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Some concepts of historical science

«The nourishing fruit of the historically understood contains time as a precious but tasteless seed.»
Walter Benjamin

Abstract — The goal of this paper is to explore the concept of historical science. Many analysts of science distinguish between historical and ahistorical sciences to argue that some practices are more «scientific» than others, that the distinction supports a particular view of proper scientific method, or that some are mere pseudo-sciences. After discussing these reasons for calling a science historical, six different analyses of the concept of historical theory or science are discussed. I conclude in favor of a pragmatic view, drawing on Danto’s analysis of narrative, in which a science is historical to the extent that it admits narrative as contributing to understanding.

The goal of this paper is to explore the concept of historical science. It is claimed that evolutionary theory and the biological sciences that use and develop it, e.g. systematics, are historical sciences. Such a claim is sometimes used to differentiate among sciences, e.g. to classify them as «hard» or «soft». Since many authors discussing the nature of systematic biology, theory, or practice assume a clear distinction between historical and ahistorical, it is helpful to examine several such concepts. The approach used here is philosophical: examining proposed distinctions between historical and ahistorical sciences (or theories) and criticizing them for failing to distinguish among sciences (or theories) that common intuition distinguishes. Since the aim in making such distinctions is either to aid understanding of the nature of the sciences or to make value judgments (e.g. to set priorities for funding or ranking for prestige), the philosophical exercise has practical merit. The aim is not, however, to criticize claims in the philosophical and biological literature in order to establish which philosophical «school of thought» has the correct analysis of science. Rather, the practical merit in the philosophical exercise is to defend the intellectual autonomy of systematics against the criticisms of theoretical and sociological reductionists.

Why call a science historical?

Three reasons one might call a science historical are to argue that: (1) a science, for being historical, is no less legitimate or valuable than other, ahistorical sciences; (2) a certain model of explanation that serves as a standard of scientific acceptability, e.g. Hempel’s deductive-nomological or covering-law model, applies to the science even though nothing like Hempel’s ideal schema appears in the main texts of the practitioners; (3) the kind of warrant, or justification, of knowledge or understanding claims is distinct from that of ahistorical science. Three more reasons just negate the first three: (1') in being historical, a science is distinctly illegitimate, unfounded, or at least inferior to ahistorical science; (2') if Hempel’s covering-law model of explanation (or something like it) does not apply to a subject, that subject is not a science at all; (3') the kind of warrant or justification in historical science in the same as for ahistorical science, and therefore the knowledge produced by historical science is weaker because the warrant is generally less for its claims.

I will consider these reasons before discussing six conceptions of what makes a science historical because they color one’s reading of the concept of «historicality». The different concepts suit different purposes, and it is not at all clear which reasons and concepts should be judged compatible. I shall not consider the comparabilities in detail, but only review four concepts and offer two more.

Positive and negative reasons are considered together because I do not aim to endorse arguments committed to one side or the other. Scientists engaging in turf battles for legitimation, authority, domination, money, power, students, laboratory space, or glory, often invoke the legitimacy reason in arguments to secure places in a «pecking order» for different sciences or to reject hierarchies of authority and social status altogether. The others are more typically used by historians or philosophers concerned to formulate general principles of science or methodological principles for their own disciplines.

Typically, authority hierarchists place physics and other physical science at the top. Then comes biology and the «historical» parts of geology and other earth sciences, then psychology, and then the lowly social sciences like sociology and anthropology. Equally typically, anti-hierarchists argue for some sort of «pluralism», with all sciences on an equal footing. For example, in «A plea for the high status of natural history», Stephen Jay Gould (1989, 280-81) takes Nobel laureate physicist Louis Alvarez to task for likening paleontologists to stamp collectors and calling them «not very good scientists». Gould defends the autonomy and authority of natural histo-
ry by invoking the power of historical interpretation on the one hand and the ignorance of physicists about natural history on the other.

The second reason, regarding the nature of scientific explanation, typically concerns philosophers of history, whose project this century was transformed by Hempel's 1942 essay, «The Function of General Laws in History». This essay set the stage for his controversial covering-law model of explanation (Hempel and Oppenheim 1948; Salmon 1989) and capped two decades of disputation about whether history was a scientific discipline or could be reduced to one.

Along with a number of other developments in the philosophy of empiricism that grew out of the logical positivism of the 1920s, Hempel's work polarized general discussion about science around the issue of the reducibility of the central theories of one science to those of another. The reduction debate was driven by three features of Hempel's conception of explanation: (1) that, ideally, explanations are sound deductive arguments from premises including statements of general laws of nature and particular conditions about the phenomenon to be explained; (2) that explanation is the core project of any science; and (3) that since deduction allows the explanation of a law by more general or «logically powerful» ones, the laws of one science might be explained by those of another, more powerful science. This last feature provides the basis for a conception of the growth of scientific knowledge as the reduction of less powerful theories to more powerful ones.

When philosophers turned their attention to sciences like geology, systematics, and evolutionary biology, it proved difficult to formalize the relevant theories along deductive lines (Fllew 1959, Smart 1963, Williams 1970). The expectation of success was formed largely through the tacit inductive argument that if a few select physical laws of mechanics can be so formulated, then all of physics can and, by appeal to principles of hierarchy and reduction, then all of science can.

Some of the qualities that made formalization of the «soft sciences» difficult align these sciences with the humanities and social sciences rather than with the physical sciences, Goudge (1958), for example, defended such an alignment, arguing that explanations in natural history have more the structure of coherent narratives than of deductive systems. Despite this, such narratives could be causal explanations. Therefore, according to Goudge, the philosophical model of deductive arrangement under general causal laws, which is lacking in natural history narratives, cannot lay exclusive claim to causal explanation. Nor, therefore, can physics lay claim to a scientific method superior to that of the «historical» sciences (Goudge 1958, 202).

The narrative quality of Darwin's adaptive explanations is manifest in the structure of this argument for evolution and natural selection (e.g. Beer 1985; Gould 1986). This «literary» quality looked problematic to those logicians who rejected as meaningless all statements lacking means of empirical verification. They accordingly tried to explain Darwin's theory away (e.g. Nagel 1961) for teleological explanations. A long general argument that such sciences are inferior science or even pseudo-science (e.g. Popper 1957, who later changed his mind about natural selection, see Popper 1972, 1978). Smart (1963) argued that since there are no general laws for particular species, evolutionary biology was not scientific. This statement helped provoked the elaboration of the philosophical analysis of species as individuals rather than kinds or classes (Ghiselin 1974, Hull 1980). The species-as-individuals view supports an argument that there should not be laws about particular species and that philosophers and others misjudged evolutionary theory in claiming it had no laws and was unscientific. Rather, they had been looking in the wrong place for laws.

Calling a science historical because it fails to reduce to physics is a reason I will not explore further. I do not believe explanation, as laid out by the Hempelian tradition, is necessarily the central project of a science, nor do I believe that there is necessarily any such beast as the «core theory» of a science in Hempel's terms. My reasons trace to suspicions of vagueness and ambiguity in the concept of «theory» and the likelihood that holding such concepts in high regard will lead to error, or worse.

Although the analytical philosophy of history (Danto 1985) has done much to widen the scope of general philosophical discussion on the topic of explanation, the latter's ultimate purpose is too narrowly epistemological to serve my goals. Philosophical theories of explanation distinguish too rigidly between «discovery» and «justification». This once useful distinction now serves only to insulate deductive inference from the scrutiny that other aspects of scientific practice routinely receive by historians and sociologists as well as philosophers of science. The question of what justifies deduction itself is part of the general problem of justification. Deduction has become a self-justifying (or unjustifiable) axiom for philosophy of science. And this in turn has masked the important fact that logical empiricist philosophers of science are themselves merely using a bit of predicate calculus as off-the-shelf technology. There is no a priori reason one could not or should not take narrative as an alternative basis for science.

Justification or warrant concerns the relation between evidence and hypothesis, while explanation concerns the relation between premises in an explanatory argument and conclusions, or between questions and answers (van Fraassen 1980). Thus, regardless of what purposes are served by invoking scientific legitimacy or the pattern of explanation, a third reason to call a science historical is that it differs in the style of justification from ahistorical sciences. That is, its evidence bears a different relation to its hypotheses. Advocates and critics of the distinction between historical and ahistorical science can agree that producing an explanation and justifying it are logically distinct operations, and yet disagree on whether the distinction between styles of types of justification, and therefore between different kinds of science, serves any important purpose.

Claims about warrant (reason three) are sometimes linked to the first reason through an argument that, because science becomes unified by a universal logic of warrant, a science is legitimate to the extent that it satisfies that logic. Scientists sometimes defend a particular science against the charge that it is inferior or pseudo-science by arguing for the epistemological unity of science (and against a distinction between historical and ahistorical science). But
scientists sharing the goal of defending the legitimacy of particular sciences can fall on either side of the unity of science question. One can analyze history itself in such a way that it is scientific, and therefore that historical sciences are scientific, or one can deny that being historical entails not being scientific on grounds other than warrant and thus still accept epistemological unity. On the other hand, one can reject epistemological unity as a goal of science and then nothing is entailed about whether a practice is scientific just in virtue of being historical. Therefore, neither commitment—to unity or disunity—is effective in defending a science against its critics.

Historians and philosophers of science also pursue common legitimization goals with different commitments to epistemological unity or disunity. Some recent essays from a symposium on evolution as a historical science illustrate this. Richards (1992) relies on the distinction between historical and ahistorical science, but to argue against the Hempel model as a basis for unity. Richards argues that Hempel's model gets the nature of explanation backwards. Hempel did not claim that acceptable explanations in actual scientific practice meet the formal conditions he defined. Rather, he offered them as an explanatory ideal to which real explanations are better or worse approximations. Real explanations might not meet the ideal by failing to explicitly mention laws, although these might be supplied by post hoc historical or philosophical analysis in particular cases. Hempel called such approximations explanation sketches that could in principle be filled in. Many commentators have taken Hempel's idealism to insulate his model from the criticism that it failed to account for actual scientific explanatory practices. Richards argues instead that historical narrative explanations—far from being mere explanation sketches—are the ideal, and that deductive-nomological explanations (even those meeting all of Hempel's requirements) are mere narrative sketches. Unity of science is preserved by standing the logic of explanation on its head.

Hull (1992) likewise relies on the historical/ahistorical distinction and likewise argues against the emphasis on laws in the Hempel model, but on different grounds than Richards. Hull defends a «particular-circumstance» model of explanation against Hempel's covering-law model to capture the sense that, even within the broad framework of deductive inference, in historical sciences the conditions or particular circumstances rather than the laws govern the character of explanations. Thus, while Richards defends epistemological unity of science by arguing that narrative rather than deduction unifies explanation, Hull defends plurality of explanatory practice within a broad epistemological unity of deductive form.

Ereshefsky (1992) does something orthogonal to both Richards and Hull. He rejects the historical/ahistorical distinction among forms of explanation and considers the Hempel model a useful (though perhaps limited) paradigm. He argues instead that evolutionary theory is historical, but because some of its central entities—taxa—are a special type of historical entity, not because its explanations are historical. Hull argues that the model of explanation in evolutionary theory takes a distinctive, particularistic form because the entities are historical, thus accepting the classical basis for the historical/ahistorical distinction in the form of explanation. Ereshefsky rejects the classical basis, but accepts a new distinction based on the nature of the entities rather than the form of explanation. He argues that this new distinction is compatible with the Hempel model and the Ghiselin-Hull philosophy of historical individuals.

Laudan (1992) rejects the distinction between historical and ahistorical science, not because she sees an alternative route to challenge Hempel's analysis of the form of «proper» scientific explanation (Richards, Hull) or because the entities of some sciences are historical entities (Ereshefsky), but because she rejects the question of explanatory form as the significant one. She suggests that philosophers of history should look again at the literature on scientific explanation (i.e., they should bring their history of philosophy up to date). They would find that in the fifty years since Hempel proposed his model, it has ceased to be the only viable formal model, and is no longer the most promising one to many philosophers.

Laudan takes the difference between sciences like geology and biology on the one hand and physics and chemistry on the other to be a scientific question about warrant, not a philosophical question about the logical form of explanation. The scientific problem is to devise means of acquiring reliable knowledge. She traces the motivation to distinguish historical from ahistorical science to «observational difficulties» about reconstructing the record of the past and argues that the gradient of difficulty in this regard among the sciences does not make for epistemological disunity, whatever form explanations in different sciences may take (Laudan 1992, 62). The fact that a laboratory physicist or geneticist has less trouble reconstructing the history of events in their experimental set-up than does a field paleontologist tracing a fossil record or a systematist reconstructing a phylogeny does not make for a difference in kind, but only in degree. Solution to the problem of stating the general terms of scientific warrant does not imply anything, therefore, about the epistemic unity or disunity of the sciences.

The objective of Laudan's argument is to deny an appeal to the complexity of geological processes and incompleteness of the data to sustain a distinction between historical and ahistorical science. Danto (1985, 340) argues the same point in a more general context: incompleteness of record is a «banal and contingent» fact about historical data, not what makes a practice historical. We shall see below why appeals to complexity and incompleteness fail to ground the distinction.

As far as devising means of acquiring reliable knowledge goes, the issue of narrative vs. deductive form of explanations is tangential to the epistemological problem of warrant (Laudan 1992, 64). The only historical/ahistorical distinction Laudan admits is that narrative explanation requires an additional justification step: connecting parts of chronologies using particular causal theories. The link between use of causal theories and parts of chronologies is important for characterizing historical science, but a proper analysis would show that the distinction can be drawn independently of arguments for or against epistemological unity (Griesemer ms).
One may doubt that arguments pursuing any of the three reasons for calling a science historical can be effective on the grounds proposed. One can argue for or against legitimacy, explanatoriness, or warrantability of a science while accepting or rejecting the claim that it is distinctively historical and while accepting or rejecting the claim that the sciences as a whole are epistemically unified or disunified. Pushing further, I will argue that what makes a science historical is a pragmatic matter of taste rather than logic, and the distinction is one of aesthetics and judgment. Put differently, questions of logical form are distinct from questions of logical relation and questions of historical form are distinct from questions of historical relation. That explanatory form and historicality are pragmatic matters in no way forces one to take up sides on the largely sterile controversies dividing relativists and realists or objectivists and subjectivists.

**What is a historical science?**

I will not discuss all the concepts needed to interpret the claim that some sciences are historical and others not. Concepts like "chronicle" and "narrative" are mentioned only to introduce an alternative view of what makes a science historical. Danto (1985) analyzes the relevant concepts.

A good start in answer to the question is made in Wright, Levine and Sober (1992). In a section authored by Sober and titled, "The Historical Character of Evolutionary Theory" (48-51), four concepts of the historicity of historical science are discussed (cf. Levine and Sober 1985).

The first conception is one that Sober dismisses as trivial, failing even to demarcate evolutionary theory from "billiard ball" mechanics. Even though I will ultimately distinguish "historical science" from "historical theory", the claims to follow are worth considering irrespective of whether science or theory was the author's intended target.

1. **A theory is historical if the statements it explains refer to two or more moments of time**

This is trivial because any science that deals with temporally extended entities, events, or sequences must refer to multiple moments in time. Here is the tasteless seed, not the nourishing fruit. The fact that billiard ball mechanics refers to the states of balls at different times suffices to classify it as historical according to criterion 1. Indeed, it is hard to see how theories of any physical processes could to otherwise, and since evolutionary theory is a theory of certain physical processes, claim 1 does not demarcate evolutionary theory from any other dealing with physical processes, nor any science based on facts of natural history from other sciences. And even if systematics is interpreted as a theory of certain temporal relations or patterns among taxa rather than processes, it would not be distinguished from a similarly interpreted physics, e.g. one interpreted in terms of functional relations rather than causes (e.g. Russell 1913).

It does not follow, however, that claim 1 does no demarcation work at all. It demarcates the sciences of pure abstractions — "Platonic objects of thought" — such as mathematics and philosophy from empirical sciences of the physical world. Abstract objects are not in time, and therefore their explanations do not require reference to moments of time.

A more promising thesis is one that Sober characterizes as attributing "the Markov property" to a scientific theory. Sober (Wright et al. 1992, 48) traces this view to Gustav Bergmann and quotes an adaptation of it by Hull (1974).

2. **A theory is historical when "knowledge of the past is necessary to predict the future. Knowledge of the present alone will not do"**

It is important not to overinterpret claim 2. It does not assert that knowledge of the past is sufficient to predict the future, but only that it is necessary. If real physical systems exhibit chaotic dynamics, then even very precise information about the past may be insufficient to predict the future (My thanks to Professor Scudo for raising this point). Claim 2 does not claim so much, but Sober rejects it on several grounds. First, he points out that in population genetics, standard models require only the gene and genotype frequencies of the population at a given time plus a specification of the evolutionary forces in play at that time. If population genetics is taken to be the core of evolutionary theory, and one claims that evolutionary theory is historical, then Sober's point has some merit: since population genetics does not (or at least need not) have the Markov property, evolutionary theory is not historical by criterion 2. One may, of course, deny that population genetics is the core of evolutionary theory or conclude that evolutionary theory is not historical without doing serious damage to claim 2.

Sober makes a more important second point about references to the past: any dynamical theory will in practice typically require knowledge of the past as well as of the present to predict the future. In order to predict without reference to the past, a theory must be dynamically and empirically sufficient relative to the choice of state variables, parameters, and state space (Lewontin 1974, Wimsatt 1980, Lloyd 1988). Otherwise, our poor knowledge of how forces combine to produce effects will require that to predict we must know things about the temporal order in which the forces operated in the past. Cartwright (1983) makes the same point when she argues that mechanics is the only example of a dynamical theory for which there is a general rule (vector addition) about how to combine forces. She argues that, more typically, the lack of general laws of interaction leads to the complex form of "phenomenological laws" used to predict in practice. These are distinct from the pristine "fundamental laws" used to explain. For a theory like evolutionary theory, one needs in addition a lot of variables and a lot of care in their measurement to avoid empirical insufficiency, which is also required to predict a future state of an evolving system from its present state alone.

Indeed, for evolutionary theory, Lewontin (1974) suggests that we will rarely meet conditions of dynamical and empirical sufficiency to make interesting predictions about evolutionary systems in nature. But this failure in practice does not lead Sober to
argue that evolutionary theory is historical. He instead argues against the interpretation that flows from claim 2. It does not follow that a theory is historical merely due to failure to meet the practical requirements of dynamical and empirical sufficiency. In practice, one can sometimes substitute a knowledge of history for satisfaction of formal sufficiency. But it would be a mistake to confuse this empirical and contingent fact with a conceptually necessary property of historical science.

While I accept Sober’s point about theories, and therefore his conclusion that claim 2 fails to demarcate evolutionary theory from others in the class of dynamical theories, there are caveats. It is not clear how far Sober’s conclusion about a rather unspecified evolutionary theory carries to other theories and theoretical structures in evolutionary biology, e.g., to theories of speciation, or more importantly to the collection of methods and assumptions that play a role in systematic practices such as cladistic analysis and phylogeny reconstruction. Sober (1988) argues for the dependency of cladistic inference on model assumptions about the evolutionary process, but that does not suffice to show that cladistic practice depends on any particular version of evolutionary theory. Therefore, the failure of claim 2 to establish the historical nature of evolutionary theory does not imply that systematics is also ahistorical.

Caution must be exercised in two ways in evaluating these arguments. One is in articulating the content and structure of theories. If theories are best presented through models rather than axiomatized laws, it may turn out that what can be done with models in practice plays a more important role that any conceptually necessary claim about the historical or ahistorical nature of the theory, making precise statement of the latter irrelevant to understanding what it means to say that the theory is historical. Second, we may be forced to distinguish between theories and sciences if practice is relevant. A science in which a theory is constructed and used to make predictions may be historical even if the theory itself fails the test of claim 2. (It should already be clear that in calling a science historical, I am not concerned with the idea that all sciences are historical because they are practiced and that all practices are historical).

Distinguishing between a science and a theory leads to the further concern that we are perhaps being overly respectful of the idea that prediction, like explanation, is a relation between premises and conclusions of arguments. The covering law model of explanation (and by Hempel’s symmetry thesis, prediction) not only leads us to think of prediction in this way, but also leads to inattention to the practice of argumentation. Hempel was sensitive to this in his claim to be formulating idealized concepts of explanation and prediction, not analyzing or criticizing actual scientific explanations. The latter amount to mere explanation sketches in Hempel’s analysis. But they are none the less powerful for failing to meet the ideal, and if the idealization is so stringent that it is never met in scientific practice, then practice, not the ideal, should be the focus of attention in trying to understand what makes a science historical. To the extent that explanatory and predictive practice falls outside the bounds of what is typically called the theory, we may need to distinguish claims about historical science from claims of historical theories so as to fix the meaning of what is necessary to predict the future.

So, the need for too many variables in practice to achieve formal sufficiency (the heart of claim 2) does not work as well as the basis of a criterion of demarcation among the sciences. We would have to know a lot more about the relation between formal structure and practical use of theories than is evident from the literature on theory structure to make claim 2 work.

Another criterion moves from claim 2’s generic appeal to knowledge necessary to predict, to a more specific appeal which Sober identifies (in philosophy of evolutionary biology) with Morton Beckner (1959).

3. A theory is historical when it contains at least one historical concept

Sober suggests that Beckner’s chosen example, that physiological theory is historical because it contains concepts of physiological states that are historical, such as «hungry», is inapt. Physiologists are quite comfortable explaining physiological processes with methods drawn from physics and chemistry that (by assumption) do not require any historical concepts. To understand «hungry» as a historical concept one would therefore be thrown back on a version of claim 2, that a theory contains historical concepts because they are the means used to refer to the past, which is necessary to predict the future. But Sober offers another, better example: «adaptation». The now standard reading of (evolutionary) adaptation is that it applies only to traits that have arisen by a «historical process» of natural selection. That is to say, present states of adaptedness are outcomes of natural selection processes that operated in the past.

But Sober rejects claim 3 as well because it implies that many concepts from physics, e.g., acceleration, are also historical and therefore that physics is a historical science. He further points out that the process laws of evolutionary theory, such as concern the principle of natural selection, are ahistorical in the sense of claims 2 and 3, so it is hard to see how appeals to claim 3 can succeed merely by application to concepts regarding the outcome of such processes.

Perhaps the main difficulty with claim 3 is also the reason for the faint hope that it could work. It merely pushes the problem of historicality of theories back to the problem of historicality of concepts. If a criterion of the latter could be produced independently of any assumed meaning of the historicality of theories, then claim 3 might succeed. Sober’s argument against claim 3 simply shows that we have not succeeded in an independent characterization, not that claim 3 fails. Appeal to the historicality of «adaptation» as a concept depends on the assumption that the process of natural selection is historical, perhaps just in virtue of being a process. While that may in fact be true, it fails as a demarcation criterion since it throws us back on claim 2 or claim 1, both of which fail, as we have seen. Unfortunately, I have no further insight on how to characterize «historical concept» independently, so I reject claim 3 as unpromising but not conclusively false.

Finally, we reach Sober’s preferred criterion:
4. A theory is historical if it has a built-in temporal asymmetry. The theory enshrines a difference between the direction from present to future and the direction from present to past.

Sober recognizes that this claim fails to demarcate evolutionary theory from all of physics. Thermodynamics turns out to be a historical science according to claim 4 through its formulation of the principle of entropy. To suggest that this demarcation failure is acceptable, Sober notes the deep parallel between evolutionary theory and thermodynamics that R. A. Fisher developed in his fundamental theorem of natural selection. Fisher (1930) argued that selection causes an increase in mean population fitness in proportion to the additive genetic variance in fitness. Sober, of course, must take care of the objection that mean population fitness need not be maximized by natural selection (e.g., if it is frequency-dependent). Professor Scudo (pers. comm.) points out that Fisher's fundamental theorem is not true in general, but the point here is not about its truth, but about the parallel to principles in physics. Since the same sort of caveat is true of increase in entropy, these objections do not undercut the comparison between biology and physics. And in accepting claim 4 as adequate, Sober acknowledges that some parts of physics are historical.

Once again, a question about practice arises. The highly mathematized, clearly formulated evolutionary theory of which Sober speaks is tethered at very few points to data about natural populations. In fact it is most successful only in highly constrained laboratory populations and abstract mathematical models. True, claim 4 works as a criterion applied to this idealized, abstracted theory and I am willing to accept it as such. But it teaches us little about evolutionary theory or evolutionary science to call an abstract model historical. Moreover, if natural selection in nature is almost always frequency-dependent, then analysis of the theory has only a tenuous grip on the practices of evolutionary biologists trying to understand evolution in nature. This not to say that the theory is not a triumph, but only that the worries raised above are still in play. Moreover, the theory is only remotely connected to the practice of systematics, and most definitely not connected to cladistic or phylogenetic patterns through successful, precise predictions. So judgment on the basis of acceptance of claim 4 that evolution is a historical science rests on the still unjustified claim that the mathematical theory of evolutionary genetics is the «core» of evolutionary theory.

Worse still, as I noted above, it is not clear that systematics has a central theory in the way that evolutionary genetics does, though I would certainly agree with Ghiselin (1969) that Darwin's theory of descent with modification and his principle of natural selection are core if anything is. My worry here is with the concept of «core», not of «theory» — we don't have a thorough understanding of the relation between theory and practice to justify the linkage required to sustain claim 4 as an analysis of historical science. My point is that a demarcation criterion like claim 4 may never be directly applicable to a science like systematics unless its application to the core theory helps us interpret systematics practices as well. Such help may eventually be produced, but the indirect application of claim 4 solely through the rhetoric of evolutionary synthesis and core looks rather dubious.

One thing claim 4 has going for it is a certain similarity to views expressed by Stephen Jay Gould (Gould, Gilinsky & German 1987). Gould argues, going back to the heyday of «nomothetic paleontology» and random clad studies by the «MBL Group», that the data of clade diversity show historical directionality. Gould et al. argue that clades that originate early in the history of a larger group tend to be bottom-heavy, i.e. to have more members in the first half of their temporal duration than in the second half. (Put differently, that their «centers of gravity» are at less than half their duration, measured by first and last appearance in the fossil record). Clades that originate late in the history of a given larger group tend to be «neutrally bouyant» or top-heavy. Thus the history of a larger group has a signature in the statistics of clade shape at any given time-plane. Thus the data themselves show directionality.

If there were a theory of clade diversity that explained the phenomenon discussed by Gould et al., then it would probably be a theory which satisfies claim 4 and would therefore be a historical theory. I do not think that such a theory currently exists. But more importantly there is some ambiguity in claim 4. The first sentence of the claim requires that «it», the theory, has the built-in temporal asymmetry. But since theories are abstract objects (on the usual philosopher's reading, which is tacit in the covering-law model of explanation), it is ambiguous to say that the theory has a temporal asymmetry built-in. Presumably this is clarified in the gloss in the second sentence: the theory enshrines a difference in two temporal directions, i.e. the theory describes or entails an asymmetry in nature. But the second sentence is also ambiguous. It either means that the theory refers to a temporal asymmetry in the phenomena, or that it makes some claim or assumption about the nature of time itself. If we assume the former, then claim 4 seems to be in line with the empirical claim of Gould et al. The data shows directionality, so a proper theory of clade diversity, if we had one, would satisfy claim 4.

But now we are left with a troubling counterfactual application of claim 4 to a science in order to argue that a theory is historical: if the data of a science have a built-in temporal asymmetry, and if there were a proper theory for such data, then the theory would be historical. This is clearly inadequate, because it does not follow from the temporal asymmetry of the data that every theory of them would satisfy claim 4, and how are we to distinguish counterfactually the ones that are historical from the ones that are not? What pressure is there from nature on our scientific interests, such that we formulate a theory historically so as to «mirror» this aspect of the phenomena? Mirroring nature is something scientists may or may not wish to do with their theories.

In the light of this lack of a proper theory of clade diversity, and of a meta-theory sufficient to infer that a proper theory of clade-diversity would meet claim 4, one might be tempted simply to cut theory out of the picture by altering claim 4:
4*. If the data have a built-in temporal asymmetry, then the science that includes those data is historical.

But claim 4* is suspect as well. Thus far we have only seen difficulties in extending criteria of demarcation for historical vs. ahistorical theories to sciences. By switching to the data of a science from the nature of a theory, we no longer have conceptual resources enough to judge whether claim 4* is satisfied. Why, for example, couldn’t we adopt Sober’s criticism of claim 1 along with the theory that time is asymmetrical to argue that all sciences of the physical world are historical? Then every pair of data points referring to distinct moments of time will exhibit temporal asymmetry: one datum is earlier than the other. We do not require some further «signature» in the data to know that data necessarily exhibit temporal asymmetry of this fundamental sort. Indeed, the very idea of data from different moments in time implies temporal asymmetry. But if we want to rule out the temporal asymmetry of time itself as the relation through which data are considered to satisfy 4*, how can a restriction be made? What makes one asymmetric relation relevant and another not? What makes a theory or a science historical, in short, is not the nature of the data, but what scientists do with them.

Thus, I conclude that the four options discussed by Sober et al. are not satisfactory. Let me add two more: one that, like the others, is unsatisfactory on conceptual grounds but nevertheless interesting because it results from considerations of problems fundamental to systematic biology, and another that does the work I want it to, though it will probably not satisfy philosophers as a respectable criterion.

Philip Kitcher (1989) revisits the question whether species are individuals or classes (sets) from the point of view of formal logical issues. Kitcher offers a criticism of the species-as-individuals view that leads to an interesting possibility for a criterion of what makes a science historical. Kitcher does not develop such a criterion per se, nor is that an aim of his essay and critique of the Ghiselin-Hull species-as-individuals theory.

In criticizing the species-as-individuals theory, Kitcher tries to disentangle what he sees as a confused and misleading claim, that it is a thesis about the ontology of species, from what he sees as an interesting thesis about what makes individuals historical. Since species-as-individuals theory entails that species are historical individuals, Kitcher expects it to formalize a position on historicity. I will use Kitcher’s formulation of such a position below to state a criterion of historicity for a science.

Kitcher introduces a concept he calls «historical connectedness» in order to say what he thinks is at the heart of the answer to the interesting problem posed by the Ghiselin-Hull theory. He offers two formulations, one in the idiom of mereology (the logic of parts and wholes or individuals), and one in the idiom of set theory (the logic of members and sets). Kitcher draws on two alternative logical schemes because he thinks that Hull’s arguments do not suffice to show that species are not sets. This does not mean that Kitcher has shown that species are sets, but only that he thinks Hull has not shown that they are not. By offering a formulation in terms of mereology and one in terms of set theory — logical schemes with different ontological assumptions — Kitcher maintains neutrality on whether the Ghiselin-Hull theory is about the ontology of species.

The mereological version of Kitcher’s criterion of historical connectedness is as follows:

...we conceive of an individual with organisms as parts to be historically connected just in case for any organismal parts x and y such that x precedes y and for any organism z, if z belongs to a population that is descendant from a population containing x and that is ancestral to a population containing y then z is also part of the same individual as x and y. (Kitcher 1989, 187.)

The set-theoretical version is:

A set of organisms is historically connected just in case it satisfies the following condition: for any organisms x, y and z, if x and y are in the set and if z belongs to a population that is descendant from a population which has x as a member and that is ancestral to a population that has y as a member then z is in the set. (ibid.)

Kitcher goes on to develop an interesting argument for the claim that if the Ghiselin-Hull theory that species are historical individuals means that species are historically connected entities, then their theory is incompatible with Mayr’s biological species concept. Kitcher argues that the biological species concept is compatible with species not being historically connected, and since his formulation of the Ghiselin-Hull theory implies that species must be historically connected, there is a contradiction. While I do not think the argument is sound, an interesting criterion for what makes a theory or a science historical can be constructed using Kitcher’s criterion of historical connectedness:

5a. An entity is historical if and only if it is historically connected

5b. A theory (or science) is historical if at least some of its objects are historical entities

Unfortunately, there is an equivocation on the meaning of the term «population» at a critical point in Kitcher’s argument that Ghiselin-Hull theory, on Kitcher’s interpretation in terms of his criterion, is incompatible with the biological species concept. This equivocation raises doubts about the general utility of historical connectedness as a criterion of historicity. The source of the trouble is that nothing follows from Kitcher’s analysis about the relation between species and their members or parts without some further specification of the relation between populations and historical individuals. Kitcher has chased the problem to the level of populations, but has not sufficiently analyzed the concept of biological population for his argument to go through.

Because of this problem, I do not think 5a + 5b is a successful criterion of what makes a theory or science historical. But I have no general argument to show that a definition of historical connectedness cannot serve as the basis for a concept of historical entity. I have only claimed that without clarification of the concept «population» and the relation between population and species, Kitcher’s use of the criterion fails as a criticism of Ghiselin-Hull theory. Extension of 5a + 5b to the concept of population may result in an adequate criterion. This would address the goal of proponents of claim 4 by identifying the source of historicity of a theory or science with properties of
the objects of study, rather than with the methods or concepts, but by identifying a particular property other than temporal relation per se and thereby solving the problem with claim 4 raised above. But absent the extension, there is much cause for skepticism: the species problem is hard enough, and now the equally hard problem of population has been added to the task.

Where do these considerations leave us? The first four criteria are out and number 5 is open to doubt. Good-faith attempts to ground historicality in reference to multiple points of time, to prediction, and to directionality in the data have failed and the last, connectedness in time, has failed to establish temporal order in the phenomena definitively. I am led to try another avenue, one that ignores theory altogether and focuses on the historicality of a science.

Before characterizing historical science, I must introduce the concept of narrative sentence (Danto, 1985, ch. 8). To the extent that the analytical philosophy of history illuminates the general problem of what constitutes history, it will illuminate the concept of historical science. Danto's view of the role of narrative sentences in understanding history is significant for my argument: "My thesis is that narrative sentences are so peculiarly related to our concept of history that analysis of them must indicate what some of the main features of that concept are." (Danto 1985, 143).

My analysis of historical science is framed in terms of the concept of narrative sentences and a pragmatic criterion related to their presence in a science. As such, the definition of narrative sentence becomes part of the criterion of historicality of a science.

6a. A sentence is narrative if it refers to at least two time-separated events, but only describes, i.e. is only about, the earliest event to which it refers.

Narrative sentences have a "teleological" character in that they refer to events in the future of a given event in order to describe and interpret the significance of the event. The sentence, "Malthus developed the basis for Darwinian evolutionary theory in An Essay on the Principle of Population," is narrative. It is about Malthus at the time of writing of this essay (published in 1798), but it refers in addition to something in the future of that event, Darwin's working out (in the 1830s) and publishing (in the 1850s) his theory of evolution by natural selection. The sentence describes Malthus in terms that lend significance to his writing due to events of which Malthus could not have known at the time. It selects his writing as significant among contemporaneous events in virtue of what happened later.

An important contrast between narrative and non-narrative sentences in biology stems from a grammatical difference between the relation "ancestor-of" and the relation "descendant-of." The sentence "a is an ancestor of b" refers to two objects, a and b, and b is in the future of a, and a is the subject of the sentence, which is therefore narrative. In contrast, the sentence, "b is the descendant of a" refers to something in the past of its subject and is therefore non-narrative. The grammar of narrative sentences of the kinds found in systematic biology has not been well explored from a logical point of view, and I only mention it here to indicate problems and subtleties that await, especially for concepts like Kitcher's historical connectedness that refer to genealogical relations.

With Danto's concept of narrative sentence in mind, I can now define "historical science." 6b. A science is historical to the extent that it admits narrative sentences as contributing to understanding.

Unlike claims 1-5, claim 6 (6a + 6b) differentiates explicitly between a historical science and a historical theory. This results from the concept of admission indicated in claim 6b. I have not specified what it means precisely for a sentence to be admitted into a science and I reject the idealized characterization of science as a "body" of knowledge and theory as a set of statements or sentences, but there is some sense in which the sentences circulating orally and in writing - among a community of scientists - are admitted into the narrative. I want also to distinguish between admission into a science and admission into theory (minimally because contradictory sentences may plausibly be admitted, consciously, into the former and not the latter). To be sure, there may be disagreement as to which sentences are admissible or have been admitted, and even lack of cooperation in the admission process, but admitted sentences are nevertheless "in play."
systematics are clades, and these «branched pieces of the evolutionary tree» lack some of the key properties of the sort of individuals that typically serve as «central subjects» in human narratives (O’Hara 1988, 152). Terminal taxa in a tree do not have continuity one with another, they are linked only by common ancestry. They also do not always have distinct endings in time, a property that O’Hara calls «closure». He observes that recognition of paraphyletic taxa is a means of imposing an artificial closure on an evolutionary group to «minimize the cladistic aspect of evolution and maximize the linear aspect» (ibid.).

Admission of narrative sentences as a criterion of historicity is intended not to prejudice the linearity or non-linearity of narrative. Since 6a only requires that a narrative sentence refer to at least two different times and that it be about only the earliest time to which it refers, nothing is implied about whether narratives must be linear or not. Whether a science, like systematics, is historical, is thus a distinct question from whether its narratives are or should be linear. O’Hara’s «tree thinking» is compatible with a variety of conceptions of historicity and narrative structure.

This is important because it is open to question whether O’Hara’s distinction between evolutionary history and evolutionary chronicle can be sustained. Danto characterizes chronicle as distinct from history as:

...just an account of what happened, and nothing more than that... The very best kind of chronicle [which gives all the details] would still not quite be history in the proper sense... Proper history regards chronicles as preparatory exercises. Its own task is rather concerned with assigning some meaning to, or discerning some meaning in, the facts allegedly reported by chronicles. (1985, 116).

Following Danto, O’Hara claims that «Systematics is the discipline which estimates the evolutionary chronicles» (O’Hara 1988, 144; emphasis his). And evolutionary chronicle is the description of a series of events without accompanying «causal statements, explanations, or interpretations» (ibid.). I can accept, as does O’Hara, the distinction between history and chronicle as it is developed in the philosophy of history, but if this distinction is pressed into systematics along certain lines it will, I think, run into problems

If O’Hara’s view is that cladistic analysis is to phylogenetic reconstruction as chronicle is to (narrative) history, then I disagree with his analysis on two scores. First, I think the practices of systematists tell against a distinction O’Hara tries to draw between human historians and systematists. He writes,

In contrast to the historian, the systematist performing a cladistic analysis is trying to use all available evidence to estimate the position of as many evolutionary events as possible; he is not trying to construct a narrative account of a selected set of those events. (146-47).

Sober (1988), following arguments by Felsenstein, suggests that the question of available evidence is a complex one in systematics. The traditional view of advocates of parsimony methods for cladistic analysis has been that only shared derived characters (synapomorphies) provide evidence of cladistic relationships. But Sober and Felsenstein have both argued through consideration of maximum likelihood methods that there are evolutionary circumstances in which shared ancestral characters (sympleomorphies) can also provide cladistic evidence. It is irrelevan here to try to decide which view of cladistic methodology is correct. My point is simply that what counts as available evidence is a negotiable matter in systematics. At the very least, choice of parsimony as a method does not lead to interpretation-free descriptions, so cladistic analysis produces something more theoretically charged than chronicle.

Second, while I agree that cladistic analysis aims at something prior to evolutionary narrative in the way that chronicle preceeds history, I think it is false to say that in performing cladistic analysis a systematist avails the kind of «selectivity essential to historical narrative. The «out-group method» for determining character polarity, for example, involves a selection from among a number of possibilities. But the selection process here is buried in the «craft work» of systematists that is not usually considered part of the cladistic analysis per se, e.g. in knowing or having hunches about what would make suitable out-groups, which is tantamount to a judgment of meaning or historical significance. Professor Urbani makes the even stronger claim that the simple choice of a statistical computer package performing maximum likelihood or parsimony methods is a declaration of faith in a given school of thought and that practicing systematists are well aware of such craft commitments (pers. comm.). My argument is with the artificial separation of craft work from high theory in the philosophical analysis of systematic practice. Moreover, Sober and Felsenstein have both vigorously argued that in order to do cladistic analysis at all, some model of character evolution must at least be tacitly assumed (Sober 1988, ch. 6). This imposes interpretive constraints and selection of data, based on events in the future of the branching sequences that cladistic analysis seeks to «estimate». For example, in assuming a neutral rate of molecular substitution, a model of character evolution refers to a process spanning the whole temporal duration of the clade being analyzed.

For these reasons, I think it will be difficult to pursue «a new philosophy of evolutionary biology», as O’Hara desires, «which reflects the discipline’s historical nature», unless the practices of the discipline are articulated along with the central theoretical constructs that are usually identified with the core or essence of a discipline. I suspect that O’Hara agrees with this conclusion: he has begun the critique of such disciplinary practices (see his 1990, 1992), focusing on the character of evolutionary narratives (see also Landau 1984). In systematics there is a tendency to equate cladistic analysis with only the final step of running a computer program on a data set to produce trees. This is misleading about cladistic practice because most of the selection and a substantial amount of interpretation are implicit in the so-called «methodological» choices already made before a particular computer package is run. Neither do such methods thereby constitute a theory. It is therefore desirable to take as primary the historicity of a science so as to include all of the discursive practices besides those nominally connected with theory-structure, and to characterize the historicity of a scientific theory derivately.

Claim 6 entails nothing that compels evolutionary biologists to treat their science historically and nothing that precludes physicists from doing so. Histori cality is a property that a science has in virtue of the pragmatic commitments that scientists make in
doing science. As such, it has no logical or physical necessity. Indeed, it doesn't even have social or cultural necessity; it is possible to practice a historical science ahistorically and an ahistorical science historically. Pragmatic commitments are negotiated parts of the social order of scientific practice, not fragments of epistemology or metaphysics.

To put this point in terms of the foregoing review of various attempted analyses of historicality, one need not refer to multiple points in time in order to describe a given moment of time. Failing to do so, however, leads to singularly uninteresting science, as the logical positivists showed with their protocol sentences as exemplary of ideal science ("Otto says a red patch in the upper left quadrant of his visual field at 4:05 pm, Thursdays"). Their thought experiment in ideal science showed that not even the level of commitment to temporality required by claim 1 is logically or epistemologically necessary, but such commitments are pragmatically necessary if science is to be interesting.

One need not invoke the past in order to predict the future either. Claim 2 says nothing about how adequacy conditions for successful prediction are arranged in conjunction with conditions for constructing predictions, and even Hempel's symmetry thesis, which brings prediction under the same umbrella as explanation, is mute on this. So, there is a wide field for deciding to refer to the past in formulating predictions and their acceptance. Perhaps some general regularities will be discovered in the prediction-forming, -testing, and -judging habits of scientists, but I doubt it.

One need not invoke historical concepts in order to formulate scientific theories, either. In the discussion of the concept of adaptation above, the claim that this concept is historical was discussed. But even if accepted at face value, it is still open to biologists to operate without the concept. Using adaptation as an evolutionary concept requires commitment, one that neutral mutationists, for example, have tried at times to do without. It is certainly open to evolutionary biologists to change the scope of their science to exclude, as physicists have done, phenomena that seem to require concepts that do not meet their methodological standards of taste.

One need not have intrinsic directionality in the data in order to commit to interpreting data historically. Temporal asymmetry is automatic in data, as I suggested, just in virtue of the fact that time itself has a direction and events and objects are located in time.

Finally, one need not have historically connected objects as subjects for a science to be historical. If Kitcher is right that the biological species concept allows species to be historically unconnected, then in so far as evolution was a historical science when the biological species concept was uncontested, it does not need historically connected objects. On the other hand, if it turns out that Kitcher is wrong and the biological species concept implies that species are historically connected, it is still plausible to think that evolutionists could proceed without their science being historical. They could, for example, embrace with enthusiasm what Dobzhansky reluctantly accepted as a working hypothesis in 1937: evolution is change in gene frequency. This commitment to the much maligned bean-bag genetic view of evolution, coupled with the view expressed in criticism of claim 2, that population genetics is just one more dynamical theory (and not thereby historical), would suffice to allow evolution to be practiced as an ahistorical science.

I doubt that my pragmatic criterion 6 will be warmly received. Certainly my criticisms of the other criteria do not constitute a rigorous argument in favor of my preferred criterion, though others reflecting on systematics have championed a pragmatic criterion of explanation, if not of historicality per se (see Ghiselin 1969, p. 29). Claim 6 seems to admit a rather literary quality of historical science that many have been at pains to avoid, for one or more of the reasons I discussed. But one virtue of the pragmatic criterion is that it highlights something the others have in common: they all try to find properties that make a theory or science necessarily historical. While I do think that the sciences can be demarcated, one from another and from other parts of society and culture, I do not think that seeking a necessary condition for a distinction among sciences as the first four criteria do is the right approach. The pragmatic criterion focuses attention squarely on two things: the social process of commitment and the nature of historical narratives. Many of the reasons for labeling a science (or theory) historical can be addressed by investigating a central feature of narratives: they assume a periodization of history which serves as a theoretical model in narrative construction. It is this aspect construction of theoretical models, that puts historical science on an equal footing with ahistorical science and defeats the hierarchical view of scientific authority. But natural historians have not articulated the view that their science is fully theoretical (Griesemer 1990). Analysis of the structure of theoretical historical models would go far toward answering the critics (Griesemer ms).

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