

The British Society for the History of Science

---

Review: The Structure and Strategy of Darwin's 'Long Argument'

Author(s): M. J. S. Hodge

Review by: M. J. S. Hodge

Source: *The British Journal for the History of Science*, Vol. 10, No. 3 (Nov., 1977), pp. 237-246

Published by: Cambridge University Press on behalf of The British Society for the History of Science

Stable URL: <http://www.jstor.org/stable/4025843>

Accessed: 19-04-2016 16:31 UTC

---

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at

<http://about.jstor.org/terms>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).



Cambridge University Press, The British Society for the History of Science are collaborating with JSTOR to digitize, preserve and extend access to *The British Journal for the History of Science*

## ESSAY REVIEWS

### ◀ THE STRUCTURE AND STRATEGY OF DARWIN'S 'LONG ARGUMENT' ▶

**Charles Darwin's Natural Selection, Being the Second Part of his Big Species Book Written from 1856 to 1858.** Edited from manuscript by R. C. Stauffer. London and New York: Cambridge University Press, 1975. Pp. xii + 692. £20.00

As Robert Stauffer explains at the very beginning, *On the origin of species* was largely written as an abstract of what Darwin called his 'big book'. This treatise was well over half finished and running to more than a quarter of a million words when Wallace's famous letter precipitated the Linnaean Society presentation of the theory of natural selection. But although Darwin had the *Origin* ready in only nine months after that meeting he never went on with the parent work. He did not abandon the manuscript altogether for he later developed the first two of its ten and a half chapters into *The variation of animals and plants under domestication* (2 vols., London, 1868), and allowed portions of the tenth chapter, on instinct, to be included in a book by George Romanes, *Mental evolution in animals* (London, 1883), that appeared shortly after Darwin's death. What is edited and printed here for the first time are all of those ten and a half chapters except for the first two (apparently no longer extant) on variation under domestication.

Having had the author's own long abstract for over a century, we expect no great surprises or revelations in the present text. And so we find it. As Dr Stauffer says himself, what we now have that we formerly lacked is mainly more abundant and detailed examples supporting and illustrating the arguments of the *Origin*, together with the comprehensive citation of sources that is so impressive in *Variation*.

Anyone who has worked with the manuscript can confirm what is manifest from this exemplary edition of it: the task of transcribing Darwin's messy text and tracking down his myriad references must have been dreadful to contemplate in advance and exasperating to execute once undertaken. Wallace put us all in his debt by moving Darwin to produce a reasonably readable exposition of his theory; Dr Stauffer has put us no less in his by giving us this careful, judicious, and informative edition of Darwin's definitive account of the origin of species by means of natural selection.

Introducing Darwin's own words are two chapters by Dr. Stauffer that explain fully how the 'big book' was written, starting in 1856, and the editorial decisions taken in preparing this printing of it. In addition, each chapter is preceded by a discussion of its particular place in Darwin's schedule of work, especially as shown by his correspondence and diary entries from that time. Then at the end there are printed as appendices various 'fragments of the manuscript, letters and related materials' that supplement the main text. Finally, to follow the 'extensive bibliography, Dr Sydney Smith and three collaborators have prepared a valuable collation of the *Origin* and *Natural selection* and a truly excellent index to the big book. This collation and index will, needless to say, provide great help for the many detailed exegeses of particular passages that we can expect Darwinian scholars to present in the years to come.

However, *Natural selection*—this book seems destined to be known as that—helps us not only in understanding particular passages in the *Origin*, but also in seeing how that sometimes seemingly formless work is organized as a whole. For pretty certainly we now have in print almost all the surviving pages of every

version Darwin ever wrote of the *Origin*, so that if we are to improve our grasp of the strategy and structure of its argumentation it must be largely by comparing the recently published 'outline and draft of 1839', the *Sketch* of 1842, the *Essay* of 1844, *Natural selection* (1856–1858) and the six editions of the *Origin* itself, starting with the first in 1859 and ending with the sixth of 1872.<sup>1</sup>

Stauffer has identified *Natural selection* as the 'second part' of Darwin's big treatise, and he explains that it is the second part of a three-part work. With a couple of qualifications this identification is correct as far as it goes; the two qualifications, to be developed further below, are that the first of the eight and a half chapters printed here is really from the first part of the work; while the half chapter at the end belongs to the third part.

It might be thought that nothing very much depends on how Darwin divided his case for the origin of species by natural selection, whether in his own mind or in his published writing. But in fact we can learn a great deal that is of interest by trying to clarify this point as thoroughly as we can. In this task, I want to suggest, there are two things which are of decisive importance: (i) the realization that there is a sense in which the *Sketch*, and every subsequent version of it, is a two-part work, and a sense in which it is a three-part work; and (ii) the realization that Darwin's commitment to the *vera causa*—or 'true cause'—principle shows us that the bi-partite division is more fundamental than the tri-partite one.

Now before turning to the *Sketch*, *Origin*, and lastly *Natural selection* to confirm these points, it will be as well to take care right away of a pair of objections that are likely to be made. For after all the suggestion that the doctrine of *verae causae* is relevant here may seem implausible: first, because there is a historical gap of nearly two hundred years between Darwin's *Origin* and the 'rule of philosophizing' in Newton's *Principia* which was the *locus classicus* for all later discussion of 'true' or 'known' causes; and second, because we usually expect a vast gap of credibility between scientists' aphoristic professions of methodological principle and their actual performances in expository practice, let alone their private thoughts as recorded in research notebooks and diaries.

But as it turns out, both of these gaps can be closed without difficulty or dispute in the present case. We may take the historical one first. Vincent Kavaloski, in a penetrating and comprehensive study of the history of the *vera causa* principle (hereafter VCP) from Newton to Darwin, has no trouble in documenting the explicit and far from inconsequential acceptance of various versions of the principle shared by numerous scientists and philosophers (Whewell was a notable dissenter) in the two centuries after 1687.<sup>2</sup> Indeed the principle, as he shows, was eventually invoked by people on both sides of the controversy over Darwin's theory—Huxley and Sedgwick, for example. Even more instructively for our business here, Kavaloski shows that the two people Darwin most hoped to impress with the arguments of the *Origin*, John Herschel and Charles Lyell, were at one in seeing the principle as a crucial constraint and indispensable standard for theories not only in physics but also in geology and biology. In offering the principle as a main clue to the argumentative ideals, and so also to the organization of the *Origin*, I am going beyond Kavaloski's own account of the VCP's history, but in ways that are only possible after his pioneering study.

Appropriately enough, one of the best introductions to the VCP is provided by Herschel's discussion of it when comparing Lyell's theory of geological climate change with his own. But since Herschel begins with a nod towards Newton, we too had better have the *Principia* before us. In the original edition, the four rules of philosophizing called in the second and third editions 'Regulae

philosophandi' were actually included in a list of nine 'Hypotheses'.<sup>3</sup> But in the case of the first rule, it was only this denomination and some of the supporting comments which changed, not the crucial opening sentence expressing the rule itself:

Hyp. I Causas rerum naturalium non plures admitti debere, quam quae et vera sunt et earum Phenomenis explicandis sufficient.

Hyp. I We ought to admit no more causes of natural things, than such as are both true and sufficient to explain their appearances.<sup>4</sup>

With Reid and other eighteenth-century commentators it became customary to read this rule as laying down, in Reid's words, 'two conditions' to be met by explanatory causes: first they must be 'true', that is, *known* to have a 'real' existence and not to be conjectured so as to have a merely 'hypothetical' existence; second they must be adequate, must suffice to 'produce the effect'.<sup>5</sup> To avoid confusion, then, we will take a *vera causa*, a true cause, a real cause, a known cause, to be one meeting the first condition, and a VCP cause to be one meeting both conditions.

As a rough approximation, then, we may take the whole rule or principle to specify the following: in explaining any phenomenon, one should invoke only causes whose *existence* and *competence* to produce such an effect can be *known* independently of their putative *responsibility* for that phenomenon. As an elementary but not too misleading example, consider the explanatory challenge presented by a dead rabbit in the garden; the neighbour's cat or lightening bolts seen the night before would both be true and sufficient causes for this, for both are already known to exist and to be adequate for this sort of effect, so that their providing a possible explanation for this particular phenomenon would not be the sole evidence for their existence and competence to cause it. By contrast, to ascribe the rabbit's death to a burst of cosmic radiation conjectured to accompany every sunrise would be to dodge the requirement of independent evidence for the existence of explanatory causes; while to blame the neighbour's hamster would be to violate the requirement of independent evidence of competence.

Herschel's discussion brings out well the expository corollary of the separateness of these two evidential requirements of the VCP from one another, and, no less, the separateness of both from the requirement of independent evidence for causal responsibility. To anticipate, we can see in Herschel's and Lyell's upholding of the VCP the source for Darwin's taking up, in the *Sketch*, the following in turn: (i) the case for the *existence* of natural selection; (ii) the case for its *competence* to produce new species; and (iii) the case for its having been *responsible* for the production of extant and extinct species. And to anticipate further, we can see here the source for Darwin's presentation, there and thereafter, being fundamentally bipartite. For, with the existence case (i) and competence case (ii) both required to establish natural selection as a VCP cause for species production, these cases contribute a first VCP half of the whole exposition, which must come before the transition to the responsibility case (iii) that makes up the second half. Looking still further ahead, we can see one obvious potential source of confusion over Darwin's equally explicit tripartite division of his exposition. For Darwin himself sometimes distinguishes in that VCP half of his exposition two parts, but not an existence case (i) part and a competence case (ii) part. Rather he distinguishes a part I on variation under domestication from a part II on variation under nature, a division that does not coincide with that between case (i) and case (ii). Indeed, one of the keys to understanding the structure of the *Sketch* and so the *Origin* is to see how this

part I/part II division is related to, or rather unrelated to, the existence case (i)/competence case (ii) division.

To illustrate what Newton and others understood by the term *vera causa*, Herschel explained how both Lyell's and his own very different causal candidates for the cooling of the earth since the geologist's secondary eras were true causes. Lyell's—the changing distribution of land and sea relative to the poles and equator—is a real cause because that distribution is known now to be affecting climate; and Herschel's candidate—the slow diminution of the eccentricity of the earth's orbit around the sun—is similarly real. But Herschel stresses that whether either of these two causes is adequate, whether it could ever cause a change of the full amount required, is 'another consideration'. Moreover it goes without saying that which of them is more likely to have been responsible for the climate change since the secondary eras—if indeed it was either of them at all—would be yet another consideration.<sup>6</sup>

But is there any evidence from the *Sketch* and the descendant texts that Darwin really did adapt his exposition to the demands of this principle? There is; and what is more there is a postscript to a letter (22 May 1863) of Darwin's to the botanist and erstwhile logician George Bentham that shows us how to find that evidence.

P.S.—In fact the belief in Natural Selection must at present be grounded on general considerations. (1) On its being a *vera causa*, from the struggle for existence; and the certain geological fact that species do somehow change. (2) From the analogy of change under domestication by man's selection. (3) And chiefly from this view connecting under an intelligible point of view a host of facts. When we descend to details, we can prove that no one species has changed [i.e. we cannot prove of any particular species that it has changed]; nor can we prove that the supposed changes are beneficial, which is the groundwork of the theory. Nor can we explain why some species have changed and others have not.<sup>7</sup>

At first sight this sequence of three considerations may seem out of order. Surely the second should proceed the first, seeing that variation under domestication is always taken up before variation in nature? But a telling text in the *Sketch* itself shows that this query is mistaken. In a passage headed 'summing up this division', Darwin reviews the whole first half of his entire exposition in the *Sketch* as follows:

If variation be admitted to occur occasionally in some wild animals, and how can we doubt it, when we see thousands of organisms, for whatever use taken by man, do vary. If we admit such variations tend to be hereditary . . . If we admit selection is steadily at work, and who will doubt it, when he considers amount of food on an average fixed and reproductive powers act in geometrical ratio. If we admit that external conditions vary, as all geology proclaims they have done and are now doing—then . . . there must occasionally be formed races . . . differing from the parent races . . . Take *Dahlia* and potato, who will pretend in 5000 years that great changes might not be effected: perfectly adapted to conditions and then brought again into varying conditions. Think what has been done in few last years, look at pigeons, and cattle . . . And therefore with the [adapting] selecting power of nature, infinitely wise compared to those of man, I conclude that it is impossible to say we know the limit of races, which would be true to their kind; if of different constitutions would probably be infertile one with another, and . . . adapted in the most singular and admirable manner . . .—such races would be species.

The existence and competence of this cause for species are then established. But what of its responsibility? Darwin continues by assuring us that that is just what he is going to take up next, as an independent issue:

But is there any evidence that species have been thus produced, this is a question wholly independent of all previous points, and which on examination of the kingdom of nature we ought to answer one way or another.<sup>8</sup>

If we turn to the opening half of the *Sketch* that this summary recapitulates, we find a sequence of topics as follows: I(a) ability of new conditions to cause hereditary variants in domestic species (b) tendency of free crossing to swamp such variation (c) ability of artificial isolation and selection to counteract that swamping tendency; II(a) geology shows wild species often get exposed to changed conditions that would therefore cause hereditary variations (b) so these are variations that could be accumulated by any selective breeding (c) the struggle for existence entails that just such a selective breeding exists (d) this natural selection has longer to work and is more discriminating than artificial selection, so (e) if artificial selection has been able to produce races, this natural selection will have been able to produce races which are not merely races, but which meet the criteria for specific distinction. So extant and extinct species *could* have originated as races produced by natural selection.

So far we have passages I (a) to II (c) arguing for the existence of natural selection. These passages correspond, then, to consideration (1) in the letter to Bentham; for (especially *via* the struggle for existence and geological causes of hereditary variation) they establish natural selection as a *vera causa*, as a true, not a fictional, cause, analogous to artificial selection. Equally clearly, passages II(d) and II(e), arguing for the sufficiency, the competence, of this existing cause to produce species, correspond to consideration (2) in the letter to Bentham. Appropriately then, Darwin follows II(e) with passages taking care of two 'Difficulties on theory of selection', two *prima facie* objections to the case for its *competence*: (f) the difficulty of 'perfect organs' useless in their early stages, and (g) the difficulty of species differing in instincts and mental powers (there being no question from the argument and phrasing of (g) that Darwin really sees it as on the same footing as (f), although he inadvertently obscures this equivalence of the two difficulties by giving the topics in (g) a 'separate section'). And it is, of course, after (g) that we have the summing up prior to the transition to the second half of the book, the half devoted to consideration (3) in the postscript to Bentham.

Why then do we need to distinguish, in the topics prior to the 'summing of this division', between those we have numbered here I(a)—I(c) and those numbered II(a) to II(g)? We need to, simply to mark Darwin's own implicit designation of I(a)—I(c) as constituting a part I on variation under domestication, and II(a)—II(g) a part II on variation under nature. But if Darwin does not actually indicate it in his manuscript, what is the evidence for this implicit designation? As Francis Darwin records, the new beginning that follows the 'Summing up [of] this division' just quoted is headed in the manuscript 'Part III'.<sup>9</sup> And this heading, as he acknowledged, accords well with an outline plan in his father's hand, a plan possibly for the *Sketch* itself but in any case for some such exposition of the theory of natural selection, and very likely dating from about 1842:

- I The Principles of Var. in domestic organism.
- II The possible and probable application of these same principles to wild animals and consequently the possible and probable production of wild races, analogous to the domestic ones of plants and animals.
- III The reasons for and against believing that such races really have been produced, forming what are called species.<sup>10</sup>

But Francis Darwin was being reasonable enough in dividing the *Sketch* not according to this tripartite plan and the 'Part III' heading that follows the 'Summing up [of] this division', but according to the more fundamental

bipartite structure of its argumentation. For this bipartite structure was explicitly recognized by its own author—as his filial editor understood correctly—when he promised that a discussion of ‘whether the characters and relations of animated beings are such as favour the idea of wild species being races descended from a common stock’ would form ‘the second part of this sketch’.<sup>11</sup> For the reference here is clearly to the chapters (beginning with the geological ones) concerned with the responsibility case that was to follow the cases for the existence and competence of natural selection as a VCP cause of species. Equally correctly, Francis Darwin realized that this bipartite division did not replace and supersede that tripartite plan. Indeed, it was to that concerto-like plan that Darwin returned in the 1860’s, in distributing his writing efforts on behalf of natural selection into three distinct books. As he explained in introducing *Variation under domestication* (1868), he was in that work treating, ‘as fully as my materials permit, the whole subject of variation under domestication’; then in a ‘second work’ he would present variability ‘in a state of nature’, including, of course, chapters on the struggle for existence and natural selection itself; before, finally, in ‘a third work’ going on to ‘try the principle of natural selection by seeing how far it will give a fair explanation’ of ‘several classes of facts’, from geology, geography, morphology, embryology and the like.<sup>12</sup>

But it should now be clear that this tripartite scheme always represented a convenient division of Darwin’s personal labour, rather than a natural articulation in his public argument. For of course variation under domestication, no less than variation under nature, was appealed to twice over in the VCP argumentation making up the opening half of his whole exposition: first of all, in establishing that a natural selection of hereditary variation *existed* in the wild, he appealed both to the known tendency of new conditions to cause such variation indirectly in domestic species, apparently by affecting the reproductive system itself, and to the known fact that such variation can be accumulated by selective breeding; then secondly, in establishing the *competence* of natural selection to produce *species*, he pointed to the known power of artificial selection to produce distinct *varieties*, and to the reasons for thinking that the natural selective breeding entailed by the struggle for existence would have the greater effect—*species*—within its power because it is more precise and prolonged than any practised by man.

A tabular view of the structure of the *Sketch* will provide the best way to summarize the various suggestions just offered. It will also allow us to see directly that, in writing the *Essay*, *Natural selection* and the *Origin*, Darwin made successive departures from this early version of the structure, and that, though really minor and superficial, these departures were eventually enough to render the strategy and organization of his most famous book unhelpfully and quite unnecessarily obscure. (See Table I, page 243).

Moving now to the *Essay* of 1844, we can see this orderly three-part, two-division and three-case scheme beginning to be violated. For the first thing we notice is that the difficulties for the *competence* case posed by ‘perfect organs’ and by instincts have been moved, so that they now come after the principal summary of that case and not before it as in the *Sketch*; while the second thing to stand out is that Division Two now opens with a topic not recognized separately as such in the *Sketch*, namely the difficulty for the *responsibility* case posed by the lack of intermediate stages of morphological change among fossil organisms.

Moreover when we reach the *Origin* itself we find further such changes in its first edition, with the result that that book appears, quite misleadingly, to have the following three stages in its exposition: A. Four chapters developing

TABLE 1. The structure of the *Sketch* of 1842

Part I Principles of variation and selection under domestication	Topic a b c	Chapter I Variation under domestication	Consideration 1 Existence case		Division One Natural selection established as VCP cause for species
Part II Application of those principles to species under nature	a b c d e f g	Chapter II Variation in nature, and natural selection	Cons. 2 Competence case	The case made	
	Ch. III Instinct etc.	Difficulties considered			
Part III Trial of theory of natural selection as explanatory of species production		Ch. IV Geology Ch. V Geology Ch. VI Geography Ch. VII Classification Ch. VIII Morphology Ch. IX Morphology	Consideration 3 Responsibility case		Division Two Natural selection as, on balance, probably responsible for species
		Ch. X Recapitulation			

the arguments for natural selection; B. A rather miscellaneous collection of chapters devoted to sundry digressions and difficulties—i.e. Ch. V on laws of variation; Ch. VI difficulties posed by, *inter alia*, perfected organs; Ch. VII difficulties of instincts and the like, Ch. VIII difficulties with hybrids; Ch. IX difficulties with missing intermediates in fossils; and then C. Four chapters triumphantly returning to favourable facts from geology, geography and the rest. But, in fact, the two-division and three-case structure originally required by the VCP has not been replaced by anything so casual and arbitrary as this apparent three-stage sequence might suggest. For Ch. V is really a supplement to Ch. IV ('Natural selection') which makes the competence case for natural selection; while Chs. VI, VII and VIII all take up difficulties for the competence case as made in Ch. IV. By contrast Ch. IX opens what is in effect the *Origin's* Division Two, and opens it just as it was opened in the *Essay*, by taking on right away the obvious paleontological objection to this Division's responsibility case. So in outline we have a structure as in Table 2, page 244.

Returning, finally, to what we have of *Natural selection*, we can see from

TABLE 2. The structure of the *Origin* (1859)

Part I Variation and selection under domestication	Chapter I	Consideration 1 Existence case		Division One Natural selection established as VCP cause for species
Part II Variation and selection under nature	II	Consideration 2 Competence case	The case	
	III		Difficulties considered	
	IV			
	V			
	VI			
	VII			
VIII				
Part III Trial of theory of natural selection as explanatory of species production	IX	Consideration 3	Geological difficulty	Division Two
	X	Responsibility case	Evidence favouring responsibility	Natural selection as probably responsible for species production
	XI			
	XII			
	XIII			
Recapitulation	XIV			

the text itself, and even more from Darwin's own detailed table of contents for it, that it should be mapped as in Table 3, page 245.

From this table together with the other two, we can see that Chapters III and XI in the big treatise would have belonged in the first and third, respectively, of the three works Darwin hoped to complete in the 1860s and 1870s. In the case of Ch. XI this is obvious from its subject, geographical distribution; and in the case of Ch. III, we have Dr Stauffer's own report that Dr Alice Guimond found material from that chapter worked into *Variation* while helping with his edition of *Natural selection*, which accords well with the place of its topic—crossing—in the part of the *Essay* devoted to domestic variation.

It is surely unfortunate that Darwin did not stick more closely, in *Natural selection* and in the *Origin*, to the format of the *Sketch* or at least to that of the *Essay*. For even in the *Essay* there is manifest the relation of the two fundamental divisions—their chapters numbered separately as Francis Darwin emphasized—and there is no mistaking there the instructive adaptation of the whole to the VCP, and thereby to the precedent set by the structure and strategy of Lyell's *Principles of geology*. For of course Lyell's Books II (on the physical world) and III (on the organic) had explicitly sought (after Book I had put the general case for adapting the Huttonian system to the current state of the science) to establish various 'known' agencies as true and sufficient causes—the struggle for existence as such a *vera causa* for species extinctions for example. While his Book IV (later split off to form the *Elements of geology*) argued for these causes probably having been responsible for various past effects recorded in the rocks on

TABLE 3. The structure of *Natural selection* (1856-8)

Part I Domestic variation and artificial selection	[Chapter I]	[Variation under dom.]	Existence and competence cases	Division one  Natural selec- tion as VCP cause	
	[ II]	[Variation under dom.]			
	III	Crossing etc.			
	IV	Natural varia- tion			
	V	Struggle for existence			
	VI	Natural selection			
	VII	Laws of varia- tion			
	VIII	Perfect organs etc.			Difficulties for competence case
	IX	Hybrids			
	X	Instinct			
Part III The theory tested	XI	Geographical distribution	Responsibility case	Division Two  Natural selection as responsible	

the grounds that they offered at least as comprehensive and coherent explanations for these records as did the causes invoked by those geologists who denied the adequacy of any present causes acting with their current intensity to bring about certain past effects.<sup>13</sup>

But Darwin, it hardly needs remarking, had little of that love of schematic clarity and consistency which kept Lyell to the same format through no less than twelve editions of the *Principles* and more than forty years of revisions, some of them substantial—even radical. Nor was this lack a failing or a handicap—not at the time anyway, for the cognoscenti would have had little trouble recognizing the basic organization of the *Origin*, even obscured as it was by Darwin’s mingling of arguments and shuffling of topics that he had once had explicitly, though privately, arranged in their natural order. It does, however, make it easy for his readers today to miss the wood for the trees. Now, with *Natural selection* apparently completing the publication of all the versions of the book still extant, we must take full advantage of Darwin’s monumental researches and Dr Stauffer’s meticulous scholarship in seeking to improve our mapping of that wood even as we discern ever more of the detail in the twigs on the trees.

M. J. S. HODGE

<sup>1</sup> For the first of these, see P. J. Vorzimmer, ‘An early Darwin manuscript: the “Outline and draft of 1839”’, *Journal of the history of biology* viii (1975), 191-217. Vorzimmer discusses in detail the dating of the outline and of the draft, admitting that while some evidence favours 1839, the two documents may be of different dates and both later than 1839. The thirteen-page draft deals

only with domestic variation. The outline, published earlier by Francis Darwin in his introduction to the *Sketch* and *Origin*, identifies the three parts of a projected work on the origin of species by natural selection, as I have explained later in this review. Apart from Stauffer's edition of *Natural selection*, I refer here to the edition of the *Sketch* and of the *Essay* in C. Darwin and A. R. Wallace, *Evolution by natural selection* (Cambridge, 1958. Reprinted, New York and London, 1971) which includes Francis Darwin's 1909 introduction to those writings, and to the facsimile of the first (1859) edition of the *Origin of species* (Cambridge, Mass., 1964).

<sup>2</sup> V. C. Kavaloski, *The vera causa principis: a historico-philosophical study of a metatheoretical concept from Newton through Darwin*. Ph.D. dissertation, University of Chicago, 1974.

<sup>3</sup> A. Koyré, 'Newton's "Regulae philosophandi"', in Koyré, *Newtonian studies* (Cambridge, Mass., 1965), pp. 261–72.

<sup>4</sup> Text and translation in Koyré, op. cit., p. 265. For a fuller citation of the texts in Newton's three editions, see A. Koyré and I. B. Cohen (eds.), *Isaac Newton's Philosophiæ naturalis principia mathematica. The third edition with variant readings* (Cambridge, 1972), ii, 550.

<sup>5</sup> Thomas Reid, *Essays on the intellectual powers of man* (Edinburgh, 1785) as quoted in Kavaloski, op. cit., p. 6.

<sup>6</sup> J. F. W. Herschel, *Preliminary discourse on the study of natural philosophy* (London, 1830), p. 148.

<sup>7</sup> F. Darwin (ed.), *The life and letters of Charles Darwin* (3 vols., London, 1888) iii. 24–5.

<sup>8</sup> Darwin, *Sketch* in *Evolution by natural selection*, pp. 57–8.

<sup>9</sup> *Ibid.*, p. 59.

<sup>10</sup> As in F. Darwin, 'Introduction', op. cit., p. 29. Cf. Vorzimmer's discussion of this text in his article cited in note 1.

<sup>11</sup> *Sketch*, loc. cit., p. 46. Cf. F. Darwin, 'Introduction', pp. 29–30.

<sup>12</sup> *Variation of animals and plants under domestication* (2 vols., New York, 1868), i. 13, 15 and 21.

<sup>13</sup> For this division of the *Principles* into four books—not explicit in the first edition—see the table of contents in *Principles of geology* (5th edn., 4 vols., London, 1837).

#### ◀ COULD DARWINISM BE INTRODUCED IN FRANCE? ▶

**L'introduction du darwinisme en France au XIX<sup>e</sup> siècle.** By Yvette Conry. Paris: Librairie Philosophique J. Vrin, 1974. Pp. 480. 120 francs.

Since 1887, when T. H. Huxley's essay 'On the reception of the "Origin of Species"' was published in the second volume of Darwin's *Life and letters*, a large and variegated literature dealing with the history of evolutionary thought in the later nineteenth century has appeared. To summarize this literature in brief would clearly be impossible, but the greater portion of it—48 titles to be exact—is amenable to the following informal historiographic analysis: considering titles *per se*, there are thirteen accounts of a 'reception', ten of an 'impact', nine of 'reactions', six of an 'influence', four of 'effects', three of a 'response', and one each of 'implications', 'attitudes', and an 'incorporation'.<sup>1</sup> A moment's reflection on these concepts—and concepts they must be if the authors intend their titles to delineate the substance of their work—reveals a two-fold emphasis in histories of post-Darwinian evolutionary thought: on the one hand, an emphasis on the human 'subjects' of history, their receptivity, reactions, response, and attitudes; and on the other, an emphasis on the 'object' of historical encounter—Darwinism, evolution, etc.—its impact, influence, effects, and implications. Though these emphases are not mutually exclusive (e.g., influence may be judged by attitudes and responses may be effects), they individually lack the wholeness of an interpretive concept which can represent indifferently human subjects as the creators of historical objects and historical objects as the comprehensions of human subjects: that is to say, a concept which can fully represent historical individuals as the *locus* of evolutionary thought. The notion of 'incorporation' may hold promise in this regard, though its use does not seem as yet to be especially profound.<sup>2</sup>

Now, however, with the publication of Yvette Conry's thesis for the Doctorat ès Lettres, completed in 1972 under the direction of Georges Can-