Explanation, Description, and the Meaning of 'Transformation' in Taxonomic Evidence

R. H. Brady*

Abstract

The nature of description and its role in scientific procedure is examined. Observation is theory laden, but not necessarily with explanatory theory. Darwin attempted, in the *Origin*, to absorb all the known facts of current morphology in his notion of homology. His insistence on literal "transformation" between forms, however, forced a confusion between explanation and description that he did not notice. Recent papers have attacked the "transformation approach", arguing that the hierarchy of forms is essentially *static* rather than successive. While this rejection of Darwin's literalism appears correct, the emphasis on stasis may be misplaced. Pattern reconstruction must take logical precedence over process explanation, but topological relations can be dynamic without reference to succession. What I have termed "literalism" understands the dynamic as the temporal, and this confusion between the realms of law and event continually leads to a substitution of explanation for description.

Introduction

Explanation and description are generally recognized activities within the biological sciences, but their respective positions are hardly equal. Explanation is, according to most accounts, the real task of science, and description one of the procedures serving this end. Darwin gave the same emphasis to explanation, but in Chapter l3 of the *Origin* he argues that the known patterns of morphology and classification await explanation, implying that both result from a purely descriptive activity. Darwin never gives an account of the science that produces these patterns, but his assumption that taxonomic relations are discovered prior to the inception of explanation remains an interesting one. Recent arguments in taxonomy suggest that the basis of the opposition, between proponents of "pattern cladistics" on the one side and "phylogenetic systematics" on the other, is generated by an emphasis on description and explanation respectively. If so, the nature of description needs examination, as does its place among scientific procedures.

The eclipse of descriptive science

Because natural history consists almost entirely of description, one would suppose biology to be the science that possessed the best account of this stage of science. Indeed biologists do take descriptive procedures for granted, both as ends in themselves and as steps toward the formation of classifications, which represent a higher level of description. Unfortunately, the importance of the stage is often eclipsed by that of explanation, with the result that the independence of description is compromised. Despite his remarks in Chapter thirteen, Darwin contributed to this eclipse in several ways.

The most obvious manner in which Darwin places emphasis on explanation was through his rejection of Baconian methodology. In a letter to Fawcett, written in 1861, Darwin complained of the notion that one ought to collect facts without a theory:

> About thirty years ago there was much talk that geologists ought only to observe and not theorize; and I well remember someone saying that at this rate a man might as well go into a gravel pit and count the pebbles and describe their colours. How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service. (See Hull 1973, p. 9.)

The passage gains interest by comparison to the *Autobiography*, where Darwin claims that he "worked on true Baconian principles without any theory and collected facts on a wholesale scale" (See Hull 1973, p.8). The public fiction, writes Hull (1973, p.lO) "was important to the sociology of science, both for the sake of the scientist's own reputation and for the sake of the acceptance of his theory by the scientific community", for the Baconian philosophy was the prevailing one. He adds, however, that Darwin had privately rejected Baconian approaches, realizing "that data cannot be gathered efficiently without some hypothesis in mind" (p.9).

Two years after his letter to Fawcett, Darwin made the following remark in a letter to a young scientist:

> I would suggest to you the advantage, at present, of being very sparing in introducing theory in your papers (I formerly erred much in geology in that way); *let theory guide your observations*, but till your reputation is well established, be sparing of publishing theory. It makes persons doubt your observations. (See Hull 1973, p. 10.)

The argument that "data cannot be gathered efficiently without some hypothesis in mind" might well cast doubt on observation, and Hull (1973) went on to explain why he was not worried. We may guess his position, however, from the Darwin quote above, "all observation must be *for or against* some view if it is to be of any service" [my italics]. Throughout the *Origin* Darwin searches not only for observations that support the theory, but also for those that seem to contradict it, in order to examine potential falsifiers. The possible contradictions were carefully explained away, and Darwin could claim that his theory, however it originated, was thoroughly *tested* against observation. Hull not only agrees, but is evidently of the opinion that the scientist has no choice, since some hypothesis must guide observation. But Hull's discussion is framed by the Baconian position which is only a straw-man today. Given that Darwin had to deal with that ideology, no one now supposes that explanatory theory arises by spontaneous generation from a mere collection of facts.

The problem that should come to focus here turns on Darwin's own insistence that his theory was the best explanation of the patterns of natural history, which patterns he laid out in Chapter 13 of the *Origin*. He does not comment on how we have originally come by this compendium, which is a matter of some interest, given his account of scientific method. If observation is to be guided by explanatory theory, and such theory is advanced to explain known facts, it would seem that the canon of known facts were acquired without observation. Of course, it might be better to suppose that these facts were acquired without explanatory theory.

Must we credit any explanatory theory for the *descriptive* content of natural science? If we are so inclined, Darwin's list in Chapter 13 should give us pause, for various elements are shared by representatives of very different persuasions (including Aristotle). Darwin himself is clear that he views the material as generally accepted — he calls the taxonomic hierarchy a "grand fact" — and calling for explanation.

Description seems to be taken for granted by Darwin and dismissed by Hull (1973), for neither takes any serious notice of it. Hull has maintained his lack of interest since that book, and the supposition that the business of science begins and ends with explanation is ubiquitous for many taxonomists. De Queiroz and Donoghue (1990), for example, insist that "phylogenetic systematics" should be preferred to a "cladistics" independent of Darwin's theory due to the "explanatory power" of the former, a property that description would not possess. Presumably, they would not recognize a descriptive taxonomy as worthy of consideration.

Why have so many (including Hennig 1966, Chapter 1) ignored Darwin's apparent satisfaction with a canon of known patterns which were not only discovered prior to his own work, but could stand on their own, needing no justification from explanatory theory? The most popular single response (all the recent voices mentioned above make it) seems to be the warning that all observation is theory-laden, and is therefore never innocent of our explanations. The comment is so popular and so unreflective that it deserves a section of its own.

The theoretical component of observation

N. R. Hanson, who was a possible source for the phrase above, had his own "Baconians" to deal with — the naive sense-data theorists who supposed observation to be a simple physical process of recording sense-data. Against this, Hanson (1972) argued very cogently that observation must be framed in the conceptual and linguistic world of a research programme to be of any use. After all, the scientist not only looks through the eyes of his theory, attempting to organize observation in such a manner as to find evidence of objects and events *postulated by the theory*, but also couches the observational report *in the concepts and language of the theory*. Thus the very activity of observing is both motivated by and structured through the same theory that the observations are to test. As a result, Hanson concludes that the eye is hardly innocent of theory, and some circularity is unavoidable, if undesirable. The discussion reminds one of Darwin contra Baconianism: theory builds in a particular way of looking at things, but without theory there is no point to investigation.

Hanson (1972) is best known for his argument on the theory-laden *language* in which observation is reported, but he also spent a great deal of effort pointing out that the very act of seeing, *before* any linguistic report of its results, is still a conceptual deed. Those who are fond of his phrase do not seem to realize that this is a very different point. Thus, the human perceiver interprets (linguistically), not merely what he or she sees, but also "interprets" in an entirely different sense *in order to see*. We are all familiar with the first case. We place interpretations — sometimes several successive ones — on whatever observations we make. We are less aware that any observation is already a product of something like interpretation, for the human perceptual process cannot be innocent of conceptual ordering.

The fact that concepts must be included in perception was discovered for the modern world by Kant, but it has antecedents that stretch back through medieval thought into antiquity. However one cares to argue the point, all accounts have the dichotomy between the sensible and the conceptual in common. The heart of the argument is that while the operations of our sense organs present us with sensations without conscious effort on our part, all *relations* are conceptual, and must be added by the intentional activity of the observer. Because nothing is intelligible without relations, whether internal or external or both, observation is the result of conceptual elaboration of the sensible report (which report would indeed be useless, because unintelligible, without conceptual elaboration).

Thus, even prior to any linguistic report, observation is still a product of "theory" in the sense that it is a product of conceptual elaboration. But this conceptual content exists prior to the addition, through linguistic expression, of a place in a particular universe of discourse. Let us take a simple example to clarify the point. Consider Fig. 2.1, below.

If we ask an average audience, "what do you see?", some will reply that they see a cube slanting down and to the right, others a cube slanting up and to the left, others will say both. Eventually, of course, everyone will see both cubes. They will also be able to see a flat pattern, a diamond from above (convex), or the same shape from below (concave), etc. They do this by assigning the spatial relations to the elements in the diagram — particularly those of depth.

These differing configurations are not added by thought after the object is perceived, but are *intended* by the perceiver in the act of perception. (I borrow the term used in phenomenology for the thinking we use to formulate perception rather than think about the result.) Contrary to what most laymen and some sense physiologists would assume, seeing is a cognitive activity — we must think (intend) the elements of the seen into place. And since "to place" the elements is to relate them, to "see" a particular figure is to cognize the same figure — each act of seeing is necessarily an act of understanding.

The example provides a simple test. If we attempt to make the choice of cubes voluntary, we find that in order to exchange one cube for another no further change is required than thinking (intending) the other cube. Simply look at the cube you presently see and think or imagine the alternate cube until you see it. The shift should take place in a few seconds. For those who seem to be dependent upon squinting, changing focus, moving the head, etc., the fact that this alteration can be done just as easily with the after-image, which is fixed in the optic nerve, should settle the matter. Only an intentional change is necessary, since only a shift in relations is desired.

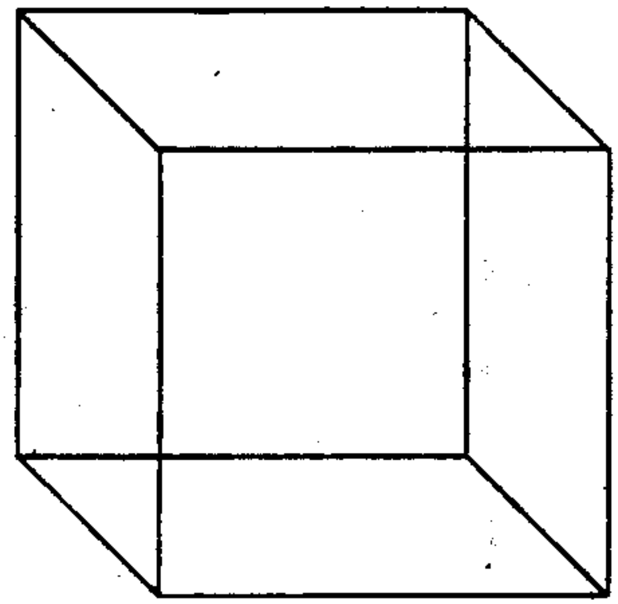

Fig. 2.1 A cube.

Of course, we do not propose relations arbitrarily. In the example above the information provided by the sensible input is not enough to pin down a particular figure, so the design remains ambiguous — subject to change. Yet the changes are of a limited nature — just those that will fit the sensible input, for the sensible signal and the intention reciprocally determine the final form of both. If we were looking at an actual box, and allowed to approach, circle, and touch it, ambiguity would disappear in the richness of the signal. Given the impoverished nature of the actual pattern under discussion, we understand as soon as we witness the visual shifts that it is ambiguous, that is it may be configured several different ways, each a lawful projection of possible spatial relations, with no particular advantage possessed by any.

By reflection on the experiences just reviewed one can see that in order to have them we must be able to make a judgment of the "fit" between each intention and the sensible input, and compare the results. "Fit" here must mean the degree to which our intention relates the sensible elements, bringing them into a unity. Of course, we can only judge this after the fact of combination, for only then is the unity so produced available to our judgment. Given the varied images we can produce from Fig. 2.1, we judge that each unifies the sensible input, and thus the sensible given is ambiguous. If we substitute an actual box, the *added* input would radically alter these results, since as we know it narrows down the possibilities of intentional unification, in most cases to one.

The central activities in perception are therefore *combination* (of intention and sensation) and *judgement* of the product: our intentional contribution proposes "what" is perceived and the fit with the sensible report determines "that" it is perceived, i.e. whether the proposal of "what" is successful. The usual attitude towards perception does not allow for so much activity on the observer's part, yet we are all familiar with the similar situation of the "double-take", in which the first "take", having resulted in contradiction or ambiguity on some level, is dissolved again and replaced by the second "take" that is sufficiently rich in fit. Here we apparently propose a set of relations and observe the result, which result, when found to be inadequate, is rapidly replaced by a second proposal and a new observation. The whole procedure takes place so quickly that it is easily neglected, but it becomes transparently clear when examined according to the discussion above.

As Darwin's letters argue his position against the Baconians, Hanson (1972) argued against the sense-data theorists, who supposed that perception was innocent of concepts. He was right to reject that thesis, but since he produces his arguments in opposition to it he does not further investigate the distinction between the conceptual contribution necessary to *see* and that which *accounts for* the thing seen (by placing it within a familiar context of explanation). When we hold the distinction in mind, however, it seems clear that in order to *describe* the cube slanting down one must invoke the very concepts of relation that must be intended in order to see it. After all, a good description would allow another perceiver to see the same thing, or replicate the original perception. Considering the technical nature of morphological "seeing" that detects taxonomic characters, it should be obvious that the replication of perception is a central goal of morphological description.

Since perception is the combination of intention and sensation, if either component varies the resultant perception must vary as well. On the other hand explanation varies independently of perception, and several explanatory hypotheses may be advanced to explain the same observation. An explanatory theory begins *after* the synthesis described above has produced an intelligible unity and adds hypothetical relations which are not part of observation. For this reason explanatory hypotheses are judged by comparison to completed observations, that is against the result of description. Both description and explanation are conceptual, but the concepts of the former cannot be changed without producing a new observation, while the latter set may vary freely against a fixed set of observations.

Hull's (1973) original statement that "data cannot be gathered efficiently without some hypothesis in mind" was correct, but his application dubious. The statement must change its meaning when the referent changes from explanatory to descriptive stages. Given an explanatory theory we look for independent observations that might confirm or disconfirm the expectations it creates. Given a descriptive intention we attempt to form a perception. When I told the reader to see alternative cubes the reader presumably formed those observations by bringing forward alternative intentions. In the first case we test by comparing "theory" to "observation", where both are pre-existent entities. In the second we "see what we can do", that is we attempt to create a successful observation. An intentional stance can be understood as a hypothesis, for it guides us in gathering data and will be judged by the result, but it must produce its data, and can only be judged by its own products. Even if human psychology hardly supports an absolute separation of these two, the attempt to separate them is still a necessary one. A confusion of these two very different senses of "hypothesis" could be fatal.

If we insist on ignoring the difference we shall think nothing of couching description in terms of explanation by using the language of explanation to describe the data. Yet since explanatory narratives contain relations logically in excess of perception, when observation is couched in the language of explanation the hypothesized relations are added to it by this linguistic procedure. It now becomes a *product* of that explanation, and the resultant circularity is debilitating for all empirical purposes. There is no point in testing explanations against observation if our language guarantees that the observations will agree.

Should we apply Darwin's advice to the descriptive phase of science, we would build explanation into observation, producing that most desired of outcomes — experience that explains itself. As I understand it, the notion that texts explain themselves, whether we mean by "text" a set of writings or a set of phenomena, is one of the strategies of fundamentalism. It does simplify the world, but also makes investigation unnecessary.

Taxonomic description

Darwin's "grand fact ... of the subordination of group under group" is, when examined, a proper descriptive claim. Since to see is to understand on some basic level, once we *have* seen we have also understood — we have a logical grasp on what is seen. We may then abstract those relations which constitute the phenomenon (intended relations) in order to trace relationship between phenomena. We may recognize, for example, that we may "see" in each of several distinct objects the same set of topological relations. If this perception is unambiguous, that is we can find no other way of seeing that produces a better fit between the sensible input and our proposal of relations, it follows that all the forms seen are the same on the level of topological constancy, and distinct on some other level.

If we turn to how we see difference it will follow that if our original proposal of topological constancy holds, differences will be reduced to different variations of the same topology. Given three or more distinct forms, some of these variations may be shared by smaller groups within the whole. We may call these variants homologies, or characters; or whatever we like, but more importantly we may use them to arrange the forms in an order reflecting the distribution of shared and unshared characters. Since the characters generally shared bring all into a group, but those shared by a lesser number unify only that number, it is the latter (synapomorphies) that indicate more complete sharing or closer relationship.

This categorical elaboration merely makes explicit the relations implicit in the intentional act, but as Edmund Husserl (1921) argued exhaustively, categorical elaborations cannot alter or add anything to the original perceptions: "They can do nothing to them, cannot change them in their own being, since the result would otherwise be a new object...". The point of the remark turns on the nature of intentionality. In order to make any logical sense, our categorical elaboration must represent the intended relations by which we brought the object to perception. Since that is the case, we can see that were they to add or subtract anything from our previous intentions we would generate a new object of observation, since a change in intention produces a change in perception. But such elaboration does not produce a new object, simply a new light — it puts a new face on the same data, like the difference between seeing a group of trees and seeing that there are five of them. We have not added anything to the trees by numeration. Even so, we do not add anything to the topological variants by arranging them hierarchically. If hierarchical relations — various levels of sameness and difference — were not already part of the observed structures they could not be abstracted to form a hierarchy.

Of course, other relations may be present as well, such as circular arrays or periodic orders. As the history of taxonomic schemes will testify, any scheme that produces a good fit with the data is worthy of consideration, and the argument between competing schemes must be settled on the same descriptive basis, that is the best fit between the conceptual order and the data. Nelson and Platnick (1981) attempted a short history of taxonomic schemes, and while they were successful in presenting the problem as one of comparative descriptions, their review demonstrates the need for a far more extensive study of the same material. Even without reference to the historical material, however, we can see that the choice between alternative classificatory schemes must either be settled on descriptive grounds or cease to have any empirical validity.

I am not sure why this point is not more generally grasped. de Queiroz and Donoghue (1990), for example, claiming that data may be arranged in other orders besides a hierarchical one, argue that the basis for selecting hierarchical relation is provided by the theory of common descent. While they mention the historical analysis produced by Nelson and Platnick (1981) they object that: "The observation of nested, hierarchical elements throughout the ages may be attributable to nothing more than the fact that grouping entities into nested sets is a convenient way of ordering knowledge". The suggestion that categorical elaboration is derived from our thought rather than the observed objects would only make sense only if our thought could *add* something to the data by categorizing. But they make no arguments to that end, nor do they attempt to compare the fit of other possible arrangements. Thus, without any logical or empirical basis that I can detect they advance the claim that description alone is indeterminate, that is the fit of the different schemes is equal, and conclude that explanatory theory should dictate how the world is perceived.

While I cannot believe that these authors would be entirely satisfied with the resulting fundamentalism, if one accepts their judgment as expressed the theory of common descent would provide our reasons for determining the order of taxa to be hierarchical. Since our belief in this arrangement would now follow from our belief in the theory, the hierarchical order or taxa could no longer be considered as evidence for evolution. Even Darwin would have to abandon ship here.

Since Darwin's remarks against Baconian methodology do not apply to the descriptive phase of science, and the argument against the sense-data theorists that "observation is theory laden" does not mean that description must be ruled by explanation, there is no reason to suppose that the contest between competing schemes of taxonomic order cannot be settled empirically. Actually, I would suppose that many taxonomists were already of the opinion that the hierarchical order was evidence for rather than an artifact of descent theory, and did not need my review. Yet the second manner in which Darwin contributes to the eclipse of description is more seductive and difficult to detect. The manner in which we think of morphological transformations is one of these, and it is here that we are most affected by Darwin's grasp of the relation, or better, Darwin's imagination of it.

Darwin's "literal" transformation

In Chapter 13 of the *Origin* Darwin reviews the agreement between known patterns of natural history and his theory. When he comes to morphology, he begins by calling to mind the biology of common plan:

> What could be more curious than that the hand of a man, formed for grasping, that of a mole for digging, the leg of the horse, the paddle of the porpoise, and the wing of the bat, should all be constructed on the same pattern, and should include the same bones, in the same relative positions? Geoffroy St. Hilaire has insisted strongly on the high importance of relative connexion in homologous organs: the parts may change to almost any extent in form and size, and yet they always remain together in the same order. (Darwin 1859 [1972, p. 434]).

Here is the discovery of topological constancy, with proper credit to the contributions of Geoffroy St Hilaire. Darwin continues by acknowledging that this description of phenomena demands an explanation — the underlying topological similarity needs a cause — and after arguing that past attempts at explanation have failed, he turns to his hypothesis:

> The explanation is manifest on the theory of natural selection of successive slight modifications, — each modification being profitable in some way to the modified form, but often affecting by correlation of growth the other parts of the organisation. In changes of this nature, there will be little or no tendency to modify the original pattern, or transpose parts. The bones of a limb might be shortened and widened to any extent, and become gradually enveloped in thick membrane, so as to serve as a fin; or a webbed foot might have all its bones, or certain bones, lengthened to any extent, and the membrane connecting them increased to any amount, to serve as a wing: yet in all this great amount of modification there will be no tendency to alter the framework of bones or the

> relative connexion of the several parts. If we suppose that the ancient progenitor, the archetype as it may be called, of all mammals, had its limbs constructed on the existing general pattern, for whatever purpose they served, we can at once perceive the plain signification of the homologous construction of the limbs throughout the whole class. (Darwin 1859 [1972, p. 435]).

Realizing that he has altered the original meaning of Geoffroy St Hilaire's "philosophical relations" (Geoffroy's term for the topological relations he traced), Darwin now tries to explain the linguistic change he is making:

> Naturalists frequently speak of the skull as formed from metamorphosed vertebrae; the jaws of crabs as metamorphosed legs, the stamens and pistils of flowers as metamorphosed leaves, but it would in these cases probably be more correct, as Professor Huxley has remarked, to speak of both skull and vertebrae, both jaws and legs, &c. — as having been metamorphosed, not one from the other, but from some common element. Naturalists, however, use such language only in a metaphorical sense; they are far from meaning that during a long course of descent, primordial organs of any kind — vertebrae in the one case and legs in the other — have actually been converted into skulls or jaws. Yet so strong is the appearance of a modification of this nature having occurred, that naturalists can hardly avoid employing language having this plain signification. On my view these terms may be used literally; and the wonderful fact of the jaws, for instance, of a crab retaining numerous characters, which they probably would have retained through inheritance, if they had really been metamorphosed through a long course of descent from true legs, is explained. (Darwin 1859 [1972, pp. 438-9]).

The point is that the descriptive "transformation" argued by Geoffroy may be called "figurative", because it does not refer to alteration over time but only "philosophic", that is ideal, transformation in the sense that two distinct forms are called "transformations" of one another in topology because both can be mapped to the same scheme of connections. The elements that are connected in one remain connected in the other, and those that are unconnected remain unconnected. Darwin argues that he has discovered the "true" meaning of topological similarity, but Huxley's warning reveals a problem.

Darwin had assumed that topological (philosophic) transformation may be equated with historical transformation. But topological transformations and even continuous morphoclines are found between forms that appear simultaneously rather than sequentially, such as members of a serial homology, or sister species when ancestry is not suspected. This is why Huxley warns Darwin to change language. Dealing with this same objection in an earlier chapter Darwin wrote:

> I have found it difficult, when looking at any two species, to avoid picturing to myself forms directly intermediate between them. But this is a wholly false view; we should always look for forms intermediate between each species and a common but unknown progenitor; and the progenitor will generally have differed in some respects from all its modified descendants. (Darwin 1859 [1972, p. 280]).

The argument works on the level it is made, but notice that this form of it cannot apply to the metamorphosis of stem leaves into stamens and pistils in the section quoted above. As Huxley detected, Darwin just cannot get away from a direct equation of topological and historical transformation, because his imaginative grasp of the phenomena is equally direct.

Throughout the text, when attempting to characterize relation between organisms or organs Darwin calls our attention to "intermediate forms" or "gradations", whether these are found in serial homologies within a single organism or between two distinct species. He asks us to see these as "transitional" between the two terminal forms, and in order to overcome the sensible distance between members of the series the rhetoric slowly eliminates even that distance, modifying the intermediates with "numerous", "a multitude", "a long series", "numberless", "innumerable", "interminable", and finally "infinite" and "insensible". Of course "infinitely numerous transitional links" commits us to the requirement that the difference between stages be so "infinitesimally small" as to be "insensible", and the series now *approximates continuous movement*. Thus the summation in Chapter 11, "Doubtful Species":

> Certainly no clear line of demarcation has as yet been drawn between species and sub-species — that is, the forms which in the opinion of some naturalists come very near to, but do not arrive at the rank of species; or, again, between sub-species and well marked varieties, or between lesser varieties and individual differences. These differences blend into each other in an insensible series; and *a series impresses the mind with the idea of an actual passage*. [My italics.] (Darwin 1859 [1972, p, 51].)

Here we see the core of Darwin's literalism. The argument, as reconstructed, identifies historical transformation with the graded series, and by extension, with all topological transformation. Thus "naturalists can hardly avoid employing language having this plain signification", that is sequential transformation in history, because "the appearance of a modification of this nature having occurred" is "so strong", that is "a series impresses the mind with the idea of an actual passage". The imaginative act at the center of the argument is the equation of a series with an "actual passage", and even Huxley's warning cannot correct the impression in Darwin's mind.

Because the same imaginative act is embedded in all Darwinian thought and language, it is also ubiquitous to much systematic literature today. It represents, however, the sort of confusion between description and explanation that has been my concern from the outset of the discussion. In order to see this with greater definition, let us look somewhat more closely at the visual impressions that ground Darwin's argument.

Observing a series

Since Darwin does not exclude serial homology, the series of stem leaves of Fig. 2.2 will provide an adequate example. If we desired to reconstruct the order in which the leaves appear on the stem the loop would be read from left to right, but without knowing this one could survey the series from either end, and whatever our choice the "passage" that Darwin indicates would be apparent. In the literalist outlook we are imagining a "movement" between the leaves that "is not really there", since the leaves are static. Thus we speak of transformation only by a figure of speech that describes the leaves "as if" they had once gone through a sequential metamorphosis. Yet under examination this "movement" of the static series is much more than an impression of past events.

Let the reader imagine, for a moment, how one could decide whether an additional form, not included in the series as yet, could be placed within it. By what criterion could the judgment be made? (Since I have performed the experiment with luckless classrooms of students — mostly ignorant of biology — I can report that the solution is almost immediate for most observers.) The forms of a graded series have the peculiar property of appearing to be arrested stages — we might call them "snapshots" — of continuous "movement". If we begin with the first leaf on the stalk (lower left) and follow the transformation to the last (lower right), or begin with the last and follow the movement to the first, we have the sense that we are in fact watching the form on one end of the series turn into the form on the other end. Notice that whatever the direction of our imagined movement the same intermediates will come to mind. Because we "see" the series in the context of this imagined or "intended" movement (to use the phenomenological term), an adequate criterion for accepting or rejecting a new member is near at hand.

We must reflect that the "movement" of the forms becomes more apparent in the actual phenomena to the degree that the "missing pictures" — the forms transitional between the shapes we have — are supplied. The movement we are *intending* would, if entirely phenomenal, be entirely continuous, leaving no gaps. Thus as gaps narrow the impression of movement is strengthened, and the technique by which a new form can be judged consists in placing that form within one of the gaps or at either end of the series and observing the result. When the movement is strengthened or made smoother the new form may be left in place. But if the impression of movement is weakened or interrupted, the new form must be rejected. Thus we have an appearance of necessity, and the context of intended "movement" is itself a criterion by which we accept or reject new forms.

If we imagined an immediate transformation between the leaves this property (the criterion that assigns leaf shape) would be provided by the necessity of shape number one to pass through shape number two before it could get to number three. Since by observing the growth of the stem we know that one leaf never changes directly into another, we must look elsewhere for the appearance of necessity. If we remove the explanatory reference to past events and simply describe the relations that govern present appearances, we can understand that the direction of our imagined movement has no bearing on its use as a criterion of inclusion or exclusion. That which is exactly the same for either

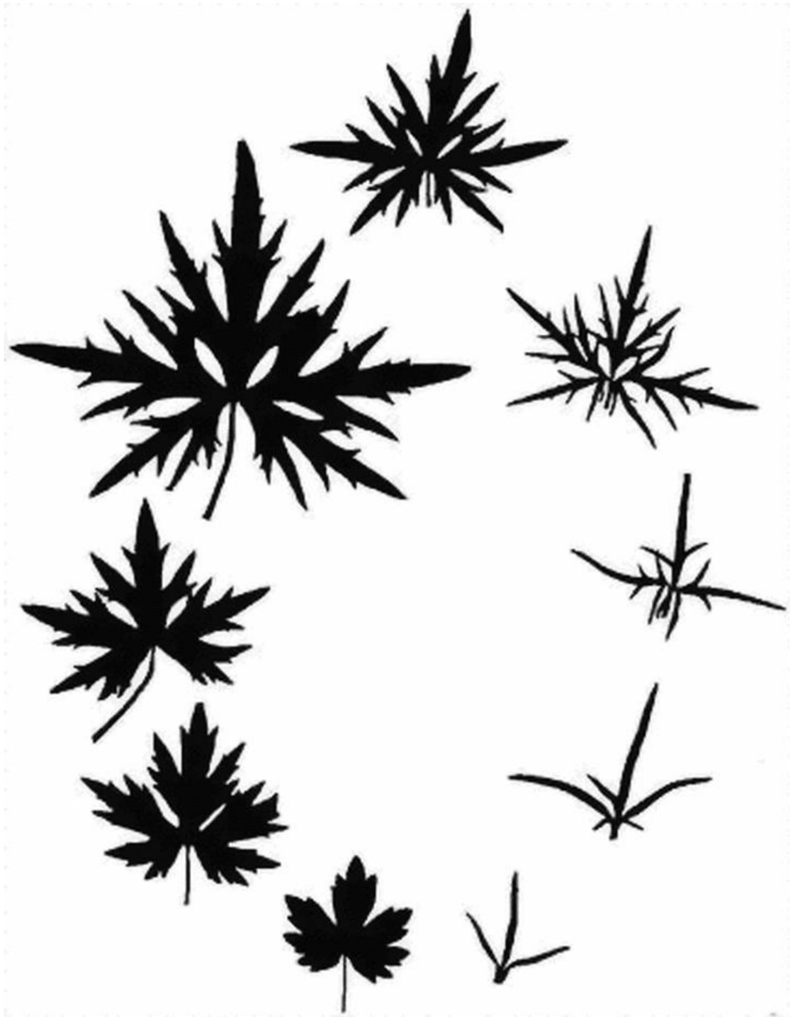

Fig. 2.2 A graded series of leaves from a single stem.

direction is the graded difference between members, or a continuous differential. That property alone may be able to account for our impressions.

The differential, once detected by our sense of movement, becomes an intended context for the members of the series. We arrange them in agreement with it. Because it is continuous, we may imagine any number of intermediates that are not actually present, but which are *potential* forms of the series, and any actual form may be matched to these potentials to find its position. This is how it can act as a standard of inclusion or exclusion with regard to the introduction of new members. But upon reflection, if all potential forms are specified by the movement, it must *generate* those intermediates which we do not actually find in nature. After all, we can make forms visible to our imagination that are not present in the observed series. The differential specifies form, for the mind at least, by generating it.

A common generative principle is a way of relating forms, but the relation can be displayed only by a sequence of forms that portrays the range of possibility. The result of such a display is an impression of completely continuous passage from the first form to the last (in either direction). We can now see, however, that this impression is not an imagination of an external event, that is one form actually changes into another, but the intention of a continuous potential. In the context of that intention the actual leaves become realized stages.

So far we have examined only the perceptual experience of the observer, but evidently the criterion we have uncovered approximates some lawful aspect of the organism itself — the differential that the mind recovers from the leaves can be observed on the stem. The differential is therefore a rule followed by stem leaf development, and as such a characteristic of the ontogeny of the species. It is recovered by an imagined movement through the leaves, but as we see this is transformation only in an ideal sense, since each form has its own unique development. There is another parameter visible here as well, although it is less obvious at first glance. The range of *potential* forms does not tell us anything about which potentials will become actual. Whether an actual leaf will occur at this or that point is determined by something else — something which determines the interval between each leaf. Of course Darwin's imagined "actual passage" is also actually continuous, and contains no intervals between forms.

The generative function of the differential is also responsible for the impression that the whole series is "the same thing". If the reader will work up the movement of the series again, in both directions, until it is effortless, and then consider the very different members of the series juxtaposed in Fig. 2.3, the impression should be clear. When these two forms are viewed in the context of the movement just developed, the perceivable difference between the forms is diminished. In fact, if the reader is successful with the exercise the two forms will clearly resemble one another, sometimes to the point of seeming momentarily like the same form. This result follows from what we have already determined about our perceptual structures.

Fig. 2.3 Two widely separated, different forms of Fig. 2.2 juxtaposed

If we cancel the stasis of the discreet forms in order to emphasize their continuity, each form will now represent, not an independent particular, but an expression of a potential. Since every form expresses the same generative principle — the source of all potentials — each now "stands for" the same whole. Thus two different forms appear to be the same form because in the context of the series they are both expressions of the same differential.

These investigations may have interest for the botanist (they did for Goethe) because such a series is characteristic of the species, and really does predict the nature of an intermediate leaf. They should also be of interest, however, to the reader trying to fathom Darwin's rhetoric, for they allow us to understand something of his mind. It is the perception of sameness above that Darwin inadequately translates into physical change of one form into another, for he looked to explain what he observed of biological forms by reference to past events. Given his vision of these events, his perception of the continuous differential was mediated by the language of that vision.

He struggles to approximate imagined events, for example, with the series of adjectives mentioned above — the account of a transformation by means of "very gradual", or "numberless", or "innumerable", and "interminable" stages, resulting in "infinite" and therefore "insensible" gradations. The obvious hyperbole of these adjectives was demanded by the explanatory model. We begin from an observed differential within a series of discrete stages. If we now imagine that series being produced by an immediate transformation of one form into the next, the number of stages must become infinite in order that the movement become continuous. Darwin insists on seeing an actual passage rather than a set of relations, and the failure of literalism here is the predictable result of a substitution of event for relation. A relation may specify infinite potentials, but events are finite.

If the analysis above is correct, the entire problem should remain within the "philosophic relations" of pure observation which Darwin rejected. The continuity of the graded series provides an order that assigns position to any given member. In this a type of necessity is immediately perceived, just as Darwin thought, but its "plain significance" is not the literal but rather the metaphoric meaning of the words. The "movement" that runs *either way* is a representation of the generative differential, which is perceptible from the forms alone, and which remains the same through change. We are not looking at the same object, but at products of the same rule.

Modern literalism

Darwin's language, following his imagination, read his narrative into the data and habitually substituted events for relations. Although he is no longer on the scene, his habits have been updated and modernized. Commenting on the distinction between what he calls the "taxic" and "transformational" approaches (following Patterson 1982), Rieppel writes:

> If ... the question is raised whether jaws could be derived from the mandibular arch or the tetrapod limb from a sarcopterygian fin, the taxic approach is implicitly abandoned and the analysis becomes one of transformations. Problems of evolutionary transformations entail no hypothesis of subordinate grouping, but a hypothesis of graded similarity mirrored by the serial arrangement of organisms in a continuous morphocline ... The taxic approach results in a hierarchy of fixed types representing logical relations of form, and in that sense it is ideal as well as static. The transformational approach, however, puts the hierarchy of homologies into a temporal sequence of form in order to ask the question of evolutionary transformation. The static hierarchy of types is reduced to a temporal sequence of forms, homology comes to denote *relations of succession*. (Rieppel 1988, pp. 111-12.)

Rieppel (1988) treats the two approaches as alternate styles of science, but I have difficulty understanding the transformational programme. It is clear that the "evolutionary transformation" theorized above is the "actual passage" of Darwin, with a similar appeal to a continuous morphocline. Homology is no longer described as a level of topological identity, but an historical event of transformation. Yet the only "event" that historical transformation can refer to is the transformation of ontogeny (or of the causal parameters of ontogeny), resulting in an alteration of adult forms. As we see above the questions asked in the transformational approach depend on the imagined transformation of one adult form directly into the next.

Science advances where evidence is determinate. If we ask taxic questions the evidence will determine a hierarchy, or a set of most parsimonious hierarchies. If we ask transformational questions the evidence should determine a transformation, or at least a set of best fitting transformations. As I understand it, the process of questioning will proceed by a reconstruction of the assumed transformation between a sarcopterygian fin and a tetrapod limb by arranging forms within a continuous morphocline. But although we can arrange both fins and limbs in a series, whatever progression we construct will always lack forms intermediate between the last fin and the first limb. If such intermediate forms were found, or at least forms that seemed to provide graded intermediaries, our attempt to trace a common topology between the two appendages would be far less ambiguous.

But here the investigator proposes to reduce the ambiguity by imagining “forms *directly* intermediate between them”, as Darwin did.

As my analysis above shows, a differential will generate forms, and thus we may easily generate potential intermediaries (but not their interval) by proposing this or that differential. But here the parallel ends. In our exercise with the series in Fig. 2.3 the differential was determined by the selection of forms. The selection was made for us, since we are dealing with a serial homology — the leaves in Fig. 2.2 were gathered from a single stem. But were the collection of leaves less specific — were we to mix the leaves of multiple species and attempt to find a differential — the results would be ambiguous. Multiple differentials would then suggest themselves because the collection is not specific enough.

Either a known series gives us a differential or a known differential gives us the potentials of a series, but we must start with one of these. In the case of the fin and limb, the intermediate series is under reconstruction and therefore unknown. Because we cannot derive it from a given series, and we do not know the underlying laws or generative mechanisms that would determine it, the differential is also unknown. If the evidence contains neither of these we can neither find nor generate intermediates, and the search is indeterminate. Worse yet, since we also lack the *interval* between the forms of the unknown series, we do not know if there ever were intermediate stages. And with the possibility that there were no intermediates comes the possibility that the two forms cannot be homologized by composition, but only by external connections, that is the organs are not the same.

A logical examination of the evidence in this case can reveal it to be indeterminate, but presumably those who still publish papers tracing hypothetical transformations think that their work has some empirical basis. I can only suppose that by projecting their explanatory narrative — gradual transformation through many intermediates — into their observations they have added something to the perceptible relations. As I have just argued, the relations within the data determine neither *what* intermediates existed nor even *that* intermediates existed. But if our theory convinces us that intermediate forms did exist, we are apt to suppose that we learned this from the evidence. For our theory-enhanced understanding, something more seems to be presented in the data than mere observation can detect. Unfortunately, those who do not embrace the same belief cannot discover from the evidence why we do.

Perhaps the imagination of a narrative is easier than the discovery of determinate relations in the data. I am reminded of a rather prophetic passage in Hegel’s *Philosophy of Nature* (1970) which notes that a contemporary account of biology has the first animals developing out of chemical reactions in water, and the higher developing from the lower (although all the material in the text was set down between 1805 and 1830). While admitting that “Nature is to be regarded as a system of stages, one arising from the other”, Hegel argues that a “metamorphosis” from one stage to another cannot be understood as the transformation of one individual into another but a development of the “inner idea”. As I understand it one example of this would be the development by which the same topology comes to new forms of expression. Since constant relations are what Hegel terms the "inner" necessity of "outward" events, only the former can explain the latter, and these relations can be studied only by examining what is available to us — the observable stages. The paragraph comes to an end, but then, as if another thought struck him, Hegel appends a "Remark" to this discussion, which begins:

> It has been an inept conception of ancient and also recent Philosophy of Nature to regard the progressive transition of one natural form and sphere into a higher as an outwardly-actual production which however, to be made *clearer*, is relegated to the obscurity of the past. (Hegel 1970, p. 20.)

Some twenty-nine years before the publication of the *Origin* Hegel was able to see a problem in supposing an "actual passage" between given stages. There may yet be some profit to be had in considering his meaning.

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* *Ronald H. Brady taught in the school of American Studies at Ramapo College in Mahwah, New Jersey. This article was originally published in* Models in Phylogeny Reconstruction, *edited by Robert W. Scotland, Darrell J. Siebart, and David M. Williams, pp. 11-29. Oxford: Clarendon Press, 1994.*

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