

# **THE RATIONAL AND THE SOCIAL**

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How to Understand Science in a Social World

James Robert Brown

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SOCIAL THEORY



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Volume 55

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JAMES ROBERT BROWN

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# **Preface to the Reprinted Edition**

It has been about twenty-five years since this book first appeared. Much has changed. It was written at a time when those of us who took reason and evidence seriously were doing battle with social constructivists. Of course, champions of constructivism would have objected to the way I just put it, saying they, too, believe in reason and evidence. It is true; they did. But they put it in a social form, robbing it of the normative force that “rationalists” took to be crucial. The trick at the time was trying to convince readers that reason is a thing in its own right, while also allowing for the presence of genuine social factors that shaped the development of science.

I’m not, however, interested in refighting those battles. Much more interesting is the way that things have developed. Philosophers of science today pay more attention to social factors than they did. They owe their enriched views in part to sociologists of knowledge and in part to feminist critiques. On the other hand, social constructivists, including the ones discussed in this book and the newer generation, are much more sensitive to reason and evidence, even if it is not discussed specifically in those terms. It would be going too far to say there has been a complete convergence of views. But each side now profits considerably from the work of the other. I would be pleased to think the original edition of this book played a tiny role in this happy outcome.

**James Robert Brown**

Toronto, November 2013

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# THE RATIONAL AND THE SOCIAL

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JAMES ROBERT BROWN



ROUTLEDGE  
London and New York



For those I grew up with

my father, Andrew  
my mother, Isabel  
my brother, Alan  
my sister, Andrée  
my brother, Gordon

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# PREFACE

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Ever since Plato took up the cudgels against Protagoras, when the latter said that ‘man is the measure of all things’, the battle over relativism has been fought again and again. It may seem like an endless cycle of claims and counter-claims, but we would be mistaken to think that nothing is ever learned in any of the successive clashes.

Among the main contenders in the present debate are, on the one hand, a small but vigorous and influential group of sociologists and historians of science centred in Edinburgh. They preach (and practise) a radically sociological approach to the understanding of how knowledge (scientific knowledge, in particular) is acquired. On the other side of this debate are their rationalist opponents, including most philosophers of science and traditional historians of ideas. They see ‘evidence’, ‘good reasons’, and ‘rational belief’ rather than non-cognitive ‘interests’ as the guiding force behind the development of science.

This book is a contribution to the present debate. It is, for the most part, squarely on the rationalist side. The first chapter is an exposition of the new sociological way of doing things, the so-called ‘strong programme for the sociology of knowledge’. This chapter is followed by two more devoted to critically examining some of the chief tenets of the sociological movement. David Bloor’s ‘science of science’ and Barry Barnes’s ‘finitism’ are the main targets of chapters two and three, respectively. Some special topics in experimentation come up in the fourth chapter. The anthropological metaphor comes into play in the fifth and especially the sixth chapters, which are mainly about the role history might play in determining just what rationality is. The account given leads to

## PREFACE

a richer, and I think proper, way of understanding how science works. It is at this point that we begin to get a glimmer of how the social really enters science. Its penetration is quite significant, but its *locus* is not where the present debate between rationalists and sociologists is usually focused. The final chapter is devoted to this issue and, in particular, to what we might do about it; that is, it's about philosophical interventions to make science better.

That, in brief, is what the present book is about, but a preface should say something more; it should also say something about the author's larger intentions and motivations. I find that I have several. One of my concerns is to defend the old ideal of 'knowledge for its own sake'; but holding forth on this invariably induces cynicism or yawning, so the topic is best avoided. (Of course I do not wish to defend a childish view of the purity of science, and would readily admit that sociologists have taught us much about the sorts of things we must avoid in defending such an ideal.)

The other motivation has to do with political concerns. I am convinced that the social goals many of us hanker after, namely, a socialist world with equality among the classes, sexes, and races, is best served by rationality. It is true that some scientific theories (often pseudo-scientific, I would claim) have done considerable harm. Lots of biological theories, for instance, have been racist or sexist or have been used in defence of the most pernicious forms of free enterprise. This is undoubtedly why so many who share my political goals find themselves attracted to the sociologists' account of science. (There are speculations along this line about the sociologists' motives in the second chapter.) I am quite persuaded, however, that such a view is mistaken, even dangerously so, for the scepticism and relativism which result from it tend to produce a quietism and inaction rooted in a sense of hopelessness and pointlessness. One of the great forces for evil today is the religious right. Its members clamour for equal time with Darwin for 'special creation', since after all, both are 'just theories', as Ronald Reagan put it. This, of course, is merely a front for a not-so-hidden agenda; the suppression of women, trade unions, and third-world people is part of the bargain. Several relativist-minded sociologists of knowledge despise these political trends just as I do, but how are they to fight the battle? By saying, 'Yes, all theories are equally good; they merely serve different social interests'? Surely we can do better than that.

## PREFACE

On the other hand, science should not be left to its own devices. It can be improved, and I shall try to say how in the final chapter. But in spite of its shortcomings, science, on balance, is a force for liberation (as well as being a joy to behold in its own right). Perhaps this book will go a little way toward convincing some readers of this fact. We have every good reason to think that rationality serves the demands of justice just as it serves the demands of curiosity. Corny as it may sound, science is a friend of the oppressed just as surely as it is the glorious entertainer.

Well, enough of the missionary talking to cannibals; let's get on with it.

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Writing a book makes one aware just how much is owed to others. Morsels of information and good sense obtained by osmosis often make their way to the front of the mind when the time requires it. How much I owe to others I cannot say, but I do not doubt that there are traces on every page. But, inevitably, only some of these can be singled out for explicit thanks: Ian Hacking, Noretta Koertge, Larry Laudan, Andrew Lugg, John Nicholas, Alex Rosenberg, and Michael Ruse. I also learned a great deal from the contributors to two recent volumes, Hollis and Lukes (1982) and Brown (1984). There are others to whom I owe even more. They include: William Newton-Smith, who invited the book in the first place, offered several valuable suggestions for improvement, and displayed great patience at my slow progress; David Bloor, who has been a most valuable correspondent of long standing and who spent a memorable day riding trains and sitting in Paddington station arguing these issues with me; and Robert Butts, with whom I have had numerous valuable conversations on these issues over the last dozen years. My greatest debt of all is to Kathleen Okruhlik, with whom I have argued these issues for longer than either of us cares to remember. Several ideas have been borrowed from her, only a few with acknowledgement. She read the penultimate draft and greatly improved it. To these and to those I haven't named, many thanks.

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# THE SOCIOLOGICAL TURN

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The problem we are concerned with is just this: How should we understand science? Are we to account for scientific knowledge (or rather, belief)<sup>1</sup> by appeal to the various social factors which may have been prevalent when the theory was being formulated? That is, should we appeal to the ‘interests’ which a group of scientists may have had? Undoubtedly, social factors play some role, but are social causes totally responsible for the production of belief? Or should we instead take a different approach and account for scientific knowledge in a fashion which mirrors the very accounts that scientists themselves might have typically given to justify their theory choices? Perhaps we should be citing the ‘evidence’ for the beliefs in question; perhaps we should be providing ‘good reasons’ as part of the explanation for holding the belief. Which approach to understanding science is right?

The most effective modern champions of social causation are the members of the ‘Edinburgh School’, a very vigorous group of sociologists and historians of science largely centred in the Science Studies Unit of the University of Edinburgh. The principal opposition to this way of viewing science comes from philosophers as well as from the more traditional sociologists and intellectual historians. But the problem we are faced with is as old as it is tricky; so before elaborating on the modern debate, a backward glance would not be out of place.

## THE NATURAL VERSUS THE SOCIAL SCIENCES

If we are to set the stage properly for the concerns of this book, we could hardly do better than to start with a brief look at Karl



Mannheim's view. In *Ideology and Utopia*, the *magnum opus* of the father of the modern sociology of knowledge, Mannheim characterizes the discipline this way: 'The principal thesis of the sociology of knowledge is that there are modes of thought which cannot be adequately understood as long as their social origins are obscured' (Mannheim 1936:2). In itself, this seems quite innocuous; it expresses a sentiment that we all might agree to, for we all concur that *some* people believe *some* of the things they do because of social factors. Quarrels do not arise until it becomes a question of *which* people and *which* beliefs. Things usually become heated when it is suggested that all of our *scientific* beliefs have a (possibly contaminating) social origin.

There has been a long tradition in the sociology of knowledge which has sharply separated beliefs into two kinds. A boundary is drawn between mathematics and the natural sciences on the one hand, and almost everything else on the other. Included in this latter collection are such 'cultural' things as: religious beliefs, morals, 'practical wisdom', and, often enough, the social sciences. The natural sciences are viewed as pristine, uncorrupted by any considerations of interest, while everything else is at least suspect, viewed as likely ideology, tarnished with subjectivity, and corrupted with naked or concealed self-interest.

Mannheim is part of this dualistic tradition (at least in some of his writings; he tended to be ambivalent on the issue). In one place he expresses the dichotomy this way:

It may be said for formal knowledge that it is essentially accessible to all and that its content is unaffected by the individual subject and his historical-social affiliations. But, on the other hand, it is certain that there is a wide range of subject-matter which is accessible only either to certain subjects, or in certain historical periods, and which becomes apparent through the social purposes of individuals.

(Mannheim 1936: 150)

By 'formal knowledge' he means the natural sciences and mathematics. In another passage, Mannheim again gives voice to the duality between the natural and the social:

Are the existential factors in the social process merely of peripheral significance, are they to be regarded merely as

conditioning the origin or factual development of ideas (i.e. are they of merely genetic relevance), or do they penetrate into the 'perspective' of concrete particular assertions? This is the next question we shall try to answer. The historical and social genesis of an idea would only be irrelevant to its ultimate validity if the temporal and social conditions of its emergence had no effect on its content and form. If this were the case, any two periods in the history of human knowledge would only be distinguished from one another by the fact that in the earlier period certain things were still unknown and certain errors still existed which, through later knowledge were completely corrected. This simple relationship between an earlier incomplete and a later complete period of knowledge may to a large extent be appropriate for the exact sciences (although indeed today the notion of the stability of the categorical structure of the exact science is, compared with the logic of classical physics, considerably shaken). For the history of the cultural sciences, however, the earlier stages are not quite so simply superseded by the later stages, and it is not so easily demonstrable that early errors have subsequently been corrected. Every epoch has its fundamentally new approach and its characteristic point of view, and consequently sees the 'same' object from a new perspective.

(Mannheim 1936: 243)

Admittedly, Mannheim hedges on the 'stability of the exact sciences'; nevertheless, a sharp distinction between such things as physics, chemistry, and mathematics on the one hand, and what he calls the 'cultural sciences' on the other, is made.

In consequence of this distinction, we should have a similar dichotomy in our approach to accounting for belief. If we wish to explain why a certain scientist or community of scientists believes (or did in the past believe) a particular theory of physics (or some other natural science) we should look to the evidential reasons which were available. However, if it is a belief in a theological, moral, or economic doctrine that is to be accounted for, then 'evidence' would have little or nothing to do with it. Instead, we must look to the social factors prevalent at the time the theory choice was made, for it is these social factors which would have caused such a decision. We should point to the evidence to account for the growth of natural science; and we should point to social causes to account for every other kind of belief.

It is very important to notice, in the passage cited, why Mannheim makes the distinction between the cultural and the natural sciences. He thinks that the exact or natural sciences exemplify a 'simple relationship between an earlier incomplete and a later complete period of knowledge'. This reflects a view of the development of science known as the 'cumulative' account: Once a fact has been discovered, or a theory established, it is never abandoned; only more facts are added to it.

Mannheim is not alone in holding such a view of natural science; nor is he alone in thinking it is a good reason for a hands-off attitude toward it by sociologists. In a recent exegetical piece on the sociology of knowledge, Werner Stark voices the same sentiment when he writes, 'Because man must take the facts of nature as he finds them, while the facts of culture are his own work, the social determination of knowledge will be different in the two instances' (Stark 1967: 477, vol. 7).

This is, however, a view of the natural sciences that few today give any credence to. In fact, quite the contrary. Whether they are proponents of a 'rational' or of a 'sociological' approach to the understanding of science, virtually all contemporary commentators think the natural sciences have been revolutionary, not cumulative. Many past theories and even many past 'facts' have been completely overthrown.

Indeed, attacks (especially that found in Kuhn's *The Structure of Scientific Revolutions*) on the cumulative account of science have done much to inspire the contemporary sociological turn. One of the leading figures in the recent sociological movement, Barry Barnes, puts it this way:

It is well known that as scientific knowledge has developed, numerous mechanisms and theories have been postulated and successively set aside. This is, indeed, why so many philosophers of science have struggled to maintain a fact/theory distinction, and to base their justificatory rhetoric on the accumulation of facts. But there has also been a good deal of informal faith placed in the progressive quality of this sequence of theories and mechanisms. Recent historical studies, however, in particular those of T. S. Kuhn (1970), effectively undermine this faith; they demonstrate that fundamental theoretical transitions in science are not simply rational responses to increased knowledge of

reality, predictable in terms of context-independent standards of inference and evaluation.

(Barnes 1977: 23)

Most philosophers would not put it as Barnes has done, but they would agree with the upshot: Mannheim's reason for distinguishing between the natural sciences and the social sciences is not such a good reason after all. The natural sciences do not develop in a cumulative fashion. But does this then mean that we must look to social causes to explain the developments in the natural as well as the social sciences? Must we explain *all* beliefs by citing social factors? Or should we never turn to the social to account for any beliefs at all and look instead for 'good reasons' to account for every sort of belief? Perhaps there are yet other ways to look at it.

## THE ARATIONALITY PRINCIPLE

Though the dichotomy between the natural and the social sciences is not one which will stand up, nevertheless, its breakdown need not lead automatically to a fully-fledged sociological account of *all* belief. One might maintain that what Kuhn and other historians have shown is not that the natural sciences require a sociological account, but rather that we need a new and richer notion of rationality. It will have to be a notion which, among other things, does not require a cumulative history of science. And this is just what several contemporary, post-positivistic, philosophers of science have been trying to do.

Larry Laudan is one prominent philosopher who is working in this direction. His *Progress and its Problems* contains a number of interesting suggestions and proposals, but one of the most important is his denial of any sharp boundary between science and non-science, or between the natural and the social sciences. What is good method in science, he claims, is good method anywhere that there are cognitive aims. Thus, physics and theology, mathematics and metaphysics, geology and economics are all on a par as far as they have the same method for correctly pursuing their cognitive ends. Of course, the practitioners of one or the other of these disciplines may not be following the method properly, but if the various practitioners were to be completely rational they would use the same general procedures. Scientific rationality

simply means following the right method, and it is the same method for all.

As well as rejecting a demarcation between science and other cognitive activities, Laudan would also reject the idea that the practitioners of the natural sciences have made nothing but rational choices. Indeed, no one seriously believes that every scientist who ever held to some theory of physics, chemistry, or mathematics, did so because of the evidence then available. It is widely thought, even by the staunchest champions of scientific rationality, that sometimes a scientist will act irrationally, either by believing something which is totally crazy or by believing the right thing for the wrong reasons. And those 'bad reasons' might often stem from social forces. It will simply not be true that every cognitive decision in the history of the natural sciences can be accounted for by appeal to the evidence available to those who made the decision.

In consequence, some sort of guideline is required, some sort of rule which will tell the historian how to approach individual cases in the history of science. A rule which has been proposed is this: If a belief can be explained as being the result of the rational examination of the evidence available then that should be accepted as the correct explanation. If, and only if, no such rational explanation is available, should we account for the belief by appeal to a social (or some other) cause. In the frequently used jargon of 'internal/external', we should only seek an external account if no internal one can be found. Robert Merton, for instance, holds such a view: 'thought has an existential [i.e., social] basis in so far as it is not immanently [i.e., rationally] determined' (1969:516). This methodological principle is explicitly adopted by Laudan and called the 'arationality principle':

basically, it amounts to the claim that *the sociology of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits*. . . . Essentially, the arationality assumption establishes a division of labor between the historian of ideas and the sociologist of knowledge; saying, in effect, that the historian of ideas, using the machinery available to him, can explain the history of thought insofar as it is rationally well-founded and that the sociologist of knowledge steps in at precisely those points where a rational analysis of the

## THE SOCIOLOGICAL TURN

acceptance (or rejection) of an idea fails to square with the actual situation.

(Laudan 1977: 202)

If a slogan is useful, let it be this: 'Sociology is only for deviants' (Newton-Smith 1981:238). The sociologist is to step in when and only when there is some deviation from the norm of rationality.

The arationality principle is probably the very antithesis of the sentiments embodied in the recent sociological turn. Though I do not accept it myself without major qualifications, its advocacy and employment are common. As a rule of thumb, it embodies the rationalist outlook. It is interesting that the (traditional) sociologist Robert Merton and the rationalist philosopher Larry Laudan concur in their endorsement of this approach to understanding science. The recent sociological turn is as much an attack on the Mertonian way of doing the sociology of science as it is an attack on philosophers of science and traditional historians of ideas. The lines of battle and the disciplinary boundaries do not correspond.

## THE STRONG PROGRAMME

The claims of the new cognitive sociologists of knowledge, especially the members of the Edinburgh school, are much stronger than the mere assertion that sometimes social factors have to be considered in order to have a complete account of an episode in the history of science. Rather, it is maintained that social causes are always present; they are the determining factors. This position is most clearly and forcefully put in David Bloor's important and influential work *Knowledge and Social Imagery*, and it goes by the name 'The Strong Programme'.

Broadly speaking, there seem to be three types of consideration used to support the new sociological approach. The first of these is based on the claim that it is the only approach to science which is itself scientific. The second stems from philosophical considerations about underdetermination and related issues, the suggestion being that there is not enough evidence to make rational decisions anyway. The third type of alleged support for the sociological approach comes from the perceived successes of recent case studies.