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**Reconstruction, Rationalization,  
and Deconstruction**

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"S'il devait un jour n'être plus possible pour les observateurs scientifiques de s'entendre au sujet des énoncés de base, cela équivaldrait à l'échec du langage comme moyen de communication universel. Cela équivaldrait à une nouvelle "Tour de Babel", la découverte scientifique s'en trouverait réduite à une absurdité. Dans cette nouvelle Babel le haut édifice de la science tomberait bientôt en ruines."

**Karl Popper, *La Logique de la découverte scientifique*, section 29.**

"But, even if there is agreement about 'basic' statements, if there is no agreement whatsoever about how to appraise scientific achievement relative to this 'empirical basis', would not the soaring edifice of science equally soon lie in ruins ? No doubt it would."

**Imre Lakatos, "Popper on Demarcation and Induction", in *The Methodology of Scientific Research Programmes*, p.145.**

Couverture: La Tour de Babel (Bruegel, 1563)  
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*Reconstruction, Rationalization,  
and Deconstruction*

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Cornelius Castoriadis once commented that science, 'like all institutions, is an inertia supported by a myth'.<sup>1</sup> Castoriadis had in mind not only the external, social dimensions of science, but also its conceptual frameworks and most specific findings. Like many contemporary theorists, Castoriadis does not hesitate to characterize research being done in a wide range of disciplines, including physics, biology, and '*les sciences humaines*' as a whole. Science is a myth, Castoriadis claims, because it is thought to provide a pristine, objective representation of reality, but does not really do so. And science is an inertia because it functions as a self-perpetuating, self-validating, and constantly growing system of cognitive practices and social arrangements.

Castoriadis is hardly alone in suggesting that science is anything but a matter of rational problem-solving giving rise to objective knowledge. Many are those who today agree that science is, at bottom, a social institution, and what is more, they agree that it is the kind of delusionary and self-perpetuating affair that Castoriadis had in mind. In this paper I want to explore and challenge this kind of attitude. I shall not contest the idea that scientific and humanistic research are for the most part conducted in, and influenced by, institutional contexts, which are a fit object for sociological enquiry. I also agree that in many respects, some institutions are indeed an inertia supported by a myth. At the same time, however, I think it is reasonable and important to ask whether there are different kinds of institutions, some of which are supported, not by a myth, but by good and sufficient reasons. Could it be the case that at least some forms of knowledge, and not just scientific knowledge, transcend the kind of cynical perspective Castoriadis and others have promoted? In an effort to pursue

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<sup>1</sup>Cornelius Castoriadis, 'Science moderne et interrogation philosophique', *Les Carrefours du labyrinthe* (Paris: Seuil, 1978), 147-217; citation, pg. 215.

this question, I shall turn to some work in epistemology and in the philosophy of science to provide a broader context for examining the cynical perspective on knowledge. I shall try to delineate the cynic's best and most prevalent arguments, and will then present some rejoinders and alternatives to them. I cover some ground here that is familiar to most philosophers; unfortunately, scholars in other fields have a tendency to overlook much of the relevant work, past and present, in epistemology and in the philosophy of science. One of my aims here, is to build a proper context for the evaluation of such recent trends as deconstruction.

### *1. Cynical critiques of epistemic rationality*

I shall be focusing in what follows on what I call 'cynical critiques of epistemic rationality', and it is best to begin by unpacking this expression a bit. My use of the term 'cynical' is based on its standard dictionary sense, but also goes beyond it a bit. The cynic, the *OED* tells us, is someone who is 'disposed to disbelieve in human goodness and sincerity'. When I refer to cynical critiques of science, I certainly have this kind of attitude in mind, but I also want to include the idea that when scientists do happen to be sincere about their knowledge claims, they are deluded if they think their results are true or justified. The basic intuition behind a cynical critique of someone's beliefs or actions is that the agent in question is either hypocritical or deluded about his or her motives, and more generally, about the reasons or causes explaining them. That's what I mean by 'cynical' in what follows, and I intend no insult either to ancient philosophers or canines.

The cynical perspective is often presented as a challenge to rationality and morality as a whole, but I want to focus today on its challenge to only one aspect of human rationality--our epistemic rationality. As the term suggests, epistemic rationality has to do with the rationality of belief, but it does not simply amount to the rationality of holding or not holding a particular belief because other, nonepistemic reasons can make a belief rational. Beliefs can

be beneficial or detrimental to the believer in a lot of different ways. It is possible, for example, that some people successfully realize some of their goals because they unwittingly indulge in wishful thinking. Such beliefs may be unjustified or false, but can nonetheless be very useful, and perhaps even rational. Yet these beliefs are not *epistemically* rational because they do not serve the properly epistemic goal, which is to maximize truth and minimize falsehood in a large body of beliefs.<sup>2</sup> A belief is epistemically rational or irrational only relative to the latter goal, and may simultaneously be evaluated with regard to any number of other, compatible or conflicting goals. I shall have more to say below about epistemic justification and its relation to the epistemic goal of seeking truth and avoiding falsehood.

The cynical challenge to epistemic rationality amounts very generally to the claim that the real causes determining our beliefs are practical interests and other factors, and not specifically epistemic ones. Reference to the latter is just a matter of rationalization, because epistemic rationality is at bottom an illusion. Thus one finds, in the literature of postmodernist theory, various phrases announcing the equivalence of knowledge and power, the collapse of properly scientific and scholarly standards into those of rhetoric and conversation. Aristotle was simply wrong to imagine that one can distinguish between theoretical and practical interests. What masquerades as a purely speculative or contemplative stance is just a practical interest in disguise. Knowledge is just one of the many masks of violence and domination.

Perhaps I should mention at least one specific example of such pronouncements so that no one may claim that I am lashing out at straw dummies. A cynical theorist par excellence is Barbara Herrnstein Smith, a professor of literature whose work is associated with 'deconstruction'. In its bluntest form, her explicitly stated view is that 'there's no such

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<sup>2</sup>William P. Alston, *Epistemic Justification: Essays in the Theory of Knowledge* (Ithaca: Cornell University Press, 1989), pp. 83-4.

thing as an honest opinion'. Anyone who publically makes a value judgement or defends a claim as justified belief is at some level engaged in deception--if only in a form of self-deception. In its hedged version, this cynical thesis reads as follows: '. . . no judgment is totally unaffected by the particular social, institutional, and other conditions of its production, and none totally immune to the (assumed) interests and desires of its (assumed) audience . . . .' <sup>3</sup> Interests and desires, then, are fatal to reason, and no vaccine is to be found.

## 2. *Responding to cynical critiques*

I shall begin my response to the cynical challenge by presenting what I consider to be an historically important, but *inadequate* way of defending epistemic rationality. This failed response merits discussion in the present context because of its significant position in the history of ideas, but also and more importantly, because it speaks directly to the kinds of objections raised by the cynic. A discussion of its shortcomings is also an effective way of setting the stage for the presentation of a more successful alternative.

The approach I want to discuss was first developed by the logical positivists and was part of the orthodoxy of logical empiricism that dominated philosophy of science for several decades. An important source for this approach is Rudolph Carnap's *The Logical Structure of the World*.<sup>4</sup> Carnap notes that our cognitive syntheses of objects do not usually take place according to a conscious procedure, but are carried out intuitively. Nor are the theses of science necessarily realized by means of a logical deduction involving an exercise of conscious, rational reflection. 'After all', Carnap adds in a phrase that stresses the

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<sup>3</sup>Barbara Herrnstein Smith, 'Judgment after the Fall', in *Deconstruction and the Possibility of Justice*, ed. Drucilla Cornell, Michel Rosenfeld, and David Gray Carlson (New York: Routledge, 1992), pp. 211-31; citation, pg. 223.

<sup>4</sup>Rudolf Carnap, *Der logische Aufbau der Welt* (Berlin: Weltkreis, 1928), trans. Rolf George, *The Logical Structure of the World* (Berkeley: University of California Press, 1967). Page numbers provided in my text are to the latter edition.

'contingencies' on which scientific judgements depend, 'the basic orientation and the direction of interests are not the result of deliberation, but are determined by emotions, drives, dispositions, and general living conditions [*Gefühl, Trieb, Anlage, Lebensumstände*]' (pg. xvii). Carnap, we see, was not unfamiliar with the Heideggerean theme of the *Seinsgebundenheit des Denkens*, the idea that various worldly conditions lurk behind the back of consciousness. These diverse factors are at work in science and in philosophy as well as in everyday life. But what is decisive in the former (if not always in the latter) is that the scientist or philosopher be able to provide a rational and empirical justification of his or her theses. To that end, it must be possible to provide a 'rational reconstruction or justification' [*eine rationale Nachkonstruktion* or *Rechtfertigung*] of the actual process of cognition. Such an approach 'allows and even requires deviations of the construction from the actual process of cognition', the aim of the analysis being to isolate the 'formal structure' of the cognitive process (pg. 220). Carnap adds that the construction 'does not even contain a fiction to the effect that the process is rational rather than intuitive' (pg. 221). The justification of scientific knowledge, then, depends not on claims about the rationality of the actual process of discovery, but on claims about the validity and empirical accuracy of a reconstructed theory.

With Carnap's work clearly in view, Hans Reichenbach made an influential attempt to develop and apply this approach.<sup>5</sup> Like Carnap, Reichenbach sought to draw a sharp distinction between the tasks of psychology and epistemology, where the former studies actual thought processes, and the latter analyses a 'logical substitute' or 'rational reconstruction' of these real processes. Psychological explanations of scientists' reasoning

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<sup>5</sup>Hans Reichenbach, *Experience and Prediction: An Analysis of the Foundations and the Structure of Knowledge* (Chicago: University of Chicago Press, 1938), especially pp. 5-8. For a detailed discussion, see Thomas Nickles, 'Introductory Essay: Scientific Discovery and the Future of Philosophy of Science', in *Scientific Discovery, Logic, and Rationality*. Boston Studies in the Philosophy of Science, Vol. 56, ed. Thomas Nickles (Dordrecht: Reidel, 1980), pp. 1-59.

are part of what Reichenbach calls 'the context of discovery', while the epistemologist's analysis of scientific theories focuses on 'the context of justification'. Reichenbach adds that epistemology has distinct *descriptive* and *critical* tasks.

Reichenbach does not have much to say about these tasks and about their relation to rational reconstruction. He claims that it would be a mistake to try to build a theory of knowledge that is 'at the same time logically complete and in strict correspondence with the psychological processes of thought' (pg. 5). Epistemology need only 'construct thinking processes in a way in which they ought to occur'. To that end, the epistemologist can 'construct justifiable sets of operations which can be intercalated between the starting-point and the issue of thought-processes, replacing the real intermediate links' (pg. 5).

But what, precisely, is the relation between the reconstruction and the actual thought process? The rational reconstruction of a discovery, he notes, is not arbitrary, but is 'bound to the actual thinking by the postulate of correspondence' (pg. 6). Reichenbach does not say anything more about the latter postulate, but he does say that the reconstruction expresses what the scientist 'really meant' and thereby advances our understanding of scientists' actual thoughts. Yet the correspondence between reconstructions and scientist's thoughts is partial. When we set out to provide a rational reconstruction we are supposed to 'suppress the traces of subjective motivation' at the origin of scientific theories. Rational reconstruction is also a critical process: 'the tendency to remain in correspondence with actual thinking must be separated from the tendency to obtain valid thinking' (pg. 7). Even so, Reichenbach adds that a rational reconstruction can contain unjustified chains of thought, in which case the scientist's knowledge claim would be exposed to criticism.

What is somewhat puzzling here is the exact status of the rational reconstruction and its relation to the scientist's actual thought processes. In its descriptive moment, a reconstruction would appear to provide a partially corrected and idealized version of the scientist's thinking. What gets left out is the psychological background of affect and

motivation, and in some cases, steps in the scientist's reasoning. At the same time, Reichenbach suggests that the scientist's own premisses and conclusions should be retained in the reconstruction. The final product of such an analysis is an argument, some of the steps of which the scientist may never have conceived of or thought through. The point of providing such a reconstruction is to produce an argument that can be subjected to critical scrutiny, and although Reichenbach does not say so, presumably the epistemologist's task is to build the best possible argument while respecting the spirit, but not the letter, of the scientist's own premisses and conclusions. In its critical moment, the same reconstruction would either ratify or critique this idealized version of the scientist's thinking.

A first important problem to take note of here is the lack of clarity concerning the exact nature of a reconstruction's proper relation to the psychological and social realities of scientific research. It is simply too vague to say that reconstructions are meant to be selective, yet must still correspond to the actual processes, for we must know what can and cannot be left out, and what the nature of this correspondance should be. A related problem concerns the relation between rational reconstructions as described and Reichenbach's distinction between contexts of discovery and justification. Presumably if the reconstruction must in some sense correspond to the scientist's psychological processes during his or her research, then the reconstruction refers, in at least in its descriptive moment, to the context of discovery. Yet if the point of the reconstruction is to ask whether an idealized version of the final product of the scientist's work is logically valid, then it clearly should be situated within the context of justification. Rational reconstruction would appear, then, to be in the service of both the psychology of discovery and the epistemology of scientific knowledge, in which case Reichenbach's central concept straddles the distinction between these two contexts.

With Carnap, Reichenbach, and these same issues in mind, Karl Popper sought to draw an analogous, but more workable distinction. He set the matter of 'rational reconstructions' aside and insisted instead on the value of disjoining factual descriptions of

hypothesis formation from evaluative analyses of scientific hypotheses.<sup>6</sup> Whether a scientific judgement is justified cannot be determined by describing the particular motives, interests, and thought processes of the agents involved. '*Quid facti?*' or questions of fact are one thing, and '*quid juris?*', questions of justification, are another, and answers to the latter cannot be derived from answers to the former. Epistemology, then, does not even ask questions about the actual thought processes (and other factors) involved in the historical genesis of scientific theories. Instead, epistemology asks questions about the logical and empirical status of the content of scientific theories. Popper would appear to have tidied matters up considerably, but the solution is rather draconian. Reconstruction is abandoned in name, but not in practice, and Popper does not tell us how we are to determine which aspects of the history of science are relevant to Kant's question, so his epistemological project remains underdefined.

Returning now to the cynic's critiques, we can see that the logical empiricists' distinctions deny the very pertinence of the kinds of claims the cynic musters in a critique of scientific reason. The scientist's greed, vanity, ambition, and masculine desire for domination could be central traits of his personality, just as the cynic claims, but these factors would not figure in a reconstruction of the scientist's findings. That these scientific findings themselves serve a host of non-epistemic ends, such as those of the military-industrial complex, is similarly irrelevant to the epistemologist's analysis of their logical structure and empirical basis. If, once suitably reconstructed, the scientist's arguments are justified, nothing the cynic can tell us about the context of discovery can change this fact.

As is well known, explicit critiques of this kind of approach have been prevalent throughout post-World War II philosophy of science, and there is an ever-widening consensus that logical empiricism's response to the cynic is wholly inadequate. But it is

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<sup>6</sup>Karl R. Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959); first edition, *Logik der Forschung* (Vienna: Springer, 1934), section 2: Elimination of Psychologism.

crucial in this regard to distinguish between several distinct theses that may be associated with such critiques of logical empiricism. One thesis, with which I am in full agreement, is that a satisfactory overall account of science is impossible if the analysis is limited to the context of justification. But that is not a point that would have been denied by Carnap, Reichenbach, or Popper. Popper, it is true, was rather sceptical about the possibility of elucidating a *logic* of discovery, but it does not follow that there can be no worthwhile descriptive and explanatory research focusing on the sociological and psychological processes that make the growth of scientific knowledge possible. Popper's own analyses of traditions and research communities is a case in point.<sup>7</sup> It should be added that sociological and psychological accounts of scientific knowledge were in the works long before such figures as Paul Feyerabend questioned the logical empiricist's distinction between discovery and justification.<sup>8</sup> Robert K. Merton's research is a major example, and it is wrong to complain that he thought sociological explanations were only necessary in cases where science had failed.<sup>9</sup> Merton's work on simultaneous discoveries, and his attempt to define the *ethos* behind successful scientific research, are two examples worth mentioning in this context.

A first, and in my view correct thesis holds that there is more to science than the context of justification, if by justification is meant an epistemological analysis of the

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<sup>7</sup>Karl R. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge* (New York: Basic Books, 1962).

<sup>8</sup>Paul Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge* (London: Verso, 1978), ch. 14. Feyerabend's brief discussion of the topic is strikingly inconsistent and equivocal, for having advocated 'abolishing' the distinction, he goes on to use it in several different senses. It is not for nothing that in Feyerabend's index, the item 'rhetoric' is followed by *passim*.

<sup>9</sup>See, for example, Robert King Merton, *The Sociology of Science: Theoretical and Empirical Investigations* (Chicago: University of Chicago Press, 1973). For the 'asymmetry' charge, see James Robert Brown, ed., *Scientific Rationality: The Sociological Turn* (Dordrecht: Reidel, 1984). See also Richard C. Jennings, 'Truth, Rationality, and the Sociology of Science', *British Journal of Philosophy of Science*, 35 (1984), 201-11; and Harvey Siegel, 'Discovery, Justification, and the Naturalization of Epistemology', *Philosophy of Science*, 47 (1980), 297-321.

scientist's knowledge claims. Yet such a thesis hardly puts in question the kind of distinction that Reichenbach and Popper wanted to draw; nor does it incriminate the epistemologist's perspective on science. The response to the cynic remains intact.

But there are other, more serious objections that can be raised against the advocate of a neat distinction between contexts of discovery and justification. Thomas Kuhn's influential book, *The Structure of Scientific Revolutions*, is often cited as a source of such objections. Ronald N. Giere, for example, contrasts Kuhn and Norman Hanson on precisely this point. Hanson is said to have believed, *contra* Popper, that it is possible to elucidate a logic of scientific discovery. Kuhn, on the other hand, implicitly asked whether there was a logic *of justification*, and advanced historical evidence in order to argue that there is no such thing. Instead, there is only an historical process of competition among segments of a scientific community.<sup>10</sup> It would follow that Reichenbach's distinction is inappropriate, and the door is open for the cynic to contend that both normal and revolutionary science are driven by non-epistemic motives.

Kuhn is ceaselessly cited in support of such views, but his text hardly supports such a reading, and his own critique of logical empiricism was more subtle. At the end of his introduction, Kuhn raises a rhetorical question concerning the possibility that he has violated the distinction between discovery and justification. He goes on to suggest that the distinction is 'problematic' and theory-bound, yet he also says that it could be highly important if 'appropriately recast'. Far from saying that the history of science can fully replace epistemology, he ends his introduction simply by asking how one could doubt that epistemology should be applicable to the history of science.<sup>11</sup> The point, then, is not that

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<sup>10</sup>Ronald N. Giere, *Explaining Science: A Cognitive Approach* (Chicago: University of Chicago Press, 1988), pp. 32-3.

<sup>11</sup>Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed., in *Foundations of the Unity of Science*, ed. Otto Neurath, Rudolf Carnap, and Charles Morris (Chicago: University of Chicago Press, 1962), Vol. 2, pp. 53-273; see especially pp. 70-1. An author who foregrounds the fact that strikingly different accounts may be pulled from Kuhn's

justification is either irrelevant or impossible, but that epistemology must be responsive to the historical data. Reichenbach's distinction is sound, but any epistemological analysis that is contradicted by the actual history of scientific research should be rejected. Logical empiricism is wrong, then, insofar as it held that the context of justification was fully autonomous with regard to the context of discovery.

A related and more sophisticated claim has been voiced more recently by the scientific realist Richard Boyd. "The "logic of confirmation", he notes, 'must be somehow related to psychologically real inductive procedures for theory invention if scientific practice is to be epistemically reliable at all. The question "Just what is the relationship?" is simultaneously a question in empirical psychology and a question in the epistemology of science'.<sup>12</sup> For example, scientists' and epistemologists' reliance on abductive inferences entails the inadequacy of an epistemological analysis situated uniquely in an abstractive context of justification. Since abduction, or the 'inference to the best explanation', is a matter of claiming that some hypothesis explains the data better than any other hypothesis does, scientists' psychological capacities come directly into play, for it is only with reference to the adequacy of scientists' reasoning and imaginative abilities that we can conclude that all relevant hypotheses have been surveyed and assessed. Boyd concludes, then, that the context of justification is not autonomous with regard to claims about the psychological and other conditions of scientific research. But he does not support the cynic's more general thesis concerning epistemic justification.

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writings is Joseph Rouse, *Knowledge and Power: Toward a Political Philosophy of Science* (Ithaca: Cornell University Press, 1987), pp. 26-40.

<sup>12</sup>Richard N. Boyd, 'Observations, Explanatory Power, and Simplicity: Toward a Non-Humean Account', in *The Philosophy of Science*, ed. Richard Boyd, Philip Gasper, and J. D. Trout (Cambridge: MIT Press, 1991), 349-78; citation, pg. 376. For background on abductive inference, see John R. Josephson and Susan G. Josephson, *Abductive Inference: Computation, Philosophy, Technology* (Cambridge: Cambridge University Press, 1994).

Let's take a closer look now at the cynic's arguments. The basic contention is that the rationalized beliefs yielded by a reconstruction are neither rational nor justified. To support this conclusion, the cynic exploits the difference between the following two questions:

Q1: What reasons, if any, justify some agent's belief?

Q2: What attitudes/reasons explain why the agent held that belief?

Cynics claim that the answers to these two questions can and often do diverge, so that even if the agent, or someone else engaged in offering a rational reconstruction, formulated a flawless *post hoc* justification of the belief, this is just a rationalization that does not tell us what really happened. Thus we have a negative response to a third question:

Q3: Was the agent's belief in fact justified?

Any analysis situated uniquely within the context of justification cannot yield sound epistemological results; in order to know whether a belief was rational and justified, we must know something about the reasons that guided and caused the relevant agent's thinking. It is not enough that the scientist's results be rationalizable, nor even that the scientist was aware of good reasons for holding a particular belief. Instead, scientific rationality and the justification of its knowledge claims requires scientists to hold beliefs for the right reasons, which is a matter situated squarely in the context of discovery. If the justification is to be anything more than a rationalization, the reasons offered to justify the belief must also explain why the agent held the belief, and this explanation must describe the relevant causes of the belief.<sup>13</sup>

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<sup>13</sup>For the concept of rationalization, see Robert Audi, *The Structure of Justification* (Cambridge: Cambridge University Press, 1993), 405-30.

Now we can formulate the cynic's best critique of the distinction drawn by Reichenbach. Scientific rationality cannot be justified or grounded in an independent context of justification because the cogency of the scientist's reasoning depends crucially on facts about the context of discovery, including, most importantly, causal factors explaining the fixation of belief. And in many cases, facts in the latter context reveal scientific rationality to be nothing more than an epistemologist's rationalization. It follows, then, that epistemological analyses situated in a context of justification are not autonomous and are at least sometimes wholly misleading.

These are important conclusions, but I hasten to add that they need not be taken as lending any genuine support to the more virulent forms of cynicism that one finds in the literature. Note that it is an empirical question, that is, a matter of fact, whether the reasons given in a reconstruction actually were the scientist's real reasons. If the scientist did in fact reach the right conclusion for the right reasons, the cynic's critique would be inapplicable.

What the cynic has not proven is that all knowledge claims are unjustified rationalizations. To grasp this point, consider the difference between the following two questions:

Q3: Was the agent's belief in fact justified?

Q4: Would someone today be justified in holding an analogous belief?

Sometimes the cynic wants to argue for a negative response to the latter question, and various claims about other peoples' past reasons for holding the belief are presented to that end. But even if the cynic is right about the historical matters, the cynic's argument is not valid when it is a matter of drawing conclusions about our current situation. It would also have to be shown that we do not have good reasons for holding the belief. In other words, the only way to know whether we are today justified in holding an analogous belief is to ask whether

we now have the right reasons. The reasons relevant to this question and to analogous situations in the past may frequently overlap, but they are not logically equivalent. Sometimes we are right to decide that someone in the past reached the right conclusion for the wrong reasons, but we may go on to reach the same conclusion for the right reasons. Or again, past researchers may have had good reasons for holding a particular view, and someone today could retain that belief but for new, inadequate reasons.

What the cynic would have to show in order to support a more general critique of our current epistemic rationality is that what we take to be our own good reasons for holding a belief today are just rationalizations. In some cases, there may be plenty of evidence to support such a claim. But to defend any more general thesis about the impossibility of justification, the cynic has to show us that we, today, are somehow 'fated' to make unjustified decisions whenever we ratify or revise someone else's judgements. But has it really been established that all reconstructions are rationalizations?

An astounding fact about the contemporary intellectual scene is that such wild and sweeping moves are not only frequently made, but are also frequently applauded. Jacques Derrida's deconstruction of all past, present, and future discourses of rational legitimation is an especially bold instance of this kind of thing. This is the higher cynicism, and the theological overtones of that phrase are not out of place. Consider, for example, the following passage in Derrida's text on the philosophy of law:

But as a performative cannot be just, in the sense of justice, except by founding itself on conventions and so on other anterior performatives, buried or not, it always maintains within itself some irruptive violence, it no longer responds to the demands of theoretical rationality. Since every constative utterance itself relies, at least implicitly, on a performative structure [ . . . ], the dimension of *justesse* or truth of the theoretico-constative [sic] utterances (in all domains, particularly in the domain of the theory of law) always thus presupposes the dimension of justice of the

performative utterances, that is to say their essential precipitation, which never proceeds without a certain dissymmetry and some quality of violence.<sup>14</sup>

It is hard to see why anyone thinks we could have sufficient reason to be convinced by these heady proclamations.<sup>15</sup> Derrida *claims* here that all truth claims, in all domains, ultimately depend on 'performative' actions that have their basis in non-deliberative violence. If that is right, then any decision to accept Derrida's claim would itself depend ultimately on the same sort of violence, and could therefore never be properly justified in terms of reasons and evidence. So Derrida's claim is self-defeating because his own reasoning implies that it is based on the same violence he decries.

Relativists often complain that the *tu quoque* charge is circular and proves nothing. I have yet to see that rebuttal presented in a convincing version, but am still willing to set aside the *tu quoque* charge so as to ask what good reasons there might be for thinking that the Derrida's claims are right. Derrida's approach in the essay in question is a clear instance of the 'all or nothing' conceptual strategy characteristic of deconstructive writing. The citation belongs to a section in which Derrida sets forth what he calls an 'aporia', bearing the subtitle, 'the urgency that obstructs the horizon of knowledge'. Derrida opens this section by claiming that a truly just decision would require 'infinite information' and 'an unlimited knowledge of conditions, rules or hypothetical imperatives', which simply cannot be provided in any finite context of decision-making. And so Derrida agrees with Kierkegaard that the 'instant of decision is a madness'.<sup>16</sup> The background here is the absurdist,

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<sup>14</sup>Jacques Derrida, 'Force of Law: The Mystical Foundation of Authority', in *Deconstruction and the Possibility of Justice*, ed. Drucilla Cornell, Michael Rosenfeld, and David Gray Carlson (New York: Routledge, 1992), pp. 3-67; citation, pg. 27.

<sup>15</sup>Deconstruction is critiqued with great detail and accuracy in a number of sources. See, for example, Alexander J. Argyros, *A Blessed Rage for Order: Deconstruction, Evolution, and Chaos* (Ann Arbor: University of Michigan Press, 1991), and Thomas Pavel, *Le Mirage linguistique: essai sur la modernisation intellectuelle* (Paris: Minuit, 1988), trans. *The Feud of Language: A History of Structuralist Thought* (Oxford: Basil Blackwell, 1989).

<sup>16</sup>Derrida, 'Force', pg. 26.

theological account of decision-making that Kierkegaard develops, in *Fear and Trembling*, by means of what Kierkegaard himself calls a 'dialectical lyrical' reading of the biblical story of Abraham and Isaac.<sup>17</sup> One may well ask whether such a story provides a suitably paradigmatic instance of decision-making.

Derrida leaves us with a false, all or nothing dilemma: either we must prove with absolute certainty that we have apodictic, perfect knowledge, or all our so-called instances of human knowing are just madness and delusion. Either we can take forever to settle on our beliefs, or we might as well take no time at all. What this false dilemma leaves out is the possibility that the justification of belief is not an all-on, all-off affair. Making qualitative distinctions between significantly different degrees of justification is important and valuable to us. Some beliefs are very well justified by a lot of evidence and experience, others are just guesses and speculations, and having even a rough sense of the difference between the two is crucial. Such differences matter to us, but Derrida, the prophet of difference, systematically ignores them. Either theoretical reason offers a perfect, god's-eye infallible perspective, or it can only rest on violence.

Derrida is surely right to point out that our deliberations take place within a temporal framework, and this fact has significant consequences for the theory of human rationality, as Michael Bratman and others have recently argued in a constructive, problem-solving spirit.<sup>18</sup> But the temporally finite nature of all human deliberations does not entail that our judgements cannot have a reasonable measure of epistemic justification. Derrida in fact drops the specific issue of temporal constraints on rationality and goes on to make his claim about a 'structural' form of finitude, which involves the above-cited passage about the violent basis of all

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<sup>17</sup>Søren Kierkegaard, *Frygt og Bæven*, Vol. 5 in *Samlede Værker*, ed. A. B. Drachmann (Copenhagen: Gyldendal, 1962).

<sup>18</sup>See Michael E. Bratman, *Intention, Plans, and Practical Reason* (Cambridge: Harvard University Press, 1987); 'Planning and the Stability of Intention', *Minds and Machines*, 2 (1992), 1-16; and Alfred R. Mele, *Springs of Action: Understanding Intentional Behavior* (New York: Oxford University Press, 1992). See also Paisley Livingston, 'Le dilemme de Bratman : Problèmes de la rationalité dynamique', *Philosophiques*, 20 (1993), 47-67.

theoretic reasoning. Derrida's use of the term 'violence' in this passage is a characteristic instance of rhetorical equivocation. He provides us with no good reason for thinking that the psychological and other causal processes at work in human decisions are truly *violent* in any informative sense of the word. 'Violence' evokes aggression, domination, and harmful actions, but what Derrida describes is the supposedly abrupt, precipitous, and non-deliberative basis of all decisions. His use of terms like 'irruptive violence' is meant to make us distrust the idea that reasons and ideas could be the basis of some of our judgements and decisions. After all, who wants to defend theoretical reasoning if it is at bottom just a manifestation of violence? But such a worry is not justified by Derrida, for it is hard to see how various scholarly decisions are in any direct way linked to violence. If Derrida's idea is the plausible one that we cannot simply will our beliefs or exercise direct control over them, it does not follow from this observation that the relevant thought processes are violent, or that we are never more or less justified in holding a belief.

The plausible, empirical claims about temporality, then, give way without explanation to rhetorical equivocation about violence and to the all or nothing dilemma. Such is Derrida's aporetic approach.

#### *4. Reconstruction regained*

To sum up before moving on, I have argued that cynicism's sweeping critiques of epistemic rationality fail, even though some of the cynic's arguments do serve the useful purpose of revealing the shortcomings of logical empiricism's manner of defining and defending epistemology. What, then, are the implications for the concepts of reconstruction, discovery, and justification?

In trying to defend the specificity and cogency of epistemic rationality, I have not defended any claim to the effect that the epistemological value of a belief or explanation is its only or its highest value. The relations between our different ways of assessing beliefs are in

fact far more complex than any such simple thesis allows. It is far from clear to me that the good life is to be achieved by single-mindedly pursuing the goal of maximizing true beliefs and minimizing false ones. When it comes to scientific and humanistic research, however, I do think that epistemic considerations have a special place. In this regard, I am inclined to defend what I call the 'epistemic norm of research', which runs as follows:

The primary goal of research in the sciences and in the humanities is a specifically epistemic one, namely, the production of knowledge. In the evaluation of the rationality of research, epistemic criteria have a lexical priority in relation to all nonepistemic goals.

What do I mean in speaking here of the *lexical priority* of epistemic values over nonepistemic ones? A simple example of lexical priority is the fact that "azure" comes before "balloon" in the dictionary, even though the "z" in "azure" ranks way behind the "a" in "balloon" in the linear order of the alphabet. In granting lexical priority to matters epistemic, the epistemic norm of research states that some work does not satisfy the properly epistemic criteria, it does not satisfy the norms of the rationality of research. Once the epistemic norm has been satisfied, other criteria can be used to rank the results of research in terms of an overall assessment of merit or value. None of this, by the way, implies that the results of rational research have to be infallible or a matter of absolute certainty. Those are, I think, theological, not scientific or humanistic desiderata.

It follows from the epistemic norm of research that although rhetorical values play an important role in a scientific work, when it is a matter of evaluating the overall contribution to research, the work must first of all be weighed in terms of properly epistemic values, such as clarity, concision, coherence, determinacy of meaning and reference, descriptive accuracy, explanatory and evidentiary adequacy, and so on, which together make the difference between genuine and spurious contributions to knowledge.

I am well aware of the fact that providing a properly epistemic assessment of knowledge claims is no small problem, and I have not here defended any specific account of epistemic justification. So a few remarks on that score are in order. I have pointed out that justification is not absolute and is a matter of degree. The primary situation involved in assessments of epistemic justification is one where some agent is more or less justified in holding some belief. I am inclined to prefer an 'internalist' conception, which means that what justifies a belief is the evidence available to the believer; i.e. the degree of justification an agent has for a belief depends directly on how good that agent's evidence for the belief is. And what is the nature of good evidence? The fallibilist approach published recently by Susan Haack strikes me as highly promising in regard to this difficult question.<sup>19</sup> Haack's basic strategy in dealing with the problem of elucidating epistemic justification is to retain and refine the insights that make both coherentism and foundationalism attractive. Beliefs do not stand or fall in perfect isolation, just as the vertical and horizontal lines of a crossword puzzle intersect at multiple points. Thus, like the coherentist, Haack notes that beliefs receive pervasive, non-circular mutual support from each other. Yet some beliefs carry a significant degree of independent conviction, just as one can be almost certain that a particular word is the right answer to one of the clues in a puzzle, even in cases where no other overlapping solutions have been found. Thus, like the foundationalist, Haack allows that the chain of justification terminates in beliefs that are justified to some degree (but not absolutely) independently of any further beliefs. In this regard, she articulates belief's partial anchoring in "experiential evidence." But this is not the kind of traditional foundationalist view that is so frequently condemned in postmodernist writings. Writers like Richard Rorty and Stanley Fish fail to observe some crucial distinctions when they condemn foundationalism. One view that may be associated with that term, which is, I think, to be rejected, is the idea that *a*

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<sup>19</sup>Susan Haack, *Evidence and Inquiry: Towards Reconstruction in Epistemology* (Oxford: Blackwell, 1994).

*priori* epistemology is the basis of all scientific knowledge. Another bad idea traditionally associated with foundationalism is that the justification of belief depends exclusively on a privileged category of basic beliefs; and in some caricatural versions of foundationalism, these basic beliefs have to be infallible. But the idea that some beliefs are to some extent justified independently of further beliefs can still be defended, just as it makes good sense to hold that the criteria of justification are founded by their relation to truth, and these are in a sense foundationalist ideas. As Robert Audi puts it, "foundationalism can be moderate, fallibilist, common-sensical, and psychologically realistic. It can also provide a role for coherence in understanding justification, and in some contexts, in generating it."<sup>20</sup> As Audi, Haack and other contemporary epistemologists point out, moderate foundationalism does not imply the idea that we enjoy a definitive refutation of academic scepticism, or foundations in the sense of a "God's eye perspective." Yet we have good grounds for holding that at least some inquirers achieve a modest level of success in realizing properly epistemic goals, such as rejecting false beliefs, arriving at true ones, and realizing valuable measures of explanatory integration in a web of related, empirical beliefs.

I shall conclude with some remarks on the logical empiricist's concept of reconstruction. Epistemological reconstructions of past research should never be presented as overall accounts or global assessments of that research. Nor should they be imagined to be somehow autonomous with regard to the various factors that shaped past contexts of knowledge production. Kuhn was right, then, to insist that historical factors are directly relevant to epistemology. But more has to be said on this topic, for there are different ways in which historical factors are relevant, and in some cases, there is, in principle, a limit to which kinds of factors are relevant. It all depends on which goals are aimed at by the

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<sup>20</sup>Audi, *Structure of Justification*, pg. 13. See also Paul K. Moser, *Knowledge and Evidence* (Cambridge: Cambridge University Press, 1989).

reconstruction, and one of the most important shortcomings of the positivist and empiricist literature was to fail to discuss these different goals of reconstruction.

One such goal is to ask whether some past agent's beliefs were epistemically rational. In an attempt to answer that question, a wide variety of nonepistemic factors, such as whether the believers were motivated by desires for wealth and power, are relevant to the reconstruction, since the goal is to determine whether the agents in question lived up to the epistemic norm of research. In other words, to know whether someone's past belief was epistemically rational, I must know whether the good reasons that the agent could have had for holding the belief were in fact the agent's real and decisive reasons. To know that, I must examine the various factors orienting the agent's behaviour. So when a rational reconstruction of historical knowledge aims at this goal, many facts about the context of discovery are relevant, and certainly not only the scientist's premisses and conclusions, as Reichenbach seems to have wrongly assumed.

Another goal of rational reconstruction may be identified, however, and this one sets a different limit to the kinds of factors that are relevant. It is one thing to ask whether a past *agent* was rational in holding a belief, and it is something else to ask whether the belief was, in some abstractive and impersonal sense, rational. Here we are interested in the so-called propositional content of the belief for its own sake, as a potential candidate for our own acceptance. The question whether holding such a belief made anyone rich or excited is strictly irrelevant in this context, and I need not trouble myself with the matter. What I want to know is whether it could now be epistemically rational for me to hold this view. This goal brings us much closer to the kind of epistemological reconstruction Carnap and company had in mind. What matters is the content of the belief and the epistemic grounds one can have for holding it.

It would be a mistake to think that there is a simple, direct competition between these two different goals of reconstruction and the two kinds of rationality they target. The one is

part of a historical project focusing on agents' rationality, the other one is situated in the present and concerns an impersonal epistemic rationality of belief. If we want to know whether a reconstruction is a rationalization of the past, it is the former reconstructive goal, and not just the latter, that must be realized.<sup>21</sup>

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