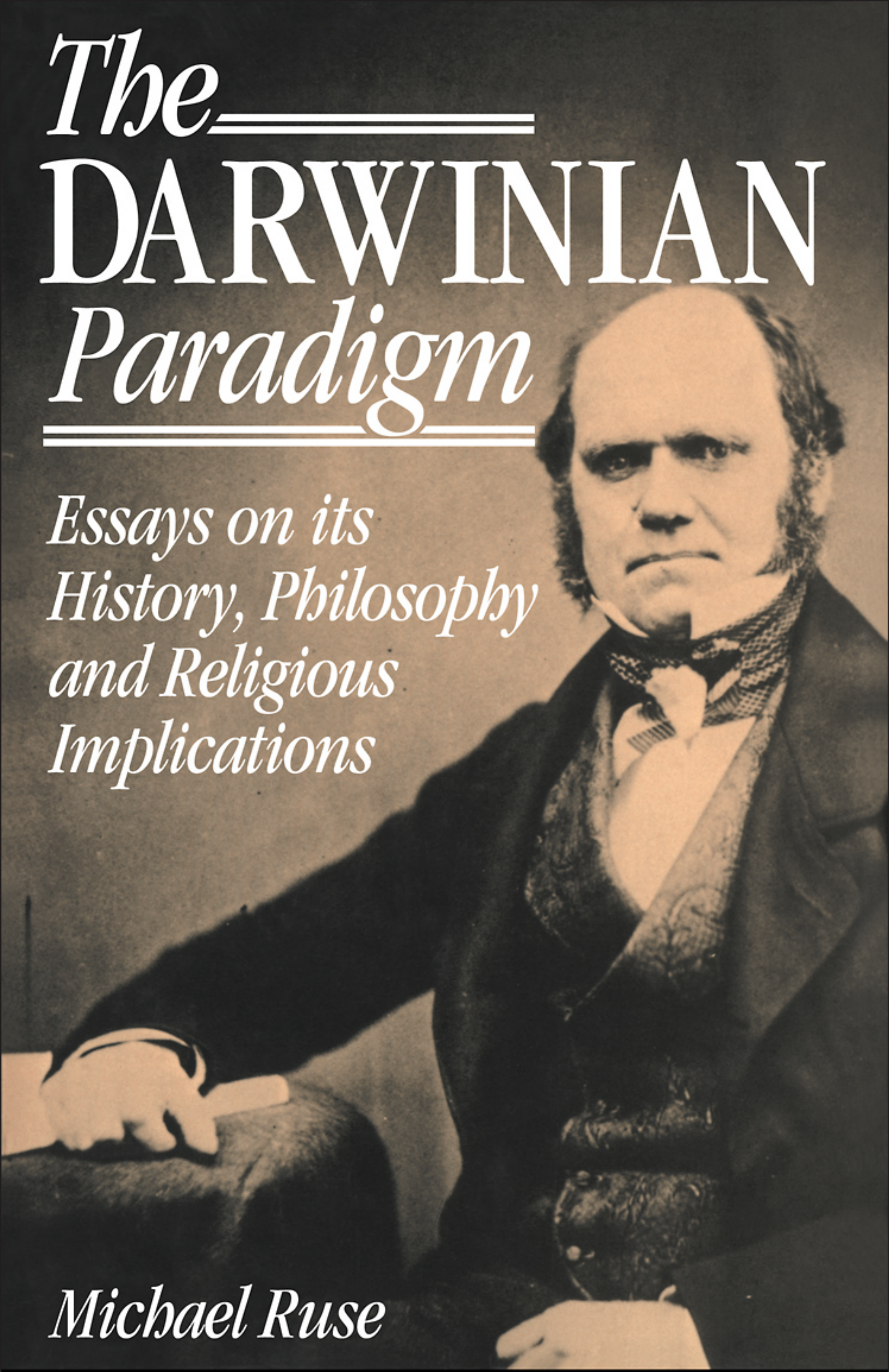
The DARWINIAN Paradigm

Essays on its History, Philosophy and Religious Implications

Michael Ruse



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For William and Ursula Ruse with affection

Aristotle's *Physica,* Ptolemy's *Almagest,* Newton's *Principia* and *Opticks,* Franklin's *Electricity,* Lavoisier's *Chemistry,* and Lyell's *Geology*-these and many other works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners. They were able to do so because they shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve.

Achievements that share these two characteristics I shall henceforth refer to as 'paradigms'.

Thomas Kuhn,
The Structure of Scientific Revolutions (1962)

CONTENTS

ACKNOWLEDGEMENTS

The following essays have been published before. I am grateful for permission to include them in this collection.

'Darwin's debt to philosophy: an examination of the influence of the philosophical ideas of John F.W.Herschel and William Whewell on the development of Charles Darwin's theory of evolution', *Studies in History and Philosophy of Science* 6 (1975) 159–81.

'Charles Darwin and group selection', *Annals of Science* 37 (1980) 615–30.

'What kind of revolution occurred in geology?' in P.Asquith and I.Hacking (eds) *PSA 1978,* East Lansing, MI: Philosophy of Science Association, 1981, 2:240–73.

'Biological species: natural kinds, individuals, or what?' *British Journal for the Philosophy of Science* 38 (1987) 225–42.

'Teleology and the biological sciences', in N.Rescher, (ed.) *Current Issues in Teleology,* Landham, MD: University Press of America, 1986, 56–64.

'Biological science and feminist values', in P.Asquith and P. Kitcher (eds) *PSA 1984,* East Lansing, MI: Philosophy of Science Association, 1985, 2:525–42.

'Is rape wrong on Andromeda? An introduction to extraterrestrial evolution, science, and morality', in E.Regis, (ed.) *Extraterrestrials: Science and Alien Intelligence,* Cambridge: Cambridge University Press, 1985, 43–78.

INTRODUCTION

I first read Charles Darwin's masterpiece, *On the Origin of Species,* some twenty years ago. At once I fell under its spell—an emotion which is as strong within me today as it was then. For all of Darwin's problems and gaps and inconsistencies (and over the years I have played my part in bringing these to light) at base, I think he was right. With Darwin, I believe that the organic world came into being through a natural process of evolution, that by far the main mechanism was natural selection, something which speaks directly to the most pervasive feature of organisms namely their adaptedness, and that (although this is only hinted at in the *Origin*) for all of the obvious qualifications one must make, the ideas apply absolutely and completely to humans.

In various writings, I have explored aspects of 'Darwinism', most fully in what evolved into a trilogy of works: on history, *The Darwinian Revolution* (1979); on science, *Darwinism Defended* (1982); and on philosophy, *Taking Darwin Seriously* (1986). And yet still I feel dissatisfied—or rather, still I feel my understanding is just beginning. One thing in particular which puzzles me more and more as the years go by is why so many of my fellow professionals, particularly the philosophers of science, tend not to have the same overwhelming convictions about Darwinism as I. One must not exaggerate. I do not pretend to be a better or more profound scholar than anyone else, nor am I (explicitly or implicitly) implying that most people do not take seriously evolution or natural selection or our own natural origins. I am not even saying that I am the truest or most orthodox Darwinian that there is. In biology itself, there are many more ardent pretenders to that title.

Nevertheless, it is true that a large number of people feel that they should revise or supplement Darwin's thinking about mechanisms with various alternatives and additions, while at the same time they resist the enormous commitment to organic adaptedness which so pervades the *Origin*. And when it comes to humankind and today's extensions of Darwinism, there is a positive philosophical stampede to other positions. What I look upon as thrilling moves forward, others regard as pernicious collapses into darkness and confusion.

The obvious reason for all of this, a reason which has tempted me more than once, is that I am right and others are wrong. An even more satisfying reason is that, when it comes to our own species, I have the courage of my convictions and others do not. But I am coming to see that matters are more complex and interesting than this. What separates the ultra-Darwinian like myself from the critics and doubters and revisers and extenders is less a simple question of fact and argument, and more one of general perspective. As I have said: at base, I think Darwin was right Others do not.

This all leads one to suspect, subject to qualifications and reservations, that we have here what Thomas Kuhn in his *Structure of Scientific Revolutions* described as a 'paradigm' difference: a gap between different world pictures. This, at least, without wanting to impose an artificial sense of unity, is the theme I hope to illustrate in this collection. I want to show you just why Darwinism, even (especially) extended to humans, just 'feels right' to me. At the same time, I hope I shall avoid being mushy and mystical. Evolution through natural selection must succeed on its own merits, and not through ill-defined yearnings for meaning.

My aim is primarily positive rather than negative. With some few exceptions, I am much more interested in defining and expanding my vision of Darwinism than in criticizing others. Basically, what I hope to show is how one thinker, over the past several years, has taken the legacy of the *Origin* and tried to understand himself and the world around him. For me, certainly, Charles Darwin's achievements have had the two essential characteristics of paradigmhood.

I am happy to acknowledge that over the years I have received much help and advice from many historians, philosophers, biologists, and most recently (especially through the Institute on Religion in an Age of Science) theologians. Closer to home, in preparing this collection I have been helped by my research assistant, Constance Matthews-Cull and my secretarial

assistant, Gail McGinnis. I have left unchanged previously published essays; they must stand on their own, warts and all. However I have imposed a uniform style and collected all references into one joint bibliography.

Part I

HISTORICAL THEMES

These first three essays look toward the past, but, perhaps uncomfortably so for most of today's historians of science, they were written with at least one and a half eyes on our thinking today. The first on Darwin and the philosophers, the oldest in the collection, explores the structure of Darwin's thinking in the *Origin* and some of the influences on this thought. I was certainly not the first to pick up on this aspect of Darwin's theory—pioneering work was done previously by Michael Ghiselin (1969) and David Hull (1973a)—but I would like to think I carried debate forward and helped provide a foundation for what is now a cottage industry.

In a funny way, however, I now see the real strength of the paper in something for which it was not primarily intended. Then, my real aim was to further and support the logical empiricist philosophy of science, a viewpoint which stresses that scientific theories consist of laws of nature bound together in tight deductive (axiomatic) structures—the best exemplar of this being Newtonian mechanics. Having analysed contemporary biology from this perspective (Ruse 1973c), I wanted then to show that Darwin's work fits the pattern (see also Ruse 1975c). I still think there is much life in logical empiricism (Ruse 1981c), but for me what counts now about this paper is the way it shows that Darwin took seriously the leading methodologists of his day. Even though Darwin has a rather easy, self-depreciating style, you should not think his ideas rest simply on the surface. The theory of the *Origin* is a very subtle piece of work. Darwin's sheer professionalism, reflected in his theory, is what counts.

The second essay of the section backs up this point. I certainly would not pretend that every idea we hold dear today was back there in 1859, the year in which the *Origin* was published. It seems to me that Darwin was hopelessly confused about heredity, and no amount of special pleading will prove otherwise. Yet, despite his notorious inability to think mathematically, Darwin's thought was often sufficiently sophisticated to bear re-examination for insights on problems which plague us today. One such worry is that focusing on the level at which natural selection is supposed to operate. Crucial to modern Darwinism, both in its application generally (especially over long periods of time) and in its application specifically to humankind, is the belief that selection works almost exclusively on the individual. There is no place for selection of collections of organisms, whether this be through so-called 'group selection' or through so-called 'species selection'. (See Brandon and Burian 1984 for details.) As we shall see later, this stance has major implications for our thinking about social behaviour. And as we see here, although about twenty years ago evolutionists with great fanfare discovered the merits of individual selection, Darwin was before them. He had already thrashed out the pertinent issues with natural selection's co-discoverer Alfred Russel Wallace, and had taken a firm individualistic stance for the very reasons which guide today's thinkers.

Finally, in the one essay in the whole collection not directly on biology, I consider the nature of the recent revolution in geology. I include this essay for three reasons. First, because historically Darwin started in science as a geologist, and moreover was hoping to find *the* overarching causal theory, as he was later to succeed in doing in biology. The arrival and acceptance of plate tectonics was the successful culmination of Darwin's own programme (although he himself thought of continents more in terms of their moving up and down than sideways). Second, because, as I argue, the influences on today's geologists are precisely those which were on Darwin, and they succeeded for the same sorts of methodological reasons as he. Third—and this starts to point us towards themes to be considered in the next section—because in the essay I explore elements of Kuhn's thinking, particularly about the nature of paradigms. This prepares ground for thoughts which I have about the paradigmatic nature of Darwinism and its rivals.

Parenthetically, let me note that when I wrote this third essay about ten years ago, there was almost no philosophical analysis of the geological revolution. That struck me as a scandal then and still so strikes me. However, I must note that, apart from the symposium in which this essay first appeared, seminal studies of the episode have been produced by Henry Frankel. (See especially Frankel 1979.)

Chapter One
DARWIN'S DEBT TO PHILOSOPHY

Charles Darwin went up to Cambridge as an undergraduate in 1828.[1] He set off on his voyage around the world on the *Beagle* in 1831, returning in 1836. About the time of his return he became an evolutionist, and he hit upon the evolutionary mechanism for which he is most famous, natural selection brought on by the struggle for existence, in the autumn of 1838. In 1842 he wrote a short sketch of his theory, and in 1844 he expanded this into a fairly substantial essay (Darwin and Wallace 1958). At the urging of his friends, in 1856 he started to prepare for publication a massive evolutionary work incorporating his basic ideas (Stauffer 1959, Darwin 1975). This work was interrupted by the arrival of A.R.Wallace's essay on evolution, one in which he mirrored Darwin's ideas in an uncanny fashion, in 1858. Thereupon, Darwin dropped all else, wrote an 'abstract' of his evolutionary ideas, and this was published as the *Origin of Species* in 1859.

In this paper I argue that an important factor in Charles Darwin's development of his theory of evolution through natural selection was the philosophy of science in England in the 1830s. When this factor is recognized, then new light is thrown both upon Darwin's discovery of his evolutionary mechanism and upon the way in which he prepared his theory for public presentation.

PHILOSOPHY OF SCIENCE, 1830–40

England's most influential philosopher of science in the 1830s was the famed astronomer, John F.W.Herschel, whose philosophical reputation rested upon the deservedly popular *Preliminary Discourse on the Study of Natural Philosophy* (1831)[2]. Not surprisingly, for Herschel the paradigmatic sciences were the physical sciences, particularly Newtonian astronomy (of the 1830s), and the claims Herschel made about the way science is, or ought to be, reflect this bias. Consequently, many of Herschel's major claims have a curiously familiar ring to today's reader, for in important respects he anticipated the modern philosophical school which also looks to physics for its ideals, so-called 'logical empiricism'. I shall now sketch those tenets of Herschel's philosophy which might have been of interest to a budding scientist; that is, I shall ignore Herschel's metaphysical speculations on the ultimate nature of science and concentrate exclusively on his methodology. I shall consider what, in Herschel's opinion, was the kind of theory a scientist ought to aim for and the kind of evidence a scientist ought to offer. I shall not at present consider any methodological directives that Herschel thought peculiarly applicable to the biologist, although I disclose no secrets by admitting that Herschel was not sympathetic towards evolutionary theories.

Essentially Herschel saw scientific theories as hypothetico-deductive systems. Thus he wrote that

> the whole of natural philosophy consists entirely of a series of inductive generalizations…carried up to universal laws, or axioms, which comprehend in their statements every subordinate degree of generality, and of a corresponding series of inverted reasoning from generals to particulars, by which these axioms are traced back into their remotest consequences, and all particular propositions deduced from them. (Herschel 1931, p. 104)[3]

Moreover, Herschel made clear that what distinguishes scientific axiom systems from other such systems is that the former, unlike the latter, contain laws; these are universal, empirical statements 'of what will happen in certain general contingencies' (p. 98). What elevates a law above a mere catalogue of empirical facts is that in some sense it expresses the way things must be, that is, to use modern terminology, it allows for 'counterfactuals': if *A* were to occur (even if it does not), then *B* would follow. 'Every law is a provision for cases which *may* occur, and has relation to an infinite number of cases that never have occurred, and never will' (p. 36).

Herschel distinguished upper level laws, 'fundamental laws,' from lower level (derived) laws, or 'empirical laws' (1831, pp. 178, 200). Newton's laws of motion and gravitation are the highest of all fundamental laws, Kepler's laws are prime examples of empirical laws (p. 178). It goes almost without saying that although empirical laws have an indispensible role in science, the ultimate aim of the scientist is fundamental laws, and there are strong hints in Herschel of the distinction modern logical empiricists draw between observable and unobservable concepts (reference to the latter occurring in the axioms of a scientific system and reference to the former occurring in the lower-level derived laws of the system). Thus Herschel wrote that 'the agents employed by nature to act on material structures are invisible, and only to be traced by the effects they produce' (p. 193). Herschel argued also that the best kind of fundamental or higher law is *quantitative;* for instance, the law of gravitation, 'the most universal truth at which human reason has yet arrived' (p. 123), gives exact ratios for gravitational attractions.

One point which Herschel emphasized at length is the need of the scientist to make reference in his fundamental laws to (and thus to explain through) *causes*. In particular, the scientist should aim at explaining through *verae causae,* where these are causes 'competent, under different modifications, to the production of a great multitude of effects, besides those which originally led to a knowledge of them' (1831, p. 144). In other words, the scientist must aim to get away from *ad hoc* putative causes, proposed just to explain one set of phenomena; he must try to relate phenomena of different kinds and to explain them through one embracing all-sufficient cause or mechanism. Only then can the scientist be reasonably certain that he has 'causes recognized as having a real existence in nature, and not being mere hypotheses or figments of the mind' (p. 144). Needless to say, at the top of *verae causae* is force; indeed, Herschel speculated whether all causes might not reduce ultimately to some kind of force (p. 88).

Finally, what should be mentioned is a point Herschel made so frequently about the confirmation of theories that it might well be regarded as the *leitmotif* of his book, namely that the mark of a truly confirmed theory, one which absolutely has to be taken as true and resting on a *vera causa,* is that the theory be found to explain phenomena in ways unanticipated when the theory was first devised or to explain phenomena which seemed hostile to the theory when first devised.

> The surest and best characteristic of a well-founded and extensive induction, however, is when verifications of it spring up, as it were, spontaneously, into notice, from quarters where they might be least expected, or even among instances of that very kind which were at first considered hostile to them. Evidence of this kind is irresistible, and compels assent with a weight which scarcely any other possesses. (Herschel 1831, p. 170; see also pp. 29–34, 97–8)

The other important philosopher of science in the period being considered was Herschel's close friend, William Whewell.[4] Herschel and Whewell came to differ quite considerably over what I have called the 'metaphysical' aspects of science, Herschel inclining more to empiricism whereas Whewell was much influenced by Kant. However, they differed little, if at all, with respect to 'methodological' questions, the kind of theory a scientist should aim to produce and the way he should try to confirm it. This is perhaps not surprising because, I think, Herschel and Whewell worked out their philosophies far more in conjunction than independently, and (the Cambridge-educated) Whewell agreed fully with (the Cambridge-educated) Herschel that the finest of all sciences is Newtonian mechanics, particularly Newtonian astronomy. Indeed, in an address to the British Association in 1833 Whewell spoke of Newtonian astronomy as being the 'queen of the sciences',[5] and in his *History of the Inductive Sciences* he wrote that

> Newton's theory is the circle of generalization which includes all the others; the highest point of the inductive ascent; the catastrophe of the philosophic drama to which Plato had prologized; the point to which men's minds had been journeying for two thousand years. (Whewell 1837, 2, p. 183)

Whewell's major work on the philosophy of science, *The Philosophy of the Inductive Sciences*, did not appear until 1840; but in various writings in the 1830s he managed to show his support of many of the important tenets of Herschel's philosophy. Thus, for instance, Whewell wrote an enthusiastic review of Herschel's *Discourse* in the *Quarterly Review* for April 1831. He adopted and emphasized Herschel's point about the best kind of laws being quantitative laws. Then in 1833, in his book on natural theology, Whewell agreed not only that the aim of science is to find laws, 'rules describing the mode in which things *do* act; [things] invariably obeyed' (Whewell 1833b), but he advocated, explicitly, the hypothetico-deductive ideal for science (p. 325). And then in his *History*, Whewell followed Herschel in distinguishing between two kinds of laws, speaking of 'formal' or 'phenomenal' laws and 'physical' or 'causal' laws, the models for this division being, once again, Kepler and Newton (Whewell 1837, books 5 and 7).

Finally, there is the question of confirmation. In his *History* Whewell was at great pains to show that the strength of great theories, particularly Newtonian mechanics, is the ability to explain in many different areas, including those unthought of before the theory was discovered.[6] As is well known, in his *Philosophy* Whewell labelled this process the 'consilience of inductions', and, like Herschel, made much of the element of surprise: 'the evidence in favour of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis' (Whewell 1840, 2, p. 230). Hence, both with respect to theory-nature and with respect to theory-proof Herschel and Whewell spoke with almost one voice.[7]

DARWIN AND THE PHILOSOPHERS

That Darwin was aware of and responded positively to this philosophy of science is undeniable. Take the influence of Herschel. Darwin first read Herschel's *Discourse* early in 1831; he reacted enthusiastically to it at the time, urging his cousin to 'read it directly',[8] and, late in life looking back over his career, he spoke of Herschel's work in the highest possible terms.

> During my last year at Cambridge I read with care and profound interest Humboldt's Personal Narrative. This work and Sir J.Herschel's *Introduction to the Study of Natural Philosophy* stirred up in me a burning zeal to add even the most humble contribution to the noble structure of Natural Science. No one or a dozen other books influenced me nearly so much as these two. (Darwin 1969, pp. 67–8)

Darwin reread the *Discourse* late in 1838,[9] by which time he knew Herschel personally. Their social circles overlapped and, more interesting, they both appear to have been active members of the (London) Geological Society.[10] Darwin wrote of Herschel that 'He never talked much, but every word which he uttered was worth listening to' (1969, p. 107). I shall show later that Darwin always thought highly of Herschel and craved his praise.

Darwin's relationship with Whewell is most interesting. Whewell was a violent anti-evolutionist, and I suspect that in later life neither he nor Darwin was over-keen to emphasize their earlier intimacy. But such intimacy there certainly was. Whilst an undergraduate Darwin knew Whewell well: for his full three years at Cambridge Darwin attended the lectures on botany by the Revd. J.S.Henslow, as also did Whewell.[11] Whewell and Darwin met also at Henslow's weekly scientific evenings, Darwin walking home with Whewell. About Whewell Darwin wrote that 'Next to Sir J.Mackintosh he was the best converser on grave subjects to whom I ever listened' (1969, p. 66). It goes without saying that, given the context, these 'grave subjects' would have included much about science: no doubt in 1831 the enthusiastic Darwin and the equally enthusiastic Whewell talked about Herschel's *Discourse*.

After his return from the *Beagle* voyage Darwin lived (early in 1837) in Cambridge for three months, but his most important contact with Whewell was through the Geological Society. Whewell was president in 1837 and 1838 whilst Darwin was on the council, and this led to fortnightly meetings.[12] Whewell seems to have pushed Darwin's scientific career strongly: he urged him to get on with the publishing of the results of the *Beagle* voyage, he pressed him into accepting a secretaryship of the Society;[13] in his second presidential address to the Society, he heaped the highest possible praise on Darwin (and hinted, incidentally, that he felt some credit due to himself as one of Darwin's teachers). In letters to Whewell, Darwin thanked him for having 'shown so much interest and kindness in all my affairs' and for 'the manner of your whole intercourse with me, since my return to England'.[14]

I think Whewell's major influence on Darwin would have been through conversation, but Darwin did read several things by Whewell. These include Whewell's address to the British Association (Whewell sent Darwin a copy[15]), the *Bridgewater Treatise* (Darwin read this twice, in early 1838 and in 1840[16]), and the *History*. Darwin owned a copy of this last-named work; he skimmed it at some point in 1838, probably in early October, and then, just after his rereading of Herschel, read it very carefully, annotating it fully.[17] He liked the work, praising it to Whewell and to others.[18] Moreover, Darwin who was notoriously so careless of his books, had the volumes leather-bound. I doubt if Darwin ever read Whewell's *Philosophy*, but he did respond with great interest to a large detailed review of Whewell by Herschel. '—From Herschel's Review Quart. June 41 I see I MUST STUDY Whewell on Philosophy of Science.'[19]

Darwin was therefore fully aware of the Herschel-Whewell philosophy of science, and all the direct evidence points to an enthusiastic reaction. Moreover, the genuineness of this reaction is supported, both by comments which Darwin made about scientific methodology and by the scientific works which he produced. We have seen that central to the philosophy was the taking of Newtonian astronomy as the paradigm for science. Many comments made by Darwin show that he accepted this claim entirely, and that, indeed, his aim was to be the Newton of biology. Thus, for example, he wrote as follows in a private notebook in 1837.

> Astronomers might formerly have said that God ordered each planet to move in its particular destiny. In same manner God orders each animal created with certain form in certain country, but how much more simple and sublime power let attraction act according to certain law, such are inevitable consequences—let animal be created, then by the fixed laws of generation, such will be their successors. Let the powers of transportal be such, and so will be the forms of one country to another.—Let geological changes go at such a rate, so will be the number and distribution of the species!! (Darwin, B, pp. 101–2)

And when he was presenting his theory again and again Darwin defended himself against possible criticisms on the grounds that he was being more Newtonian than any would-be critics. Thus, in his first full-length exposition of his theory (the *Essay* of 1844), Darwin asked 'shall we then say that a pair, or a gravid female, of each of these three species of rhinoceros, were separately created…? For my own part I could no more admit [this] proposition than I could admit that the planets move in their courses, and that a stone falls to the ground, not through the intervention of the secondary and appointed law of gravity, but from the direct volition of the Creator' (Darwin and Wallace 1958, pp. 250–1).

Were one to single out from the Herschel-Whewell philosophy the two features most likely to be manifested in any scientific theory consciously influenced by the philosophy, they would probably be: first, the hypothetico-deductive model, and secondly the use of one central mechanism or cause to explain phenomena in widely different areas. Both of these features are manifested, to a significant extent, in Darwin's theory in the *Origin*, and they can be traced back to Darwin's earlier versions of his theory, the *Sketch* of 1842 and the *Essay* of 1844.[20] Furthermore, these were features Darwin intended his theory to have and he took pride in the fact that (as he thought) his theory did have them.

Take first the hypothetico-deductive ideal. Darwin's following of this is particularly apparent in what one might call the 'core' arguments of his theory. Darwin's major mechanism of evolutionary change, natural selection, is something which embodies the notion that in each generation there is a differential reproduction of organisms, more organisms being born than can survive and reproduce, and the notion that the survival of the successful organisms is in part a function of characteristics which they, unlike unsuccessful organisms, possess. Darwin did not just drop natural selection into his theory, unannounced. Rather, he argued first to a struggle for existence and then to natural selection, and these arguments to the struggle and then to natural selection approximate closely to the hypothetico-deductive ideal (Ruse 1971). Thus Darwin started his arguments with statements which seem very much like laws (understood in the Herschelian sense), for instance, that given any species of organisms they will be found to have a tendency to increase their numbers at a geometrically high rate. And this, he tried to show, is something which *must* hold for any species you like to name, even the most slow breeding of species. Then, from lawlike statements like these, Darwin tried to show that his conclusions, first about a struggle and then about selection, *must* follow. And, of course, this is what deduction is all about.

Even more obvious than Darwin's attempt to satisfy the hypothetico-deductive ideal was his attempt to use his mechanism of evolutionary change, natural selection, to explain phenomena in many widely different areas. Thus Darwin showed how natural selection solves problems of geographical distribution, of instinct, of geology, of classification, of comparative anatomy, of embryology, and so on. All of these various areas come under the umbrella of selection just as so many areas of physical enquiry come under the umbrella of Newtonian gravitational force. And, as I have mentioned, Darwin intended and took credit for having shown both this fact and the former fact, namely that he had manifested the hypothetico-deductive ideal. He wrote constantly of showing how things, first like the struggle and then like the phenomena of geographical distribution, follow 'inevitably' from laws. (See Darwin 1859, pp. 80–1, 489–90.) And whenever challenged about the truth of this theory Darwin pointed always to the wide scope of his mechanism: 'I must freely confess, the difficulties and objections are terrific; but I cannot believe that a false theory would explain, as it seems to me it does explain, so many classes of facts' (Darwin and Seward 1903, 1, p. 455).

It cannot be denied, as critics were quick to point out, that Darwin was not entirely successful at achieving the Herschel-Whewell theory ideal. (See Hopkins 1860.) In particular, many of the inferences in Darwin's theory taken as a whole were far from being rigorously deductive. However, this is not to deny Darwin's intentions, and one's estimation of the success he actually achieved becomes much increased when one compares Darwin's theory against the works in the 1830s of other non-physical scientists. Thus, although Lyell's (1830–3) chief aim was to show that the past world can be explained by laws of the present world, he never achieved even the limited hypothetico-deductive success of Darwin, preferring rather to make his points with strings of related examples. And the same goes for the work of someone like Henslow (1835), who relied on description and example rather than the axiomatic method.

In concluding this section, let me make one caveat I argue that Darwin was influenced by the Herschel-Whewell theory ideal and I have given reasons to suggest that this would have been a direct influence. I do not, however, want to suggest that this was an entirely exclusive influence. I think that pretty well everybody in the 1830s accepted this philosophy of science and that Darwin would have received it from others as well. For example, Lyell and Whewell had a continuing debate over whether one ought to be a uniformitarian or catastrophist in geology, and both the uniformitarian Lyell and the catastrophist Whewell defended their respective positions as being more Newtonian than the other's![21] And I am sure that a major reason why Darwin did not change his theory in any significant way after its first formulation was because there was no significant change in the philosophy of science (*qua* theory-nature ideal) between the writing of the *Sketch* (1842) and the writing of the *Origin* (1858–9). Even J.S.Mill, in his influential *System of Logic* (1843), managed to incorporate many of the salient features of the hypothetico-deductive approach, though he differed from Whewell at least in his estimate of the sufficiency of that method to yield a doctrine of proof. But then, as I shall show later, at this point where Mill diverged from Herschel and Whewell, Darwin sided with the earlier philosophers rather than with Mill.

But, whilst admitting this caveat about other possible influences on Darwin one must be careful not to underestimate Herschel and Whewell themselves, and certainly one must be careful not to fall into the trap of thinking that because Herschel and Whewell were anti-evolutionists they cannot have been significant influences on Darwin. Nigh-on everyone was an anti-evolutionist in the 1830s; Lyell, probably Darwin's greatest intellectual influence, was one of the leaders of the attack against evolutionary theories, and indeed, Lyell's position was practically indistinguishable from Herschel's.[22] Nor should one assume that Darwin's theory was bound to be the way it was, because every scientific theory was that way. As I have just pointed out, Darwin hardly got the salient aspects of the Herschel-Whewell philosophy from the work of men like Lyell and Henslow, because these aspects were absent from their work. The direct influence of Herschel and Whewell, although not exclusive, should not be discounted; in any case, many of Darwin's other influences like Lyell and Henslow probably got their philosophy of science from Herschel and Whewell in the first place.

NEW LIGHT ON DARWIN

I shall argue now that recognizing the importance for Darwin of the Herschel-Whewell philosophy of science enables us to solve several puzzles in the Darwinian story. I take first the question of Darwin's discovery of his theory, and in particular the role played in this discovery by the thought of Malthus. As mentioned earlier, we know that Darwin came upon, or recognized, his main mechanism of evolutionary change some time in the autumn of 1838. His discovery seems to have been a two-part process; he grasped the principle of natural selection by analogy from breeders' use of artificial selection on domestic organisms, and then, after reading the *Principle of Population* by T.R.Malthus, he saw in some way how he could use the struggle for existence as a kind of driving force behind natural selection. Thus, to Wallace, Darwin wrote: 'I came to the conclusion that selection was the principle of change from the study of domesticated productions; and then, reading Malthus, I saw at once how to apply this principle' (Darwin and Seward 1903, 1:118).

Recent Darwin scholarship has shown that Darwin's route to discovery was less direct than he himself implied. (See Herbert 1971, Limoges 1970.) For a start, before the reading of Malthus (about 28 September 1838) most of the comments Darwin made show that he, like everyone else at the time, looked on the domestic world as pointing *away* from a mechanism of evolutionary change, rather than towards it. For instance, one comment Darwin made shortly before reading Malthus was: 'It certainly appears in domesticated animals that the amount of variation is soon reached —as in pidgeons no new races' (notebook D, p. 104, written 13 September 1838). However, despite comments like these, it does now seem that Darwin was definitely led to the mechanism of natural selection from the analogy with artificial selection. In particular, Darwin got the concept of natural selection in mid-1838 from reading animal breeders' pamphlets, which pamphlets talked not only about artificial selection but also about natural selection (not by that name); and explicitly drew an analogy between the two kinds of selection.[23] Nevertheless, a puzzle about Malthus still remains. Why was it necessary for Darwin to read Malthus before he recognized that in natural selection he had a mechanism of evolutionary change? Before reading him Darwin gave no hint that he differed from the breeder's assessment of natural selection, namely that it was something which would cause only limited change *within* a species. It cannot be just that Malthus drew Darwin's attention to the struggle for existence, because Darwin knew all about the struggle long before reading him. The struggle is described explicitly and in detail in Lyell's *Principles of Geology*, two editions of which Darwin had read by mid-1837. Indeed, Lyell even talks of the struggle for existence *by that name*.[24]

Understanding the importance for Darwin of the Herschel-Whewell philosophy, Malthus' contribution to Darwin's discovery becomes readily explicable. Malthus showed Darwin how he could locate the struggle, with the consequent selection, in a hypothetico-deductively organized network of laws; of laws which were, moreover, *quantitative:* in Herschel's and Whewell's eyes the best kind of laws. Malthus argued that a struggle for existence amongst humans would inevitably ensue, unless prevented by moral restraint (or something unmentionable like contraception), because humans have a tendency to increase in number at a *geometrical* rate whereas their food supplies can increase only at maximum at an *arithmetical* rate. Darwin seized upon this argument, generalizing to all animals, thus eliminating the alternatives to the struggle. (See Ruse 1973b.) He now had quantitative laws, leading deductively to the struggle, which he was then able to extend to selection. Thanks to Malthus, Darwin was able to put his mechanism for evolutionary change into a satisfactory context, a context, that is, which satisfied the Herschel-Whewell theory ideal.

But Malthus was important for Darwin for another, related reason. The Herschel-Whewell philosophy demanded that one explain through causes, the best kind of which, perhaps the only kind of which, were forces. Through Malthus, Darwin saw the struggle as being a kind of force, which would in turn, as it were, propel the force of selection. As soon as he read Malthus the excited Darwin scribbled in his notebook that

> Population is increased at geometrical ratio in FAR SHORTER time than 25 years—yet until the one sentence of Malthus no one clearly perceived the great check amongst men.—there is spring, like food used for other purposes as wheat for making brandy.—Even a *few* years plenty, makes population in man increase and an *ordinary* crop causes a dearth. take Europe on an average every species must have same number killed year with year by hawks, by cold etc.—even one species of hawk decreasing in number must affect instantaneously all the rest—The final cause of all this wedging, must be to sort out proper structure, and adapt it to changes.—to do that for form, which Malthus shows is the

> final effect (by means however of volition) of this populousness on the energy of man. One may say there is a force like a hundred thousand wedges trying [to] force every kind of adapted structure into the gaps in the oeconomy of nature—or rather forming gaps by thrusting out weaker ones. (Darwin, D, p. 135)

As this passage shows, Malthus enabled Darwin to see the struggle and the consequent selection in terms of force. Hence Darwin, working in the light of the Herschel-Whewell philosophy, felt able to regard selection as a possible evolutionary mechanism.

If, as I argue, the Herschel-Whewell philosophy was an important factor in Darwin's response to Malthus, one might naturally ask if the philosophy played any role in Wallace's discovery of natural selection, because he like Darwin acknowledged an important debt to Malthus (Wallace 1905, 1, pp. 361–2). Although Wallace certainly read Whewell's *History* (McKinney 1972, p. 24), I suspect the real key to Wallace's response lies in Robert Chambers' *Vestiges of the Natural History of Creation*. McKinney argues that 'the influence of the *Vestiges* [on Wallace] …can scarcely be overemphasized' (1972, p. 12). But a major aim of *Vestiges* is to show that as good Newtonians we must accept a biological evolutionary theory. Wallace, I think, whilst rejecting as inadequate Chambers' own evolutionary theory, entirely accepted Chambers' research programme, to find the biological analogue of Newtonian astronomy.[25] Thus I would suggest that Wallace, like Darwin, may have reacted favourably to Malthus' ideas because he could then start to see his way towards a biological equivalent of Newtonian astronomy. Hence I think that Darwin and Wallace quite possibly started from similar philosophical positions, although I have no reason to believe that they drew on exactly the same immediate sources for the philosophies. Indeed, I doubt that their sources were exactly the same, for, as I shall show next, Darwin took an altogether different methodological step from Wallace because, I think, he wanted to present a theory which would satisfy Herschel's criteria of theory-excellence.

As 1838 drew to a close, Darwin had his major mechanism of evolutionary change. He had now to start to think about converting his mechanism into a full-blown theory, one which he would present to the world. An understanding of the Herschel-

Whewell influence remains crucial to the grasping of Darwin's reasonings, particularly the way in which he used the analogy from artificial selection.

Darwin knew well that any theory of evolution was going to be highly controversial, to say the least. That meant he had to make the best possible case, particularly the best possible case in the eyes of the ultimate arbiters of scientific acceptability, Herschel and Whewell. He felt he had to satisfy their criteria of good science. Indeed, interestingly, Darwin always felt this way. By 1859, the year of publication of the *Origin,* the long-invalid Darwin moved in different circles from the philosophers, Whewell particularly. Nevertheless, Darwin sent copies of the *Origin* to both Herschel and Whewell, and he prefaced the *Origin* with a quotation by Whewell to the effect that the world works exclusively according to law (as if to point out that he, Darwin, was merely following Whewell's prescriptions),[26] and, most significantly, waited with interest and trepidation for Herschel's evaluation of his theory. When the great man was reputed as having characterized the *Origin* as 'the law of higgledy-piggledy,' Darwin spoke of Herschel's evaluation as 'a great blow and discouragement' (Darwin 1887, 2, p. 241). (Actually, as we shall see, Herschel's verdict was not entirely negative.)

In December of 1838, Darwin turned seriously to the question of how best he ought to develop and present his theory. To this end, he reread Herschel's *Discourse* and went very carefully through Whewell's *History*. Gauging his interest in the latter work from the extent to which he annotated and marked the various sections of his own copy, Whewell's volumes were of particular interest for two reasons. On the one hand, Darwin wanted to see what were the precise merits of a theory like Newton's, what made it so exceptional a theory. On the other hand, he wanted to see what was the strongest possible case that could be made by an anti-evolutionist: Darwin wanted to leave no possible criticism unconsidered. Thus, when Whewell claimed that every evolutionist would be saddled with Lamarckian assumptions about necessary progressive evolutionary tendencies and constant creation of new sparks of life ('monads'), Darwin exclaimed in the margin that 'These are not assumptions, but consequences of my theory, and not all are necessary' (Whewell, 1837, 3, p. 579).

Now, Whewell's major criticism of the evolutionist, one which was to be found in both Cuvier and Lyell, was that present evidence, particularly that of animal and plant breeders, pointed away from rather than towards the creation of new species. Hence, argued Whewell, new species cannot have been created naturally in the past. Darwin realized that if he were to make his case he had to counter this criticism, and as is well known, the way in which he tried to do this was by arguing that Whewell and others were wrong to cite modern breeding techniques and results as evidence *against* evolution. Darwin argued that in fact such techniques and results were evidence *for* evolution (Ruse 1973b). But why did Darwin employ this strategy? We saw that earlier, in 1838, Darwin himself seems to have agreed that the domestic world points away from rather than towards evolution. Why did Darwin not employ the kind of strategy employed by Wallace in his 1858 evolutionary essay, and argue that since the domestic and the wild worlds are so drastically different, one cannot possibly draw any analogies between the two, and that hence the failure to produce new permanent forms in the domestic world does not prove that no such forms can be produced in the wild world?

Part of the reason why Darwin adopted the particular argument that he did stems, no doubt, from the fact that by the end of 1838 he was beginning to doubt the conventional wisdom on animal and plant breeding; he was starting to get evidence that artificially induced changes could be fairly permanent. But this was not the main reason why he suddenly became so keen to *stress* the analogy between domestic and natural selection;[27] why he suddenly swung round completely from his earlier position and wrote in a notebook 'It is a beautiful part of my theory, that domesticated races of organics are made by precisely same means as species—but latter far more perfectly and infinitely slower' (notebook E, p. 71).[28] The answer to Darwin's switch lies in the doctrine of *verae causae,* a doctrine, as we have seen, that was absolutely central to a Herschellian philosophy of science.

Herschel argued that one must aim to base one's reasonings on *verae causae*, and Darwin was desperately keen to show that his evolutionary reasonings were based on a *vera causa,* natural selection. But how was Darwin to show beyond doubt that natural selection was a *vera causa?* Here, Herschel's discussion becomes of vital interest: the most convincing evidence that something is a *vera causa,* Herschel argued, occurs when we can argue