Rational Reconstruction

Philosophers of science do many things. Nevertheless, the demand arises on occasion for philosophers to give a general account of the relation of the work of philosophy of science to work in the sciences. To this general question, several answers (logical analysis of scientific and metascientific concepts; an explicit account of scientific method). One answer that has been employed in various places and times in the twentieth century is that philosophy of science engages in a rational reconstruction of science. This answer seems to raise as many questions as it answers. Does not the need for a "rational reconstruction" of science at the hands of philosophers seem to indicate that science as practiced is in some important sense not (wholly) rational? What about science needs to be reconstructed in order better to exhibit its rational structure? What are the proper tools for reconstructing science?

This essay seeks to provide a brief but balanced account of the point of rational reconstruction in the two projects that most importantly advanced that understanding of the proper business of philosophy of science, logical empiricism and Imre Lakatos's methodology of scientific research programmes. Rational reconstruction is connected with many central issues of philosophical method within analytic philosophy of science. Arguments over rational reconstruction connect also to large debates about proper method in philosophy generally and, in particular, to the debates in the late nineteenth and early twentieth centuries over the relations of philosophy to psychology and sociology (for excellent introductions to these debates see Kusch 1995 and 1999).

Logical Empiricism

Within analytic philosophy of science, Rudolf Carnap's *Der logische Aufbau der Welt* (The logical construction of the world) of 1928 is the work often cited as first articulating an understanding of the business of philosophy of science as rational reconstruction. Carnap's project in the *Aufbau* is a rational reconstruction of the objects of science through exploitation of the technical resources of the new mathematical logic. He sketches a system of logical definitions that shows how all proper scientific claims can be translated back into a language that makes reference only to experience. Logical definition and inference are, of course, exemplary rational procedures, and, thus, the constructional system rationalizes psychologically attained knowledge, exhibiting its objective conceptual meaning.

It is sometimes suggested that "rational reconstruction" was introduced by the young Carnap (Reichenbach 1938), and is an expression of a radical break between early logical empiricism and the German epistemology that was pursued in the generations before it. Carnap's use of "rational reconstruction"–*rationale Nachkonstruktion*–in the *Aufbau* was, in fact, a move within a well articulated debate among neo-Kantians, phenomenologists, positivists, Marxists, and others over the proper methods of epistemology (Richardson 1999). For example, the positivist Theodor Ziehen, in his (1914) meta-epistemological work, *Zum gegenwärtigen Stand der Erkenntnistheorie* (On the present state of epistemology), distinguished between two methods used in then current epistemology. He called the first "the genetic method" since it traced representations used in judgement back to their psychological origins and he called the second "reconstructive method" because it "to a certain extent reconstructs the world of impressions from representations and judgements" (Ziehen 1914, p. 29). That is, the proponents of the reconstructive method say that the world of impressions is not the content of knowledge

but is itself only an object of knowledge when placed within a system of representations and judgements. Ziehen associates the genetic method with positivism and naturalism and associates the reconstructive method with neo-Kantianism.

Within this general framework, "reconstruction" was closely associated with "rational." Indeed, some neo-Kantians used the language of reconstruction and tied it directly to the rationalizing of the experiential world. For example, Jonas Cohn, in his (1908) *Voraussetzungen und Ziele des Erkennens* (Presuppositions and goals of knowing), calls pure mathematics an a priori "constructive" science because it constructs its own objects. Pure experience is not, for him, the source of knowledge so much as the rationally opaque starting-point of knowledge that is brought under rational control through mathematized natural sciences. These sciences use the resources of mathematics to rationalize and objectify experience (Cohn 1908, 342):

> All reconstruction is partial rationalization. The concepts of the particular reconstructive sciences are also, considered in and of themselves, constructions and, as such, fully transparent. But the new epistemic task that they serve gives them a close relation to the inexhaustible and opaque experiential reality.... One can say, therefore, that the particular reconstructive sciences concern themselves with individuals only insofar as these can be captured under general determinations.

Carnap's articulation in 1928 of the point of epistemology as rational reconstruction of the results of cognition is a move, therefore, in an on-going meta-epistemological debate. The formal logic of Russell and Whitehead's *Principia Mathematica* was to be used to show how the objects of knowledge that are picked out by ordinary cognitive processes and the more articulated

processes of science can be arrived at logically and discursively. Thus, the process of rational reconstruction has a curious feature: it yields the results independently already attained in science but replaces rationally opaque processes with transparently rational definitions and inferences. (Carnap 1967, p. 89):

> The fact that we take into consideration the epistemic relations does not mean that the syntheses or formations of cognition [*Erkenntnis*], as they occur in the actual process of cognition, are to be represented in the constructional system with all their concrete characteristics. In the constructional system, we shall merely reconstruct these manifestations in a rationalizing or schematizing fashion; intuitive understanding is replaced by discursive reasoning.

Rational reconstruction replaces the rationally opaque psychological processes by which knowledge is typically attained with explicit logical definitions and inferences that show how the results of those processes are genuinely objects of rational knowledge.

The rational reconstructionism of the *Aufbau* fits into two more disputes central to German epistemology at the time. The first dispute is over the rational status of metaphysical claims. Carnap's principal philosophical use of rational reconstruction is not so much positive regarding the sciences as negative regarding metaphysics. Carnap argues that the claims of the metaphysicians *cannot* be rationally reconstructed: such talk has no touchstone in experience and no rational control from logic; it is without content and form. The second dispute is the by-then long-standing question of the relation of epistemology to psychology. Carnap's rational reconstructionist epistemology distinguishes the questions of the objectivity of knowledge that are properly epistemological from the workings of cognition in the causal order that is a matter

for empirical study of psychology. These two debates come together since presumably there is some psychosocial causal story about why some people talk about metaphysical matters such as the relation of Dasein and Nothingness. Carnap's view was that whatever causal story there is that explains the existence of such talk in a culture, the talk itself cannot be rationalized and has no meaning. Thus, the task of rational reconstruction is not simply to reproduce willy-nilly whatever the results of any psychological process may be. By 1932, Carnap (1959) explicitly claimed that the processes giving rise to metaphysical talk were affective or conative, not cognitive, psychological processes.

The first logical empiricist work in English to make much of the notion of rational reconstruction was Hans Reichenbach's *Experience and Prediction*. In this work, the need to distinguish properly epistemological from psychological concerns is very much in the foreground, and rational reconstruction is marshaled exactly here (Reichenbach, pp. 5f):

> Epistemology does not regard the processes of thinking in their actual occurrence; this task is entirely left to psychology. What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system; or to construct justifiable sets of operations which can be intercalated between the starting-point and the issue of thought-processes, replacing the real intermediate links. Epistemology thus considers a logical substitute rather than real processes. For this logical substitute the term *rational reconstruction* has been introduced; it seems an appropriate phrase to indicate the task of epistemology in its specific difference from the task of psychology.

Indeed, Reichenbach introduces his famous distinction between the contexts of discovery

and justification as a "more convenient determination" (Reichenbach, p. 6) of the notion of rational reconstruction. Reichenbach stresses the way in which the justification of scientific claims is a matter of public communication whereas the psychology of scientific discovery can be a subjective and intuitive matter. Only the former is the proper concern of epistemology, and even in this area, epistemology substitutes a fully logically articulated structure for the more inchoate and suggestive arguments actually found in the writings of scientists engaged in reasoned persuasion. Following Reichenbach, the term "rational reconstruction" was routinely employed by logical empiricists to explain the point of their philosophical enterprise.

Logical empiricist rational reconstruction was not without its critics. The most famous is, of course, Willard van Orman Quine, who argued that once logical empiricists themselves rejected radical reductionism–the translation of all significant discourse into the language of experience–there was no further point for rational reconstruction (Quine, p. 78):

> To relax the demand for definition, and settle for a kind of reduction that does not eliminate [the defined terms–AR], is to renounce the last remaining advantage that we supposed rational reconstruction to have over straight psychology; namely, the advantage of translational reduction. If all we hope for is a reconstruction that links science to experience in explicit ways short of translation, then it would seem more sensible to settle for psychology. Better to discover how science is in fact developed and learned than to fabricate a fictitious structure to a similar effect.

Quine's move toward psychology and naturalism here has been widely followed. One can read Carnap's movement away from the term "rational reconstruction" to "explication" as a sort

of response to Quine, however. The point of philosophical work is not to find out how science has developed and been learned, but to provide precise resources for its future development through the conceptual tools of logic. That is, rational reconstruction or explication are, for Carnap, less forensic norms for the purpose of understanding and evaluating how things have gone in science and more deliberative resources for aiding in a clearer language of science for the future. This is, in fact, how he connected rational reconstruction and explication in the preface to the (1961) second edition of the *Aufbau* (Carnap 1967, p. vi):

> By rational reconstruction is here meant the searching out of new definitions for old concepts. The old concepts did not ordinarily originate by way of deliberate formulation, but in more or less unreflected and spontaneous development. The new definitions should be superior to the old in clarity and exactness, and, above all, should fit into a systematic structure of concepts. Such a clarification of concepts, nowadays frequently called "explication," still seems to me one of the most important tasks of philosophy....

Rational reconstruction, especially when deployed as a strict distinction between scientific discovery, and justification has been subject to other important objections. Thomas Kuhn and others in the historical philosophy of science have stressed a continuity of discovery and justification; Kuhn (1977, 328) remarks, for example, "considerations relevant to the context of discovery are then relevant to justification as well; scientists who share the concerns and sensibilities of the individual who discovers a new theory are *ipso facto* likely to appear disproportionately frequently among that theory's first supporters." Indeed, Kuhn's work on multiple discovery suggests an even deeper lesson: in the absence of successful justification,

scientific discovery has not even happened. Discovery is generally assigned to whomever can best marshal the resources of persuasion, and a claim that is not substantiated in the scientific community is not a scientific discovery at all.

Lakatos

The second major attempt to explain philosophy of science as rational reconstruction was at the very center of historical philosophy of science that had rejected logical empiricist rational reconstructionism. Imre Lakatos importantly revived the notion of rational reconstruction in a new setting: he argued that philosophy of science engaged in rational reconstruction of the history of science. Lakatos argued that any history of science begins with a prior normative sense as to what counts as a scientific achievement in the first place, and this normative sense is an implicit or explicit philosophy of science. An explicit philosophy of science is, thus, an account of the constitutive norms of scientific achievement, and a history of science based on such a philosophy presents an internal history of scientific rationality according to this norm. Lakatos summarizes his view of the relation of philosophy of science and history of science this way (Lakatos 1978b, p. 102):

> (A) philosophy of science provides normative methodologies in terms of which the historian reconstructs 'internal history' and thereby provides a rational explanation of the growth of objective knowledge; (b) two competing methodologies can be evaluated with the help of (normatively interpreted) history; (c) any rational reconstruction of history need to be supplemented by an empirical (socio-historical) 'external history'.
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> A philosophy of science, thus, determines what internal history of science is, which, also,

determines which historical questions about science are relegated to an external, social and psychological history. Philosophical presuppositions, therefore, set the problems that an historian must solve if she is to write a history of the rational development of science, while, simultaneously, assigning some problems to a nonrational or even irrational external history. For example, a falsificationist methodology of science can assign a rational meaning to an experiment only if it can find a theory that the experiment attempts to falsify. Thus, if an experiment is presented by the scientists who designed it as independent of theory, the falsificationist historian must find hidden in the historical record a theory that the experiment in fact tested. In the absence of such a theory, the falsificationist cannot assign a rational point to the experiment. If no such theory can be found, then, the falsificationist must assign both the scientist's own account of science and her experimental activity to a nonrational external history–perhaps a history of false consciousness due to faulty scientific pedagogy.

It does seem plausible that an historian of science must go to the historical record with certain presumptions about which activities are properly scientific–what Robert Boyle did with his air pump is part of the history of science; what Robert Boyle had for his breakfasts and what his butler did with the air pump are, neither of them, events properly in the history of science. Lakatos's pronouncements on how to write history, however, struck many as so theory-laden as to be a deliberate falsification even of what is obviously internal to science. In a famous passage, Lakatos writes, for example (Lakatos 1978b, p. 119):

> Internal history is not just a *selection* of methodologically interpreted facts; it may be, on occasions, their *radically improved version*. One may illustrate this using the Bohrian programme. Bohr, in 1913, may not have even thought of the

> possibility of electron spin. He had more than enough on his hands without the spin. Nevertheless, the historians, describing with hindsight the Bohrian programme, should include electron spin in it, since electron spin fits naturally in the original outline of the programme. Bohr might have referred to it in 1913. Why Bohr did not do so, is an interesting problem which deserves to be indicated in a footnote.

When called upon to diagnose Lakatos's curious attitude toward history, some theorists, notably Hacking and Koertge, have, quite rightly, pointed to Lakatos's lingering Hegelianism. Unlike most empirically-minded Anglo-analytic philosophers, who think that the set of historical events is simply the set of all things that have happened, Hegelians have a teleological sense of history and any event that does not fit into the thread of the story is not part of history. To have a diagnosis of Lakatos's attitude is not yet to convince anyone else to adopt his view. Many have found Lakatosian rational reconstructions of history of science to be grotesque parodies that cover up more than they reveal about the dynamics of science. Kuhn indicated, indeed, how Lakatosian rational reconstructions (which must have looked like highly theoretical versions of the textbook histories of science he inveighed against) violate the norms that historians work within Kuhn (2000, 151n32):

> The problem is not that philosophers are likely to make errors–Lakatos knows the facts better than many historians who have written on these subjects, and historians do make egregious errors. But a historian would not *include in his narrative* a factual report which he *knew* to be false. If he had done so, he would be so sensitive to the offense that he could not conceivably compose a footnote

calling attention to it.

Conclusion

Philosophy of science has been–and continues to be–motivated in large measure by a desire to explain the rationality of science. Rational reconstruction was, to an important degree, a response to the demise of inductivist accounts according to which science was rationally constructed via an inductive method. Rational reconstruction worked together with conventionalism and hypothetico-deductivism to grant a freedom and creativity to the work of scientists while rescuing the rationality of science, which was now located in *post facto* rational evaluation. Debates over rational reconstruction, thus, are ultimately debates about whether philosophers should be defending the rationality of science and what resources are available to do so.

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