

Temperate Rationalism: An Option for the Methodology and Understanding of Scientific Enterprise

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Abstract

One of the fundamental difficulties that have bugged the minds of contemporary philosophers of science is the extent to which Popper and the Popperians' rational model can be adopted for explanatory purpose in science. Examining closely the controversy between the rationalists and the adherents of the so-called "strong programme in the sociology of scientific knowledge" – who attack the whole notion of "rational" model. This paper argues that both models represent extreme perspectives which, taken on their own, cannot produce a satisfactory notion of scientific enterprise, especially when the notions of goal, change, progress and truth are subsumed in the understanding of scientific methodology. It, therefore, proposes "temperate rationalism" as an option for scientific rationality on the grounds of relevance of its general picture of progress both in theories and in methodology.

1. Introduction

The controversy between the proponents of rationalism namely, Popper and the Popperians (Lakatos and Laudan) and the proponents of cognitive sociology, namely Kuhn, Feyerabend, Bloor and Barnes over the notion of scientific enterprise, rests ultimately on the rationalists' inability not only to sustain their perspective on scientific enterprise, but also to articulate their claim of providing the model that provides a viable account of scientific change or what makes one theory better than another. As our discussion does not commit us to present the details of any particular rationalist or sociologist's model, we will be contented with the exploration of the central and contentious theoretical issues that are inherent in their programmes for scientific enterprise.

For this purpose, the first two sections of this work is devoted to the analysis of the rational model of Popper and the Popperians and the challenges of the so-called "strong programme in the sociology of scientific knowledge" respectively. Analysis such as this is expected to highlight the problems, if any, of either accepting or rejecting, the one rather than the other, or even of envisaging the possibility of treating both programmes as complementary rather than as directly opposing models for explanatory purpose.

This opens to us, the way to the analysis of other entailments in accounting for scientific change, for example, in explaining an individual scientist's or community of scientists' action or belief in abandoning one research programme for another. Here we are indebted to Newton Smith's notion of "minirat" accounts of scientific change (1981: 243-246), which is the concern of section three. The argument here is that although it may be adequate to appeal to "minirat" accounts for actions and beliefs that may determine mere change in science, there is something more than just scientific change which will require not only our consideration of the rationalists' "normative" account or approach, but also the consideration of the external – sociological and/or psychological – factors that perhaps may be explained contextually. The presupposition here is that interest both in normative accounts and in external (social) factors may explain changes of scientific actions and beliefs.

The last section before our conclusion explores the possibility of a more viable account or strategy that avoids the errors of either limiting the notion of rationality to the rationalists' normative perspective which, in effect, reduces science to a static rather than a dynamic knowledge-yielding enterprise or of assigning any specific domain to sociology and psychology for explanatory purpose. And, this strategy, the paper contends is possible by an appeal to "temperate rationalism", which not only allows for an evolution in scientific methodology, but also views scientific enterprise as progressively capturing more truths about the physical world along with improved conception of scientific methodology in the history of science.

2. The Analysis of the Rational Model of Scientific Change: A Normative Perspective

The rational model advanced by Popper and the Popperians embodies a normative account. It is normative in the sense that it specifies the rationale or internal factors that ought to govern theory choice. Thus while the individual rationalist offers considerably different accounts; they are united by a belief in the importance of articulating how one ought to decide which of a number of rival theories is most likely to be the best, relative to a given body of evidence. Their assumption first is that such an account will assist us in making progress in science. And, second, that their model is intended not only to guide us through the methodological roles in making decisions about which theory to adopt, but also in explaining the particular changes of allegiance that have occurred within the history of science. Thus in providing such an explanation:

... the rationalist appeals to his model, which specifies both the goal of the scientific enterprise and the principles of theory of comparison. Thus by this model, a transition from a theory T_1 to a theory T_2 is accepted as explained by showing that relative to the evidence at the time, T_2 was a better theory (Newton-Smith, 1981:237).

In other words, a theory transition especially in the case of mature science, such as physics, presupposes marked progress in science. However, rationalist concedes that there may be occasions when a sociological or psychological explanation of the change is appropriate. These include, in particular, occasions when there are deviations from the rules that are implicit in rational model.

This ultimately implies occasions when irrationality flourished; i.e. occasions when the change, in the rationalist's judgement, was not progressive because it was not rationally justified and, therefore, unscientific. In other words the rationalist allows that external factors (sociological or psychological) should be appealed to when, and only when, there are deviations from the norms implicit in the rational model. Thus:

... Whenever a thinker does what it is rational to do we need inquire no further into the causes of his action; whereas when he does what is in fact irrational even if he believes it to be rational we require some further explanation (Laudan, 1977: 188-189).

Understood from this perspective the rationalist programme involves the differential assessment of the belief that informs scientific change in so far as the rationalist demarcates or specifies a domain for the sociologist of scientific knowledge in terms of handing-over to the sociologist the explanation of the changes which he, the rationalist, passes as unjustified and, therefore, unscientific.

This, however, raises a number of fundamental puzzling issues concerning the extent to which this model can be accepted for explanatory purpose and whether the rational model expresses all that it takes to explain things the rational way; or whether it is possible to provide a better explanatory model of scientific change that would exclude a normative assessment of the internal factors required in such an explanation. Reflections on issues such as these would help us to properly assess the rationality of the rational model of explaining science. Thus we can see them all as subsumed under our primary task of exploring the extent to which a rational model can be used for explanatory purpose vis-à-vis the rationalist claim – that his model provides a viable account of what makes one theory better than another. In order to address these issues adequately it is necessary to examine some of the challenges posed against the rational model by the proponents of the so-called “strong programme in the sociology of knowledge”.

3. Anti-Rationalists' (The Cognitive Sociologist's and Psychologist's) Attack on Rational Account of Scientific Enterprise

The most significant of the rationalist's claims which we have pointed out is that of possessing the scientific method of generating a logic of justification; that is, of providing a rational technique or procedure for the objective appraisal of scientific theories, as well as the pursuit of the worthy aim of science – truth, knowledge and explanation. This image explains Popper's and the Popperians' claim to a methodological rule that could be employed not only for the discovery of scientific theories, but also for the explanation of scientific change and progress. This projected image became so popular on account of the great successes of science, especially modern physics. The argument is that such successes would not have been recorded in science, particularly in physics, if there wasn't some privileged method and a community that dispassionately applied the method and thereby sustained the superior achievements of science in history.

Understood from this perspective, the study of scientific methods is assumed to cover two apparently different activities, namely an attempt to discover rules or techniques to be adopted in the discovery of theories, on the one hand, and an attempt to uncover objectively justifiable principles for the evaluation of rival theories in the light of available evidence. Of the two activities, the former is generally considered to be suspect. On this note:

... most philosophers of science hold that while the study of justification is a legitimate and important enterprise there is no systematic useful study of theory construction or discovery. Such a study properly belongs to the lawless domain of intuition, inspiration and luck or unlucky hunches and guesswork (Newton-Smith, 1981: 125-126).

Following this line of thinking, the image of science represented by the rational model, in spite of its popularity, has come under severe attack from various historians, cognitive psychologists and sociologists as well as philosophers of science. Accordingly, Feyerabend (1978) presents what seems to be the liveliest and entertaining critique of scientific “method” interpreted as “logic of justification”, which may be summarized in his dictum:

... Free the society from the strangling hold of an ideologically petrified science, just as our ancestors freed us from the strangling hold of the one True Religion (Feyerabend, 1978: 128).

The above dictum clarifies his position against the vulnerable traditions of searching for a system of methodological rules which it was held, ought to guide scientists in the business of theory choice. According to Feyerabend, therefore, no such rules could be found; and to adopt any particular (methodological) rule would only result in impeding scientific progress. His explanation is that if anyone wants to have exception-less rules that can be applied, come what may, then such rules would be so vacuous and so indefinite that nothing is ruled out by them. In other words, for him, there are no rules that have any real content or force that can be abstracted from scientific procedure or method. Indeed he had, in another work noted plausibly that:

... the idea of method that contains firm, unchanging and absolutely binding principles for conducting the business of science meets considerable difficulty when confronted with the results of historical research. We find then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other. It becomes evident that such violations are not accidental events, they are not results of insufficient knowledge or inattention which might have been avoided (Feyerabend, 1975:23).

In addition, Feyerabend, Kuhn and other sociologists of knowledge tend to believe that not only does the idea of scientific enterprise articulated by the rational model fail to live up to the image projected by the scientific community but it also fails to do so because it embodies untenable assumptions concerning the objectivity of truth, the role of observational evidence and the invariance of theory of meanings in cases of shifts of allegiance by the community. Hence, the rational image is not even capable of serving as an ideal which the practice of science ought to aspire for realization.

Moreover, Feyerabend views the pretensions of the scientific community as representing a distorting ideology which is propounded in order to serve its interest. Kuhn (1970) has also argued that the history of science, too, does not support the image articulated for science by the community in so far as the “the history of science itself is a tale of multifarious shifting of allegiance from theory to theory” (77). In such cases much of scientific activity consists in accounting for change in terms of “paradigm shifts” (Kuhn, 1970: 77-79) rather than in the accumulation of theories for selection as the rationalist believe.

This shifting of allegiance, although may be seen as a type of change that requires no single methodological rules, it indeed requires further explanation which eventually is to appear in radically divergent types of explanatory sketches, which in turn will indicate a different perspective over the objectivity of truth, the possibility of rational discourse, and, above all, different perspectives over the nature of values, language, meaning and explanation among others. In other words, by the community’s projected image of science, one is not justified in regarding scientific enterprise as the very paradigm of rationality since, by this image, there is no possibility of justifying the claim that scientific change is rationally explicable; i.e. by appealing to the internal or logical content of theories.

The point the anti-rationalists, particularly Feyerabend, are raising against Popper and the Popperians, here is that any attempt to appraise theories objectively merely by considering their internal or logical content or verisimilitude –

that is in terms of providing a normative assessment – must be rejected especially on ground of Feyerabend’s incommensurability thesis according to which theories are incomparable due to the problem of radical meaning variance (RMV) – whereby the meaning of a theoretical term also changes as the theory itself changes – and “on pains of a circle or an infinite regress” (Feyerabend, 1978: 67-68; Hempel, 1965: 183; Newton-Smith, 1981: 150-151 Kordig, 1971:2).

The consequence is that there cannot be any rationally justified reason for thinking that any theory is better than another. This, of course, is a logical consequence, too, of Feyerabend’s denial of the rationalists’ vulnerable tradition, of searching for a system of rules, which is held, ought to guide scientists in the business of theory choice. There is no such system of rules for the comparison of theories in science. The explanation is that “the application of such a rule would produce an equally plausible counter-rule, the use of which would be counter-productive” (Feyerabend, 1975: 29).

However, Feyerabend’s view does not commit us to deny the possibility of there being instantiations when a change of certain methodological rules or principles may lead to positive results that equally indicate progress. But neither agreeing that methodology can change can constitute a threat to the rationalists’ perspective, nor need any rationalist maintain that his methodological rules are absolutely binding and exception-less. The logic, on the one hand, is that the rules referred to are inductive rules – helping us to determine which of a pair of rival empirical theories is better to adopt in the face of available evidence – in so far as the expected logical gap between the evidence of a theory and its truth or approximate truth can be bridged only by an inductive inference. On the other hand, no matter how successful an empirical rule is, in general, it may in some particular contexts lead us to choose what turns out to be inferior. For instance:

... one might seek to show the unsatisfactory character of a particular rule by establishing that it has led use wrong more often than right (Newton-Smith, 1981: 129).

But this notwithstanding, it could be argued that to have had evidence of a number of occasions in which a rule has led us astray is not necessarily to have adequate reason for doubting the acceptability of the rule. It may be, these exceptions notwithstanding, that our chance of making progress in the long run is greater if we employ that rule.

Newton-Smith (1981) identified two major reasons why the rational model faces these challenges. The first is the rationalists’ “inability to provide a link between their methodology and their projected goal for science” (268), which accounts for their inability to provide any sustainable explanation for thinking that following their methodology is a means likely to lead us towards the goals they posited for science – prediction, explanation and increased verisimilitude.

The second reason pointed out by Newton-Smith (1981) is the rationalists’ use of an inappropriate strategy of arguing from methodology to raising the question of progress in science. He suggests that a more viable and promisingly operative strategy for accounting for progress in science is arguing from the evidence of progress in science to the viability of a methodology. His explanation is that:

...in choosing between theories, the scientists do not act capriciously. Rather they deliberate and in a dialectical process of discussion, provide reasons for their choice. Thus, given that there has been progress in science, we have the reason to belief that the procedures the scientist followed are by and large evidential Newton-Smith (1981:268-269).

This implies that if the anti-rationalists (Feyerabend, Kuhn and others) who deny the existence of scientific method acknowledge the fruits of science, then it would be reasonable for them to equally acknowledge that those special fruits (whatever) clearly indicate that there must be something special about method. But only a deeper study and analysis of the so called “strong programme of the sociologist of knowledge” can give us a deeper insight into the controversy.

4. Analysis of the Anti-Rationalists’ Perspective of Scientific Enterprise.

The central idea of the rational model, from our analysis so far is that all aspects of scientific change and progress are rationally explicable. In this sense, rationality amounts to merely providing a normative assessment of a given transition in science. In doing so the rationalist tends to define a province for the sociologist in terms of limiting them to the treatment only of irrational behavior. This is so because the rationalist sees sociology as concerned with social

causation – giving causal or inductive explanations of actions and changes in beliefs by reference to social structure which Popper refutes as irrational. But our contention is that in the explanation of the evolution of science, there are no special classes of features that really ought to be given over to sociological treatment.

In contrast, the sociologists of scientific knowledge, who attack the very notion of rational model of current science, hold as a crucial tenet, that in explaining why someone (including a scientist) or some group (including the scientific community) holds a particular belief or why a belief transition took place is not relevant to consider; whether the belief in question is true or false, has high or low truth-content, reasonable or unreasonable. Rather, Barnes (1974) and Bloor (1976), two major proponents of the Strong Programme in the Sociology of Knowledge believe that scientific explanation involve four basic tenets, namely: First, that such an explanation should be causal in structure. That means it should be concerned with stating the initial conditions that will bring about a belief or state of knowledge. In this case “there naturally will be other types of causes other than social causes co-operating in bringing about the belief in question” (Bloor, 1976: 4-5).

Second, that such an explanation should be impartial with respect to truth and falsity, rationality or irrationality, success or failure” (Bloor, 1976: 4-5). In other words, both sides of these dichotomies will require explanation.

Thirdly, Bloor and Barnes agree that such explanation should be symmetrically couched. That means, the same types of cause should explain, say, rational and irrational belief, true and false belief, and so on. In this sense, Bloor’s and Barnes’ symmetrical thesis essentially implies a rejection of any differential treatment of beliefs and belief transitions, which Barnes regards as an “additional habit” (1974:25).

Finally, that such an explanation should be reflexive. In other words this pattern of explanation should be “applicable to sociology itself, otherwise sociology would be a standing refutation of its own theories” (Bloor, 1976:5).

But there seem to be some difficulties in accepting the sociologist’s programme as the methodological principle for the evaluation of beliefs. The argument is this, that if differential treatment of beliefs is regarded as illegitimate, and hence the concepts of truth and rationality are jettisoned from the explanation of belief or belief transition, then the sociology of knowledge deprives itself of a subject matter. Moreover, it would be difficult to determine what a person’s beliefs are independently of assessing to some extent, the truth or falsity of the beliefs in question. Again, if we try to avoid differential treatment of beliefs, then a residual difficulty of having access into the belief system of others will be created. Thus, it is noted that:

...unless such assessment is allowed as legitimate in the determination of beliefs, the sociology of knowledge, which purports to be a scientific activity, has no subject matter. The practitioners of this programme not surprisingly do not live up to their own methodological assumption (Newton-Smith, 1981:250).

In other words, the legitimacy of scientific enterprise, including scientific sociology of knowledge, is dependent on the legitimacy of the concepts of “truth” and “rationality”, and so on, in explaining belief, belief transitions and actions. Therefore, for there to be a scientific sociology of knowledge, its practitioners ought to learn to live with this fact.

Thus following this account, Newton-Smith avers that actions can be displayed as rational by developing a correct “minirat” account (1989:257-258), according to which an individual scientist’s action in abandoning one research programme or theory for another can be accounted for by showing that, that action was most likely to be the best means to achieving his goal, given his beliefs, without evaluating the reasonableness or unreasonableness, truth or falsity of either his goal or belief. Thus a minirat account of action fits Bloor’s and Barnes’ conditions of impartiality and symmetry in so far as our judgment about the desirability of the scientist’s goal or reasonableness of the belief he holds is irrelevant to the acceptability of the explanation. What matters is his general belief to act on true beliefs rather than to act on false beliefs for the purpose of realizing his goals.

On the other hand, Newton-Smith argues that a minirat account of why someone or some group (scientist or community) holds a particular belief works only by showing that in the context, the belief was reasonable, in that he (the scientist) or it (the community) had better reasons for believing it than believing in its negation or for suspending the belief. Thus if the belief he holds is incompatible with the dictates of reason in the context, he should look for a further explanation, which “may be given in terms of the distorting effect on his judgment of special interest” (Newton-Smith, 1981: 257).

This means that, in a sense, Boor's symmetry thesis must fail. This must be so, not because a reasonable belief is somehow self-explanatory, but because we have a special interest in following the dictates of reason. That interest explains why we act in a certain way. It follows therefore that:

In not following the dictates of reason we are not following the interest and hence we require an account specifying the interest that is affecting our judgment. In explaining why someone holds a general belief by giving an account of his reasons for that belief we have to decide whether the belief was reasonable for him to hold in the context. We do not have to decide whether we, in our context, find the belief to be unreasonable (Newton-Smith, 1981: 258).

This implies that if this second interpretation of what Newton-Smith calls "minirat" account is intelligible, then the rationalists' strong interest in deciding whether the beliefs of past scientists in methodological principles were reasonable or unreasonable must be suspect. Similarly their normative model or methodology along with the rules of comparison of theories, adopted by them as correct, must also be rejected as scientifically rational.

Thus Newton-Smith suggests that in order to give a scientifically rational explanation for a belief held by a particular scientist, one is required to show that the believer (the scientist concerned) in coming to hold the belief in question, was following the dictates of reason and that his reasoning was scientifically respectable relative to the state of scientific methodology at that time. What Newton-Smith implies here is that a belief which fails to be scientific or rational in the Popperian (rationalist) sense, may nonetheless be rational in the sense that the believer has been following the dictates of reason. And in displaying that this is so, explains why the belief was held.

What is implied here, in turn, is that we do not have something requiring sociological or psychological treatment; i.e. something to be assigned specifically to the domain of sociological assessment or explanation just because it is not "scientifically" rational; i.e. "not rational" in the sense of "not being scientific" in Popper's and Popperian interpretation. Such assessment arises only when the believer in question has been operating contrary to the dictates of reason. And simply because:

...we recognize a general standing interest in following the dictates of reasoning for our survival, we want to know what other interests have competed successfully with this interest when someone has acted contrary to the dictates of reason (Newton-Smith, 1981: 256-257).

In other words, we should be interested in searching for the socio-economic factors that might have contributed in determining the interest people have in holding or changing from a particular belief or acting in a particular way. We should do so in cases of all actions of scientists, be they scientifically rational or not.

Thus while the symmetry thesis fails, strictly speaking, Bloor and Barnes are correct to seek in all cases, further, deeper explanations of actions by searching for the factors which determine goals – rational or irrational, scientific or non-scientific. Our conjecture here is that, it is likely that such explanations will characteristically be causal and will depend, most likely, on biological and socio-economic factors. And such explanations will still be accepted as rational explanation though not in the rationalist's sense of rationality. This presupposes that there is no single privileged method – "rational" or "non-rational" – to guide the choice of theory and of explanation of changes.

Thus an adequate explanation of belief or belief transition/change, be it scientific or unscientific, will involve differential assessment of beliefs, most especially, as the history of science involves not merely changes but also progress, the latter of which the rationalists wish to account for. And an adequate account of progress, contrary to Bloor's and Barnes' advocacy of the symmetry thesis, will require a differential assessment in so far as the pursuit of truth is a fundamental concern of scientific methodology.

In other words an adequate account of progress in science is not based on the methodological belief held by a scientist or community of scientists but on the truth-content of the held methodological belief. The explanation is that scientists are more interested in capturing more truth using their methodological beliefs. Thus:

The Einsteinians captured more truth than the Newtonians, who had more truth than their predecessors. The only possible explanation of this fact is that they both held methodological beliefs of some truth – content and made their decisions on the basis of

these beliefs and not because of sociological or psychological factors (Newton-Smith, 1981: 260).

This means that although we can explain why a person changed his mind by appealing to his methodological belief, we cannot do so when we want to explain why there was progress. That is, it would be inappropriate to hold that there was progress because he held certain methodological belief. Those beliefs will explain progress only if it can be shown that they were true or contained some truth.

But from the actual practice of science, it seems that the more we can minimize the goal of science, the easier it is to vindicate the claim that there has been progress in science, especially when it is viewed as a dynamic knowledge-yielding enterprise. And really, it is in viewing science as a dynamic enterprise that we can relate its explanatory and predictive roles with its “truth-related” goal – “Truth-related” in the sense that, if science is rational, then its goal cannot be truth “per-se” on account of pessimistic induction which gives us good reason to think that we shall never hit upon powerful theories that are, strictly speaking, true.

In order to meet this problem, there is need, perhaps, for us to recognize with Popper (1989:231, 234) and Lakatos (1978:191) that scientist’s aim at “truth or theories of ever-increasing verisimilitude”, contrary to Laudan’s (1977) attempt to make the goal of science that of “increasing problem-solving capacity” (125-127). The argument is that it is not wrong to adopt the hypothesis that there has been progress towards greater verisimilitude in science in so far as that hypothesis provides the best explanation of the increase in the predictive and explanatory powers of science; and especially as the notion of verisimilitude has a legitimate place in a theory of science.

This implies that the prospects are dim for a successful account of scientific enterprise which does not make “truth” or “ever-increasing verisimilitude” its goal. But if we accept this position, then the rationalist is challenged to prove the truth of this hypothesis. But Popper’s and Lakatos’s inability to meet this challenge on account of their skepticism about induction and consequently their inability to forge a link between their methodologies and the goal they posit for science, provide us no reason to think that following the method they try to articulate, is a means likely to lead us towards their projected goal for science.

In order to meet this challenge this paper concedes that contrary to the rational approach of arguing from methodology to the fact of progress, a more viable procedure is to argue from the fact of scientific progress to the viability of a methodology. The explanation is that from the undeniable evidence of scientific progress we can conclude that the methodology followed was by and large evidential and viable. Implicitly, this means that, in general, at least, the considerations that motivate scientists in selecting theories are fallible indicators of verisimilitude.

Thus the vindication of a methodology rests in first demonstrating that it has been operative in bringing about progress. And to say that there is progress in science implies accepting that there is something special about scientific method. But to say so we are not committed, like the rationalist, to accept that there is some verbally specifiable exhaustive set of binding algorithmic rules, the application of which is bound to bring about success.

Indeed scientific method cannot be exhaustively specified in some articulated system of rules, at least, in so far as there are cases in which rules conflict. And indeed, it is Popper’s and the Popperians’ attempt to provide methodological rules for the determination of change and progress that represents the methodology of science as static. The explanation is simply that such an attempt fails to recognize the fact that methodology, like science itself, as well as its goals evolve. But the evidence of changes in methodologies and evolution of goals in science presuppose that a rational model of science ought to be dynamic.

Thus instead of a single model we require a sequence of models, each of which represents the principle of comparison that operates during a period of time, to the extent that, if a model changes, then we will need a dynamic model that represents such process. Temperate rationalism represents this process by offering a dynamic theory of science.

From our discussion and analysis so far one can immediately recognize the extreme polarities of the views of both the rationalists and the proponents of the so-called strong programme of the sociology of knowledge as the former regard the latter as unscientific and therefore irrational, while the latter reject the former’s conception of rational model and the non-existence of anything like method, in so far as they represent extreme views that amount to serious misconceptions of scientific enterprise. What then are the promises of temperate rationalism?

5. Temperate Rationalism as an Option for a theory of Science

Our introduction of a dynamical factor into the description of our theory of science is not the only thing that makes a difference between temperate rationalism and the radical rationalism of Popper and the Popperians. Unlike the rational model, temperate rationalism recognizes the important status of judgment which lies at the heart of the account of scientific method, in so far as it recognizes the possibility of disagreements among scientists in matters of judgment. This omission makes the rationalist's explanatory model of scientific change seriously deficient. The point is that, it is inadequate for the rationalist to show merely that a certain change or transition fits their own model, in the sense that relative to their normative model, the best theory triumphed. They need, in addition, to show that their model encapsulates the goals and methodology of those concerned in the transition.

Moreover, given that there is evolution in the method as well as in the goal of science, we cannot necessarily use the normative account of the facts that currently govern scientific practice in explaining past transitions. Such an approach would reduce science to a static, rather than a dynamic enterprise. Thus we need to investigate, for each historical epoch, the conception of the good-making features of theories which were then operative. In this sense "temperate rationalism concerns itself with the evolving series of models in giving rational accounts of scientific activity" (Newton-Smith, 1981: 271).

Consequently, unlike the rational model of Popper and the Popperians, temperate rationalism does not hand over, for sociological explanation any past episode in the history of science just because it fails to conform to the evaluation they make on the basis of their current conception of methodology.

In order words, it does not demarcate, "a priori", any particular province for the sociologist of science. Rather, if a temperate rationalist finds that an individual scientist or community is following the dictates of reason in formulating his or its beliefs he seeks an explanation in terms of interest as to why that scientist or community followed the dictates of reason. And this may be accounted for simply in terms of the fact that we have a general interest in following the dictates of reason because it is an evolutionarily successful strategy. But, equally, he will be interested to seek for a sociological explanation for why it is that there is an individual or community that chooses the goal of science as his or its goal.

Thus a temperate rationalist does not turn over an episode for external explanation by the cognitive sociologist of science just because it is not "rational" as suggested by Popper's conception of scientific methodology. Whether an episode is properly described as rational or not depends on the conception of scientific methodology operative at the time. And if an episode is rational in a sense, then sociological explanation is not ruled out. The explanation is that we require an account in terms of external facts as to why a particular conception of scientific methodology should have arisen, and why individual scientist or groups had interest in acting under that conception.

6. Conclusion

This work has drawn our interest to the rationalists' (Popper, Lakatos and Laudan) conception of scientific enterprise, and the challenges from their major opponents, the proponents of the so-called "Strong Programme in the Sociology of Knowledge". They have been keen to appraise the activities of past scientists by reference to their own rational model which this work describes as "normative" and "static". Our analysis and discussion reveal that the rationalists' judgments of past scientists on the basis of our current perspective of scientific methodology are irrelevant to the explanation of the past activities of scientists and to the question of whether such activities were scientifically rational or not.

However, such judgments have a functional status only if the scientists were interested only in the question of "progress" as opposed to mere "change". We should therefore expect to find that the conception of scientific methodology which operated then had some truth in it. Consequently, it would be of interest to appraise it normatively in terms of our current conception of how to do science, their conceptions. But if we were to find that, relative to our current conception of how to do science, their conception was totally wrong-headed, then we should be faced with a major puzzle. The explanation is that unless we are able to explain why they made progress while operating under what we regard as a mistaken conception of scientific method, we ought to re-examine our description of the current state of scientific methodology.

The temperate rationalists while rejecting both the "rational model" and the "Strong Programme in the Sociology of Knowledge", as two extreme and misleading views recognizes the role of evolution in scientific methodology. They see science as gradually capturing more truth about the world process accompanied by an enriched and improved

conception of scientific methodology as a dynamic activity. Consequently, they expect a normative appraisal of past conceptions of scientific methodology; but a normative appraisal is of interest to them not because it is part of the explanation of scientific change, but because of its relevance to his general conception of progress both in theories and methodology. If this defense of “temperate rationalism” as a more viable alternative model to both the rational model and the Strong Programme in the Sociology of Knowledge is successful, then a viable perspective has emerged.

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