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Partition epistemology and arguments from analogy

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Abstract Nineteenth and twentieth century philosophies of science have consistently failed to identify any rational basis for the compelling character of scientific analogies. This failure is particularly worrisome in light of the fact that the development and diffusion of certain scientific analogies, e.g. Darwin's analogy between domestic breeds and naturally occurring species, constitute paradigm cases of good science. It is argued that the interactivist model, through the notion of a *partition epistemology*, provides a way to understand the persuasive character of compelling scientific analogies without consigning them to an irrational or arational context of discovery.

Keywords Interactivism · Partition epistemology · Argument from analogy · Natural selection

In this paper I will be following a pattern familiar to those versed in the interactivist literature, that of tracing a recalcitrant problem through the failure of various historical attempts to resolve it, diagnosing this recalcitrance as a function of entrenched encodingism or substance metaphysics, and showing how the interactivist model points the way toward a relatively straightforward solution. In this case, the problem is the persistent failure of philosophers of science to make sense of arguments from analogy. This failure has been recalcitrant since at least the first half of the nineteenth century, arguably considerably longer. The interactivist solution can be described in a number of ways; in my account, it involves proper attention to the implications of a *partition epistemology*, an epistemology in which knowledge involves the ability of

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the epistemic system to construct, and successfully locate itself within, partitions of its possible environments.

1 A historical datum: Darwin's analogy in *Origin of Species*

In his autobiography, Charles Darwin famously described *The Origin of Species* as "one long argument from beginning to end, and it has convinced not a few able men." It is always open to us to quibble with an author's description of his own work, but it is certainly true that the *Origin* won many adherents within the first years of its publication, and remains a persuasive read today.

But what kind of it *argument* is it? Nineteenth century philosophy of science recognized two kinds of inference, certain and probable—or deductive and inductive. But the main hypotheses of the *Origin*, the hypothesis of evolution by variation and selective retention, and the hypothesis of common descent, do not appear to be defended by either sort of reasoning. The driving argument of the *Origin* is an *argument from analogy*. In this approach, Darwin remained strongly influenced by an author he had read and greatly admired at Cambridge, the eighteenth century theologian William Paley.[1] Paley's analogy, likening the organism to the watch, is our main source for the canonical eighteenth century version of the argument from design. The substance of Darwin's analogy is obviously different, but in one respect its object is the same: that of explaining the diversity of life on Earth.

As David Hull has shown (Hull 1972) two of Darwin's greatest contemporaries among British philosophers of science, William Whewell and John Herschell, were initially utterly dismissive of the *Origin*. A third, John Stewart Mill, was less so. But he was not persuaded by Darwin's book, nor did he think the careful reader ought to be. On the one hand, as Henry Fawcett informed Darwin in a letter of July 16, 1861, Mill viewed Darwin's work as "in the most exact accordance with the strict principles of logic."[2] But Mill's own words, in a footnote added to his *Principles of Logic*, are more cautious. He allows that Darwin's argument accords with the rules of induction, but only because "the rules of Induction are concerned with the conditions of Proof. Mr. Darwin has never pretended that his doctrine was proved. He was not bound by the rules of Induction, but those of Hypothesis."[3] But the rules of Hypothesis govern the *formulation* of hypotheses only, not their justification. So Darwin can only have understood Mill's assessment as an instance of damning with faint praise.

I believe that what Whewell, Herschell, and Mill all missed is that there are uses of analogy in science other than those they acknowledged.[4] More specifically, there are situations in which analogies have *explanatory* worth, and in which this explanatory worth lends support to hypotheses articulated by means of the analogies in question.

[1] In addition to being explicitly acknowledged in his *Autobiography*, Darwin's admiration for Paley has been well studied. See e.g. Krasner (1990).

[2] Burkhardt (1994, p. 204).

[3] Cited in Hull (1972, p. 31).

[4] For a detailed discussion of Darwin's exchanges with Herschell (and other like-minded contemporaries) on the substance of his analogies, see Young (1985).

In the case of Darwin's *Origin*, the driving analogy, in Ch. 1–4, is that between the domestic breed and the naturally occurring species. Insofar as species are like breeds, their origins are explicable, because the origins of breeds are explicable—by recourse to the hypothesis of evolution by variation and selective retention.

In the later vocabulary of Karl Popper, Mill's assessment of Darwin's argument amounts to the claim that his analogy functions in the context of discovery, but not in the context of justification.[5] To thus dismiss the argument of *On the Origin of Species* is to dismiss its evident and well-documented persuasive power as somehow extra-scientific. But surely to dismiss Darwin's success as, say, *merely rhetorical*, would be to dismiss one of the paradigm cases of science—it would be to discard important *data* of the history and philosophy of science, in order to preserve a *theory*. This point becomes clear when we take into account two further considerations. First, no one who rejected the analogy between breeds and species would have had the slightest reason to even *entertain* Darwin's hypothesis, let alone accept it. Second, the first two chapters of the *Origin*, in which the analogy is laid out, contain numerous reports of observations. These observations are presumably meant to count as *evidence* of some kind—indeed, without this evidence, the analogy between breeds and species would be far less plausible. But the *discovery* of a hypothesis does not require evidence. It may be, to cite a much-abused example, that Kekulé's dream led him to the discovery of a structure for the benzene molecule. But no one has ever claimed that the dream constituted evidence of any sort.

So, Darwin's argument from analogy is persuasive, and not merely rhetorically. Further, this argument makes use of *evidence* in a way the philosophers of science who dismissed Darwin appear unable to explain. Though this paper isn't on Darwin or Darwinism per se, I have dwelled on Darwin's use of analogy at some length in order to show the full significance of the recalcitrant failure of philosophers of science to make sense of arguments from analogy. A philosophy of science that can't make sense of Darwin is clearly in desperate need of completion or correction.

2 The recalcitrance of the problem

The inability to find an appropriate place for arguments from analogy in the taxonomy of scientific reasoning is not confined to the middle of the nineteenth century. In 1906, French physicist Pierre Duhem insisted that models and analogies ought to be dispensed with in mature scientific explanation, which would consist of mathematical statements (Duhem 1906). The rejection of any significant role for analogy persisted with the logical positivists, and survived them; and so Carl Hempel, in 1965, asserted that "all references to analogies or analogical models can be dispensed with in the systematic statement of scientific explanation" (Hempel 1965, p. 440).[6]

[5] See e.g. Popper (1959, pp. 31–32).

[6] See Leys Stepan (1986) for a concise rehearsal of twentieth century dismissals of analogy in the philosophy of science.

Thomas Kuhn, in his 1962 *Structure of Scientific Revolutions*, initiated a significant change in how philosophers of science think of scientific change, and of the scientific communities, scientific theories, and scientific explanations that experience such change. Notoriously, his presentation made use of a notion whose precise definition proved elusive, the notion of *paradigm*. In subsequent work, Kuhn attempted to restrict his use of the word 'paradigm' in such a way as to refer only to "exemplary solutions to exemplary problems" in science. A scientific revolution would thus consist in casting down one set of exemplars, and elevating another set of problems, and solutions to those problems to the status of exemplars.

Two aspects of Kuhn's model of scientific change are of interest for our purposes. First, the modes of scientific reasoning held up as canonical by Whewell, Herschell, Mill, Duhem, and Hempel typically function most effectively in periods of what Kuhn called "normal science," scientific activity conducted during a period of consensual agreement on which exemplars are paradigmatic. It is in such periods that hypotheses are straightforwardly subject to inductive confirmation or falsification, because it is only in such periods that there is general agreement on boundary conditions, auxiliary hypotheses, and so on. Second, as Kuhn hints in *Structure*, and makes explicit in a later essay, "Metaphors in Science" (Kuhn 1979), his model is open to metaphors and analogies serving among the paradigmatic exemplars.

The difficulties with Kuhn's model, and the protracted, trenchant criticisms they provoked, are too numerous and well known to merit rehearsal here. Still, Kuhn's proposal, along with the work being done around the same time by Max Black (*Models and Metaphor*, 1962) and Mary Hesse (*Models and Analogies in Science*, 1966), constitute the leading edge of a new way of thinking about analogy that, once informed by a partition epistemology derived from the interactivist model, has the potential to address our recalcitrant problem. Curiously, Max Black calls his view the "interaction theory of metaphor," though 'interaction' doesn't mean the same thing for him as it does for advocates of interactivism. According to Black, an interaction metaphor, and the analogy it expresses, *cut both ways*. A metaphor of this sort is not simply an asymmetrical comparison in which the one side is held fixed, while the other is said to be "like" the first in some more or less specified way. Instead, the meanings of the two terms of the metaphor *interact*, generating a new meaning, with the potential to shed new light on the referents of both terms.

Returning to the philosophy of science, Richard Boyd (in his 1979, titled "Metaphor and Theory Change" and published in the same volume as Kuhn's "Metaphor in Science") argued for the existence of a class of what he called "theory-constitutive metaphors," metaphors essential to the very articulation of a given theory, and thus constitutive of any research program conducted with a view toward testing the theory.

When we apply Black's notion of an interaction metaphor to the study of scientific analogies, as Kuhn did in his "Metaphor in Science," we are led to consider the prospect that "the same similarity-creating process which Black has isolated in the functioning of metaphor is vital also in the function of models in science" (Kuhn 1979, p. 414). Combining Black's interaction metaphors, Boyd's theory-constitutive metaphors, and Kuhn's paradigms, historian of science Nancy Leys Stepan is led to assert that, in at least one instance, "the metaphors functioned *as the science itself*. . .without them the

science did not exist. In short, metaphors and analogies can be constituent elements of science" (Leys Stepan 1986, p. 267).

3 Partition epistemology

The interactivist model has long sought to account for many sorts of normativity, including epistemic normativity, and has issued numerous promissory notes in the philosophy of science. Significant steps toward honoring them by addressing such questions as induction and scientific progress, may be found in Bickhard (2002). Of interest for our purposes is an interactivist proposal known as *partition epistemology*.

The notion of a partition epistemology, and its position within the general framework of an interactivist approach to representation, have been briefly articulated by Bickhard in a number of unpublished works. A schematic account will suffice for the present purposes.[7]

Partition epistemology, and the closely related interactivist account of truth-value, should first be distinguished from epistemologies based on a correspondence model of truth. Crudely, the correspondence model of truth ascribes truth-value to propositions (or statements, or sentences) in virtue of the correspondences between names and particulars, between general terms and sets of (tuples of) particulars, and so on. By contrast, on the interactivist model, truth-value consists in the success (or failure) of an organism's interactive anticipations. These anticipations, in turn, express the functional presuppositions inherent in the organism's maintenance of its far-from-equilibrium condition, and by its differentiations of environment states. Together, functional presuppositions and differentiations define equivalence classes within the space of the organism's possible environments. Let each such equivalence class be called a "partition" of that space. It should be clear that these partitions are unlikely to be non-intersecting; that each may be bounded in any of many dimensions; and that the success of the organism's interactive anticipations consists, in part, in its ability to approximately determine its *actual* interactive environment by imposing such partitions.[8]

Suppose the epistemic subject is such an organism. The expansion of the subject's knowledge will consist in two processes. First, the subject will construct multiple, overlapping, and increasingly fine-grained partitions of the space of its possible environments. Concurrently, the subject develops correspondingly fine-grained anticipations, the success and failure of which allow it to further refine the partition structure as anticipations forge connections among equivalence classes of interactive environments. Second, the subject comes to situate itself—albeit fallibly—within the increasingly

[7] The following sketch is greatly indebted to Bickhard (in prep).

[8] In this connection some modal consequences of interactivism are worth noting. The familiar picture of the space of possible worlds, with the actual world at its center, is no longer appropriate, here. This is because, on the interactivist model, *possibility* is primitive, but *actuality* derived. One can only *narrow down* the space of one's possible interactive environments—by imposing ever more ramified, multidimensional partitions over it, and devising increasingly fine-grained predictions and tests. But one can never determine with precision in which of these possible environments on *actually is*. Actuality becomes merely the limiting case of possibility: possibility narrowed to the furthest extent of an organism's finite capacity for differentiation.

finely partitioned space of its possible environments. It comes to know *where it* is in this space: *what its world is like*.

If this sketch is correct, the epistemic subject's enquiries will serve either to *test anticipations* or to *generate novel anticipations*. Either sort of enquiry has the potential to be epistemically fruitful. I claim that compelling scientific analogies, including Darwin's, derive their power from the novel anticipations to which they give rise in an epistemic subject prepared to entertain them. Compelling scientific analogies compel our attention because they are *productive*. They are productive because they either define new equivalence classes over the space of possible interactive environments, or refine existing partition structures.

Furthermore, this account of scientific analogies not only explains their appeal, it also provides the kernel of an account of why it might be *rational* for the scientist to pursue them. A scientific analogy engenders anticipations. These anticipations may connect members of a given equivalence class of interactive environments, they may connect environments across partitions, or they may relate entire structures of partitions. But regardless of the sorts of anticipations engendered by a scientific analogy, and irrespective of the roles played by these anticipations in a subject's overall framework of cognitive representations, *anticipations remain the sort of thing that succeeds or fails*. Many, perhaps most scientific analogies may turn out to give rise to bad predictions, failed anticipations. But on orthodox Popperian grounds, this is precisely what the scientist wants. Falsifiability is the best guarantee of testability.

If I am right about the role of scientific analogies in inducing and refining partitions of the space of possible interactive environments, the hypotheses generated by a scientific analogy are certainly unlikely to turn out to be *unfalsifiable*. An analogy does not partition the space of possible interactive environments in a *random* way, nor does it saddle the subject with anticipations that are unlikely ever to definitively fail or succeed. The whole point of an *analogy* is to replicate anticipations that have succeeded before, albeit in other domains, in interactive environments whose structural similarity to the present interactive environment has yet to be determined. In this respect, scientific analogies not only generate testable hypotheses, they also provide a kind of weak warrant for those hypotheses: they warrant the expenditure of further resources for testing.

Partition epistemology allows us to reconstruct Leys Stepan's synthetic account of science-constitutive analogies in a way that is congruent with the interactivist model. This reconstruction will have the further virtue of illuminating our datum: the persuasive appeal of Darwin's argument from analogy.

An interaction metaphor, in Black's sense, derives its utility from the fact that the meanings of both its terms *interact* to engender a new meaning with the potential to illuminate the referents of both terms. Suppose, for the moment, that it does this by, as Bickhard says, stipulatively inducing "partitions into equivalence classes regarding which environments will fall in a particular differentiation category or which will support a particular interactive anticipation" (Bickhard in prep., p. 189). If so, an interaction metaphor allows the reflective epistemic subject who accepts the analogy to entertain novel interactive anticipations. In testing these anticipations, the system determines whether it is, in fact, in an environment belonging to the partition induced by the interaction metaphor.

If (to touch briefly on another old Popperian chestnut, the question of *demarcation*) whether or not a branch of enquiry counts as a science depends on whether its theories have testable consequences, it is now clear why an interaction metaphor of this sort can express a science-constitutive analogy. The analogy induces a partition on our possible environments; this partition engenders a set of anticipations; and the anticipations either succeed or fail over the course of a series of interactions, interactions with the environment conventionally called *observations*.

Returning to Darwin's analogy between domestically occurring breeds and naturally occurring species, we begin by noting that it is, in Black's sense, *interactive*. In other words, entertaining the analogy illuminates our understanding of both species and breeds. Of species, it suggests that, like breeds, they emerge by variation and selective retention. Of breeds, it suggests that they *really are*, in Darwin's phrase, *incipient species*.

But why is an argument grounded in this analogy rationally compelling—or why should it be? This is because the analogy *constitutes* a new empirical science, along with the attendant research programs. This, of course, is exactly what a Kuhian paradigm was meant to do. The science so constituted is worth pursuing, because it engenders anticipations the testing of which tells us more about where in the continuum of possible interactive environments we find ourselves. In other words, it entails testable hypotheses. It is fruitful and productive. The respects in which Darwin's analogy between species and breeds has proved productive, still giving rise to testable hypotheses 150 years after its first publication, are too numerous, and too well-known, to merit further discussion here.

Finally, we might ask, is there any respect in which Darwin's analogy provides a *warrant* for his hypotheses, irrespective of any promissory notes it issues on future productivity? Are there *evidentiary* grounds for distinguishing between the analogy to domestic breeds, and Kekulé's dream? The interactivist reconstruction of science-constitutive analogy suggests an affirmative answer to both questions. Entertaining Darwin's analogy between species and breeds involves accepting a partition of the space of interactive environments, a *partition known not to be empty*. In other words, by virtue of our encounters with interactive environments containing domestic breeds—by virtue of our interaction *with* such breeds, including such interactions as are necessary for domestic selection to occur—we know that there *are* possible interactive environments of the sort in question. Kekulé's dream, by contrast, does not reference an interaction of this sort. Surely the case for the evidential status of Darwin's analogy ought not to be overstated; the sort of warrant it provides, in the absence of further observation, is doubtless not particularly strong. But I conclude by noting that, however we might want to quantify such warrants, they are *not* merely inductions from one case. Instead, they establish certain modal facts, facts without which science would be difficult to conceive.

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