reçu ses propriétés particulières, et ait différé par-là de tous les autres ?' F.J. Gall and G. Spurzheim, *Des Dispositions Innées de l'Ame et de l'Esprit, du Matérialisme, du Fatalisme et de la Liberté Morale, avec des Réflexions sur l'Education et sur la Législation Criminelle* (Paris, 1811), p. 100.

[37] Pietro Corsi, *The Age of Lamarck: Evolutionary Theories in France, 1790–1830* (Berkeley, CA, 1988), p. 63.

[38] Georges Cuvier, *Essay on the Theory of the Earth,* 2nd edn (Edinburgh, 1815), p. vii.

[39] [John Fleming], Review of Jean-Baptiste Lamark, *Histoire Naturelle des Animaux sans Vertèbres*, *Edinburgh Review* 3:4 (1820), pp. 403–18.

[40] John Fleming, *The Philosophy of Zoology or a General View of the Structure, Functions, and Classification of Animals,* vol. 1, (Edinburgh, 1822).

inheritance of acquired characteristics, concentrating rather on a defence of vitalism against Lamarck's materialist theory of life. While Fleming was generally dismissive of Lamarck's theories, his friend Robert Grant, lecturer at John Barclay's extra-mural anatomy school and invertebrate zoologist, was an enthusiastic admirer.[41] Charles Darwin got to know Grant well in his time at the University of Edinburgh between 1825 and 1827, and we have his testimony for Grant's opinions on evolution. Darwin recounted in his autobiography how Grant 'burst forth in admiration of Lamarck and his views on evolution' one day while they were on a collecting trip together.[42] Although Grant published a number of transformist articles in the *Edinburgh New Philosophical Journal* in the mid-1820s, none of these mentioned Lamarck by name, or referred specifically to the inheritance of acquired characteristics. An anonymous transformist article entitled 'Observations on the nature and importance of geology' and published in the same journal in 1826 did openly praise Lamarck, but again failed to mention his ideas on heredity.[43] As I will explore below, Lamarck's views on heredity may simply have had such common currency in this period as not to have been considered worthy of comment.

These, along with a few other scattered references to Lamarck, for the most part negative, are the scant evidence for the reception of Lamarckian transformism in Edinburgh in the 1820s. The one thing they have in common is that they were written by individuals with an interest in invertebrate zoology, for it was for his important contributions to invertebrate taxonomy that Lamarck was principally known at the time. It seems that his espousal of the inheritance of acquired characteristics was not considered important or contentious enough to merit a mention, let alone refutation, in published accounts of his theories. Outside the

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[41] See Adrian Desmond, 'Robert E. Grant: The Social Predicament of a Pre-Darwinian Transmutationist', *Journal of the History of Biology* 17:2 (1984), pp. 189–223.
[42] Charles Darwin, *The Autobiography of Charles Darwin* (London, 1958), p. 49.
[43] Anon, Observations on the nature and importance of geology', *Edinburgh New Philosophical Journal* 1 (1826), pp. 293–302. James Secord has suggested that the author of this article may well have been Robert Jameson; see James A. Secord, 'Edinburgh Lamarckians: Robert Jameson and Robert E. Grant', *Journal of the History of Biology* 24: 1 (1991), pp. 1–18.

natural history circles that included Jameson, Fleming and Grant, there is little or no evidence of any wider interest in Lamarck's theories in Edinburgh before the critique of Lamarck in volume 2 of Lyell's *Principles of Geology* brought his theories to a wider audience in 1832. There is no evidence that any major Edinburgh phrenologist before Robert Chambers, author of *Vestiges of the Natural History of Creation* (1844), took any interest in transformism, and the *Edinburgh Journal of Phrenology* contains no articles touching on the subject between its foundation in 1823 and Combe's review of *Vestiges* in 1845.[44]

Combe himself seems to have been so little acquainted with developments in natural history, that when he turned to the subject to bolster his views on universal progress in the 'introductory remarks' to the People's Edition of the *Constitution* (1835), he relied largely on lengthy quotations from Humphrey Davy's *Consolations in Travel, or, the Last Days of a Philosopher* (1831). Combe was so unsure of himself on this territory that he wrote a letter to the botanist and phrenologist Hewitt Cottrell Watson in 1836 to ask for his opinion on the geological history of the world presented in the 'introductory remarks', and the lengthy borrowings from Davy in particular. In his reply Watson advised Combe to 'retain the passage, but intimate that it is merely a remote analogy, on which you lay no particular stress, & whether correct or not so, of no essential importance to your own views touching on the progress of mankind.'[45] He appends to his letter his own account of the 'Progress of the Earth's Changes', significantly more cautiously worded than the passage quoted from Davy, in which Watson raised the possibility that the absence of fossils of higher forms of life from older rocks may simply be a product of the vagaries of the fossil record and the relative scarcity of higher forms in comparison with the lower. In the end Watson concluded that in his opinion 'the evidence shows oscillation to

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[44] [George Combe], Review of *Vestiges of the Natural History of Creation*, *Phrenological Journal and Miscellany* 18 (1845), 69–79.
[45] Hewitt Cottrell Watson to George Combe, 14 September 1836, Correspondence of George Combe, National Library of Scotland, Ms.7241, f.160 recto.

& fro, without any onward or backward course in continuity.'[46] Despite Watson's misgivings, subsequent editions of the *Constitution* retained the passages from Davy. Evidently Combe had chosen to ignore Watson's advice on the matter. It would seem that the analogy between the progressive nature of the constitution of the globe and of the constitution of man were of too much importance to Combe for him to quietly drop the offending passage or amend it in line with Watson's recommendations. Combe seems to have been enraptured by the harmony between the perfectibility of human nature and the progressive story of the history of the globe presented by modern geology. As Combe wrote in the 'introductory remarks' to the People's Edition of the *Constitution*:

> The constitution of the world … appears to be arranged in all its departments on the principle of slow and progressive improvement. Physical nature itself has undergone many revolutions, and apparently has constantly advanced. Geology seems to show a distinct preparation of it for successive orders of living beings, rising higher and higher in the scale of intelligence and organisation, until man appeared.[47]

It should be noted, however, that the progressive picture painted here of the fossil record is entirely in keeping with the writings of Georges Cuvier or William Buckland, both fierce opponents of transformism, and could be as easily explained by successive creations as by evolution.[48]

Although it seems highly unlikely that Combe was directly influenced by Lamarck's theories, it cannot be denied that his brother Andrew had almost certainly been exposed to transformist ideas while he was studying in Paris between October 1817

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[46] Ibid., f.161 verso.
[47] Combe, *Constitution of Man*, p. 2.
[48] See, for example, Georges Cuvier, *Essay on the Theory of the Earth* (trans. Robert Kerr), Edinburgh: William Blackwood, 1815 and William Buckland, *Geology and Mineralogy Considered with Reference to Natural Theology*, 2 vols (London, 1836).

and July 1818. We know from the records of the Jardin du Roi that Andrew Combe had enrolled for Lamarck's course on invertebrate zoology in 1818.[49] Many years later George Combe wrote an account of his brother's time in Paris in his biography of him. Here he related that while in Paris Andrew had attended lectures at the Jardin du Roi on botany by René Louiche Desfontaines, on chemistry by M. Langres, on geology by Barthélemy Faujas de Saint-Fond and on physiology by Anthelme Richerand.[50] However, there is no mention of Andrew Combe's attendance at Lamarck's lectures. If Combe had considered his brother's attendance at these lectures significant, he would surely have mentioned them in his biography along with the others. Either he actively suppressed the attendance of his brother at Lamarck's lectures, not wishing to link his brother with controversial transformist ideas, or he simply neglected to mention them. From the lack of evidence from other sources that Combe engaged either positively or negatively with Lamarck's theories, the latter seems the more plausible.

Some further indication of Combe's attitude towards the transmutation of species can be gleaned from his reaction *Vestiges of the Natural History of Creation*, the best known work presenting an evolutionary theory to the English-speaking public before Darwin's *Origin of Species*. In October 1844 Combe wrote a letter to the anonymous author via his publisher in response to the complimentary copy he had received, not realising that the author was in fact Robert Chambers, his friend and fellow phrenologist. The letter is full of admiration for the work, and he thanked the author 'for much pleasure and instruction'. However, he concluded that the author's 'leading idea is not yet proved; it was impossible for you to do so in the present stage of science & of man's experience; but you have invested it with

[49] Pietro Corsi, 'Chronologie de la vie de Jean-Baptiste Lamarck, Le registre d'inscription au cours de Lamarck pour 1818', *Jean-Baptiste Lamarck: works and heritage* (www.lamarck.cnrs.fr/auditeurs/index.php?rech=scan&send=scan&nom=tous&cours=1818&inscri=tous&offset=48&nbmax=4 (Consulted 5 January 2014)).
[50] George Combe, *The Life and Correspondence of Andrew Combe* (Edinburgh, 1850), pp. 56–60.

probability, and brought so strong an array of phenomena to support it'.[51] Significantly, there was no explicit reference to the transmutation of species, the most controversial aspect of his theory. By the 'leading idea' he appears to have been referring to the general concept of universal development laid out by Chambers, of which the transmutation of species was but one manifestation.

When the third edition of *Vestiges* was published, Combe also received a complimentary copy of the new edition. Again he wrote to thank the anonymous author, this time at greater length.[52] As in his previous letter, Combe made no comment on the doctrine of the transmutation of species, but rather devoted a substantial part of the letter to a detailed critique of the section of the book dealing with the development of human society, a subject clearly closer to Combe's heart. While we might have expected an avowal of Combe's views on transformism in a letter to the author of *Vestiges*, he has nothing to say on the subject. It can only be concluded that, while Combe was not in any way shocked or scandalised by the transformist doctrines expounded in *Vestiges*, as many of his contemporaries clearly were, neither were they of great interest to him. While he was prepared to countenance the possibility that Chambers' transformist doctrines might have been correct, he certainly did not consider that the case for them had been proved.

There is also strong evidence from one of Combe's staunchest critics that he was no transformist and that any analogy between his theory of human progress through the hereditary transmission of mental characteristics and Lamarckian transformism was almost certainly coincidental. In 1836, spurred on by the success of the People's Edition of the *Constitution* published the previous year, William Scott (1782–1841) published an attack on Combe's ideas entitled *The Harmony of Phrenology with Scripture: Shewn in a Refutation of the Philosophical Errors Contained in Mr Combe's*

[51] George Combe to [Robert Chambers], 30 October 1844, Combe papers, National Library of Scotland, Ms.7388, 780.
[52] George Combe to [Robert Chambers], 1 March 1845, Combe papers, National Library of Scotland, Ms.7390, 66–71.

'Constitution of Man'. Scott had himself been a leading phrenologist in Edinburgh and was a former president of the Edinburgh Phrenological Society. He knew Combe well, but had broken with him over Combe's doctrine of the natural laws and advocacy of universal progress. He left the Edinburgh Phrenological Society in 1830 in the aftermath of a power struggle between Combe's faction and a group of evangelical phrenologists, of which Scott was one of the leading figures. Combe and his supporters had emerged from this struggle as the dominant faction within the Society, now largely purged of its evangelical members. In his book, Scott dealt harshly with Combe, describing his theories as 'a low and grovelling system.'[53] He reserves particular scorn for Combe's progressivism, stating that:

> We have, therefore, every kind of evidence, positive and negative, for asserting, that neither in the vegetable nor in the animal creation is there any such thing as a natural state of progression; and that no race or species of either has ever, as a species, improved itself, or shewn any symptom of 'possessing within itself the elements of improvement.'[54]

However, one thing Scott never accused Combe of was advocating transformism. Rather he noted that Combe 'distinctly and correctly states, that each new race of plants or animals was the result of a separate act of creation; and he states, moreover, in the very outset of his work, the general fact, that every creature, and every physical object, "has received its own *definite constitution*."'[55] Scott knew Combe and his views well, and was under no obligation to spare him if he had evidence that he held opinions that could be used against him. It therefore seems more than likely that we can take Scott at his word when he declared Combe innocent of advocating the transformation of species.

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[53] William Scott, *The Harmony of Phrenology with Scripture: Shewn in a Refutation of the Philosophical Errors Contained in Mr Combe's 'Constitution of Man'*, 2nd edn (Edinburgh, 1836), p. 325.
[54] Ibid., pp. 16–17.
[55] Ibid., p. 12.

Combe and the inheritance of acquired characteristics

Far from being an unusual or radical belief in the late eighteenth and early nineteenth centuries, acquired characteristics were widely acknowledged to be heritable. Combe would not have had to go searching in Lamarck's treatise on invertebrate zoology to find the principle of the transmission of acquired traits; the idea was a relatively commonplace one at this time. In a treatise published in 1775, the famous Scottish surgeon John Hunter (1728–93) had written that 'there is no manner of doubt that peculiarities acquired by men do descend to their posterity.'[56] Hunter applied these ideas both in a medical context, to conditions such as gout, scrofula and madness, and also to the effect of climate on racial characteristics.[57] The German physiologist Johann Friedrich Blumenbach (1752–1840) agreed with Hunter; in the third edition of a treatise on race published in 1795 he commented that 'the aboriginal Ethiopians have been for a long time and for many series of generations exposed to the actions of that climate … . So we must not be surprised if they propagate unadulterated, even under another climate to succeeding generations, the same disposition which has spread such deep and perennial roots in their ancestors from the most distant antiquity.' [58] In the second decade of the nineteenth century the theories of Blumenbach and Hunter were still debated by medical students at the University of Edinburgh in dissertations given to the Royal Medical Society. In the dissertation book for 1810–11 can be found a paper entitled 'What are the Varieties of the Human Species; & their causes?' by J.W. Stirk, in which it is stated that

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[56] John Hunter, *Disputatio inauguralis quedam de hominem varietatibus, et harem causis exponens* (1775), in *The Anthropological Treatises of Blumenbach and Hunter* (trans Thomas Bendyshe) (London, 1865), p. 204.
[57] Hunter, *Disputatio inauguralis*, p. 386.
[58] Johann Friedrich Blumenbach, *De Generis Humani Varietate Nativa* (1795), 3rd edn, in *Anthropological Treatises of Blumenbach and Hunter*, p. 204.

> changes, the effects of which blend with the general actions of the body, and ultimately form the character of a climate and nation, are progressively carried on through several generations, till the causes that produce them have attained their utmost operation, by becoming perfectly congenital to the system.[59]

The following year Nicholas C. Pitta expressed the same opinion that changes effected by the climate could become heritable in another dissertation on race.[60] Combe could certainly have come into contact with these ideas through the many medical men among his phrenological associates, including his brother Andrew.

However, Combe's evidence for the transmission of acquired traits seems to have come less from scholarly sources than from tales of natural curiosities, whether published or anecdotal, which were in circulation in the early nineteenth century. This was the evidence that underpinned Combe's argument for social progress based on development of the phrenological organs; without a mechanism for the inheritance of the higher state achieved, the advances made by individuals would not be automatically passed on to succeeding generations. I would argue that his adoption of the inheritance of acquired characteristics was therefore a natural consequence of Combe's hereditarian views, his belief in the perfectibility of the faculties and his unerring faith in human progress. Although Prichard largely rejected the inheritance of acquired characteristics, his discussion of the subject may also have been a crucial source for Combe, influenced as he was by Blumenbach and Hunter. In the following paragraphs I will examine Combe's specific beliefs regarding the inheritance of acquired traits, starting with his rejection of Prichard's arguments against such transmission.

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[59] J.W. Stirk, 'What are the Varieties of the Human Species; & their causes?', The Royal Medical Society, Dissertations 65 (1811–11), pp. 317–18, Library of the Royal Medical Society.
[60] Nicholas C. Pitta, 'What is the influence of climate on the human species? And what are the varieties of men which result from it?', The Royal Medical Society, Dissertations 66 (1811–12), pp. 284–5, Library of the Royal Medical Society.

In Prichard's *Researches into the Physical History of Mankind* he noted that 'changes produced by external causes in the appearance and constitution of the individual are temporary, and, in general, acquired characteristics are transient; they terminate with the individual, and have no influence on the progeny.'[61] However, as H.F. Augstein has pointed out in her survey of Prichard's anthropology, his view on heredity evolved in the course of the four editions of his *Researches*. By the second edition in 1826 she notes that he had come to the rather awkward conclusion that, while the environment was responsible for racial differences in humans, such acquired attributes were not subject to hereditary transmission.[62] The evident problems with this position must surely have been apparent to Combe when he set out to refute Prichard's views. Combe presented a number of counter arguments to Prichard and in favour of the inheritance of mental traits, with evidence drawn from a variety of sources, which he organised under four headings. These were: '1*st*, The transmission of *factitious or temporary conditions of the body*; 2*ndly*, The transmission of *acquired habits*; 3*rdly*, The appearance of *peculiarities in children, in consequence of impressions made on the mind of the mother*; and, 4*thly*, The transmission of *temporary mental and bodily qualities*.'[63] All of these forms of transmission were based on well-established beliefs that could be supported by Combe with evidence from a variety of contemporary sources. It is worth noting that Combe's list does not include the transmission of permanent physical modifications of the body, such as those caused by injury, as he was in agreement with Prichard on the impossibility of this. I will look at each of these modes of transmission in turn, examining the evidence that Combe presents for their reality.

The first type involves the transmission to offspring of temporary states of the body, such as illness or other mental or physical incapacity. Combe first noted that

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[61] Prichard, *Physical History of Mankind*, vol.2, pp. 536–7.
[62] H.F. Augstein, *James Cowles Prichard's Anthropology: Remaking the Science of Man in Early Nineteenth Century Britain, Clio Medica 52* (Amsterdam, 1999), p. 115.
[63] Combe, *Constitution of Man*, p. 44.

Prichard believed that this was impossible, as only the original attributes of the parents could be transmitted to offspring. However, Combe presented a number of instances of the supposed transmission of temporary states as evidence that Prichard was wrong, including the case of a man who, after suffering from a brain injury, had two children who were both 'idiots', but on regaining his faculties after an operation his subsequent children were normal, as was a child conceived before his accident. The story had been related to Combe by an unnamed medical practitioner from the Isle of Man. Of course, the effect of temporary mental states did not always have to be negative: 'even very inferior characters, in whom the moral and intellectual organs are deficient, may be occasionally exposed to external influences which, for the time, may excite those organs to unwonted vivacity; and, according to the rule now explained, a child dating its existence from that period may inherit a brain superior to that of the parent'.[64] Combe also gave an example of the transmission of a temporary physical state, in this case the transmission of udders enlarged by milking in cows, drawn from an article on 'America' in the seventh edition of the *Encyclopaedia Britannica* (1830–42).

As for the transmission of acquired habits, Combe gave examples of the behaviour of dogs, sheep and horses to show how patterns of behaviour could be inherited among animals. As is the case throughout Combe's work, his examples are culled from a wide variety of disparate sources taken from his own eclectic reading. The most lengthy example concerns the behaviour of pointer dogs. The information regarding the apparently hereditary hunting behaviour of these dogs was drawn from a review from the *Edinburgh Review* of August 1825 of a work published anonymously by the poet and translator William Stewart Rose (1775–1843) entitled *Thoughts and Recollection by One of the Last Century*. Combe does not give any examples of inherited habits among humans, but relies entirely on examples from the animal world.

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[64] Ibid., p. 47.

Turning to Combe's third manner of transmission, resulting from impressions made on the mind of the mother, it is noteworthy that Combe considered that relatively fleeting impressions or states of mind during pregnancy could have a profound influence on the character of the child. He relied heavily on anecdotal evidence as well as published works to support his assertions regarding the transmission of temporary states of the mind and body. These include much hearsay evidence regarding the power of strong mental impressions made on the mother during pregnancy to affect the child subsequently produced. For example, he presents the case of a shoemaker's wife which, he says, 'fell under my own observation'. This woman by chance met 'an idiot lad' while she was pregnant. The boy made such 'a strong impression' on the woman that several months later she gave birth to a son 'who is in a state of idiocy' and 'had the slouched and slovenly appearance of the[original] idiot'.[65] Prichard had also mentioned fanciful popular tales of 'mothers longing for various objects, or frightened by unexpected sights during pregnancy, and producing children marked with strawberries, currants, or having visages that resemble pigs faces', which he considered to be 'as false as they are improbable'.[66] In this he showed himself less prepared to accept somewhat doubtful anecdotal evidence than Combe. However, even he was prepared to concede that the opinion 'that at the period when organization commences in the ovum, that is, at, or soon after the time of conception, the structure of the foetus is capable of undergoing modification from impressions on the mind or senses of the parent, does not appear altogether so improbable.'[67]

As for the final manner of transmission, Combe relied heavily on the authority of Charles Caldwell (1772–1853), an American physician and phrenologist, when addressing the issue of the 'transmission of temporary mental and bodily qualities'. Combe quotes from Caldwell's *Thoughts on the True Mode of Improving the Condition of Man* (1833), a version of which was reprinted in the *Phrenological Journal* in 1834.

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[65] Ibid., p. 45.
[66] Prichard, *Physical History of Mankind*, vol.2, p. 555.
[67] Ibid., p. 555.

Caldwell was also convinced that acquired dispositions could be inherited; for example, he noted that children 'partake of the constitutional qualities of the parents, for the time being. Years and circumstances alter those qualities, and the offspring produced under the influence of them, thus modified, are correspondingly altered.'[68] For this reason Caldwell was opposed to couples marrying when they were too young, when their constitutions would not yet be mature, or when they were too old, when their constitutions would be enfeebled, lest they give rise to inferior offspring. While Prichard and Caldwell tended to dwell on the inheritance of harmful traits, Combe was equally concerned to show how beneficial characteristics could be inherited.

Although the transmission of acquired characteristics could lead to negative consequences in particular instances, in general Combe was optimistic about its effect on the human species. By exercising the higher faculties of the mind in a manner in harmony with the laws of nature, it was possible for the human race to achieve ever greater perfection in the development of its constitution, just as by allowing the lower, animal faculties free reign and neglecting the natural laws, humanity risked degeneration. But the scales were weighted in advance in favour of development, for the 'Creator has so arranged the external world as to hold forth every possible inducement to man to cultivate his higher powers, nay almost to constrain him to do so.'[69] In the People's Edition of the *Constitution* (1835), Combe presented a view of 'the constitution of the world and of human nature' in which 'the world, including both the physical and moral departments, contains within itself the elements of improvement, which time will evolve, and bring to maturity; it having been constituted on the principle of a progressive system, like the acorn in reference to the oak.' According to Combe, this view 'affords the richest and most comprehensive field imaginable for tracing the evidence of Divine power, wisdom

[68] Charles Caldwell, 'Thoughts on the true mode of improving the condition of man', *The Phrenological Journal and Miscellany* (1832–6) 8, p. 606.
[69] Combe, *Constitution of Man*, p. 2.

and goodness in creation.'[70] The happy outcome of the development and inheritance of the mental faculties was that

> 'there would be improvement in the organic, moral, and intellectual capabilities of the race; for the active moral and intellectual organs in the parents would tend to increase the volume of those in their offspring – so that each generation would start not only with greater stores of acquired knowledge than those which its predecessors possessed, but with higher natural capabilities of turning them to account.[71]

Conclusion

We have seen that Combe drew his ideas and evidence on heredity from a range of both contemporary and historical sources. Given their clear relationship with various elements of eighteenth- and early nineteenth-century discourse on heredity and the inheritance of acquired characteristics, there was nothing particularly novel in Combe's ideas on the subject, except perhaps in his linking them with his rather grandiose vision of human progress. His belief that the faculties could be developed during the lifetime of the individual was a natural outcome of Combe's progressivism. Likewise the inheritance of these acquired characteristics was a necessary prerequisite for the progress made to be passed on to the next generation. It is important to note that Combe stopped at the development of existing organs, and did not envisage that entirely new organs could be gained or existing ones lost through this process. There is no indication that Combe ever imagined that a new species could arise in this manner, or that he ever even considered this possibility. There was therefore a definite limit on how great a change could be accomplished in this way, underlined by Combe's insistence that all living things had received a 'definite constitution' from their creator, which could not be altered.

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70 Ibid., p. 4.
71 Ibid., p. 61.

The belief that new varieties, but not species, could be generated through environmental influences, whether in nature or under domestication, was widely held in the late eighteenth and early nineteenth centuries, and was not generally associated with the much more controversial doctrine of the transmutation of species. Combe's belief in the perfectibility of man therefore gives no grounds for linking his theories with contemporary transformism. While not everyone accepted the inheritance of acquired characteristics – Prichard, for example, largely rejected it – the idea also had sufficient currency in both the domain of popular anecdote drawn on by Combe and the expert discourse of medical men such as Hunter and Blumenbach to constitute part of the common sense understanding of the period. I have argued that Combe's thought developed independently and in all likelihood in ignorance of Lamarckian transformism, although both ultimately drew on hereditarian beliefs which were widespread in the late eighteenth and early nineteenth centuries. Combe's use of the concept of the inheritance of acquired characteristics harks back to the optimistic late-Enlightenment belief in the possibility of universal human progress rather than forward to emerging evolutionary understandings of inheritance. It would therefore be misleading to suggest that the inheritance of acquired characteristics necessarily belonged to a coherent constellation of ideas that included the transmutation of species in this period.

This does not, of course, mean that Combe's book and the hereditarian ideas it contained may not ultimately have influenced the popular reception of evolutionary theories later in the century. As Secord has rightly pointed out Combe 'saw his book as a revolutionary attempt to bring an understanding of human action into the realm of law, and as the founding document in a campaign to create a science that could ensure the future of the human race.'[72] His view on the perfectibility of

[72] Secord, *Visions of Science*, pp. 173–4.

humanity through the doctrine of hereditary transmission was part of that vision. Given the spectacular publishing success of the *Constitution of Man*, Combe's potent blend of hereditarian and progressivist doctrines was to reach a vast audience. As John van Wyhe has rightly pointed out, Combe's promotion of hereditarianism and naturalistic progressivism may well have prepared the ground for the wide acceptance of Robert Chambers' *Vestiges of the Natural History of Creation* in 1844, and ultimately Charles Darwin's *Origin of the Species* after 1859. This is perhaps the most important legacy of George Combe's remarkable book.