Published paper
Hodge, M.J.S. (2005) *Against "Revolution" and "Evolution",* Journal of the History of Biology, Volume 38 (1), 101 - 121.

Against “Revolution” and “Evolution”

Jonathan Hodge

Division of History and Philosophy of Science

School of Philosophy

University of Leeds

Leeds, LS2 9JT

UK

E-mail: m.j.s.hodge@leeds.ac.uk

Abstract

Those standard historiographic themes of “evolution” and “revolution” need replacing. They tend to perpetuate the limitations of mid-Victorian scientists’ histories of science. Historians’ history of science does well to take in the long run from the Greek and Hebrew heritage on, and to work at avoiding misleading anachronism and teleology. As an alternative to the usual “evo-revo” themes, a historiography of origins and species, of cosmologies (including microcosmogonies and macrocosmogonies) is developed here. The advantages of such a historiography are illustrated by looking briefly at a number of transitions: the transition from Greek and Hebrew doctrines to their integrations by medieval authors; the transition from the Aristotelian Aquinas to the Newtonian Buffon and to the no less Newtonian Lamarck; the departures the early Darwin made away from Lamarck’s and from Lyell’s views. Issues about historical thinking about nature and about essentialism are addressed in an attempt to challenge customary stereotypes. Questions about originality and influence are raised, especially concerning Darwin’s “tree of life” scheme. The broader historiography of Darwinian science as a social ideology, and as a “worldview”, is examined and the scope for revision emphasised. Throughout, graduate students are encouraged to see this topic area not as worked out, but as full of opportunities for fresh contributions.

Keywords

Historiography, origins, species, cosmologies, Greeks, Hebrews, Buffon, Lamarck, Darwin.

The need for historiographical critiques and alternatives

The negative thesis of this paper is that historians of science should abandon any notion of a Darwinian revolution. The positive thesis is that other historiographies can be developed, especially by younger people in the years ahead[i].

There are some reasons for giving up all talk of a "Darwinian revolution" that are very general reasons indeed:

1. Go to the literature on political theory and political history – the home ground of "revolution" talk obviously – and one finds no agreement about the nature, causes and consequences of revolutions. Likewise for the philosophy of science: the various attempts to borrow "revolution" talk for characterisations of change in science have proved conspicuously unpersuasive.
2. If a revolutionary change in science is a sudden, total and irreversible regime shift, then historical scholarship has found no convincing cases. Whenever there has been a fundamental change in science, it has not proceeded both suddenly and as a shift from one exclusive consensus of beliefs, practices and authorities to another such unitary consensus.
3. Historians in various fields are currently questioning "revolution" as an interpretative category. Even historians of Europe in the "age of revolutions" (roughly 1789 – 1848) are warier of the category than they were. Again, the "Industrial Revolution" is now widely regarded as a misleading concept. Equally, the concept of "the Scientific Revolution" (roughly 1543 – 1687) has been deliberately discarded by many specialists in that period.

For these three reasons, it is surely an inappropriate moment to reaffirm an uncritical commitment to a “Darwinian revolution”. More positively, this could be a good time to encourage graduate students to do without this commitment.

The appropriateness of their doing so can be confirmed by two further considerations arising directly from the history of Darwinian science:

4. It can often aid assessment of a historiographical tradition to examine its original rationale. The notion of a “Darwinian revolution” was first promoted by Darwin’s supporters in the 1860s, in efforts to convince people that Darwin had wrought such a comprehensive and irresistible transformation of biological science that, as in a political revolution, no compromise with the old regime was possible. Ever since Darwin the notion has served the partisan purposes of evolutionary biologists. Moreover, in recent decades a much younger profession, history of science, has increasingly adopted this same mid-Victorian scientists’ historiography. This adoption is surprising, because a contrast is often drawn between scientists’ history of science and historians’ history of science; and because historians of science are trained and paid to exemplify this contrast by keeping their critical distance from scientists’ partisan views within and about science. More positively, any graduate student wanting to do historians’ history of science should want to do without any “Darwinian revolution” historiography.
5. To go with their “revolution” theme, Darwin’s earliest supporters composed an “evolution” theme. For they insisted that “evolution” – identified by them as a modern, scientific idea to be contrasted with “creation” as an ancient, religious idea – had matured gradually over centuries before its sudden,

> revolutionary triumph with Darwin. Surprisingly, this "evo-revo" historiographical package still dominates professional history of science today. To be sure, it is now proposed that the "evolution" that triumphed in the nineteenth century was not evolution by Darwinian natural selection, but evolution by other, non-Darwinian, means (orthogenesis, mutation, inheritance of acquired characters, and so on); so there was an "evolution revolution" but not a "Darwinian evolution revolution". Again, one is told that evolution in the eighteenth century was conceived as a serial escalation, not, as it would be later, as an arboriform descent. And it is emphasised that the word "evolution" changed its meaning between the 1680s and the 1860s. All these theses are, however, offered as variations on the old "evo-revo" themes, not as replacement for them. The meanings and uses of the word "evolution" are not merely indispensable resources for all such variations on "evo-revo" themes. They have become essential to the very professional identity of those scholars who now categorise themselves as "historians of evolutionary biology". This collective self-categorisation is, however, oxymoronic, insofar as it is inconsistent with the aims and practices of historians' rather than scientists' history of science. The positive corollary of this inconsistency is, fortunately, that any graduate students seeking alternatives to the traditional "evo-revo" enterprise can find their motivation in their very ambition to understand science historically.

This articulation of five reasons for abandoning this enterprise has been designed to gently prompt discussion of very broad issues going beyond debates within the Darwin industry. Graduate students sometimes say that their specialist duties leave no time for big picture questions; or they urge that in their field, as elsewhere in our post-modern epoch, edification is found in what is local not global. This section of the paper will have succeeded if it suggests that not all the worthwhile questions are narrow ones.

The very long run

In any effort to think about the history of science more as historians and less as scientists do, it often helps to adopt three strategies: first, to go back a very long way, well before today's theories were thought of; second, to set aside the terminology of today's theories and to work instead with the terms of those older times; third, to settle on some early starting points that can be seen as points of departures, so that what comes later is understood as various movements away from what came earlier, rather than as movements toward what will come later. In sum, one goes for the very long run while trying to avoid question-begging anachronism and teleology.

For all Western thought the very long run means, predominantly, two ancient heritages: the Hellenic and the Hebrew. On the Hellenic side, an obvious starting point is Parmenides' fifth century BCE conclusions about being, what is; for those conclusions explicitly precluded any truthful knowledge of origins or *archai*. Theorists about *archai* as diverse as Plato, Aristotle, Democritus and Empedocles were soon disagreeing with each other over how to avoid Parmenides' impasse. The contrast between Plato and Democritus is especially instructive. For Plato's ontology and cosmogony, species, *eide*, as Forms, are beings and are *archai*, origins so original that they cannot have origins. For Democritus atoms are beings, and with the beingless void are *archai*, origins for the whole cosmos, including its species.

Generalising from such post-Parmenidean contrasts and comparisons suggests one way – there might be others obviously – to avoid the question-begging anachronism and teleology inherent in any "evolution" historiography: namely, to stop thinking of oneself as a historian of evolution – as idea, concept or theory – and switch instead to being a historian of accounts of origins and species, or, more comprehensively, as a historian of cosmologies (including cosmogonies) and ontologies.

This switch helps no less on the Hebrew side in general, and with the book of *Genesis* in particular, which was in its final form by about 400BCE. For the earliest translators of its Hebrew into Greek read it, too, as an account of beginnings and kinds (*archai* and *gene*). Further, in studying the works that integrated the Hebrew and Hellenic accounts, works by such authors as Philo Judaeus in the first century CE, Augustine in the fourth and Aquinas in the thirteenth, one can confirm the advantages of this switch. For it allows justice to be done to both the agreements and the disagreements in these works. With these authors' common preference for the Platonic over the Democritean legacy, species as Forms, now construed as Ideas within the Divine Mind, are given their origin in God. However, Augustine integrates these Ideas with the Stoics' "seminal reasons" in understanding how all individuals that would ever live of every species were made as invisible seeds at the first moment of creation and so of time; whereas Aquinas integrates the Ideas with Aristotle's account of forms and matter in arguing that new individuals, but not new species, do arise in the ordinary course of nature since the cosmos was given its constitution of forms, specific natures, in the first six days.

Not that mere attention to such authors offers circumvention of "evo-revo" themes. Indeed, sadly, some theologians have proclaimed Augustine an "evolutionist" while others translate Aquinas' Latin for "more perfect" as "more evolved". Theologians' history of theology can be no more congruent with historians' history of science than the histories biologists and philosophers write. However, what attending to the medieval syntheses of the Hebrew and Hellenic legacies can do, if one allows it to, is to bring into focus indispensable issues concerning forms, laws and order.

Consider how Aquinas, as a Christian Platonist and Aristotelian, construes the constitution given the universe by God. Like the Hebrews' God Aquinas' is a giver of laws. But these are laws for Aristotelian specific natures: one law for lions, to act in accord with their nature and so to perpetuate their specific kind, another for tigers. With as many laws as there are specific natures or forms, Aquinas' is a universe ordered, as Plato's and Aristotle's were, by forms. As the *archai*, the *principia*, of the lawful order of nature, species are origins that can have no origins within that lawful order. What is more, with the universe an Aristotelian cosmos, for God to create and order the cosmos is for him to make a spherical, stationary earth, surrounded with spherical heavens; so that there is no geogony distinct from the entire cosmogony.

Fast forward next from Aquinas' Paris in the 1270s to Buffon's in the 1770s. The laws of nature are now not the numerous laws of Aristotelian specific natures, but the few universal, Newtonian, laws for matter everywhere moved by the passive force of inertia and the active attractive and repulsive forces of gravity, heat, electricity and magnetism. What is more, the earth as a planet and its inhabitants, the individuals and their species, can have their origins long after the universe, with its numerous suns and comets, first conformed to those laws, in a cosmos not ordered in accord with specific forms, but in accord with those universal laws. By tracking these cosmogonical and ontological themes throughout Buffon's natural philosophy and natural history, one can read him as he wrote: as someone responding to the departures he knew the seventeenth century had made from the thirteenth, rather than reading him as someone preparing the way for the nineteenth century.

As Buffon knew, Descartes, in the 1640s had produced the first geogony that presupposed that the earth was a planet of the sun, and that matter had one common essential nature with universal laws for its motions throughout a universe indefinite in extent, and with lawful motions before the earth was formed from its vortical antecedents. Burnet, in the 1680s had integrated the Biblical narrative with this new Cartesian natural philosophy and cosmology; while Whiston, in 1696, had integrated that narrative with Newton's alternative account of matter, the laws of motion and gravitational force. Whiston had, however, explicitly judged various productions beyond the powers of this lawful Newtonian nature and credited them to Divine miracles. The earth's orbit required such a miracle, for instance; and, invoking the new theory of pre-existent germs, *emboîtement*, Whiston has God miraculously making every individual that will ever live, small but perfectly formed, boxed up in the first members of their species.

Buffon deliberately constructed a Newtonian macrocosmogony for the solar system and for the earth, and a no less Newtonian microcosmogony for living beings, that included no such miracles and no appeal to Biblical chronologies. His two cosmogonies appeal to the attractive power of gravity and the repulsive power of heat working with the inertial power of all matter. Heat acting on certain brute matters produces organic molecules. These molecules could, when the earth was much hotter and so nature more powerful than today, form even the grandest animals in spontaneous generations.

A synopsis of Buffon's integration of his macrocosmogony and microcosmogony, and so of his natural philosophy and natural history, allows one to see how Lamarck relates to Buffon. Lamarck's principal claim, from 1800 on, was that all living bodies are nature's productions. The simplest are produced directly in spontaneous generations; the more complex indirectly over vast eons and countless generations by the constant, recurrent complexification of the simplest, a complexification due to the action, within all organised bodies, of contained fluids moving within a matrix of solid containing parts. There is no single scale of organisational perfection, nor, therefore, any single chain of all being, rather several series. These series are series not of species, but of large families and classes. The true classification, arrangement and grading of these classes gives, then, the natural order of production. For the Newtonian Lamarck, as for Buffon, nature's ultimate active powers are the attractive and repulsive forces of gravity and heat. But, for Lamarck, the intermediaries in the production of animals and plants are not any organic molecules (explicitly discredited by his theory of mineral composition) able in ancient hotter times to assemble themselves as readily into a mammoth as they do now into an infusorian. There has been no planetary cooling; thanks to the endless recycling of the matter of heat through the sun. So, nature never had greater powers than she now has to produce directly animal and plant organisation. The intermediaries, all that nature has ever been able to produce directly, are the simplest plants and animals. The successive, progressive production over eons of all the others, that requires the indefinite mutability of species, is required by the limitations and constancy in nature's powers of direct production.

By concentrating on his main arguments for his principal claims, one can understand Lamarck's theorising as he did: as a new Newtonian alternative to Buffon's earlier Newtonian alternative to *emboîtement* and pre-existent germs. One can also, then, place Lamarck in the very long run of cosmologies and ontologies. By embracing explicitly a cosmology with a stable solar system and stable terrestrial heat, Lamarck circumvents macrocosmogonical challenges; but his account of spontaneous generations, and of the inner fluid actions within organised bodies, is a microcosmogony no less than was Buffon’s account of organic molecules and internal molds. Equally, his Newtonian natural philosophy of subtle fluids, attractive and repulsive in the forces they exert, is just as much an ontology as Buffon’s no less Newtonian natural philosophy of heat and gravity.

Wider issues

By understanding Lamarck’s theory, as he did, as a theory of the direct and indirect production of organised bodies, rather than as a “theory of evolution”, one is prompted to ask why he formulated it as he did, in those terms, with those assertions and arguments. Experience from teaching Lamarck in this way shows that insisting on a total ban on all “evolution” talk is necessary if not sufficient to get such historical questioning underway. None of the reasons historians have given for retaining “our” talk of “evolution” (or of “transformism” and the rest), rather than adopting Lamarck’s own terms in discussing his theories, is remotely respectable or convincing; as graduate students, trying to understand Lamarck, can confirm for themselves by simply dispensing with “our” talk and all other unhelpful anachronisms, including those unfortunately infecting many translations of his writings.

As for teleology, good teleology can drive out bad if one lets it. The bad teleology reads Lamarck as preparing the way for Darwin or as groping his way towards Darwinian views. All such readings can be avoided by asking what Lamarck understood his intentions, ambitions and goals to be. To ask these good teleological questions requires the historian to break various taboos that sundry behaviourists, eliminative materialists and poststructuralists would impose on what can be said about people, their actions, beliefs, desires and motives. But, since most insightful and instructive ways of understanding what people have done require breaking these taboos, the historian of science would do well not to credit the authors of these taboos with too much authority.

The good teleology of intentions can aid in appreciating what is wrong and what is not in “postcursor” and “precursor” talk. Lamarck thought and wrote as a knowing postcursor of Buffon. Darwin later would think and write no less knowingly as a postcursor of Lamarck. Is Lamarck, then, a precursor of Darwin? In one sense no: Lamarck obviously could not understand himself as such, and so could not be influenced by such an understanding; and the historian can not, then, be aided in understanding why Lamarck did what he did by categorising him as a precursor of Darwin. No less obviously, however, a historian can be aided in understanding why Darwin thought and wrote as he did by appreciating how Darwin saw Lamarck as one of his precursors. Attend to chronology, intentions and influences, just as one does in reading a novel or a newspaper, and the fallacies about “precursors” can be avoided; but notice how difficult the fallacies are to articulate and discredit if one signs up to a historiography that forbids, as some historiographies have, taking intentions and influences, and so influences on intentions and intentions as influences, into account.

Lamarck, needless to say, was not merely a postcursor of Buffon, he was a postcursor too of Linnaeus. Familiarly enough, mid-to-late 1700s natural history was dominated by Buffon and Linnaeus who had deeply different agendas of inquiry and explanation. Buffon’s agenda was predominantly cosmogonical; Linnaeus by contrast saw himself extending and reforming the systematic or taxonomic enterprise already pursued by Ray, Tournefort and before them Cesalpino and the Renaissance Italian's ancient master Aristotle. (Historians now question this Linnean view of Aristotle, but that point is not pertinent here). Buffon had repudiated the coherence and value of this enterprise, especially its quest for a natural rather than artificial system of classification; while Linnaeus had (not forgetting his account of Eden) left macrocosmogonical theories of the earth and (not forgetting his cortex and medulla proposals) microcosmogonical theories of generation mostly to others; accordingly he did not engage the newer natural philosophies of the Cartesians and Newtonians. These contrasts make entirely implausible any historiography, such as Foucault's, that has Buffon and Linnaeus singing off the same *episteme*, the same structuring rules for discourse. More generally, thanks to their deep debts to the positivist Comte, Foucault and his mentor Canguilhem can offer little help in doing historians' rather than scientists' history of science.

Faced with the contrasting legacies left by Buffon and Linnaeus, historians of science have to make up their minds about various very wide issues that, once again, connect with the very long run from ancient to modern times. Four clusters of such issues are introduced in the balance of this section of this paper: essentialism; historical thinking about nature; classifications, arrangements and gradings; and arboriform representations.

Provoked by Mayr's sweeping claims that almost all pre-Darwinian authors could not be "evolutionists" because they were, like Plato, "essentialists" about species, recent revisionists have emphasised how many were not essentialists. This revisionism is salutary; but its scope is too much limited by what it opposes. It does not take in issues about species as Forms and so as *archai*; nor issues about different accounts for different kinds of kinds. Plato gave Triangle, Man, Fire and Justice a unified ontological account. With Locke in 1690, geometric figures and ethical norms are not on the same footing as the real and nominal essences of gold, chalk and tigers. By 1800, chemical elements, mineral sorts and organic species are being given explicitly distinct treatments. Moreover, when studying conceptions of species, the historian does not always do best to follow Mayr and his revisionist opponents in looking mostly at the doctrines and practices of systematic botanists and zoologists. It was not as a systematist, but as a cosmogonist, natural philosopher and writer on the varieties of humankind, that Buffon integrated his thinking about species with his theorising about organic molecules and about internal molds; those more or less stable, self-perpetuating configurations of active, penetrating Newtonian forces responsible, as Buffon held, for the distinct reproductive succession of individuals constituting distinct species.Again, around 1800, the most sustained discussions of what it is to be a species are by authors such as Blumenbach and Prichard writing on whether humankind is one species or many. In the early 1830s, it will be the geologist Lyell, much indebted to Prichard, who will write at greater length than anyone else had ever done in answering questions explicitly about species, and who will consequently have the greatest influence on the early Darwin’s thinking about species.

Fresh historiographic stances need equally to be taken concerning history and time. For consider the standard stereotypings. The Greeks, it is routinely remarked, did space and geometry and vision; the Hebrews time, history and hearing. For Greeks time was a circle, for Hebrews a line. Seventeenth-century science, as represented by Descartes, was geometrical, and had to be repudiated by humanists to make possible a science of history. Newton had a boring repetitive universe, Locke a fixed human nature, and the French Enlightenment followed them in these views;

only later did German counter-Enlightenment thinkers liberate Western thought from this heritage, and show the world how to think historically about nature and man. “Holists” (such as Hegel) are more historical thinkers than “atomists” (such as Bentham). Deists (such as Hutton and Lyell) do ahistorical science, like Greeks, while, like Hebrews, Christians, such as Cuvier and Buckland do historical science.

There are plentiful grounds for challenging these stereotypings. Unhelpful chauvinisms and axe-grindings abound on these topics; most obviously German and Christian chauvinisms and axe-grindings. And, in any case, a historiography of cosmologies and ontologies may just lead to direct discreditings of these stereotypings. Burnet, Whiston and Buffon were thinking historically about the earth because and not despite of the influence on them of Descartes and Newton. Darwin learned to do historical, that is geological, biogeography not from the German Romantic Humboldt – who explicitly excluded geology from his geography – but from the Scots Enlightenment deist Lyell. More generally, it may well be misleading to assume that as a more “evolutionary” thinker than Buffon, Lamarck must have been more of a “historical” thinker also; misleading, for there is much more in Buffon’s treatment of the Old and New Worlds that is historical and geographical than there is in Lamarck’s works. So, “evolution” talk can only mislead here, too. Again, any suggestion that spatial thinking and temporal thinking exclude one another can be unhelpful. To understand the history of life or of society as full of contingencies, rather than as dominated by necessitation, may well require taking geographical accidents – chance migrations, invasions and other happenstance encounters – into account. Geographical views of history may, then, be usefully contrasted with what could be called developmental views that give less emphasis to circumstantial contingency. Whatever one decides on as a historian of science, it is manifest that one needs to be willing to challenge the usual distinctions, contrasts and comparisons invoked by the standard sterotypings and often perpetuated by "evolution" themes.

On turning to taxonomic science it may seem that large issues and longer runs are less relevant, but this impression can be misleading. Classification was not, in the decades after Linnaeus and Buffon, as simple an activity as that name may indicate, suggesting as it does merely the assembling and dividing of groups. For, associated with classifying were two other practices: arranging and grading. Thus, as a classifier, someone might group together all the backboned animals, and then divide this Vertebrate group into four divisions: Birds, Fishes, Reptiles and Mammals; and these divisions might be taken as natural not artificial. If so, he or she might then decide that these four are not equally close to one another in overall character or natural affinity, and so arrange them at the four corners of a rectangle, or at different points along a line. Going further, it might be judged that the Mammals are the highest in their degree or grade of organisational perfection, with Birds next and then Reptiles and Fishes.

What makes the interpretation of this classifying, arranging and grading so intriguing for a historian are presuppositions about designs and degrees in structure and function, presuppositions tracing all the way back to Plato and Aristotle and other ancients. Take Fishes and Birds for instance. Ancients and Moderns could agree that all Fishes are alike in structure and function because fitted for swimming in water; and, as for Birds, they resemble each other and differ from Fishes because fitted for flying for air. But consider now the resemblances between Birds and Fishes and how they all differ from insects. Here reference to adaptation, to designs and fittings to this or that way of life may seem explanatorily inadequate. And for centuries appeals were made to differences and similarities in degrees of animation or, later, organisation. Insects were so different in structure and function because so much lower in degree. Conversely, the resemblances between Fishes and Birds are due to their closeness in degree of perfection.

Note the explanatory priority given to degrees or grades. The resemblance, the likeness in structure and function, is explained by reference to the closeness in degree; not the other way round. Note, too, that the explanatory appeal presupposes a definite limitation. For any one degree level, structure and function are uniquely determined. There is only one way to be structured and to be functioning at that degree or level of animation or organisation. This presupposition is made not only about mankind at the highest level, but throughout the series. Degree or level judgements could therefore condition arranging and classifying decisions; for, if two groups are judged to be close in degree of organisation, it follows that they must be alike in the character of their organisation, and so should be placed close to one another in an arrangement by affinity and grouped together in a natural classification.

Once recognised, this ancient explanatory tradition can be seen living on in various versions in the eighteenth and nineteenth centuries. Buffon explains why similar or different animals and plants are formed, at various times and places, by referring to the different or similar degrees of heat active there and consequently the different or similar levels of organisation produced. Lamarck assumes that whenever the same degree of organisational perfection has been reached by successive complexifications, then animals of the same type will have been produced. Chambers, in his1844 *Vestiges of Creation*, explains what he takes to be the independent production of simian animals in the Old and New Worlds as due to life having risen to the same levels in each place; while mammals are missing from the

Galapagos, not because of migrational difficulties, but because life there has not yet had time to develop to that high level.

A historian may reasonably make a conjecture on this topic. With the authors just mentioned, resemblances not referable to common adaptive needs are referred to common levels of perfection. In other authors, starting in the early nineteenth century, those resemblances will be referred instead to a common structural plan or archetype. And, then, later Darwin will refer them to a common inheritance from a single, common ancestral stock. The conjecture is, then, that common plan and common ancestor explanations came to replace common level explanations, and so mark two ways of departing from a very ancient explanatory program indeed.

One virtue of such a conjecture is that it concentrates our minds on a central feature of Darwin's theory: namely, that it was a theory of common descent and that, in his view, common ancestries made appeals to common plans or common levels largely, if not entirely, explanatorily redundant. Hence the connection with trees and with branchings: obviously, branching tree diagrams are required in Darwin because his is a theory of common descents. However, it is seriously mistaken to assume that all arboriform representations in natural history have had a rationale consonant with that Darwinian rationale. For a start one might distinguish (i) the practice, going back to ancient times, of representing the successive divisions in a classification as like tree branchings (ii) the practice common in the eighteenth century, if not earlier, of comparing a branching arrangement – as opposed to a linear or rectangular arrangement say – to a tree (iii) the branching diagrams in Lamarck's depiction of the order of production of some animal groups, and (iv) Darwin's tree of life representations of successive, branching species propagations. "Evolution" historiographies have consistently led even sophisticated analysts to assume, quite wrongly, that Lamarck and Darwin had the same rationale for their branching diagrams. Only by keeping out of one's mind everything suggested by the word "evolution", and by looking at Lamarck's and Darwin's very different assertions and arguments, can one appreciate that their reasons for invoking branches were entirely and very instructively different, and not merely because Lamarck's branchings were not representing common descents.

This section of the paper will have succeeded if it suggests that, even for the eighteenth and nineteenth centuries, an appropriate attention to the very long run can help in avoiding the anachronisms and the teleology inherent in "evolution" historiographies.

Originality, influence, "ideas" and "worldviews"

When scientists do the history of science they often ask how original, how influential and how true were the ideas of some great hero. Indeed, the greatness ratings depend largely on these judgements. Historians are usually content to leave questions about truth and decisions about greatness to scientists; but they do take questions about originality and influence to come within their remit. With Darwin, however, there has seemed recently to be an anomaly emerging. According to a longstanding formula, found even in texts by historians of science, Darwin's *Origin of Species* (1859) contains two ideas: evolution plus natural selection; and while the second was new the first was not. Moreover, according to this formula, Darwin was influential because he made an old idea, evolution, credible by providing it with new adequate evidence and a new adequate cause. The anomaly has emerged, then, because historians are now agreed that the cause, natural selection, was accepted hardly at all for nearly half a century after Darwin's death in 1882. So, where Darwin was influential, as an evolutionist, he was not original; and where original, as a natural selectionist, he was not, until well into the twentieth century, influential.

There will be no quarrel with part of this anomaly here, the part concerning natural selection. When one looks at what others were doing before Darwin published, when one looks at Darwin's early, private notebook theorising before, during and after his gradual (several months) first arrival at his theory of natural selection, and when one looks at the long post-Darwinian run; then, indeed, one has to conclude that in formulating and arguing for that theory as he did, Darwin was being highly original, but was not successful in getting agreement from others, even if he influenced many people, negatively so to speak, by prompting them to disagree with him and to develop alternatives to his theory.

Leaving to other papers in this issue of this journal discussion of the origin and fate of the theory of natural selection, the balance of this paper will concentrate on "evolution" rather than on natural selection in Darwin. Needless to say, the first step must be to drop "evolution" talk, and to stay with Darwin's terms: the "tree of life" and "common descent" and so on. Less obviously, it will be argued that help can be had in understanding how the *Origin* was received by looking, again, to Darwin's early notebooks and seeing how he arrived at his first versions of the tree of life scheme familiar from the *Origin*. To anticipate, help can be had for a simple enough reason. For when one sees how remarkably complicated was Darwin's arrival at those first versions of that scheme, and when one sees how he only did so by departing in several major ways from anything that others had done before, then a very useful conclusion becomes hard to avoid. The conclusion is that it is entirely misleading to read Darwin in the *Origin* as providing "new evidence" for "an old idea", namely "evolution". Indeed, it is misleading to go on contrasting the newness of the "evidence" with the oldness of the "idea", because such an analysis does not concentrate our minds sufficiently on Darwin's argumentation. For, the tree of life scheme was novel in ways that required the evidential argumentation to be novel too. What is more, in appreciating the novelty of both scheme and argumentation, one can begin to appreciate why many readers were persuaded to agree with Darwin, readers who had been unpersuaded by various earlier theorists, such as Chambers, often lumped with Darwin under the "evolution" theme. Here, then, by contrast with the theory of natural selection, one finds Darwin not only being novel in his arguments, and in what he was arguing for, but being persuasive because rather than despite that novelty.

As just one instance of what is involved, an uncontroversial instance, consider what many historians now agree upon: namely, that Darwin's tree of life scheme was the first comprehensive proposal for a theory of common descents; with, that is, branching and rebranching divergences descending from single, ancestral species to many diverse descendent species. For Darwin to argue, evidentially, for such a scheme in citing morphological facts, or in citing biogeographical facts, was for him to argue that the common structures in some order of species, or the common origination in some region of some genus of species, were most probably the result of a common ancestry rather than any other cause. So, readers who were persuaded by Darwin's argumentation were not won over by a new, unprecedented weight of evidence for an "old idea", but by new argumentation for a new scheme.

One could discern that much without going back to the notebooks, just by comparing and contrasting Darwin's argumentation, what he was arguing from and to, with earlier publications by other authors. The notebooks can help, however, by alerting us to the novelty in the presuppositions and implications of Darwin's tree of life scheme.The most pertinent notebook pages are a mere four dozen at the opening of *Notebook B*, pages first filled by Darwin around July 1837. Once again, in understanding what Darwin is doing in these pages, one needs to read him as he wrote: as someone departing in various ways from Lyell's views and from Lyell's version of Lamarck's views, and as someone given a lifelong preoccupation with generation (or reproduction) by his Edinburgh mentor, Grant, and as someone inspired by the precedent set by the chapter on generation in the book *Zoonomia* (1794-6), by his grandfather Erasmus Darwin.

For a start, consider the successive agreements and disagreements Darwin has with Lyell's views. In the two earliest years of the voyage (1831-33), Darwin comes to embrace Lyell's account of the physical and organic worlds. His first disagreement about species concerns their extinctions. In 1835 and for the next three years Darwin explains extinctions as due, not as Lyell held to external competitive circumstances, but to an inherent limitation on species lifetimes, analogous to an individual's lifetime limitation. Next, most likely around mid-1836, Darwin disagrees tentatively with Lyell's view that species are fixed in character and originate as independent, special creations. Then, some months after returning to England, Darwin, in March 1837, decides to side with Lamarck's entire system as expounded and rejected in Lyell. Accordingly *Notebook B* opens with a sketch for a comprehensive zoonomical system matching the precedent of Lyell's version of Lamarck. Very soon, however, that opening system has been revised and replaced by Darwin with another. This system for the first time includes a tree of life scheme like the one familiar from the *Origin*.

By keeping these successive departures from Lyell in mind, one can appreciate, also, how Darwin has made three departures from Lamarck's own version of Lamarck's system. For Lyell's version of Lamarck departs fundamentally from

Lamarck's own, not least because in Lyell's version the system is one of branching and rebranching common descents as it is not in Lamarck's own system. So, by siding with Lamarck in Lyell's version, Darwin has departed from Lamarck himself. Next, note that Darwin's opening notebook system makes further departures from Lyell's rendering of Lamarck. So, by the time one reaches the forty fourth page of *Notebook B* and Darwin's revised replacement of his opening system, one needs to distinguish four successive systems: Lamarck's own; Lyell's version of it; Darwin's initial, modified emulation of that version; and, finally, Darwin's new replacement for that initial system.

It is worth facing up to the complexities in Darwin's successive agreements and disagreements with both Lyell and Lamarck for a fundamental reason. These two were uniquely and massively influential in Darwin's early theorising and so for all his later theorising. Yet his theories are hugely different from either of theirs. And there is no paradox here, as anyone can confirm who has studied the Aristotle-Plato or the Marx-Hegel relationships or Kant's relations with Hume and Leibniz. "Evo-revo" historiographies do not easily accommodate such relationships. For they ask us to choose between saying that Darwin owed a lot to Lamarck so that Darwin's "evolution" is like Lamarck's "evolution", or saying that Darwin can not have owed much to Lamarck because Darwin made a "revolutionary" break with all that went before. The trouble arises here because influence is taken to entail similarity, as in heredity. Well, what is shown by a close study of the agreements and disagreements in Darwin's successive departures from Lyell and Lamarck is that intellectual work, brainwork if one prefers, does not have to work that way.

As just one example of this general reflection, consider a consideration that moved Darwin to replace his first with his second *Notebook B* system. In the first he had tried to explain a manifold correlation: among animals with the highest grades of organisation, there were more branched and less linear affinities, shorter species lifetimes and greater character gaps than among lower animals. Now, what Darwin goes on to do with this correlation is not to give it a new explanation; rather he decides that greater gaps within and between groups correlate not with higher perfection in the groups, but with greater taxonomic width. For, in the buddings and splittings in the tree of species branchings, when one ancestral species has a dozen descendent species, there must be eleven lines ending without splittings in extinctions, given (as Lyell thought) that the total species number is not increasing. In this greater multiplying of species in the diversifying descent of a large group, an order say rather than a genus, there will be many more extinctions and so bigger gaps of character within and between groups. In this arboriform process, any species as a quasi-individual is born, lives and dies but once, and so likewise with any supraspecific group issuing from its single, ancestral species. Moreover, only one species in an ancestral group has had descendants in any particular offspring group, so there is no general tendency for fish species, say, to have mammalian descendants. One fish species did so once, due presumably to exceptional circumstances as all the rest have not. Darwin has here reached, therefore, an abstract, referentially anonymous scheme, like the one familiar from the *Origin*. Like that scheme, the diagramming for this scheme is labelled not with the names of particular groups, fish, say, or mammals, but only with letters and numbers representing quite abstractly the cumulative, arboriform outcome from the births, lives and deaths of species in any indefinitely long run of past time.

Darwin's new tree of life has now departed fundamentally from any scheme, such as Lamarck's own, of recurrent escalations of life through a given, permanent, array of higher and higher organisational types. So, while Darwin is explicit in holding that each individual mammal's ontogeny recapitulates the fish ancestry of the mammals, this is a recurrent ontogenetic recapitulation of a phylogenetic descent that has only happened once. Mammal embryos have causes within them ensuring that they all undergo that recapitulation, but there has been nothing within or without fishes ensuring any phylogenetic recurrence.

What *Notebook B* shows more vividly than the *Origin* is that Darwin drew not merely on the branching structure of trees in his tree of life analogies. The growth of trees as colonies of buds was decisive too for this protégé of Grant and Erasmus Darwin, with his lifelong preoccupation with comparing and contrasting sexual and asexual generations and individual and colonial lives. Thus in that summer of 1837, the tree of life was conceived as growing asexually, in that a group of offspring species issued from a single parental species not from a pairing of two. However, species propagations were in a sense quasi-sexual, in that a species acted upon by altered circumstances was quasi-mating with those circumstances. Without the quasi-sexual influence of fresh circumstances, the species would die childless, with no successor species, when its limited vital duration expired, just as, Darwin thought, any asexual tree grafting succession does. Like an asexual graft succession that can only avoid childless death through a fresh sexual union, so a species is saved from extinction without issue by its quasi-sexual interaction with fresh circumstances.

Even this brief sampling of Darwin's tree of life theorising at one moment early in his life will show how misleading it would be to summarise his relationship to Lamarck by saying that they both had the "idea of evolution", but "evolution" was "linear" for the older man and "branching" for the younger. It shows, too, how terms, assertions and arguments make better analytic items to compare and contrast than "ideas". As the philosopher Davidson says somewhere, the very idea of an idea is not a very good idea.

It might be thought that Darwin's tree of life scheme of common ancestries has no room for any conception of levels or degrees of organisation. But Darwin does indeed eventually embrace just such a conception. It does not, however, come still accompanied by the old presupposition that made the character of any organisation uniquely limited by its degree of perfection. In the 1820s, Von Baer had deliberately distinguished the type from the degree of organisation so as to avoid that limitation. The degree of organisation is for him given by how much differentiation of structure there is And different organisms with different structural differentiations, with many different types of structure, can be differentiated to the same degree, to the same level. Darwin, by the time the *Origin* was written, had adopted this view. Accordingly, he has progress reliably if not invariably accompanying adaptive divergence. For many different descendents from a common ancestor can all have higher levels of organisation than that ancestor without all having to be of the same character as each other. There are unlimitedly many ways of progressing beyond the ancestor. Progress in any one line of descent is obviously linear, but the different lines can progress in divergent ways. The embryos in any one line recapitulate the phylogeny of that line; but that does not limit the changes made, nor then what is recapitulated in other lines of descent from the same ancestral stock. Nor are those lines limited in what can descend from them; and so it goes on: progress in the levels of organisation but without character limited by level.

Conceptions of progress are often, perhaps always and necessarily, associated with social ideologies. Historians insisting that there is more than mere biology going on in Darwin's work have increasingly and rightly insisted that there is a social ideology there too. This historiographical campaign is now won and it is good that it is. There is room for revision here, too, however. In recent decades the long run of England's social and economic life has been heavily reinterpreted, so that, for example, many Marxists now think Marx and Engels mistaken about who the ruling class was in the society Darwin grew up in. The time has come, then, to question the standard typing of the Darwins as middle class and of Darwinian science as bourgeois. The English ruling class, the Darwins among them, and Darwinian science itself, need reinterpreting as landed and gentlemanly rather than urban and industrial, but nonetheless capitalist for all that.

The other standard way to broaden the scope of any historiography for Darwinian science is to insist that Darwinism became not just a scientific theory but an entire "worldview". The philosopher Scheffler says somewhere that the whole idea of a worldview is not a wholly good idea, being, as it is, a questionable legacy from post-Kantian German idealism, and assimilating, as it does, thought and judgement to vision and seeing. There may be a case therefore for dropping "worldview" talk and sticking with cosmologies and ontologies.

Darwin himself was offering no intergration, Buffon style, of a macrocosmogony and a microcosmogony, nor then of natural philosophy and natural history. His theory of generation, pangenesis, formulated well before the *Origin* but not published until 1868, was a microcosmogony, but his geology in the *Origin* was Lyellian enough to be acosmogonical. And of course Darwin had nothing to say about the moon, much less the sun and other stars. In obvious senses, natural selection – with its unremarkable ontology of competing, hereditarily variable organisms, and its formation of species distinguished from mere varieties in the usual ways – was, especially when complemented with the account of adaptive divergence and progress, a mesocosmogony. For natural selection is fitting, ordering and improving in its workings on inherited variations that are in themselves chancy and disorderly. However, even if one integrates natural selection with pangenesis – which Darwin never did in a sustained way – there is no systematic theorising with the scope and ambition of Buffon's. A historiographer of cosmology and ontology who follows the precedents set by the historiographers of "worldviews" will therefore look beyond Darwin himself to broaden the agenda.

The usual candidates for this purpose are, quite properly, the likes of Spencer and Haeckel, and Spencer will serve well here. For he can indeed be read, in his ambitions, as a successor to Buffon. For, like the Frenchman he offers an integration of macrocosmogony and microcosmogony, and of natural history and natural philosophy, in his synthesis of Adam Smith on societies, William Herschel on heavenly nebulae, Lyell's version of Lamarck's biology, Von Baer's generalisations (from "homogeneity" – by progressive differentiation – to "heterogeneity") about embryonic maturations, and Grove, Joule and other physicists on the persistence of force, also known as the conservation of energy.

This itemisation of the main ingredients in Spencer's synthesis shows right away what a great deal has happened since Buffon. What historians of biology need to pay more attention to than they usually do is the physics. After Buffon, the rise and fall of Laplacean physics and the rise of a new physics of work and energy – indebted to sources as diverse as English and French steam engineering and German *Naturphilosophie* – transformed natural philosophy and so, obviously, transformed the relationships between natural philosophy and natural history. Those relationships went far beyond the intervention of William Thomson (Lord Kelvin) and others into debates on the age of the earth. And those relationships have continued to be influential and historiographically instructive, as one can see by looking to the disagreements about evolution and Boltzmannian thermodynamics among such people as Lotka, Wright and Fisher in the 1920s. Not that this continuation should be surprising, for thermodynamics became, in the late nineteenth century, the new theory of order, of cosmos and chaos, and a new locus for ontological disagreements between atomists and eergeticists, and between realists and non-realists. Thermodynamics' relations with biology offer, therefore, as several recent writers have recognised, very fruitful possibilities for broadening our historiographical horizons, as graduate students may be encouraged to appreciate.

Concluding remarks

As the conclusions to this paper were presented at its opening, it remains only to sound two positive notes here. First, any survey of the last two and a half millennia of theorising about origins and species shows that almost every century has seen major shifts; so it is not merely that we need a historiography that does not tend to lump together all the pre-Darwinian thought as just that: pre-Darwinian, and so presumptively creationist, essentialist or whatever. We need many historiographies that avoid the distortions entailed by the usual "evo-revo" themes and encourage looking instead at many centuries in fresh ways. Second, graduate students today are often understandably wary of getting into any topic even remotely associated with Darwin and his science, if only because they assume that it has been worked out very thoroughly by now. This paper will have succeeded if it is taken to suggest that, even on the most familiar topics in this area, there are plenty of opportunities for new inquiries, interpretations and themes.

References

Hodge, Michael Jonathan. 1990. “Origins and species before and after Darwin”. In R. C. Olby, G. N. Cantor, J. R. R. Christie and M. J. S. Hodge (eds.), *Companion to the History of Modern Science*, pp. 374-395. London and New York: Routledge

Hodge, Michael Jonathan. 1991. *Origins and Species: A Study of the Historical Sources of Darwinism and the Contexts of Some Other Accounts of Organic Diversity from Plato and Aristotle*. New York: Garland

Hodge, Michael Jonathan, and Radick, Gregory (eds.). 2003. *The Cambridge Companion to Darwin*. Cambridge: Cambridge University Press

Sapp, Jan. 2003. *Genesis. The Evolution of Biology*. Oxford: Oxford University Press

---

[i] Among professional historians of sciences, the most widely read current practitioners of what I call “evo-revo” historiography are probably Peter Bowler, Robert Richards and Michael Ruse. Their invaluable books are discussed and cited extensively elsewhere in this issue of the journal. “Evo-revo” themes are prominent in a recent general history of modern biology by another professional: Sapp 2003. My own critiques of those themes and proposals about alternatives have been developed in various articles drawn on and listed in Hodge 1990,1991 and Hodge and Radick 2003, and , mostly, included in the History of Science, Technology and Medicine bibliographic database available online through many institutions. The present paper has benefited greatly from suggestions made by Gar Allen, Jane Maienschein and Michael Ruse in their extensive and helpful comments on an earlier version.