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THE SCIENTIFIC UNDERCURRENTS OF PHILOSOPHICAL NATURALISM

Sergio F. Martínez
Universidad Nacional Autónoma de México[1]

ABSTRACT

Naturalism refers to views that consider philosophical method to be continuous with the methods of science. Most often the discussion centers on the characterization of the sort of continuity that is relevant for characterizing naturalism, and thus it is assumed that naturalization takes places with respect to a given discipline. My aim is to argue for a characterization of naturalism distinguished by the capacity of mutually supporting explanations to produce better and more encompassing explanations. Thus, such account of naturalism relies on attributing epistemic importance to the capacity of different explanations for mutuallly supporting each other, not as a consequence of a perfect fit, but through a process of accomodation that takes place in time and involves considerations that are crucial to evaluate its rationality. The issue is not supplementation or replacement of philosophical method as a whole. Naturalism is not one master stroke of a brush, but a long process of subtle strokes promoting scientific understanding.

Introduction. Broadly speaking, naturalism refers to views that consider philosophical method to be continuous with the methods of science, implying that at least some scientific methods have an impact on whatever philosophy can say about the norms of inquiry. When naturalism is used as a model for epistemology one talks of naturalized epistemology. Similarly, naturalized philosophy of science indicates a philosophy of science that is continuous with science. How to understand such continuity is a major source of controversy in epistemology and the philosophy of science.

The continuity in question is usually understood as having two different sources. On the one hand, this continuity is seen as a consequence of the realization that knowledge and justification are psychological concepts that cannot be understood through mere logical analysis. This idea can be elaborated in several ways. One possibility is to say that there is no philosophical theory of knowledge over and above natural science which spells out the methods of inquiry and thus can be used to decide the epistemic status of scientific claims. This is a strong view of continuity that simply replaces the traditional theory of knowledge for scientific method. Another possibility

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is to claim that continuity requires not replacement of the philosophical theory, but supplementation with scientific methods.

The other traditional source of continuity is associated with the recognition of the failure of logical positivism to provide the basic framework for understanding science, and in particular, with the recognition of historically minded philosophers of science that scientific methods are not *a priori* and that we have to give due importance to the history of science and other empirical studies of science in order to provide a substantive basis for the philosophy of science. In contemporary philosophy of science, the most interesting proposals blend both sources of continuity into an integrated account.

Philip Kitcher, for example, wrote a well known long paper entitled "the naturalist return" (Kitcher 1992) and a book, The Advancement of Science (Kitcher 1993), in which he combines the acknowledgment that epistemology has to be naturalized with the denial of the a priori nature of scientific methodology. Kitcher elegantly integrates the history of science in his account, but following the logicist tradition (and Hempel in particular) he considers that logical analysis is sufficient in order to identify the structure and typology of the psychologically instantiated arguments that are important in uncovering the structure of scientific advance. *The continuity between science and philosophy for Kitcher, then, is grounded not only in empirical methods but in logical analysis of the forms of argumentation that are taken for granted.*

Such a view is compelling because it is accompanied by the idea that scientific progress can be modeled as the accumulation of significant truths about the world. In 1993, Kitcher thought that significance was an objective property of truths. This makes the view plausible that it is not important how we arrive psychologically at those significant truths, or, at least, it is not important for the philosophy of science. Kitcher claims that his account is naturalistic because it gives weight to the history of science and the history of methodology in order to identify significant truths and the typology of the relevant (psychologically instantiated) arguments. But as Kitcher himself has ended up recognizing, significance only makes sense within a context, and thus the typology that is taken as a fixed point in his approach demands to be anchored.

I will argue in this paper for a different approach to naturalism, one less concerned with the history of the question within the philosophy of science, and more interested in developing a philosophical view that I think is implicit in the way science advances. This is an approach, which, like Kitcher's, acknowledges the importance of the structure of explanations as a guide for naturalism. But the source of the relevant continuity will be located elsewhere. The continuity that matters for what I will call *scaffold naturalism* is to be found in the mutually scaffolded structure of explanatory practices; the source of normativity that such naturalism aims to characterize is to be found in the way such practices come to integrate heterogeneous concepts and representations into scientific understanding.

The usual way of thinking about continuity is too much dependent on concerns arising from the non-naturalistic (logical empiricist) past of the philosophy of science. Naturalization should not be seen as taking place against the background of a given discipline. Quine thought that epistemology should dissolve in behavioral psychology

(Quine 1969), Goldman thought that epistemology should be naturalized with respect to (a versión of) cognitive psychology (Goldman 1988), sociologists of science claim that naturalization of the philosophy of science means interpretation within the explanatory framework of one sort of sociology or another. These reductive approaches to naturalization have no doubt something important to contribute to epistemology and the philosophy of science, or at least to sociology, but we are missing something crucial if we do not see that naturalization has a more integrative epistemic dimensión that has roots in the way different explanations look for mutual accommodation and thus serve as mutual scaffolding supporting better explanations. The issue is not supplementation or replacement of philosophical method as a whole. *Naturalism is not one master stroke of a brush, but a long process of subtle strokes better characterized as an evolutionary process of the interaction among practices which by comparing and constraining the scope of models, concepts and explanations, promotes its integration.* Such integration, that sometimes involves replacement, sometimes supplementation and at others a tweak of methods and norms, serves as a scaffold for further diversification (and specialization) of the tapestry of scientific practices. Since scaffold naturalism avoids reductive assumptions about the ultimate source of epistemic legitimacy, it should be seen not as a straight jacket for epistemology, but as a horizon of epistemic normativity stretching across the backdrop of our scientific understanding of the world.

One basic idea behind scaffold naturalism is that the naturalization of philosophy of science is closely related to the sort of integration associated with the search for understanding. Understanding is no doubt a main epistemic aim of science. In the logicist tradition understanding has been undervalued as an epistemic aim because it is considered to be merely a psychological phenomenon. But this is not something that should worry us. Nowadys it is widely recognized that epistemology cannot be divorced from psychology. Similar motivations have led us in the philosophy of science to dismiss understanding as an aim of science. One can argue that understanding, maybe even more than knowledge (in the sense of justified belief), is an epistemic aim in science. Most approaches to understanding characterize understanding as a distinctive epistemic virtue going beyond explanation. For example, understanding is taken to consist in the virtue of unifying explanations. Rather, in my sense, *understanding is an emergent feature of our mastering of different explanations and the way they support each other (as this feature gets embodied in practices).*[2]

In the philosophy of science the topic of understanding as a distinctive epistemic aim is a recent topic of discussion, but scientists have often talked of understanding as a major aim of science. This is not only the case in the social sciences. Darwin, in *On the Origin of Species*, aims to understand, not just to add to stores of knowledge. As Einstein famously (is said to have) said, every fool can know, the point is to understand. And it is not hard to find contemporary scientists recognizing the importance of understanding as an epistemic aim. Such recognition often makes use of an assumed relation of understanding with reductionism. But we should read further before prejudging their concept of understanding.

[2] In this paper I will not elaborate further this notion of understanding. This is not required for the argument of this paper. I hope the examples in the following sections leave sufficiently clear in what sense I take understanding to be an epistemic value. For a congenial argument for the importance of understanding as an epistemic aim see Elgin 2007. For recent collection of approaches to the issue of understanding in the philosophy of science see De Regt et al. 2009.

For example, Regev and Shapiro believe that the distinctive mark of scientific understanding is the reduction of phenomena into simpler units. But reduction is not associated with ontological monism, but with *the identification of the right abstractions, being those which allow for the integration of very different phenomena into more general and more tightly related explanations* (see Regev and Shapiro 2000).
From this perspective that takes seriously understanding as an epistemic aim, naturalization is first and foremost a philosophical attitude towards different ways in which the diversity of methods and explanations can be productively integrated into understanding. Such view goes against a deeply ingrained belief on ontological monism. Peter Gintis, for example, has been arguing that the social sciences are defective because they study human behavior from different perspectives that are not consistent to each other (Gintis 2007). He assumes that progress is related to the stablishment of a unified theoretical framework that would integrate the social sciences and thus dissolve the inconmensurabilities associated with the use of different ontologies and representations. But this is not the only way of thinking about scientific progress and naturalism, as we have just seen. And in the social sciences there are several interesting proposals that go beyond such accounts of progress and naturalism. Sperber in (Sperber 2011), for example, argues that a naturalization of the social sciences demands the ongoing naturalization of psychology, and that such naturalization does not require the flattening of ontologies. The ontologies of a naturalized social science would articulate a naturalistic description of mental and environmental events using heterogeneous concepts and representations. Heterogeneity of concepts and representations does not go against their capacity to integrate different ontologies.

2. In the preface to her book (Maddy 2007), Maddy says that when she set out to write on the subject of naturalism in mathematics, she assumed that everyone knew what it means to be a naturalist, and that her job was to show how to extend this idea into mathematics. What she discovered was that everyone, naturalist or not, had a different idea of what naturalism requires. We should not be surprised by such diversity of opinion on naturalism, since the overall tendency is to develop naturalism as part of a philosophical tradition in epistemology and the philosophy of science which from the 19th century and until very recently was interested in legitimazing its non-naturalism. An implicit claim imposed upon naturalism is that it should be consistent with a minimal non-naturalism that different authors formulate in different ways but which always includes an assumption as to the homogeneity of science. It is assumed that it makes sense to ask about the continuity between science as a whole and philosophy. The human beings that do science and philosophy are bypassed. However, if as I am arguing, our point of departure are the scientific discussions that can inform us about human nature (coming from biology, the cognitive and social sciences specially) the philosophical task is more clearly in sight. Philosophical naturalism has to follow the scaffolds of our best science, including our best social science, to come to terms with the relevant continuity for naturalism to be not only doctrine, but a guide for doing better philosophy. The underlying issue is more about putting in perspective relations of mutual support between different explanations, with respect to a certain issue, than the usual eliminativism supported by traditional reductionism.[3]

For the purpose of this paper I shall characterize strong reductionism as a reductionism that is committed to ontological monism. Such a label aims to include all reductionistic approaches in which the existence of different kinds of methods and things is not recognized as valuable resource for the characterization of scientific methodology and epistemology (see section 4 below). If we start from the assumption that strong reductionism is the right approach to understanding the way the different realms of knowledge relate to each other, then the problem of naturalism ends in the sort of excluding alternatives that Quine made famous. Either norms have an *a priori* source or we have to acknowledge that psychology (or some other discipline) is all the epistemology we need. But if we do not assume such strong reductionism, *naturalism is better approached as aiming to the construction of perspectives from which the diversity of modes of organization of practices (and their implicit and explicit norms) can shed light on explanatory depth.* This means giving suitable importance to the way in which cognitive resources get displayed socially in practices and traditions of inquiry and also to how such practices and traditions merge in a productive manner to generate, on the one hand, overarching scientific explanations and, on the other, specialized knowledge often related with the production of technological advances.

3. Traditionally, it is considered that the two main difficulties relating to a naturalized philosophy of science are circularity and the problem of normativity (or alternatively, the problem of philosophical irrelevancy*). Circularity* elaborates on the point that the use of scientific methods to investigate scientific methods is circular; whatever the evidence that we take as the point of departure, we are required to use criteria or norms of inquiry that it would be part of the business of such methods to discover. *Philosophical irrelevancy* refers to the issue that a naturalized study of science could, at most, describe scientific methods, whereas philosophy of science should have a say in how science is carried out.

Lakatos and Laudan are seen to start a discussion of models of scientific change that can give weight to the salutary criciticisms of naturalism towards logical positivism, while at the same time avoiding such difficulties. Metamethodology allows us to have a rational decision-making method about the relative merits of research traditions, and thus overcome circularity and irrelevancy. Laudan's approach has changed over time, but the underlying assumption remains, and it is a good example of the usual sort of strategy employed to resolve the difficulties of naturalism: philosophy of science can be studied without entering into the messy territory of particular scientific disciplines and specific discussions in the sciences as to the nature of our cognitive capacities, its relation to their evolutionary history, and the way they play a role in the kind of inquiry we call science.

The recognition of a metalevel (by Lakatos and Laudan), the typology of arguments assumed by Kitcher, or the typology of models assumed by Giere (in Giere 1985, for example) function in a similar way in regard to the grounding of the more usual

[3] In Martinez 2011 I defend there a view of (non)-reductionism as an important aim of science that is closely related to the view of naturalism that I present here.

versions of naturalism. They provide a stopping point for what is considered a threatening circularity and also allow their proponents to overcome the second difficulty. Normativity has its origin in the explanatory power of arguments or explanations grounded on such fundamental typology. But there is a problem with this strategy. Why should we expect actually displayed arguments or explanations in the sciences to fit these typologies? All of these authors appeal to the history of science to justify their point of departure. But such use is questionable. There are several important rebuttals of the way Lakatos and Laudan want to use the history of science for their own purposes. Kitcher integrates the history of science in a much more sophisticated way in his model of science and furthermore recognizes the importance of scientific practices in his account. Here I will concentrate on showing problems with the way Kitcher uses the history of science to gift wrap his views on naturalism.

Kitcher claims that the theory of evolution by natural selection formulated by Darwin quite rapidly generated a core consensus. For him, *On The Origin* provides naturalists with good reasons for accepting minimal Darwinism (the belief in natural selection as a plausible mechanism explaining the origin of species). Kitcher suggests that there is a well formed and clearly delimited argument that goes along with this belief and which leads to changes in views in widely different fields; the acceptance of this minimal argument has led practices to be modified taking minimal Darwinism in consideration; and when this has not happened, resistance can be explained by the importance of exogenous constraints on individual rationality associated with, for example, personal, professional and intellectual allegiances.

One important problem is that the history of Darwinism does not support such a neat account of what happened. There were many versions of Darwin's theory and important discussions as to the scope of these versions. Robert Richards, for example, has argued that "Darwin crafted natural selection as an instrument to manufacture biological progress and moral perfection" (Richards 1988), and that in this regard, Darwin's theory does not substantially differ from Spencer's views. Indeed, it seems that many contemporary naturalists accepted Darwin´s theory as a variant and more sober version of Spencer's view of evolution as a cosmic process (see Martinez 2000). Miriam Solomon has argued against Kitcher's account of the reception of Darwin's theory pointing out that contemporaries did not (as Kitcher claims) modify their practices and start producing the sort of arguments that, according to Kitcher, are associated with the acceptance of minimal Darwinism. She indicates-correctly I believe- that Darwin's supporters and opponents were not always fighting the same battle, and that they use all sorts of routes to reach their different positions. There are many overlapping and sometimes conflicting claims being supported by different kinds of empirical work, and by different traditions of inquiry often related to different disciplines, that do not lend themselves to a simple comparison and more importantly, that do not seem to support the view that progress should be identified with the accumulation of significant truths. In any case, progress would be seen to be associated with the diversification and specialization of significant truths. But this suggestion would not gratify Kitcher, because he would like to say that there are no competing significant truths as the above idea suggests.

In the case of Darwin, at least, it is far from clear what the accumulated significant truths would be. Only in retrospect, and with specific values in mind, could one argue that research traditions, for example in developmental biology, have or have not contributed to the progress of biology. Whether they are Darwinian traditions or not is a judgement that depends on our views of what constitutes evolutionary theory nowadays. From the perspective of neo-Darwinism these traditions might not have contributed to significant truths, since it is considered a major achievement of Darwin to have separated issues of development from issues of evolution. But from the perspective of contemporary evo-devo or systems biology, things do not look this way at all.

4. From an historical perspective, the sort of naturalism common in the philosophy of science of the 20th century (and in particular in views like that of Kitcher's), looks quite strange. In the 19th century the sharp opposition between science and philosophy that motivates traditional accounts of naturalism was not present. And the continuity between science and epistemology was often framed in terms of the scope of explanations. Ontological and teleological themes and discussions were common and played an important role in the formulation and scope of explanations. From the perspective of the sort of methodological fundamentalism that is pervasive in 20th century philosophy of science, the sort of fundamentalism promoted by philosophers as diverse as Laudan, Popper and Kitcher (at least in some of their work), epistemically distinctive features of science can be understood in terms of methods or typologies (or differences among them) in such a way that discussions about ontology or explanation can be bypassed or blackboxed. From this perspective it seems clear that there has to be a metalevel or some common (normative) currency that allows the comparison of methods with respect to epistemic aims independently of context. However, if we look at substantive discussions in contemporary philosophy of science, it is clear that such methodological fundamentalism is not sustained. Take for example discussions about reduction in biology. The traditional view associated with positivism is "theory reduction", according to which the most important relation between theories is a deductive relation between theories conceived of as set of statements generated by axioms and laws. As several philosophers of science have pointed out, not even the canonical example of such a relation, the relation between classical and molecular genetics, fits the model (Hull 1974, Wimsatt 1979). If one goes on to argue that even though the reduction does not take place, and that what matters for philosophy is that such reduction is possible in principle, then the question arises as to why one should think that such in principle reduction is philosophically relevant. There is nowadays a widespread agreement in the philosophy of biology that such reductionism will not do, that the diversity of methods and explanations that enter into the variety of scientific practices that conform biology cannot be reduced to a fundamental theory.[4]

This anti-reductionistic stance supports scaffold naturalism. Discussions about what is a gene or what is an species are more and more often been answered by pointing out that there are different concepts of gene and species that have a place in biology. Pluralism is not only allowed but increasingly recognized as an important resource with which to answer questions in science and in the philosophy of science. When we see to what extent this plurality of methods and explanations goes hand in hand with different ontological commitments, methodological fundamentalism looses credibility. But pluralism seems to lead to epistemic relativism. We seem to be left with a huge variety of ontological claims implicit in widely different explanations that might make us yearn for the simplicity of strong reductionism and methodological fundamentalism. But the risk of relativism is only a mirage resulting from the distance at which philosophers tend to look at science.

Brigand, for example, has provided an elaborate discussion as to how evolutionary novelties (a morphological structure or function featured in a group of organisms that did not exist in an ancestral species) can be explained in contemporary biology (Brigandt 2008). Explanations of novelty involves concepts, data and explanations from different disciplines: classical and molecular genetics, paleontology, developmental biology, biogeography and ecology, among others. Furthermore, there are changes in how different traditions understand novelty. Neo-Darwinists take novelty to be substantial change in an existing structure, whereas evo-devo theorists consider novelty as coming into existence through evolution of structure. Brigandt uses this kind of discussion as the basis for suggesting that the centrality of a (kind of) explanation as part of another explanation depends on the goal pursued. Depending on our explanatory aim paleontology or biogeography might be questioned and the other considered an unshakable point of departure for the explanation. Explanations are used as scaffolds for more general or more complex explanations. Such scaffolds put in perspective the different ontologies used in different disciplinary domains. *Explanation perspectivism and not relativism would be a more accurate way of describing the consequences of ontological pluralism*.

Another example of this sort of naturalism is the discussion about typology in biology. Darwin started the trend of getting typology away from the metaphysics of essentialism but getting away from essentialism has been harder than it was originally thought. (see Love 2008). Love shows how, *within specific scientific practices, one can transform metaphysical thinking into epistemologically sound explanatory reasoning*. As Love puts it, “typology needs to be understood as a form of thinking or reasoning, as conceptual behavior” (Love 2008). The role of typology in biology is closely related to the recognition of kinds of representations crafted in specific scientific practices through carefully weighted abstractions and approximations.[5] The choice of abstractions and approximations aim to promote the integration or alignment with some practices while distancing them from others, thereby fitting the practice within a certain tradition or research program. Love shows that concepts like Protein

[4] See for example Beckermann et al 1992, Horst 2007, Regenmortel and Hull 2002, Mitchell 2003.
[5] For an elaboration of this point see Nersessian 2008 and Martinez and Huang 2011.

domains (in molecular biology) respond to different characterizations: a)units that have stable activity or structure through manipulation; b) structural units that are observed in X-ray crystallography; c) functional units that exhibit a particular activity; as well as many others. These different characterizations are used in different contexts related to specific goals and disciplinary practices. One can think of those contexts as competing with each other, but what is important for us is the end result, the shaping of the scope of the explanation by situating it in relation to many other explanations. In a few cases the result is some kind of reduction. But this is not the rule.

This determination of the scope of the explanation is not a mere identification or discovery, but rather the crafting of a norm imbued with epistemic import. In the next section, we show how this sort of explanatory naturalism relates to a versión of the continuity thesis that was an important element of 19th century science. The rejection of the thesis of continuity as formulated and defended by many 19th century naturalists, and Darwin in particular, played an important role in the development of the social sciences, and moreover in the conviction that the autonomy of the social sciences from biology (and psychology) should be considered an important achievement. This conviction is being questioned nowdays in the social sciences, in biology and in the cognitive sciences.

5. Darwin was convinced that his theory had implications for the social sciences through its implications for understanding the evolution of our cognitive capacities. This thesis is known as the continuity thesis. The second half of the 19th century saw the publication of many books promoting numerous versions of the thesis. Romanes, for example, published several well known books developing the thesis from the 1870s to the 1890s. As part of the delineation of the borders between scientific disciplines that took place at the turn of the century, such a view of continuity fades away towards the end of the 19th century. In psychology, continuity gave place to emergentism and later to behaviorism. The claim by Lloyd Morgan in 1898 that we do not have enough evidence to support the thesis of the continuity of the animal and the human mind is a well known lapidary statement that is a good indicator of the fate of such a thesis for several decades to come. The thesis of continuity was banned as untenable, and several different views took its place. From being a banner of progress, the thesis was seen within a decade as a sign of an old approach that could only hinder the development of a scientific view of the world. In the social sciences and anthropology in particular, the sort of emergentism embraced by Morgan took the place of the thesis of continuity as a guiding methodological principle. The established consensus towards the beginning of the 20th century was that the thesis of continuity was an untenable metaphysical thesis, unsuitable for the development of sound social science. Boas' rejection of evolutionism and the embracing of "historical particularism" is a good example of the way this rejection of the thesis of continuity took place. Even if he were to accept the importance of the mechanism of natural selection and the importance of geographical dispersion as the main forces shaping the evolution of living beings, he would do so in accordance with the rejection of the thesis of continuity. For Boas, as for many of his contemporaries, Darwin's defense of the thesis of continuity (in the Descent of Man), required a view of progress that (contrary to his views in the Origin of Species) pointed to the inevitable transition from instinct to intelligence and thus supported an unacceptable view of human nature. As Wallace famously put it: (since) natural selection "could only have endowed savage man with a brain a little superior to that of an ape" we should reject the applicability of the theory of evolution by natural selection to the explanation of human cognition, since a savage "actually possesses one very little inferior to that of a philosopher" (1870: 356).

Wallace had a point, but independently of whether you are convinced or not by Wallace's argument, I think it should be acknowledged that it makes very clear why the thesis of continuity is at the center of a discussion about the scope of explanations of evolution by natural selection, and about the relation between biology and the social sciences.

Wallace is often mentioned in the history of biology as someone who did not fully understand the scope of his own discovery (as it is recognized, Wallace was, with Darwin, co-discoverer of the principle of natural selection as an evolutionary force). But things are more complicated. Wallace's view was one of the views supporting the advance of the social sciences during the first half of the 20th century. The scope of the mechanism of natural selection had to be crafted in such a way as to allow principles of the social sciences that had strong ethical and political overtones to be maintained. But, this is not the end of the story. During the second half of the 20th century, the discussion about the thesis of continuity came back as part of a crisis in the social sciences and the shaking up of the borders between the biological and the social sciences associated with new ways of extending the scope of evolutionary models, and the rising to prominence of the cognitive sciences.

For example, in anthropology the objective of turning the study of culture into a "scientific enterprise" has been an important motivation for elaborating an evolutionary model of culture. There are two lines of thought that lead to this sort of project. On the one hand, the idea originating in the 19th century that evolution is the most general and fundamental sort of change, which does indeed support a version of the continuity thesis; and on the other, the search to legitimize the social sciences by anchoring their explanations on laws of nature of universal scope, laws that would sustain social sciences' claim to objectivity. *The assumption that evolutionary (Darwinian) biology is grounded on such laws as part of its scientific status leads naturally to the view that a characterization of the scientific status of the study of culture has to be modeled as an evolutionary process subject to the same laws.* This second line of thought does not support the continuity thesis. But the separation between these two ways of promoting the use of evolutionary models of culture is not as clear cut as it should be.

However, this train of thought sets us on a path that has serious problems (Frachia and Lewontin 1999). The longing for generality is certainly related to the search for the intelligibility of human history, but models of cultural evolution, to the extent that attempt "to mimic, for no reason beyond the desire to appear scientific, a theory from another domain… are too rigid in structure to be even plausible" (Frachia and Lewontin 1999 p.78). Indeed, if the explanation of cultural change and stability

has to fit “the reductionist model in which individual actors have more cultural offspring by virtue of their persuasiveness or power or the appeal of their ideas, or in which memes somehow outcompete others through their superior utility or psychic resonance” (Frachia and Lewontin 1999, p.74) I agree. Frachia and Lewontin level much criticism to attempts aiming to extend the scope of Darwin’s theory to the social sciences based on the existence of laws that support evolutionary explanations. But such criticism would not be relevant to explanations that are not based on such laws, as would be the case if we could give credence to some version of the continuity thesis. The way in which Darwin and his supporters (like Romanes) tried to elaborate the thesis of continuity may be incomplete or faulty, but it is not the only way of developing it.[6] The cognitive sciences suggest versions of the continuity thesis that bypass the traditional objections. To start with, once we abandon the idea that “hard science” is based on laws of universal scope, and thus abandon the idea that scientific explanations have to fit big theoretical structures that systematize such laws and ground our generalizations (in the form of explanations or predictions), *models of cultural evolution can be seen to model the technologies of cognition that scaffold both the stability of culture and the sources of cultural innovation*. In this way, models of cultural evolution contribute as much to our understanding of human cognition as to our understanding of human history (see Martínez in press for an elaboration of this sort of model). Several versions of the thesis of continuity are being proposed in cognitive social sciences. But the thesis of this paper does not depend on details of different versions of the thesis of continuity. What is most relevant for us is that once continuity is an issue, the border erected as a metaphysical división between the social and the cognitive sciences through the first half of the 20th century, comes into question. This debate has important implications for philosophical issues, and not the least, on the question of scientific rationality.
In order to better understand what is at stake and the claim I am putting forward, we have to review another important discussion in the philosophy of science in recent decades: the discussion about the nature of scientific rationality and its relation to the “historical turn” in the philosophy of science.

6. Philosophy of science has devoted a lot of effort to discussions about the nature of scientific rationality. As Ian Hacking famously put it:

Philosophers long made a mummy of science. When they finally unwrapped the cadaver and saw the remnants of an historical process of becoming and discovering, they created for themselves a crisis of rationality. That happened around 1960.[7]

[6] There are many alternatives. John Dewey developed the thesis of continuity in several writings. The waning of interest in Dewey’s naturalism in the mid-twentieth century seems to be related to the widespread rejection of versions of the thesis of continuity as a way of advancing sound philosophy and good science. We will suggest a version of the thesis of continuity that is not far from Dewey’s thesis (although I will not elaborate this point here).

[7] Hacking 1983 p.1

The crisis of rationality in question started when Kuhn undermined the traditional view of rationality. (or at least this is the usual story). Many others questioned logical positivism at the beginning of the second half of the 20th century. But Kuhn captured the headlines. I suspect that one major reason for the attention given to Kuhn's ideas, as opposed to alternative proposals, like Toulmin's evolutionary model (which is, in more than one sense, a more elaborated critique of formal models of reasoning) and several others that were published around the same time, has to do with the fact that Kuhn's approach touched on central concerns of the logical positivists, for example, discussions between Carnap and Popper, as well as many others, about the relation between the history and the philosophy of science, and metaphysical or epistemological discussions about nominalism and realism. But Kuhn was catapulted to stardom not by philosophers or historians, but by social scientists and psychologists. Very soon after the publication of the *Structure of Scientific Revolutions*, social scientists in the different disciplines were talking of the need to overcome a pre-paradigm stage, thus allowing the social sciences to reach the scientific status of physics. Kuhn's ideas resonated with the ideas of the sociologist Robert Merton, who had argued for the need to abandon a narrow empiricism and speculative sociology. Merton's claim that sociology should develop specialized theories with a carefully constructed range as the basis for successful generalizations (middle range theories), that in turn could serve as the basis for further generalizations, is not far from the Kuhn's notion of paradigm (the term paradigm was actually introduced by Merton). Thus, the importance Kuhn bears for the social sciences, as several writers by now have pointed out, is closely related to the ingrained positivism in the social sciences and philosophy at that time.[8] In psychology, his influence was also quite important and not easy to understand.[9] Kuhn's ideas were most often used in a self serving superficial way. But not always.[10] In developmental and educational psychology in particular Kuhn was recruited together with Piaget and Vigotski (among others) to support theories of conceptual change like the interactionist theory of Strike and Posner from 1982 (reformulated in Strike and Posner 1992), for example.

As already said, Kuhn is not the first author to question the positivistic ideal of science as a set of theories dealing with very different subject matters but united through the vertebral column of a methodological reductionism. But the "mob psychology" dimensión of his work resonated in several áreas of psychology and education in a rather constructive way. The discussion of paradigmatic science as a kind of doing science in which questions about foundations are left aside and progress is perceived to lie in the solution of relatively well formulated problems, leads in educational

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[8] Bird 2004.

[9] O'Donohue 1993: "The extent to which psychologists find Kuhn so attractive is puzzling given the significant ambiguities and inconsistencies in Kuhn's views, his informal and unsystematic use of psychology, and his disparaging comments about psychology. .."

Coleman and Salamon 1988 found that Kuhn was the most frequently cited historian/philosopher of science, most citations (95%) highly favorable towards Kuhn. In the case of psychology, the reason for Kuhn's fame might be more superficial than in sociology. Dry experimental papers might be spiced up by quoting a philosopher of science.

[10] Nickles has argued that Kuhn's account of exemplars requires bringing in schema theory in the discussion (see Nickles 2000, for example), but Kuhn did play a consructive role in the development of schema theory for several psychologists, and educational psychologists in particular.

psychology to the development of applications of the concept of paradigm as exemplar. Whereas in the social sciences Kuhn is used mainly as leading to the attractive view (at least for positivist-minded philosophers)that a physics-like status is possible for the social sciences to the extent that a new revolutionary way of looking at things was posible. The different appreciation of Kuhn by educational psychologists and social scientists is telling. The widely recognized tension between the two ways in which science changes according to Kuhn is not a mere When seen from afar, such applications of Kuhn's ideas force us to confront the obvious problem that science seems to have two ways of changing. The way in which Kuhn talks in 1962 and the way it is often interpreted is that this "extraordinary" or "revolutionary" way of doing science is not a rational type of change. What happens when paradigms change is that old problems and ways of thinking about the central questions of the field disappear, and a new way of looking at things takes its place. In this case, it is not continuity but replacement which occurs.

The question that has most attracted philosophers' attention is the question of how we can account for this sort of non-continuous, non cumulative change. It seems rather odd to say (as Kuhn was often understood to be saying) that the most significant scientific advances, like moving from Newtonian to relativistic physics, are irrational sorts of changes. Lakatos famously said that Kuhn had reduced theory change in science to "mob psychology". One can argue that Kuhn was simply wrong, that there is no non-cumulative sort of change. One can, for example, argue that in the examples of extraordinary change given by Kuhn, there is a cumulative sort of change, a change that takes place rather fast, but cumulative at any rate (Laudan 1984, Shapere 1984). Alternatively, one can give philosophical reasons pointing to the impossibility of modeling scientific change as Kuhn suggests, unless one is willing to fall into the hole of relativism (Popper for example suggests something in this direction). Or one can try to show that indeed there are two notions of rationality that make sense.[11] Or one can try to argue that rationality is an achievement implicit in the history of science, and thus impervious to anomalies in Kuhn's sense. This can be done in many different ways, including proposals like that of Feyerabend, for whom incommensurability is an anthropological thesis, a basic organizational principle implicit in our conceptual structure, and more especially in the way objects of experience are classified. It would also include proposals like that of Lakatos 1970 and Laudan 1977.

7. But Kuhn's suggestion that there are different sorts of changes in science that are relevant in order to understand science philosophically and historiographically, is worth giving serious attention. This is ultimately the issue of incommensurability and it is a difficult question. If one stays within the straight jacket of methodological fundamentalism, it is not difficult to conclude, as Popper and many other philosophers have done, that talk of different modes of change leads directly to relativism. But if we abandon methodological fundamentalism (and the epistemology that accompanies it) and recognize the plurality of methods and explanatory frameworks that comprise

[11] Godfrey Smith has a proposal in this direction in his 2003.

science, the existence of different modes of change is no surprise.[12] Feyerabend is right in that the thesis of incommensurability is an anthropological thesis, but as we shall see, it is an anthropological thesis in a rather different sense. Godfrey Smith is correct in pointing to different kinds of rationality, but as we shall also see, this point has to be reformulated. There are not two types of rationality, but many, and how to characterize them invites us to adopt a deeply naturalist attitude that takes the empirical study of rationality (in the cognitive and the social sciences) seriously. However, before we come to this, it might be important to emphasize two things about the point of departure. The first is that (contrary to what Kuhn and most philosophers of science assume) scientific disciplines are not a stable starting point from which to discuss the naturalization of concepts like rationality or paradigm. I would like to suggest that the interesting notion of paradigm makes sense as a constraint on the sort of change that is open to scientific practices (thorugh processes of learning and through constructive interactions among practices). This is a notion of paradigm closer to what Fleck called a "style of thinking", and that I prefer to call (by reasons that will be clear later) cognitive style.[13] The second thing is that, in so far as the task of describing cannot be sharply separated from normative considerations, the interaction of efforts and the mutual supporting role of different scientific practices are already part of the process through which the scope of norms and explanations come to be taken as scaffolds for further research.

To illustrate this point, in this section I will review some recent discussions about rationality that suggest how incommensurability can be understood as an expression of different modes of change, and the way such different modes constitute mutual scaffolds for fruitful diversification and specialization of concepts and practices.

The questioning of the concept of rationality based on the theory of expected utilities has been having important implications for the way the social sciences are designed and oriented, and is leading to the blurring of the border between social and cognitive sciences. In particular, recent approaches to rationality, as well as related concepts like cooperation and decision making provide a good example of how paradigmatic thinking is a cognitive phenomenon and how paradigmatic thinking embodies different kinds of scientific change.

Central discussions about the structure of reasoning, the nature of rational thinking and decision making are nowadays carried out at the intersection between psychology, economics and neurosciences (see for example Gigerenzer and Sturm 2011, Bardone 2011, Glimcher et al. 2009, Echeverría and Álvarez 2010). The revolutionary character of those proposals have to be emphasized. Confronted with anomalies (like the famous Allais paradox), one could argue that, as Simon for example suggested several decades ago, the neoclassical models of economics and the

[12] Rouse in (Rouse 2003) elaborates a related point.
[13] Fleck provides three features of his notion of style: 1. common features in the problems of interest to a thought collective, 2.the judgement which the collective thought considers evident and 3. The methods which it applies as a means of cognition (Fleck 1981, p. 99). Styles for Fleck seem to be characterized historically and sociologically, whereas cognitive style in my sense, even if it may be addressing similar phenomena, is characterized cognitively. But this is not meant to deny the sociological and historical dimension that Fleck identifies through his account of style of thinking.

associated concept of rationality worked only under some limited circumstances. Simon approach already involves cognitive considerations, eventhough one can argue that such cognitive components play a rather passive role and can be taken as part of the background conditions in a slightly modified traditional account. This sort of suggestion can hardly be sustained nowadays. Starting with the development of constructive views, like the one developed by Kahneman and Tversky in the 1980s, it became increasingly clear that the anomalies could not be seen as isolated examples or rare cases describing extreme circumstances. Now, as Kuhn and Fleck would predict, this period of "extraordinary science" has led to a diversification of approaches. But what is interesting for us is that such approaches are not transient views destined to disappear inmolated at the door of a new paradigm. What seems to be happening is that the crisis in the standard theory of rationality is giving place to several new fields of study that are consolidating different lines of research through the integration of work practices in different disciplines.

Behavioral economics, for example, was developed as a label for a series of approaches that were united by the idea that models developed in experimental psychology should have a bearing on models of human behavior that would improve the models offered by neoclassical economics. The discussion between behavioral and traditional neoclassical economists spars about old philosophical issues, like the duality of body and mind, but also issues closely related to projects of naturalization; for example, whether scientific explanations, in order to avoid circular argumentation, should rely on normative idealized theories that provide a privileged and uncontested point of departure.[14] What is particularly relevant for us is that the discussion initiated by behavioral economics is a discussion about the normative status of certain idealizations that are being proposed as alternatives to the traditional idealization of *homo economicus*. However, the issue is not whether the new alternative idealizations are true or not, or which one is true: the discussion is about its explanatory scope and stability under explanatory use. Behavioral economics has been criticized because explanations were based on very different models, and there was a perceived need to weed out the variety of empirical models and methods use in inquiry. This is often mentioned as the motivation for the development of neuroeconomics.

The beginnings of neuroeconomy are related to early attempts to interpret intermediate variables used in models of mental processes in term of neuron mechanisms. To this extent, the suggestion is that the neurosciences could provide the sort of normative-explanatory framework required for a more consistent advance in the development of an alternative to the standard theory of decision making. No doubt part of the appeal of neuroeconomics is of course the promise of reducing the modeling of decision making to a "hard science" intelligible (if not reducible) to biology (and physics). But such veiled reductionism, even if it is a hidden motivation behind the recent enthusiasm for neuroeconomics, is not the whole story. Glimcher, for example, argues for an interdisciplinary approach to choice supported by "reductive linkages". The idea is more like the one suggested by Regev and Shapiro (see section 3) than the usual accounts of reductionism in philosophy of science. As a general rule, he says, "it is the structure of the higher-level abstractions that guides

[14] See for example Glimcher et at. 2009.

the more reduced-level inquiries" (Glimcher 2011, p.126). Thus, neuroeconomics is not meant to simply replace traditional economic theory. Mechanistic constraints, relevant to the study of choice and behavior, lie outside the neoclassical paradigm. But such constraints are not intended to be independent of the organizational structure imposed by more traditional economic theory associated with the higher-level abstractions that describe the goals guiding neuro-economics. Continuity and change go hand in hand.

The questioning of the standard theory of rationality based on the theory of expected utilities has led to the development of other important approaches promoting very different kinds of explanations that are not even being considered in behavioral economics. Institutional economics, for example, is another development arising from the recognition of the limitations of neoclassical economics. Some authors consider that institutional economics should integrate the neoclassical framework and might suggest that institutional economics is the new paradigm for economics. As Coase puts it, "modern institutional economics should study man as he is, acting within the constraints imposed by real institutions. Modern institutional economics is economics as it ought to be" (Coase 1984: 231). But this is hard to believe, at least if the idea is that behavioral economics and neuroeconomics and the other recent fields in economics branching from the same crisis should become extinct or be absorbed by institutional economics. Very probably some of these approaches will disappear and consolidation will take place, but it seems hard to believe (and it is not necessary in order to advance scientific understanding) that the future will bring one new homogeneous model for dealing with economic phenomena.

All of these branching lines of research have in common the recognition of the need to abandon the traditional blackbox account of cognition implicit in traditional models. Also, all of these new proposals share the recognition that the empirical sciences, biology, and the cognitive sciences in particular, can provide guiding principles and appropriate idealizations for advances in the social sciences. But what seems to be happening is not reduction of alternatives, but stabilization of at least some of them, stabilization that goes hand in hand with integration of approaches into configurations of explanatory frameworks that scaffold new applications.

Another example of the way in which the breaking of the normative framework provided by the traditional theory of decision making (based on the theory of expected utility) unleashes a similar process of diversification of models and explanation counterbalanced by the search for a integrative idealized theoretical framework (that limits the choice of models to be tested and discussed), is the discussion about the use of evolutionary models in archaeology. Evoutionary models in archaeology have been developed in many directions, but it is widely recognized that some sort of constraints on the possible models have to be put in place in order for sustainable advances to follow. This leads us to appeal to the neurosciences, or to a discussion about the possible use of conceptual metaphors in the sort of explanations that should be accepted in archaeology to account for historical patterns in material culture (see for example the discussion between Ortman, Hurt, Rakita and Leonard. Ortman 2001). One central point of discussion here, as in many other contemporary discussions in the social sciences, is the extent to which we are willing to abandon the view that culture is an exogenous factor that fixes implicit assumptions required for an idealization of the process to be explained. The problem with such an approach is that it ignores feedback between normative frameworks and culture. As Roerstopft puts it in a recent article:

> The underlying argument appears to be that mapping this chain of transformation in all its cumbersome detail is the key to understanding the type of society in which the object was produced, and —at least since the turn towards a cognitive archaeology (Renfrew and Zubrow 1994) — also the mindset of the people who made it. Roepstorff 2008.

An obvious consequence of these sort of discussions, is that the manner in which change is modeled cannot be left outside the empirical discussion. Change and rationality are not concepts that we can grasp outside a cultural history. As Gamble puts it in the summary to the first part of his book *Origins and Revolutions* in reference to accounts of change relevant for archaeological theory:

> I have now examined how archaeologists use the concept of upheavals in their descriptions and accounts of change. I have placed their usage in historical context and found that change is best understood not as a property of archaeology being studied but rather an outcome of contemporary concerns. (Gamble 2007, p. ¿?)

This is a conclusion that seems to be generalizable: change is an outcome from a certain perspective.[15] But that does not mean that change is in the eye of the beholder. Different notions of change in archaeology, historical sociology and behavioral economics point to some sort of incommensurability, but this sort of incommensurability is a synchronous, non-transient type of incommensurability. Such inconmensurability is not an obstacle to knowledge, but is a source of understanding. The different perspectives on change, when fruitfully contrasted, provide limitations of scope and bridges to integrate advances in different disciplines into credible explanations*. Inconmensurability is not a problem to be solved but a resource to be exploited for understanding*.

More generally, Gamble's summary of to the history of theories of human origins support the sort of account I want to give of naturalization projects in the philosophy of science (and epistemology). *Naturalized philosophy of science should not be seen as a search for the right way of doing philosophy informed or constrained by science, but as a way of thinking about science from the perspective of what are the most promising and empirically grounded contemporary accounts of what is human nature in the context of a set of disciplinary goals.*

Notice that a similar line of thought leads us to the conclusion that we should expect (as indeed empirical studies show) different notions of rationality to play a role in different situations. There is no single perspective that integrates what we know about human nature in such a way that said perspective can be taken as the normative

[15] In this paper, I am referring to all notions of change relevant for the discussion in the philosophy of science. For a defence of this view as a general philosophical view see Van Fraassen 2008, Elgin 2010.

point of departure for explaining the continuity of science and philosophy. If science spoke with a single voice, naturalism could be described as in Wittgenstein's famous account of the correct method of philosophy: *To say nothing except what can be said, i.e. the propositions of natural science*. But science does not speak with one voice, rather through different practices that conform traditions of inquiry stabilized by social-cognitive productive constraints. Such constraints do not work only on systems of beliefs, but shape the metaphors, analogies and heuristics with normative import that conform scientific styles of doing and representing that I refer to as cognitive styles.

8*. From paradigms to cognitive styles*. The *crisis of rationality* to which the discussion about Kuhn's work is famously attached goes hand in hand with the *historical turn* in the philosophy of science. I have suggested above that Kuhn's notion of paradigm is important for the philosophy of science because it leads us to confront the fact that there are different kinds of change and different ways of doing things that relate to each other in a way that cannot be modeled by traditional models of explanation and rationality. But Kuhn's concept of paradigm is too rigid.
For one, it is too closely related to assumptions about the importance of the disciplinary organization of science as the point of departure for an explanation of the factors that play a role in an explanation of the stability and change of the norms of inquiry . This disciplinary organization is important from a sociological perspective, but from an epistemological and historical perspective it is less important; disciplines have changing borders; practices coalesce in disciplines in a relatively contingent way, they come together insofar as they can cooperate (not in view of a common aim necessarily). But also it is important to take in consideration that often scientific advance involves migration of methods from one discipline to another. Practices imported from physics were crucial for the beginning of molecular biology, and mathematicians have initiated many lines of research in economics and the social sciences. Once institutionalized an important stability comes from this institutionalization and from the associated teaching practices. But such stability is rather precarious at the level of research, even though the same textbooks are used through decades.
And there is something else. Stability of relevant beliefs (for explaining conceptual stability and change) does not follow from sharing textbooks, but even if it were to follow, one would still need to show how such stability of beliefs is relevant to understand the different sorts of conceptual change important for modeling the dynamics of science. Sharing mathematical methods is quite important for the stability of practices, but sharing mathematical methods does not imply sharing beliefs about important conceptual matters. Scientists might share the view that Hilbert spaces are important in Quantum Mechanics, or that population models are crucial to formulate the theory of evolution, but they might differ as to how understand the basic concepts of the theories in question. For teaching basic quantum mechanics such differences of conceptual framework are not important, but for appraising the future of the discipline and the sort of alliances we might make to foster the discipline, it might be.

I am not claiming that stability of beliefs is not important. The stability of beliefs that is a product of shared teaching practices is important. But there is not one set of beliefs that such practices usually converge to. And further more, this is not the only sort of stability that matters for understanding conceptual change. Sharing standards of laboratory, specimens, models and know-how is also an important source of stability and constitute important resources that have to be brought into an explanation of scientific change. In order to answer the crucial question of how these different sources of conceptual stability relate to each other *requires to give due importance to the cognitive dimension of conceptual change as it manifests itself in the interaction among different practices through time*.[16]

As I have suggested above such stability can be explained as a special case of the sort of conceptual stability and change supported by cultural practices. A key ingredient of such explanation is that the relevant stability of beliefs for explaning conceptual change is the result of a complex interaction and evolution of norms implicit and explicit in different practices and institutions that in particular have to take into consideration the role of material culture in promoting such stable normative environments. But the stability of beliefs is only a transient state, which, like our geographical reference points changes through geological history. What apparently is an unchanging set of beliefs transmitted through generations of scientists belonging to a paradigm is really a changing arrangement of factors (some of which are norms or have a normative dimension) that is changing slowly in different directions, from different perspectives.

An explanation of the stability of cultural practices asks for a complex account of what human culture is; such account commits us to take in consideration the role of material culture and the cognitive scaffoldings that shape our situated cognition and that support our understanding of the world and of our human condition in particular. Those (cultural and cognitive) scaffolds cannot be analyzed fully in terms of beliefs or systems of beliefs, they have to be thought of in terms of their role as productive constraints on situated action. [17]

Hacking and other philosophers and historians have been pressing for the importance of recognizing the role of styles of thinking or reasoning in order to understand the stability and the advance of science. For Hacking, a style of reasoning crystallizes in

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[16] A different explanation of the relevant stability can be given using schema theory (see Nickles 2000). I see such an approach as compatible. The difference is that my suggestion takes into account the fact that *the stability of our beliefs and practices* depends on cognitive and cultural factors that go beyond whatever factors can be identified as playing a role in explanation of the stability and change of our theoretical knowledge. Material culture, as this is obvious for cultural practices in general, has to be recognized as playing a role in accounting for the mechanisms of stability and change of concepts, at least to the extent that norms (implicit and explicit in practices) support such stability.

[17] As Brunner puts it more than two decades ago: the cognitive revolution has to go beyond the predominance of AI and return to the original force of the cognitive revolution, a cultural psychology not preoccupied with behavior but with situated action (Brunner 1990, p8). For a converging philosophical approach see Hendriks-Jansen 1996. The shift in the cognitive sciences to model cognition as situated or embodied is nowadays not just a programmatic statement as it was for Brunner. It is increasingly recognised as a crucial element of an explanation of cognition and its relation to action.

the introduction of new objects and criteria used to judge what is said about such objects. A cognitive style in my sense is the result of complex interactions between material culture, institutions and conceptual resources that constrain our ways of learning and of doing things. A style for Hacking does not answer to external criteria and thus the objects in question are quite distinctive from the sytle. Nevertheless my notion of cognitive style does not "crystallize" in objects, but in ways of doing things, in constructing models or designing heuristics and more generally, artefacts for situated actions, the sort of artefacts that are paradigmatically articulated and produced in scientific practices. [18]

9. Concluding Remarks. From the perspective of a philosophy of science that gives full weight to the organization of science into practices the notion of paradigm does not characterize shared beliefs, but shared practices. Shared practices are most often the result of common ancestry. Common ancestry is important because it allows the transmission of whole packages of techniques, expectations, standards and norms that function as a whole in a relatively stable environment but that it can change piecemeal through small changes in the (conceptual and material) environment that is part of the complex array of factors constituting (scientific) culture. Common ancestry explains the well-documented similarities in the formulation of problems, modes of representing and the kind of expectations that lead research in different traditions and cognitive styles. The differences between different groups of practitiones tend to be inherited through their lineages, formed around the training of new generations of scientists that involve informal personal interaction (see Kaiser 2005 for a detailed presentation of the history of Feyman's diagrams along these lines) . A similar account can be given for heuristic patterns of reasoning and observational skills used in the different scientific practices (see Martínez 2003) . Such patterns of reasoning and observational skills lead some scientists to see certain phenomena and not others. Such biased reasoning skills are not arbitrary. The direction of bias is stable, as it answers to a cognitive style. That biases are not arbitrary but stable features of reasoning is one of the most important theses of Kahneman and Tversky and has also been used to characterize central features of heuristic reasoning, and of explanation by models (as argued by Wimsatt since 1974).[19] This non-arbitrariness of biases provides further evidence for our thesis. The naturalization of the philosophy of science (and epistemology) has to take roots in projects of naturalization going on in the cognitive and social sciences. These roots are continuity enough.

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[18] See Martinez in press.
[19] See Wimsatt 1976.

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