

dynamics of moving bodies. Left to its own devices, science, like a physical object, moves naturally away from where it started, and abnormal change occurs only when external forces come into play. In Laudan's words, 'a body moving at constant velocity and a man behaving rationally are both "expected states", which require no further causal analysis. It is only when bodies change velocity or when men act irrationally that we require an explanation of these deviations from the expected pattern'.³

Philosophers reflecting on these two ways of explaining beliefs often distinguish between rational and causal explanation. They see the explanation of scientific belief as involving—at least for the most part—what Morton White has called 'noncausal rational explanations'.⁴ On this view, historians of science mostly ignore the causal antecedents of beliefs and concentrate instead on the logical and methodological relations between them. For them, what matters is the abstract propositional content of beliefs, and this evidently cannot be 'existentially determined' or 'socially caused'. In short, the claim is that the historian of science has to revert to causal explanation only when attempting to account for the nature of scientific activity or the social preconditions of scientific research or the occasional irrationalities that mar the smooth development of science.

This general conception of the historiography of science continues to be widely accepted, but it has not gone unchallenged. In the view of sociologically-oriented historians, noncausal rational explanations are less than fully scientific. David Bloor, for instance, has argued at length that the sociology of knowledge should be 'symmetrical in its style of explanation', and he has dismissed models of explanation according to which 'logic, rationality and truth appear to be their own explanation (and) causes do not need to be invoked'.⁵ According to Bloor and other historians of a similar persuasion, explanations in terms of reasons should never pre-empt social explanations, and Merton and Laudan are wrong to insist that well-founded beliefs should be accorded special treatment. Bloor's uncompromising contention is that social explanation should be extended to all beliefs regardless of their rational merits.⁶

torigraphy: An Apology for Internal History of Science', *Minnesota Studies in the Philosophy of Science*, Vol. 5, Minneapolis 1970, p. 135. 'Internal history', according to Hesse, is the 'history of rational thought evolving according to its own inner logic'. See also E. McMullin, 'The History and Philosophy of Science: A Taxonomy', *ibid.*, pp. 21-23.

³ Laudan, *op. cit.*, note 1 above, p. 189.

⁴ M. White, *Foundations of Historical Knowledge*, New York 1965, pp. 194-200. See also M. Hesse, 'Reasons and Evaluation in the History of Science', in M. Teich and R. Young (eds.), *Changing Perspectives in the History of Science*, London 1973, p. 134. In Hesse's view, explanation in the history of science is usefully thought of in terms of White's general categories.

⁵ D. Bloor, *Knowledge and Social Imagery*, London 1976, pp. 5, 6.

⁶ See also B. Barnes and S. Shapin, Introduction, *Natural Order*, London 1979, p. 11.

However, such arguments also leave much to be desired. One cannot show that there are no restrictions on the province of the sociology of scientific knowledge simply by arguing against noncausal explanation.⁷ We may agree with Bloor that scientific history, like any other science, must rely exclusively on causal explanation. But this concession is fully compatible with the view that well-founded and ill-founded beliefs should be explained in different ways. For we can reject the view that scientific beliefs should always be related to their social contexts even as we deny that the history of science is primarily concerned with logical and methodological relationships between ideas.

To establish this point, it suffices to note that the historian's primary focus is on the causes of beliefs that have propositional content, not on the propositional content itself. Historians of science do not investigate relationships between disembodied ideas. With very few exceptions, what concerns them is the ways in which individual scientists come to acquire the ideas that they do. Thus, when historians explain beliefs in terms of reasons, they should not be seen as providing 'noncausal rational explanations'. What they intend to establish is rather that certain scientists accepted certain beliefs because they accepted other beliefs as reasons.⁸ Although somewhat misleading, it is simply more convenient to talk of scientific beliefs as such than to talk of scientists' acquiring and retaining beliefs.

The charge that principles like Merton's and Laudan's lead to an unsatisfactory view of the history of science can thus be turned back by the simple expedient of focusing on the acceptance of beliefs rather than on the beliefs themselves. The main idea motivating the intellectual historian of science is fully covered by the principle that a scientist's *acceptance or rejection* of an idea requires a social explanation when and only when the idea is not rationally well-founded. This principle assumes that all beliefs should be causally explained, but it also implies that rationally and nonrationally accepted beliefs should be explained in different ways. Intellectual historians should still be seen as concentrating on the rational acceptance of ideas, and social historians are still required to confine their attention to the explanation of ill-founded beliefs.

Here Barnes and Shapin speak of 'the spurious distinction between internal intellectual history of the rational growth of knowledge, and the external social history of science, dealing with allegedly irrational influences upon it'.

7 In this essay I take 'causal' in a broad sense. In particular I view any 'naturalistic' explanation as causal. This, I believe, is in accordance with the intentions of those who insist on causal explanation. But see also L. Laudan, 'The Pseudo-Science of Science', *Philosophy of the Social Sciences*, 11, 1981, 181.

8 For more on this view of rational explanation, see for example H. Aronovitch, 'Rational Explanation and Social Motivation', *American Philosophical Quarterly*, 15, 1978, 197-204.

II

But is it true that only rationally accepted belief should be rationally explained? Temporarily setting aside the question of whether rational beliefs always require 'rational explanations', I propose in this section to examine the contention that ill-founded beliefs should always be socially explained.

One difficulty concerning this view arises as soon as we consider what it is for a belief to be well-founded or rational. For many philosophers, a belief (or its acceptance) is rational provided it conforms to the dictates of a particular philosophical theory of rationality. If it conforms to the theory, we should explain it by indicating how it conforms; and if it fails to conform, we should detail the 'external forces' that caused the scientist to behave in a nonrational manner. On this view, then, history of science is subordinate to the philosophy of science. As Imre Lakatos has succinctly put the point, 'the history of science without the philosophy of science is blind'.⁹

The main problem here is not that claims like Lakatos' are self-serving, nor is it that there are almost as many theories of rationality as there are philosophers of science. It is rather that a belief may be ill-founded given the standards of the latest philosophical theory of rationality yet completely in accord with the standards that prevailed when the belief was being evaluated.¹⁰ In such cases, social explanation may be quite out of place. For the acceptance of the belief may be appropriately explained by making reference to reasons even though it is not sanctioned by present day philosophical standards. Thus, Pasteur's emphasis on crucial experiments may have been unreasonable (given what we now know), but it would be a gross error to conclude from this that his thinking should be explained in social terms. The principles underlying scientific investigation must be elicited from history, not imposed upon it. To insist that historians of science keep an eye on the latest developments in the philosophy of science is a prescription for anachronistic history, not the embodiment of a deep philosophical truth.

Moreover, even if the standards of the best theory of rationality always coincided with the standards of past scientists, departures from rationality still need not always be explicable in sociological terms. A theory may have been adopted for bad reasons, yet it may nevertheless

9 I. Lakatos, *The Methodology of Scientific Research Programmes*, Cambridge 1978, p. 202. See also L. Laudan, op. cit., note 1 above, p. 205, where Laudan insists that 'an essential prolegomenon to any adequate cognitive sociology is the choice as to a theory of rationality'.

10 It is important to notice here that the more adequate and up to date a theory of rationality, the less likely it is to yield the same prescriptions as those yielded by the standards employed by past scientists. For the philosopher's aim is to codify good scientific practice, and this is not fixed but in the process of being developed.

be entirely appropriate to explain its adoption by citing reasons rather than social or similar causes. For example, however unsound Descartes' attempt to explain the rectilinear propagation of light by invoking the notion of 'centrifugal endeavour' may have been, it would be foolish to argue that he was responding to social pressures or attempting to further his social interests. Historians are concerned with the question of whether scientists accepted theories for reasons, not with the question of whether they should have accepted those reasons. If this seems unclear, it is only because we naturally talk in terms of reasons supporting beliefs and this misleadingly suggests that what is at issue is good reasons rather than reasons as such.

The moral to be drawn from these observations is that it is more important to determine whether scientists have reasons for their beliefs than to determine whether the beliefs themselves are well-founded. We should explain beliefs in social terms when scientists do not have reasons for them; otherwise we should simply indicate the scientists' (good or bad) reasons. Laudan is wrong, it seems to me, to maintain that 'when a thinker does what is in fact irrational—even if he believes it to be rational—we require some further explanation (beyond citing his reasons)'.¹¹ What counts is whether the individual has reasons, not whether these are good reasons, still less whether they conform to some philosophical theory of rationality.

To say that social explanations are required whenever scientists do not have reasons for their beliefs improves the original principle, but it is still problematic. Consider Clairaut's first calculation of the movement of the moon's apogee. We cannot say that this provided him with reasons for thinking that Newton's theory was not entirely satisfactory. Clairaut accepted this conclusion because—as he himself later realized—he had made an error in his calculation. Thus, at least some beliefs are accepted neither on the basis of reasons, nor as a result of social influences. In particular, the explanation of Clairaut's miscalculation is more likely to be in terms of fatigue, parapraxis or some similar 'psychological' cause.¹²

This problem can, of course, easily be solved by holding that any belief not accepted for reasons should be explained in terms of social *or similar* factors. Surely, it might be argued, what is intended is that unreasonable beliefs should be explained in terms of 'non-scientific' considerations understood as including not only social factors but also psychological, economic and other factors of the same general kind. But this trivializes the original dictum. 'Social if not rational' provides guidance, but 'something else if not reasons' is entirely vacuous. The problem is that there seems to be no reason to restrict what can count as

11 Laudan, *op. cit.*, note 1 above, p. 188.

12 See also W. H. Newton-Smith, *The Rationality of Science*, Boston 1981, p. 254.

an appropriate 'similar factor', but the principle urged by Merton, Laudan and others is of no value if the notion of a similar factor is left open-ended.

III

The central concern of sociologically-oriented historians of science is, however, well-founded rather than ill-founded belief. In their view, social explanation should not be reserved for irrational belief, but should be applied to rational beliefs as well. Whereas 'the rational' and 'the social' have traditionally been held to be entirely distinct categories, it is now frequently argued that they overlap and ought to be integrated. The emphasis, as Barry Barnes has remarked, should be on 'the insufficiency of "reason" in science', not on its absence.¹³

When appraising the traditional view urged by Merton and Laudan, charity requires that we concentrate on reasonable belief rather than on belief that conforms to some philosophical theory of rationality. For social explanation may be as appropriate for rational belief as it is occasionally inappropriate for nonrational belief. Since the standards of our best theory of rationality—assuming that we have one—need not coincide with the standards employed by our historical subjects, beliefs that appear to have been accepted in accordance with these standards may actually have been accepted to further social interests or in response to social pressure.

Furthermore, explanations in terms of nonscientific, external considerations may be required even when there is nominal adherence to the dictates of our preferred standards of rationality. For there is a crucial difference between having reasons for a belief and accepting the belief for those particular reasons.¹⁴ Although rationalization is undoubtedly less prevalent in science than elsewhere, we cannot assume in advance that scientists accept beliefs for reasons whenever they have reasons for their beliefs. The interesting question, therefore, is not whether social explanations are ever in order when beliefs are well-founded or when scientists have reasons for their beliefs, but whether they are ever appropriate when scientists accept beliefs on the basis of reasons.

Some sociologically-oriented historians of science have pursued the heroic course of arguing that social explanation is always required. We should, according to these historians, explain scientists' acceptance of beliefs in social terms regardless of the reasons they have. For on the one hand, no theory can be established by empirical considerations alone, all theories being underdetermined by the available evidence; and on the other hand, no decision concerning the acceptance and rejection of a

13 B. Barnes, *T. S. Kuhn and Social Science*, New York 1982, p. 22. See also Barnes and Shapin, *op. cit.*, note 6 above, p. 11.

14 Cf. Aronovitch, *op. cit.*, note 8 above, p. 200.

theory can be fully empirical, there being no principles of theory evaluation that apply in all conceivable circumstances.¹⁵ However, despite their popularity, neither of these arguments is valid. The underdetermination of theory by data reveals the crucial role of theoretical and methodological considerations in science, not—as is claimed—the presence of a social component in theory acceptance. The fact of underdetermination would bolster the sociological approach to scientific knowledge only if intellectual considerations were always social in nature. Likewise, the relativity of standards of evaluation only reflects the fact that scientific inquiry is not simply a matter of collecting data and of applying logic and mathematics. To be sure, the required conclusions do follow given an exceptionally narrow conception of what counts as ‘empirical’, but there is little if anything to recommend such a conception.

Equally problematic is the commonly held view that social factors are reflected in theory acceptance because scientists occasionally appeal to conceptions of society and to other ‘external’ considerations.¹⁶ Such ideas may be thought of as ‘cultural resources’, but this does not preclude their being quite properly appealed to in the course of scientific deliberation. An idea may come from ‘outside’ science and it may even make reference to social ideas, yet it need not require a social explanation in any interesting sense of ‘social’. The fact that a reason is social rather than ‘scientific’ does not mean it should be afforded special treatment. It is a mistake to think that certain types of belief are ‘external’ to science, and that these and only these should be explained in social or similar terms. Any belief can function as a reason just as any belief, even the most clearly scientific belief, can be put to a social use. Surprisingly, despite their rejection of the distinction between internal and external factors, social historians of scientific ideas frequently assume an exceptionally narrow view about what is and what is not scientific. It is true that scientists often invoke ‘external’ considerations, but this does not in the least establish the need for social explanations of scientific beliefs.

To show that the rational does not exclude the social, one does not have to show that all beliefs require social explanation. We can admit—what is obvious anyway—that many explanations are quite satisfactory even though they make no reference to ‘underlying social

15 For arguments along these lines, see Bloor, *op. cit.*, note 5 above, p. 13, and M. Hesse, *Revolutions and Reconstructions in the Philosophy of Science*, Brighton 1980, p. 37. These arguments are discussed in greater detail in A. M. Lugg, ‘Two Historiographical Strategies: Ideas and Social Conditions in the History of Science’, in J. Brown (ed.), *Scientific Rationality: The Sociological Turn*, forthcoming.

16 See, for example, Barnes and Shapin, *op. cit.*, note 6 above, p. 9. According to Barnes and Shapin, ‘the contrast of “internal” and “external” factors in the history of science (should) cease to be a major source of interest or controversy’.

realities'. We can challenge the view urged by Merton and Laudan, yet agree that Newton's acceptance of the inverse-square law of gravitation, Lavoisier's acceptance of the oxygen theory of combustion, and Einstein's acceptance of the special theory of relativity can be satisfactorily explained by indicating the reasons that Newton, Lavoisier and Einstein had for their beliefs. On the other hand, if we are not to trivialize the problem we must be uncompromising about what counts as a social factor. What needs to be shown is not that scientists invoke allegedly nonempirical considerations, but that explanations of scientific beliefs however reasonable, must on occasion involve reference to the scientists' social circumstances. If we are to be as naturalistic in our explanations as critics of intellectual history demand, we must show that social causes play a role in the acceptance and rejection of theories; it is not enough to establish that their acceptance and rejection can be described as social phenomena in some weak, non-causal sense.

IV

When scientists accept beliefs for reasons, it might seem that we can always explain their acceptance of them simply by citing their reasons. As Laudan remarks, 'when a thinker does what it is rational to do, we need inquire no further into the causes of his action'.¹⁷ However, there are also cases of reasonable belief for which explanations exclusively in terms of reasons seem incomplete. Moreover, on occasion we must go beyond the scientist's reasons to the social circumstances in which they occurred. In particular, when a scientist's reasoning is sound yet puzzling, it may be appropriate to indicate why it made sense given the scientist's social context. And when different scientists accept different beliefs, we may only be able to explain their disagreement by tracing it to their differing social circumstances.

In the first type of case, a scientist accepts a theory in conformity with the prevailing standards, yet from our standpoint the adoption of the theory appears odd, inconclusive, even unreasonable. The problem here is not that we do not appreciate why the theory was accepted for the reasons it was, but rather we do not understand why anyone would take those reasons as reasons. Such accounts are incomplete not because they can be further elaborated—this is true of all explanations—but because they are unexplanatory. What we require is an indication why the scientist's reasons functioned as reasons, not just an enumeration of the reasons taken into account. Thus, since it is sometimes possible to explain why scientists found certain procedures reasonable by relating them to the prevailing social context, it is certainly possible that some reasonably held beliefs require social explanation.

¹⁷ Laudan, *op. cit.*, note 1 above, p. 188.

As for the other type of case, this is, if anything, even more straightforward. When scientists 'rationally' disagree, we cannot explain their disagreement simply by noting their respective reasons.¹⁸ This might suffice to explain why the scientists accepted the views that they did, but it does not explain why they adopted different views. As Alan Garfinkel has recently emphasized, explaining two contrasted items separately is not at all the same thing as explaining a contrast.¹⁹ In such cases, we are once again obliged to go beyond the scientists' reasons. In particular, it may turn out that the best, or even the only, way to explain a dispute between two scientists involves making reference to the fact that their social positions occasioned and circumscribed different bodies of background beliefs. Thus, as before, it may be appropriate to go beyond scientists' reasons to their social circumstances even though they were clearly behaving in a fully rational manner.

These difficulties for the principle that reasonable beliefs never require social explanation are, moreover, not merely of academic interest. Historians of science can sometimes be seen as attempting to clarify controversial reasoning by relating it to the scientist's social circumstances. For example, some historians have argued that we can better understand Darwin's acceptance of the theory of evolution if we see him as reasoning within a particular social context.²⁰ Granting that Darwin proceeded in a completely reasonable way, it is nevertheless argued that we can better understand his argument if we recognize the plausibility of Malthus's ideas to those living in nineteenth-century England and we appreciate the power that the metaphor of natural selection would have on someone so closely associated with commercial breeders. Furthermore, historians of science quite frequently invoke social considerations to account for apparently rational disagreement among scientists. Thus, it has been argued that we can better understand the debate concerning phrenology in early nineteenth-century Edinburgh if we trace the views of the parties involved to their respective positions in Edinburgh society.²¹ If we bear in mind that the phrenologists tended to be outsiders, while the antiphrenologists were mostly members of the Edinburgh elite, we shall—according to this line of thought—be able to clarify why the two groups disagreed for as long

18 For a more detailed discussion of the notion of rational disagreement see A. M. Lugg, 'Theory Choice and Resistance to Change', *Philosophy of Science*, 47, 1980, 227-43.

19 See A. Garfinkel, *Forms of Explanation*, New Haven 1981, p. 89, where it is argued that we cannot explain why boys become doctors and girls nurses by separately explaining how boys become doctors and girls nurses.

20 See, e.g., R. Young, 'Malthus and the Evolutionists: The Common Context of Biological and Social Theory', *Past and Present*, 43, 1969, 109-45, and R. Young, 'Darwin's Metaphor: Does Nature Select?', *The Monist*, 55, 1971, 442-503.

21 See S. Shapin, 'Phrenological Knowledge and the Social Structure of Early Nineteenth-Century Edinburgh', *Annals of Science*, 32, 1975, 219-43.

as they did. In a nutshell, the claim is that outsiders favoured the phrenological doctrine because they tended to be naive empiricists, whereas insiders rejected the doctrine because they were inclined to espouse a much more sophisticated form of empiricism.

Nor should it be thought that the type of social explanation being discussed here is tame and uninteresting. It is true that the main point I have been urging is simply that a scientist's beliefs may on occasion be clarified by considering their social preconditions as well as the reasons offered on their behalf. But the importance of this type of explanation should not be underestimated, it being essentially the kind of explanation repeatedly invoked by sociologists of knowledge. In particular, notice that in the case of Darwin the claim was that certain 'cultural resources' were made available by 'uneven social developments' in society at large. And the difference in the phrenologists' and anti-phrenologists' beliefs was seen to derive from the stratification of society. It may be true that the sociology of knowledge appears in many guises, but it seems to me that its major claim can be captured by observing that occasionally our concerns and interests require that we go beyond our subjects' belief to their social circumstances.

My main point, then, is that it is a mistake to think that a scientist's having reasons for a belief precludes the possibility of a social explanation. It may be appropriate to draw attention to developments in society or in the social system of science itself. Or it may be helpful to indicate how different scientists' social positions occasion different beliefs.²² But in general, there is no justification for assuming that explanations in terms of reasons are always preferable to explanations which make reference to underlying social realities.

V

These last observations can be further clarified by noting that explanation is an essentially pragmatic affair.²³ Since whenever we explain an event, we aim to satisfy specific interests, different explanations may be required given different sets of concerns. As many philosophers have recently emphasized, explanation should not be seen simply as a logical relation between something that is being explained and something that does the explaining. It also depends on a wide range of pragmatic considerations including the aims of the questioner and the background knowledge of the intended audience.

Thus, social explanations of reasonably held beliefs cannot be precluded prior to empirical research. However reasonable a scientist's beliefs may be, we may—as a result of our interests—have to go beyond

22 It is perhaps worth stressing here that disagreements can sometimes be explained by noting where scientists are located in the social system of science. For further discussion and examples, see Lugg, *op. cit.*, note 18 above.

23 See, e.g., Garfinkel, *op. cit.*, note 19 above, especially chapter 1.

the beliefs themselves and focus instead on their social preconditions. Since we cannot know in advance what pragmatic issues are involved, we cannot know whether any given reasonable belief can be 'rationally' explained. There is, then, a grain of truth in remarks concerning 'the insufficiency of "reason" in science'. But the point is generally misunderstood, it being a major shortcoming of most discussions of the explanation of scientific beliefs that they overlook the pragmatic considerations that determine whether an explanation is complete or in need of further development. Scientific explanations which make no mention of social factors are not necessarily incomplete. Whether they are complete or not is determined by the pragmatic considerations that prompt historical inquiry, not by the intrinsic character of science.

To a considerable extent we overlook the pragmatics of explanation because we tend to see the acceptance of scientific beliefs as being brought about by a chain of causes the penultimate link of which is either a reasoning process or an 'external' factor. We think that the acceptance of a belief must have been prompted either by the acceptance of reasons or by some other factor. But in fact beliefs generally arise as a result of the convergence of a host of intellectual, social, biological and other factors, and our concerns may require that we concentrate on non-intellectual rather than on intellectual factors. It is only by wrongly assuming that each belief has a single antecedent that we can ignore the problem of selecting one aspect of the causal network as 'the crucial cause'.

In response to this argument, it might be suggested that the social conditions of scientific inquiry are quite different from the causes of particular beliefs.²⁴ In particular, one might insist that the acceptance of scientific beliefs is prompted by deliberation and merely made possible by their social preconditions. But this suggestion by itself hardly establishes that social explanations are never required. For the very point at issue is whether scientists' reasons should always be selected as 'causes' of their beliefs. It is after all the central claim of proponents of social explanation that the social conditions which surround beliefs occasionally function as 'causes' rather than as 'mere conditions'. We beg the question if we argue that what actually causes reasoned belief is always the acceptance of reasons. Again the point to be made is that whether we take a scientist's reasons or the social preconditions of these reasons to be 'the cause' of the scientist's accepting a belief depends at least in part on the concerns and interests we have.

24 Compare Mary Hesse's suggestion that we distinguish between 'the historical occasions upon which scientific developments take place and the character of those developments themselves'. (Hesse, *op. cit.*, note 2 above, p. 138.) Hesse's proposal may be criticized on the grounds that it assumes the distinction between the abstract propositional content of beliefs and the historical circumstances in which they occur, which was rejected in section I as unhelpful.

-

In this regard, it is interesting to note that Laudan, who is outspoken in his support for the traditional view, also responds to the problem of selecting causes by invoking pragmatic considerations. He agrees that 'certain social factors may well be preconditions for rational belief', yet he insists that we can 'still legitimately exclude those social factors from an explanation of a certain belief, provided we can show that the most crucial and relevant antecedent to the acceptance of the belief was a well-founded reasoning process on the part of the believing agent'.²⁵ This view of the matter differs significantly from the spirit of Laudan's original claim, which made no reference to 'crucial and relevant antecedents', still less to the concerns and interests that determine what makes an antecedent 'crucial and relevant'. Of course, one may attempt to mitigate this conflict by adding—as Laudan does—that 'in those cases where agents have sound reasons for their beliefs, those reasons are the most appropriate items to invoke in an explanation of the beliefs which those reasons warrant'.²⁶ But this is simply to assume what needs to be shown, namely that reasons are in fact always the 'most appropriate items to invoke'. Is it not more plausible to think that our interests and concerns might from time to time require that we stress the social conditions surrounding a belief rather than the reasons presented on its behalf?

In summary, the principle propounded by Merton, Laudan and many other historians, sociologists and philosophers of science is doubly mistaken: ill-founded beliefs do not always require social explanation, and well-founded beliefs may sometimes be explained in sociological terms.

POSTSCRIPT ON THE DIRECTION AND RATE OF GROWTH OF SCIENCE

Just as it is often argued that well-founded beliefs should be explained in a different way from ill-founded ones, it is often maintained that the character of content of science should be accounted for in an entirely different way from its rate and direction of growth. The prevalent view is that the character of science should always be explained 'rationally', while the kind of science which is undertaken and the rate at which scientific research progresses should always be explained in social terms.²⁷ But this attempt to specify different aspects of science as being rationally and social explicable is no more plausible than the attempt to demarcate rationally explicable from socially explicable belief.

There are a number of points that need be made immediately. First, it is true but uninteresting that social factors never cause the abstract

25 Laudan, *op. cit.*, note 1 above, p.210.

26 *Ibid.*

27 Compare Bloor, *op. cit.*, note 5 above, p. 1: 'Sociologists have been only too eager to limit their concern with science to its institutional framework and external factors relating to its rate of growth and direction'.

propositional content of science. As already noted, what has to be explained is a fully causal process, namely scientists' accepting beliefs. Second, it is uncontroversial that the character of science, understood as 'the body of knowledge accepted at a certain time', varies with developments in society. Within limits, we shall learn more about a subject the more resources we invest in its study. Third, there is little to recommend the contention of some sociologists of science that social factors influence the rate of development of science but not its direction of growth.²⁸ Since technical developments in one area of inquiry can bring about changes in the research undertaken in a second area, the factors which influence how rapidly one area grows may indirectly influence the direction of growth of other areas.

These points notwithstanding, it might be argued that we should explain all aspects of science apart from its character by invoking social (or political or economic) considerations. One might insist that social factors directly determine the rate and direction of the growth of science, whereas they only indirectly influence its character. But even this conception is problematic. Social factors do not by themselves bring about scientific advances. As in the case of the acceptance of beliefs, how fast and in what direction science develops depends on the reasons that scientists devise, and we should see social factors as 'preconditions'. In other words, we can, if we choose, relegate all social factors to the status of 'mere conditions', in which case the rate and direction of growth of science is no less immune to social determination than its character of content.

More importantly, as soon as we view social factors as preconditions, the observations outlined in the last section apply with full force. We cannot hold that explanations of the rate and direction of growth of science must always be in terms of either intellectual or social factors, since the type of consideration involved will in general depend on the concerns and interests we have. In some cases it will be appropriate to concentrate on scientists' reasons, while in other cases we shall have to go beyond the reasons to the surrounding social circumstances. Given the particular concerns we have, it may be necessary to relate the ideas that prompted an area of scientific research to accelerate, retard or change direction to the social circumstances in which the scientists were pursuing their investigations. But it may also happen that we can explain the development of a research area by concentrating on the problems studied and the kinds of solutions entertained by the scientists involved in the area. Keeping the pragmatic aspects of explanation in mind, we shall not expect explanations of the rate and direction of growth of

28 Compare J. Ben-David, *The Scientist's Role in Society*, Englewood Cliffs 1971, p. 12: 'Although societies can accelerate or decelerate growth by lending or denying support to science or certain parts of it, they can do relatively little to direct its course'.

-

science to be either invariably in terms of reasons or invariably in terms of social and similar factors. The alleged boundaries do not exist.