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By Louis Caruana

Peter J. Riggs, *Whys and Ways of Science*, Melbourne University Press, 1992.

Is science a fully rational enterprise, or does it involve changes which are ultimately independent of logic and reason? This question has been central in recent debates concerning philosophy and sociology of science. Through an introductory presentation of various positions, Riggs attempts to trace an argument in favour of the rationality of science. The book is divided roughly in two parts, the first dedicated to philosophy of science and the second to sociology of science.

Riggs first clears the ground by providing a short preliminary overview of some central topics like deduction, induction, the underdetermination of theory by data, and the theory-ladenness of observation. This done, he starts by giving a useful overview of Kuhn's theory. Kuhn has been discussed in many other works. What is particular to the presentation in this book is the care with which the author defines the typical Kuhnian concepts, as for example pre-paradigm science, normal science, problems, puzzles, anomalies, crises states and so on. What is also particular to this presentation is the use of a flow chart to illustrate Kuhn's scheme of science. As a memory aid, this can be very useful. In diagrammatic form, it shows how, during normal science, researchers have to deal with puzzles. These puzzles may be shelved to wait for further work, but they may also be solved, in which case the researchers remain within their working paradigm. If the puzzle proves itself too stubborn, it becomes an anomaly. The researchers will then be in a crisis state, where normal science is abandoned for extraordinary science. The anomaly may be shelved again, but if it isn't, then a candidate for a new paradigm for science will be proposed, and if this new paradigm wins the debate, the well known Kuhnian scientific revolution occurs. The major drawback that Riggs underlines in Kuhn's theory is that it does not give enough criteria for a demarcation between science and non-science.

This leads him to consider the Lakatosian approach involving methodology of scientific research programmes. The presentation starts out with the insight that naive versions of falsificationism are wrong: one counterexample is not enough to bring down an entire theory. For Lakatos therefore research programmes have two major parts: the hard core and the protective belt consisting of auxiliary hypotheses. When the predicted novel facts do not receive confirmation, a research programme starts to degenerate. Prolonged lack of confirmation shows that the protective belt is losing its function. However it is not sufficient to eliminate a programme solely on the basis that it presently appears not to be making empirical progress. The programme could be in a degenerating phase from which it will recover. Hence the competition between rival research programmes can continue for centuries, because there is no such thing as a crucial experiment for a final decision. A scientific revolution occurs when a progressive research programme supersedes its degenerating rivals. So the lag between emergence of a new research programme and its becoming empirically progressive shows that there is no 'instant rationality'. But this is not problematic. Riggs explains well how, on this view, definite criteria are always available. In fact, at any given moment, it could be rational to stick to one's own research programme even though it is degenerating, but it is certainly not rational to deny its poor public record.

Riggs proceeds by giving an account of how L. Laudan develops these Lakatosian ideas into the theory of evolving research traditions. A research tradition is a set of general assumptions about the allowed ontology and methodology. It is thus a set of assumptions concerning the entities and processes in the domain of study, and concerning the appropriate methods to be used for investigating the problems and constructing the theories in that domain. Riggs shows how Laudan's research tradition performs much the same function as a Kuhnian paradigm or a Lakatosian scientific research programme, with an important difference. Laudan insists that the central elements of a research tradition, elements which are usually considered unrejectable, change through time. Riggs rightly points out however that there may be a dilemma here not mentioned by Laudan: the core elements are in a strong sense constitutive of the research tradition. For example, all theories within heliocentric astronomy must *ipso facto* postulate that the sun is stationary. Laudan's way of accounting for change of research traditions depends on his mini-max strategy. Riggs explains this as follows. The aim of science is maximising the number and scope of solved empirical problems, whilst minimising the number and scope of anomalies and conceptual problems for one's own theory. The choice of which research tradition to accept is therefore bound up with the factors that govern the problem-solving assessment of individual theories. The interesting thing here is that the rational choice is assumed by Laudan to be linked to factors external to the relevant scientific discipline. There is no basic difference between science and other academic pursuits. So it will be detrimental to our understanding of science if we were to divorce it from its context.

One aspect of this context is certainly the society within which science emerges. This point offers an interesting bridge from philosophical to sociological analysis of science. Riggs presents first sociology of science and then sociology of scientific knowledge. The former is primarily concerned with the social network and hierarchy of scientists in institutionalised science. In broad strokes, Riggs presents the issue as a debate between R. K. Merton and M. Mulkay. To achieve the goal of certified knowledge and to regulate its own activities, the social system of science is governed by a number of institutional imperatives. These are inferred from the moral consensus of scientists as expressed in practice, in countless writings on the scientific spirit and in moral indignation directed towards violation of ethos. That is Merton's position. Mulkay objects. He argues that the rules and moral imperatives are not in any sense binding on the members of the scientific community. A scientist's primary motivation is self-interest or interest in one's own research group. Merton's institutional imperatives should be seen, at best, as describing an ideal situation. There are no institutionalised normative principles to which all scientists conform. If there is an ethos at all, it is a very flexible one.

If sociology of science concerns the behaviour of scientists, the sociology of scientific knowledge concerns the content of scientific theories. The thesis is that this content may be socially, as distinct from empirically or rationally, determined because all societies exert pressures on their members to conform to established rules, beliefs and values. The Strong Programme in the Sociology of Scientific Knowledge holds that the content of scientific theories is greatly affected by prevailing political and social structures to the extent that the type of entities, laws or processes postulated in them are completely, or at least in large part, determined by these structures. But this view needs careful handling. Riggs rightly recalls that it is one thing to say that society plays a part in the formation of the content of theories, but it is quite another to say whether these contents do indeed correspond to what the world is like. Advocates of the strong programme conflate these two questions by embracing the interests analysis of science. In this analysis, scientific theories are viewed as means by which a group of scientists may further its interests. These interests determine in advance a scientist's choice of theory. And instrumental interests are themselves related to social interests, because the latter may dictate what sort of prediction is important or what should be considered a problem. Riggs alludes here to two prominent examples. The first involves the claim that, when historical evidence is genuinely taken into consideration, all attempts to describe science using fixed rules find severe difficulties. The conclusion is therefore that, as far as methodology of science is concerned, anything goes. This is Feyerabend's position. The second example Riggs offers is B. Latour's position. Social anthropological analysis of science is taken to provide a way of explaining how scientific facts are socially constructed. Latour employs the term ‘black box’ to signify anything in science that is uncontroversial. A sentence may be made more of a fact or more of an artefact depending on how it is inserted into the context of other sentences. By itself a given sentence is neither a fact nor a fiction. A sentence is on the way of becoming a fact when its origin becomes less and less obvious to those who read it. In this scheme, discoverers of facts are those who change sentences into black boxes. Riggs concedes that this position throws valuable light on the nature of the day to day research in laboratories, but leaves a lot to be desired in the way of explaining science itself.

In the final evaluative chapter, Riggs elaborates his own position. He argues that sociological theories of science show a ‘major failure’ because they do not take into consideration the epistemic and methodological dimensions of scientific research. There is a short-term aim of science, namely of solving problems, and a long-term aim of gaining truths about the world. He favours Laudan’s position: those theories which we come to call ‘scientific’ are efficient at advancing our cognitive aims, and in general, they do so better than theories we denote as ‘non-scientific’. This is a two step process. One’s aims justify one’s methodology which in turn is constrained by one’s theories. What is specifically rational is partly dependent on time, place and context. Yet, some general aspects of rationality are trans-temporal and trans-cultural. This is how Riggs endeavours to account for science as an activity conducted in accordance with rational principles. He suggests, rather briefly unfortunately, how Laudan’s theory may be extended. He defines a general explanation as one which accounts for a majority decision. This is opposed to individual explanations which account for individual actions or choices. Between the general and the individual explanations he envisages a ‘filter’. This filter-mechanism, the exact nature of which is case-dependent, ensures that the individual explanations that are *prima facie* inconsistent with the general explanations do not act as refuting instances of the general explanation.

Whether such an extension to Laudan’s theory is enough to ensure that some core aspects of rationality are trans-temporal and trans-cultural is unfortunately not discussed. It is probable that some readers will feel themselves deprived of a more detailed exposition of the author’s own position. Moreover, given the number of places where allusion is made to K. Popper’s great influence on a number of positions discussed, it seems that Popper could have merited a chapter on his own. But apart from these minor points, the book offers a valuable survey of philosophical and sociological theories of science, as its subtitle indicates. It is well documented with many references and contains a good number of historical examples from different branches of science. The type of reader the author had in mind is not specified, but it seems that the book is especially appropriate for advanced undergraduates of philosophy and sociology of science. It is also useful to scientists who want to learn more about recent debates concerning their discipline. Its combination of philosophical and sociological perspectives makes it useful, as a means of broadening their horizon, to those who are specialising in only one of these fields.

Louis Caruana
St. Edmund’s College
Cambridge