INTRODUCTION

This volume is one of a pair. The other volume will appear later this year and will include only articles on Charles Darwin's life and work. This volume collects articles concerning pre-Darwinan and post-Darwinian topics. The articles are here arranged in three groups. The two articles in the first group address big-picture, long-run interpretative issues, especially concerning theorising about origins and species from ancient times to the present day. The five in the second group concentrate on four ambitious pre-Darwinian theorists of modern times, theorists who were theorising comprehensively about the earth, life and man: Georges Buffon, Jean-Baptiste Lamarck, William Whewell and Robert Chambers. The principal writings of these four were spread over roughly a hundred years from the mid-eighteenth to the mid-nineteenth century. The articles in the third group treat of the theory of natural selection, as it was argued for by Charles Darwin in his 1859 book *On The Origin of Species* and as it has been developed and deployed since. Here the discussion often moves from the history of past science and past philosophy to the current philosophy of current science; so there is much in this third group of articles that will interest philosophers as much as historians.

The articles, identified by Roman numerals, are, with one exception, reproduced here just as they first appeared and with their original pagination. The exception is article (V) which is presented here in the English version that I wrote rather than in the translation given it for its printed publication. For some articles, the bibliographies have had to be extracted from composite bibliographies for the collaborative volumes in which the articles were published. But these extractions involve no additional editorial changes. Following this introduction, I have provided notes about the original publications of the articles.

A word may be useful here about the title given to this volume. The conjunction of the terms origins and species will seem familiar enough for obvious reasons. When 'evolutionists' debate 'creationists' today both sides are agreed that animal and plant species have had origins; where they disagree is over how species originate, over what origins species have: origins in gradual natural modifications of earlier species or origins in sudden, supernatural acts of God? The big divides have not always run on just those lines. Among the Greek cosmologists, Plato taught that species were those eternal, immaterial

forms – recipes and standards with no origins – looked to by a craftsman God in working chaotic matter into an ordered world; while Democritus found origins for species in fortuitous concatenations of mindlessly-moving material atoms. To favour Platonic forms over Democritean atoms was to favour one account over another of the order of the cosmos and, at the same time, to favour one account of being, of what is, one ontology, over another. From the Greeks there descended then diverse traditions that divided over issues than are at once cosmological (concerning the nature of the world's orderliness) cosmogonical (concerning the origins of that orderliness) and ontological (concerning the being of the origins and constituents of that order). As an aside, I might add that when Geoffrey Cantor and I collaborated on writing a historical overview of ether concepts to introduce a volume of papers on that subject, we saw there too the indispensability of all those old commonplaces, for such they are, about the legacy of diverse Greek integrations of cosmologies with ontologies.

The rest of this introduction will be largely devoted to discussing in turn the three groups of articles. The first group can be read as arguing that in the history of the life sciences we still need critical historians' histories of science, alternatives to what may be called partisan scientists' histories. Partisan, participants' histories are familiar enough in political history or military history, written by the ministers or the generals who took part. And one reason we have a profession of history is to ensure that there are alternatives to those histories; and likewise for the profession of the history of science: we need alternatives to the histories the scientists write. For some periods and topics that has already been achieved and the kind of campaigning I call for is no longer needed. Writers on the new natural philosophies of the seventeenth century are no longer under the spell of Ernst Mach and other physicists – often positivistic ones – as that prominent young historian of science and Newton scholar Bernard Cohen was only six decades ago. The shift to historians' history of science has been made in some places; but in others it has still to be made. To be convinced of this all one needs to do is to look at recent standard texts by authoritative historians of science, texts whose titles signal that they chart, to use conventional phrasing, the history of evolutionary biology. For if one then compares those texts with the essays and books with similar titles written over a century ago by Darwin's defender T.H. Huxley and his associates, one finds manifest continuities in how the history is interpreted and written. Indeed, in some cases, one will find explicit recognition that the historian of science of our time is knowingly perpetuating Huxleyan historiograhical precedents.

In an article written in 2005 for a special issue of the *Journal for the History of Biology* devoted to reassessing the so-called Darwinian Revolution (an article which will appear in the partner volume to this), I was tactless. I suggested that for historians of science in our time to have made no deliberate, sustained

break with that Huxleyan tradition of scientists' history of science is remarkable not to say bizarre. In 'Origins and species before and after Darwin' (I) I am not so tactless but the aim is the same: to provoke a long-overdue rejection of that tradition and a quest for alternatives. In that piece, I offer one possible alternative but without wishing to imply that only this one is all we need. The tactless stance is more evident in the second article: 'Canguilhem and the history of biology' (II). I would justify the lapses of tact in two ways. First, for good reasons, Canguilhem has enjoyed during his life and since, great, merited international academic respect. Second, again for good reasons, those folk whom one's French friends used to call the Anglo-Saxon historians and philosophers of science have often looked to the Continent for critical insights into the limitations in Anglo-Saxon views. However, neither of these considerations seems to me to be adequate grounds for not raising critical questions about just how far Canguilhem can help us as historiographers of science, especially in our efforts to make those shifts away from scientists' historiographies, shifts still needed in major areas of our discipline. It did seem appropriate therefore to ask why Canguilhem's history of science seems so often to be written in ways that remind us too much, to put it crudely, of scientists' history of science. Is it despite his philosophical sophistication, or rather because of it? The answer has to be a more complex one than I could offer in one short article. But, I argue there, it has to do, among many other things, with Canguilhem being too much under the influence of that other positivist, one Mach often sought to vindicate: Auguste Comte. The connections between positivist influences and scientists' history of science form a vast and instructive topic not faced in either of my articles. But any enquiry, into the nineteenth-century sources of those connections, would confirm that the original ideological rationale of the positivism pioneered by Comte and upheld by Mach was to legitimate the claims professional scientists were then making for unlimited autonomous and hegemonic authority for science as the only modern progressive way to think and act in any domain. So a positivist history of science must reconstruct a progress from persistent religious and metaphysical dogmas and speculations to successive scientific corrections of them; such a history is also then the most fitting, reassuring, celebratory, edifying, triumphant history for any scientists to give of their science.

Turning now to the second group of articles, there is an issue about labels and phrasing that connects directly with what has just been said about scientists' history of science. In identifying Buffon and the others as 'pre-Darwinian' as I did earlier, and in using that phrase in the title of (I), I did of course do what we should not be doing: namely, talking of the people before Darwin as those who came before him and so falling into two lots: the unwittingly prescient preparers and anticipators whose efforts were later consummated by Darwin, and the preventers, resisters and losers whose role was to be the precursors of Darwin's

own opponents. There is plainly a simple antidote to this kind of talk and to the fallacious presumptions it promotes: talk, think and write about Lamarck, say, not as someone who came before some one other with one view, but as someone who came after many others with many diverse views: Plato, the priestly redactor of *Genesis*, Lucretius, Descartes, Newton, Linnaeus and Buffon and so on and on. The advantage of working at postcursorial interpretations is not merely that successions can be read the right way round, as they happened , from earlier to later, but that the narrowing imposed by one-dimensional relatings to one future development can be replaced with much broader, plural relatings to the many past resources, influences, contexts and so on being responded to consciously or otherwise, intentionally or not so, with embracings or with repudiations and so on and on. My hope then is that the articles on Lamarck and the other three can not only exemplify such familiar dicta, but do that with enough evident, specific, detailed advantages as to make those dicta look like more than just pious and truistic homilies.

The period from Buffon to Chambers, from the mid-eighteenth to the mid-nineteenth century, spans of course the Enlightenment and its sequel the Counter-Enlightenment, including the romantic movement; and no less familiarly the French Revolution and the Industrial Revolution. Or at least that is what students used to be told. Now, quite properly, it is explained that some, if not all of these labels, should go in quotation marks to signal recent questionings. So in the case, say, of the 'Industrial Revolution' there is no denial that many of the transformations historians used to write about under that heading did indeed take place, but their timings, their pacings, their causes and effects are now understood in ways making inappropriate that old label with its connotations of suddenness and totality. Historians of science can only welcome such revisions, fitting well as they often do with their revisings of older views in their own fields. Buffon is rightly seen as an Enlightenment *philosophe* of the *ancien regime*, Lamarck as no less an Enlightenment *philosophe* but, from mid-career on, as a citizen professor in the reorganised national museum of the revolutionary regime. But attempts to relate either of them to trends in urbanisation or increases in machinofacturing or the rise of the middle classes seem less plausible. And here the historian of science can be encouraged by social and economic historians' newer emphasises on the role in the maturing of European modernity of agrarian capitalism and imperial capitalism. Too narrow a focus on the sources and consequences of urban, bourgeois machinofacturing capitalism can hamper and misguide any efforts to relate the natural history and natural philosophy of these epochs to their proper social and economic contexts in other forms of capitalist life.

Before moving to discuss the articles in the third group, it may be useful to indicate how far those in the first two groups connect to the doctoral thesis I

wrote in the late sixties, and which was published much more recently without revision but with a new extended preface in 1991. The ancient, medieval and early modern authors treated very hurriedly in the article 'Origins and species' (I), Plato, for example, and Aristotle, Augustine, Aquinas, Descartes, Burnet and Whiston, all got much more sustained, albeit pretty amateurish attention, in that thesis. Likewise, the relations of Darwin and Wallace to Lyell, relations relevant to the Whewell and Chambers articles printed here, are given extensive treatment in that thesis, more extensive, regarding Wallace's biogeography especially, than in anything I have published before or since. The new preface provided a chance to explain how the state of interpretation on various topics had moved on in the seventies and eighties. Obviously things have moved on further now. To take just two examples: anyone studying Lamarck today goes to the indispensable website on his life and work run by Pietro Corsi and colleagues and thereby engages many archival, explicational and contextual possibilities that were hitherto much less easily available, if at all; while no one takes up Chambers as a subject today without steeping themselves in Jim Secord's marvellous monograph: *Victorian Sensation: the extraordinary publication, reception, and secret authorship of* Vestiges of the Natural History of Creation (Chicago, 2000).

Turning now to that third group of articles, I can begin with a familiar pair of points about what has happened in evolutionary biology since Darwin. The first point in the pair emphasises that Darwin's theory about the main cause of evolutionary change – his theory of natural selection – is alive and well in current textbooks setting out today's consensual conclusions. Most professional evolutionary biologists are natural selectionists, as Darwin was. So there is continuity, agreement, with Darwin on that much. The second point is that, for today's biologists, natural selection works mostly with small hereditary variations just as it does in Darwin's account; but since Darwin the understanding of the sources of these hereditary variations have been transformed by three developments: the elaboration in the early twentieth century of Mendelian cytological genetics, in the mid-century of Mendelian mathematical and experimental population genetics; and, in the later twentieth century of molecular genetics. So, on what is now called the genetics of evolutionary change, major changes in theories and techniques have come in since Darwin.

The papers in this third group contribute to this question of continuity and discontinuity between Darwin and today. On the theory of natural selection as an element of continuity, they aim to contribute in two ways. For a start, the first four papers defend an analysis of what natural selection is for Darwin and of how his various arguments for it are integrated in the one long argument of the *Origin*. Put simply the main claim is that for Darwin natural selection was not a law or a principle but a cause, a causal process or agency, just as artificial selection was for him a causal process or agency. Accordingly, he argues that

this cause exists in nature, that it is capable of causing to new species to be formed in the modification of older ones, and that it has been the main process or agency responsible for producing the species that have lived on earth. In sum, Darwin adduces evidence for the existence of this cause, adduces independent, separate evidence for its power to produce species and again separate, further, evidence that it has indeed been responsible for doing so. Now, what is insisted on throughout the last batch of papers in this volume is that, as for Darwin so today, the theory of natural selection is a causal theory appropriately evidenced in just such ways; and is a theory providing, as it did for Darwin, causal explanations for the production of new species from old and so causal explanations for all kinds of generalisations – ecological, embryological , geographical etc generalisations – about species. Today then as of yore it is a causal-explanatory theory.

But now what about all those changes in thinking since Darwin about heredity? There is a simple story one can tell here and it is not entirely misleading. It says that Darwin held assumptions about heredity that did not cohere at all well with his theory of natural selection. However, the story goes on, those assumptions about heredity were discredited in the early twentieth century; so that a trio of mathematical population geneticists, Haldane, Fisher and Wright, could show that with those errors eliminated natural selection works fine with hereditary variations as newly understood.The trouble with this story is that it overlooks too many instructive complications. In one article (XII) I have tried to show how this story is too simple-minded in what it says about the changes that took place in theories about reproduction or generation and about heredity between Darwin in the 1830s and the evolutionary genetics of the 1930s. Especially I urge that in order not to be leaving vital considerations out of the history one has to focus on changes in thinking about sexual and nonsexual reproduction and even life and death themselves. And once again, the conclusion is that only by doing so can we avoid the limitations and distortions inherent in the scientists' histories of these subjects over these decades. In another article (XIII) I join others in emphasising that, in the case of Sewall Wright and Ronald Fisher, there were along with their agreements – about how to integrate the new genetics with the old theory of natural selection – deep and instructive disagreements about how to understand evolution and indeed the cosmos as a whole. For Fisher, the cosmos divides into two worlds: first, the living world subject to all the entropic (disordering) causes and subject also to natural selection, the only counterentropic (ordering) cause in the cosmos, and, second, the rest of the world which is subject to entropic causation alone. For Wright, in keeping with his panpsychist metaphysics (mind is what is ultimately real in everything all the way down to the matter of elementary particles), there is no such divide. His science contrasts not entropic and counter-entropic causes but heterogenesing

and homogenising causes; and he holds that all science whether physics or biology may make any process more intelligible by disclosing how that process involves both kinds of causal tendencies in shifting balances or moving equilibria, this last a phrase drawn from Herbert Spencer. Wright's shifting balance theory of evolution is, then, not only opposed to Fisher's mass-selectional alternative to it, it is integrated with cosmological assumptions quite at odds with Fisher's own. The controversy between Fisher and Wright, still continued today by their scientific descendants, is then yet another example of the light thrown even on much modern science by legacies from ancient Greek traditions.

Going back now to natural selection as a causal-explanatory theory: two articles, (IX) and (XIV), pursue this theme in ways that connect directly with current controversies in the philosophy of biology. In (IX) I argue that two recent, general, philosophical views about scientific theories – van Fraassen's and Kitcher's – have been used to throw welcome light on Darwin's theory in the *Origin*; but that both views fail to do justice to its causal-explanatory character. In (XIV) I contrast any correctly causal-explanatory view of the theory with any view such as the Machian positivist Pearson held early in the twentieth century: namely that Darwin's theory is properly understood as all physical theories are, not as causal-explanatory but as correlational-descriptive. Now, a view of natural selection, a view reasonably called neo-Pearsonian, is currently being defended by some philosophers of biology who think that the theory of natural selection when properly understood is not a causal theory but a statistical theory. In writing (XIV) and in arguing that Pearson's view was an aberrant and misleading minority view of the theory, I did not have these more recent neo-Pearsonians in mind, for they had not arrived on the scene yet. But anyone familiar with the lively debates they have provoked in the last few years will find in (XIV) resources of a historico–philosophical kind very adaptable to countering their stimulating but mistaken neo-Pearsonian take on the theory of natural selection. Indeed, I am working towards joining in myself in future publications. In doing so I will find myself once again taking issue with the historico-philosophical legacies of nineteenth-century positivism. But I should not let that remark give a false impression. All the articles here reprinted were not written by an author consciously bent on taking up one issue after another with those positivist legacies. It is just that countering the legacies from nineteenth-century scientists' histories of science can often develop so that it overlaps with any countering of legacies from that century's positivist philosophies of science.

In closing this brief introduction, I am delighted that there is now this chance to express my most grateful thanks to Greg Radick for first thinking of this republication project, for discussing it with Ashgate and for continuing encouragement since; to Annie Jamieson for contributing so expertly to the preparation of text copies, bibliographies and other material and for the secur-

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